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Heroes, Villains, or Something In Between? How "Right to Work" Policies Affect Teachers, Students, and Education Policymaking

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Although the *Janus v. AFCSME* (2018) decision fundamentally changed the institutional context for U.S. teachers' unions by placing all public school teachers in a "Right to Work" (RTW) framework, little research exists to conceptualize the effects of such policies that hinder unionization. To fill this gap, I exploit the different timing across states in the passage of RTW policies in a differences-in-differences framework to identify how exposure to a RTW policy affects students, teachers, and education policymaking. I find that RTW policies lead to declines in teachers' union power, but contrary to what many union critics have argued, I find that efforts to weaken unions did not result in political opportunities for education reforms nor did they improve student achievement outcomes.

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Abstract
Although the *Janus v. AFCSME* (2018) decision fundamentally changed the institutional context for U.S. teachers' unions by placing all public school teachers in a "Right to Work" (RTW) framework, little research exists to conceptualize the effects of such policies that hinder unionization. To fill this gap, I exploit the different timing across states in the passage of RTW policies in a differences-in-differences framework to identify how exposure to a RTW policy affects students, teachers, and education policymaking. I find that RTW policies lead to declines in teachers' union power, but contrary to what many union critics have argued, I find that efforts to weaken unions did not result in political opportunities for education reforms nor did they improve student achievement outcomes.

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Introduction

The *Janus v. AFCSME* (2018) decision has fundamentally changed the institutional context for U.S. teachers' unions by ruling that public sector unions cannot collect "agency fees" or "fair share fees" from non-members to compensate the union for the collective bargaining done on their behalf. In effect, this means that all public sector workers, including teachers, now must operate in a "Right to Work" (RTW) framework. This RTW legal framework and other policies hindering unionization are not new to teachers' unions. Figure 1 traces the number of the three major types of restrictive labor policies for teachers' unions over time: policies prohibiting agency fees, collective bargaining, and strikes.[1]

As is shown, over the past three decades states have experienced a resurgence in RTW policies for teachers' unions. Most states prohibiting agency fees for teachers' unions enacted those policies around the timing of the Taft-Harley Act of 1947, and then only two states prohibited agency fees between 1965 and 1990. However, beginning with policy changes for teachers' unions in Texas and Indiana in 1993 and 1995 respectively, the frequency of RTW policies has increased substantially. Eight states have adopted new prohibitions on agency fees for teachers' unions in the years from 1990 until the writing of this article. The increase has been especially prominent after the passage of Act 10 in Wisconsin in 2011. The *Janus v. AFCSME* (2018) decision then nationalized this trend and fundamentally changed labor policy in states without RTW laws by declaring agency fees in the public sector unconstitutional.

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[1] Data on the timing of these laws between the years of 1955-1996 come from the NBER Collective Bargaining Law Data Set including the Kim Reuben extension (Valletta and Freeman 1988). I have further extended the dataset to span the entire timeframe of restrictive labor laws using policies documented by the U.S. Department of Labor (1972, 1979, 1981), Sanes and Schmitt (2014), and Feigenbaum, Hertel-Fernandez, and Williamson (2018). Where Sanes and Schmitt (2014) did not match with the NBER Collective Bargaining Dataset, I referred to the U.S. U.S. Department of Labor (1972, 1979, 1981) State Profiles of Public Sector Labor Relations. If a discrepancy still existed, I referred to the referenced statutes themselves. It is important to note that these policies target teachers' unions but not necessarily other private *or public sector* unions. See Freeman and Han (2012) for further discussion of teachers' unions as specific targets of these policies.

Despite the conceivably dramatic changes to the American labor movement, little is known about the consequences of policies hindering teacher unionization (Freeman and Han 2012; Eren and Ozbeklik 2016; Han 2016; Baron 2018; Hertel-Fernandez 2018; Marianno and Strunk 2018a; Han and Maloney 2019). Much of the empirical literature on the effects of teachers' unions has focused on policies *facilitating* teacher unionization, specifically collective bargaining laws (e.g., Hoxby 1996; Lovenheim 2009; Anzia and Moe 2015; Cowen and Strunk 2015; Frandsen 2016; Lovenheim and Willén 2019; Paglayan 2019). Within this body of literature, two theories have dominated: *rent seeking* and *teacher voice*,[2] which largely mirror the "monopoly face" and "collective voice/institutional response face" of unionism that Freeman and Medoff (1984) examined in their seminal text, *What Do Unions Do?*. As explained in the section that follows, these theories are complex and overlapping; however, they are often pitted against each other in a simplified, normative question about whether teachers' unions are the heroes or the villains of the American education system (Hannaway and Rotherham 2006; La'Tara et al. 2017).

Although much of this work agrees that teachers' unions increase the cost of government, there are mixed findings in terms of what this means for students. Most studies of laws facilitating teacher unionization find negative or no effects on student outcomes, thus aligning with the rent seeking theory; however, this literature remains relatively thin with only a few attempts at identifying causal impacts (see Cowen and Strunk 2015 for a review of the literature). Some very recent work has also challenged the dominance of the somewhat black and white rent seeking view that puts teachers' interests at odds with student interests (Brunner, Hyman, and Ju 2019). In contrast to the prior conclusions that teachers' unions are harmful for students (Hoxby 1996) and that expenditures do not affect achievement (Hanushek 1989), the literature may be moving toward a new consensus about the effects of teachers' unions, particularly in relation to the costs of schooling (Frandsen

[2] Also sometimes referred to as *efficiency-enhancing* (Hoxby 1996).

2016; Jackson, Johnson, and Persico 2016; Matsudaira and Patterson 2017; Baron 2018; Lafortune, Rothstein, and Schanzenbach 2018; Brunner, Hyman, and Ju 2019; Han and Maloney 2019; Han and Keefe 2020).

Only a few studies have considered the effects of laws hindering unionization, and this nascent body of literature has tended to focused on policy implementation within one or two states and on teacher- or union-centric outcomes (Eren and Ozbeklik 2016; Han 2016; Baron 2018; Hertel-Fernandez 2018; Marianno and Strunk 2018a; Han and Maloney 2019). Current predictions about the rent seeking or teacher voice effects of policies restricting unionization, therefore, rely heavily on research about policies facilitating unionization. If there is an asymmetry in the effects of laws *facilitating* unionization and those *hindering* unionization, this gap in the literature is consequential for understanding public sector labor in a post-*Janus* America.

To fill this gap, I leverage the variation in the timing of Right to Work policies across states to bring new, empirical evidence on their impact and contribute to ongoing theoretical debates about the teacher voice and rent seeking activities of teachers' unions. I find that prohibitions on agency fees for teachers' unions lead to declines in teachers' union power, but contrary to what many union critics have argued, I find that these efforts did not result in political opportunities for education reforms nor did they result in increases in student outcomes. If anything, RTW policies have decreased student achievement, with upper bounds of estimated 95% confidence intervals ruling out a positive effect of larger than 0.015 SD. Although my evidence is somewhat more consistent with the teacher voice framework, I argue that it is likely that teachers' unions sometimes narrowly defend the interests of teachers and at other times act as productive information sharers.

Theoretical Frameworks

In this section I explain the *rent seeking* and *teacher voice* theoretical frameworks to detail their arguments, empirical literature, and predictions. Although often conceptualized as a dichotomy, I

argue that these two frameworks are not necessarily mutually exclusive, and as I explain below, they often produce similar hypotheses about the effects of restrictive labor policies like RTW policies. Where they become most divergent is the potential effect of RTW policies on students.

Rent Seeking

The *rent seeking* theory argues that teachers' unions pursue teachers' job interests at the expense of student interests and has arguably dominated the empirical literature as well as the political debate, at least in the post-No Child Left Behind era (Moe 2011; Lott and Kenny 2013; Cowen and Strunk 2015; Lovenheim and Willén 2019). From this perspective, teachers' unions engage in collective bargaining to extract *rents* such as salary increases and smaller class sizes. In the negotiation process teachers' unions advocate for teachers' job interests, particularly those that align with union interests (Moe 2011). For example, smaller class sizes can provide not only improved working conditions for teachers but also more union members to pay dues. The key concept is that unions are able to negotiate for these conditions that help teachers and their unions but do not increase district productivity and may even decrease it (Cowen and Strunk 2015).

Some empirical work supporting this framework has focused on the notion that collective bargaining increases the costs of education but decreases the efficiency of schools (e.g., Hoxby 1996; Strunk 2011; Strunk and McEachin 2011; Marianno and Strunk 2018b). Other literature has characterized teachers' unions as powerful organized interests that block reform attempts that would have otherwise made schools better for students because unions have a stake in the status quo (Moe 2006a; 2011; 2015; DiSalvo 2015). Some empirical work has supported the notion that strong teachers' unions block policies aimed at school efficiency such as school choice programs, charter school expansion, and performance pay plans, although the authors do not necessarily maintain that these reforms would have been effective at increasing school efficiency (Hartney and Flavin 2011; West and Mykerezi 2011; Finger 2018).

In sum, the rent seeking framework posits that RTW policies should improve educational systems for three interconnected reasons. For one, they should increase school efficiency by decreasing the power of teachers' unions to bargain for increases to educational inputs that do not increase student achievement. Additionally, RTW policies should lead to new reforms that increase school efficiency and student achievement by ridding education politics of the most powerful groups that are preventing productive education reforms. Further, because self-interested teachers and their unions face greater difficulties in steering the politics of education toward their own, narrow interests, the voices of education reformers, parents, and students should be amplified, and as a result, student outcomes should improve.

Teacher Voice

The *teacher voice* theory, in contrast, posits that collective bargaining and teachers' union strength can induce improved school management and higher productivity by promoting teachers' abilities to share their knowledge and opinions at the workplace (Ravitch 2007; Gyurko 2012; Choi and Chung 2016; Matsudaira and Patterson 2017; Han and Maloney 2019). Inspired by Albert O. Hirschman's (1970) notion of "voice" as a resource for organizational recuperation, this theory argues that the collective bargaining process provides opportunities for teachers to discuss working conditions and school policies collectively with education decision makers. This allows them to improve the system from the inside instead of "exiting" into other professions or labor markets. The teacher voice theory therefore argues that teachers' union power increases educational productivity because it gives "voice" to those that have the most knowledge about students and are the workers responsible for implementing school policies. In short, this theory argues that teachers' interests are aligned with student interests. This argument was revived in the 2018 and 2019 teacher strikes, which were generally received positively and considered successful, in part, because teachers were able to make that argument in a compelling way (Goldstein 2018).

The teacher voice theory holds that teachers' unions allow teachers to discuss their concerns with school and district leadership instead of merely leaving the profession (Hirschman 1970; Choi and Chung 2016). Some empirical work has found that when teachers are not able to express their voice, they are more likely to exit the system, thus increasing teacher turnover and requiring additional energy and attention directed toward hiring and training new teachers (Choi and Chung 2016; Han 2016; Roth 2017; Baron 2018). Baron (2018) explicitly connects this with student achievement, providing evidence that Wisconsin's Act 10, which prohibited agency fees for teachers' unions, decreased student achievement by lowering teacher salaries and increasing teacher turnover.

The teacher voice theory hypothesizes that RTW policies decrease the unions' power to advocate for school improvements and effective policies that reflect the conditions on the ground. If this decrease in power then translates to decreases in indicators of teachers' job interests, the teacher voice theory posits that it would also lead to decreases in indicators of student interests (e.g., lower student achievement). Under this framework, RTW laws therefore decrease student outcomes by limiting the channels by which teachers can provide valuable information about educational practices and policy viability to decision makers (Han and Maloney 2019).

As discussed above, this debate about the teacher voice and rent seeking activities of teachers' unions has largely posed these two theories in opposition to each other, and where they become most divergent is in the alignment between teachers' and student interests. Although much of the empirical evidence finds larger rent seeking effects of policies facilitating unionization, it is possible that policies restricting unionization, like RTW policies, may not affect the activities of teachers' unions in the same ways. This study therefore examines the question: To what extent have prohibitions on agency fees affected teachers' unions, school working conditions, education policymaking, and students in ways predicted by the rent seeking and teacher voice theories?

Data and Methods

Data

Based on the rent seeking and teacher voice frameworks presented above, I created an original dataset from a number of different sources. The section below describes each of the variables, their sources, and the exact years in which data are available. This information is also summarized in Table 1. Data on the timing of agency fee prohibitions for teachers' unions prior to 1996 come from the NBER Collective Bargaining Law Data Set Kim Reuben Extension (Valletta & Freeman 1988). I have further extended the dataset using Sanes and Schmitt (2014); Feigenbaum, Hertel-Fernandez, and Williamson (2018); and the U.S. Department of Labor (1972, 1979, 1981) State Profiles of Public Sector Labor Relations.[3] Table 2 displays the year of passage for each state.

The current analyses focus on the prohibition of agency fees for teachers' unions since 1990, and the years of passage for each of these recent policies are highlighted in grey in Table 2. I focus on this recent time period for a number of reasons. For one, a key differentiator between the two major theories lies in the effect of restrictive labor policies on student achievement, and standardized, longitudinal, cross-state student achievement data are only available in the period since 1990. Additionally, laws prohibiting agency fees accomplish much of the same functional result as the *Janus* (2018) decision, and so the impact of these laws can uniquely shed light on potential implications of that Supreme Court decision. Further, public sector RTW laws have been a particular goal of conservative advocacy groups who have explicitly used them as a vehicle to disempower teachers' unions (Hertel-Fernandez 2018; 2019).

I first examine the effect of state laws prohibiting agency fees on two measures of the

[3] As noted above, where Sanes and Schmitt (2014) did not match with the NBER Collective Bargaining Dataset, I referred to the U.S. Department of Labor (1972, 1979, 1981) State Profiles of Public Sector Labor Relations. If a discrepancy still existed, I referred to the referenced statutes themselves. Texas and Arizona are the most problematic states to decisively determine a year of adoption for agency prohibitions for teachers' unions. Both states had policies that may have, in practice, prohibited the collection of agency fees for teachers' unions even though they did not explicitly prohibit them prior to the enactment of such a policy in 1993 and 2006 respectively. Because of this nuance, in my main models I consider their years of enactment 1993 and 2006 respectively but test the robustness of the findings to models that do not conceptualize them as having new prohibitions on agency fees, as explained in the section below.

strength of teachers' unions: membership and political contributions. I operationalize teachers' union membership with a measure of NEA membership counts from the National Education Association "Secretary-Treasurer/Independent Auditors Financial Reports: Presented to the Representative Assembly" compiled by Mike Antonucci of the74 and extended to cover the entire time period of interest with information presented in the 1995 NEA Handbook.[4] As a second indicator of the power of teachers' unions, I follow Finger (2018) and Hartney and Flavin (2011) by examining political contributions at the state level from the National Institute on Money in Politics (NIMP). NIMP has collected a comprehensive and thorough accounting of donations to candidates running for statewide office since 2000. I downloaded these data from NIMP in July, 2019. Because of the limited time range, I am cautious about overgeneralizing from these results. These data are, however, particularly insightful because they include contributions from both the NEA and the AFT at the national level as well as their state and local affiliates.

In my analysis of the effects on school working conditions, I follow Hoxby (1996) by focusing on teacher salaries, per pupil expenditures, and pupil to teacher ratios. Data on teacher salaries come from SAGE stats and Paglayan (2019) but were missing for the 2007-2008 school year, so I supplemented those sources with data from the Common Core of Data (CCD). Per pupil

[4] Had it been possible, I would have also included an analysis of the second largest teachers' union in the United States, The American Federation of Teachers (AFT). However, they do not publish their membership counts and did not respond to my request for membership counts. Because of this, I must rely on NEA state membership counts as the indicator of teachers' union membership across the United States. There may be good reasons to assume that my results would not generalize to AFT membership, and I examine this with information on AFT/NEA Mergers and all employed union workers in education. Beginning in the latter half of the 1900s, the AFT and NEA have converged on a shared set of principles and goals, and mergers between the NEA and AFT at the national level have even been discussed for many years (Murphy 1990). Mergers at the state level have become increasingly common, and I have also gathered information on the timing of these state mergers to use as control variables in all models of NEA membership. They occurred in Florida (2000); Minnesota (1998); Montana (2000); New York (2006); and North Dakota (2013). Secondly, if I assume that AFT and NEA membership follow similar trends, then I can generalize my results across the two major education unions. Although AFT membership data are not available, I gathered data from the Bureau of Labor Statistics (BLS) on the total number of union members employed in education, training, and library services. Figure A-1 in the appendix compares NEA membership totals to these membership totals and demonstrates that they follow very similar trends.

expenditures come from the CCD as well as Paglayan (2019).[5] Pupil to teacher ratios are particularly important indicators of school conditions because they directly affect students as well as teachers and therefore more finely straddle the line between teachers' job interests and student interests. Data on pupil to teacher ratios come from the CCD.[6]

Because the rent seeking theory focuses on teachers' unions' ability to block reforms that may have otherwise increased school efficiency and student achievement (Moe 2011; Anzia and Moe 2014; DiSalvo 2015; Moe 2015), I also examine education reform policy outcomes at the state level that are aimed at increasing educational efficiency and often blocked by teachers' unions. These include charter school laws, private school choice programs, performance pay, and Teach For America expansion provided by Leslie Finger (2018) and updated using the original sources for charter laws, private school choice programs and Teach For America expansion. For updates to performance pay, I used information from the National Council on Teacher Quality State Policy Yearbook. I combined these into a single dummy variable indicating if a state has any one of these policies. I also tested each of these separately with results included in the appendix.

Finally, I investigate the impact of laws prohibiting agency fees on student outcomes, operationalized by the National Assessment of Education Progress (NAEP). For these analyses, I use publicly available state averages for fourth and eighth grade reading and math scores. I standardized NAEP scores within grades and subjects using the 1992 means and student-level standard deviations. I then averaged across grades to generate pooled NAEP scores for each

[5] Even though instructional salaries comprise a very large portion of school expenditures, I include information on both salaries and per pupil expenditures because public sector unions often bargain to maintain public sector employment and negotiate for provisions that lead to non-instructional expenditures (e.g., support staff, nurses). Focusing on total expenditures picks up a combination of changes in teacher hiring, changes to salary schedules, and changes to other non-instructional expenditures, and I can't distinguish between them.

[6] Although pupil to teacher ratios are the best cross state indicators of class size with enough years of data to be useful, they are imperfect indicators because of variation in school structures and processes across states. As explained below, any time-invariant differences across states are accounted for by the inclusion of state fixed effects in my models; however, there may be time varying cross-state differences that could still affect my results.

administration since 1992. I also present results within subjects and grades. In the appendix, I display results for gender, income, and racial subgroups.

Methods

The primary empirical strategy exploits the different timing across states in the passage of RTW policies in a differences-in-differences (DD) framework to identify how exposure affects unions, proxies for school working conditions, education policymaking, and ultimately students. The two differences in this case arise from within state changes in prohibitions on agency fees as well as cross-state variation in the timing of these changes. There is a growing body of literature supporting the use of DD to identify a causal effect when a policy is implemented in various years across multiple states (Angrist and Pischke 2009, 2015; Goodman-Bacon 2018; Hallberg et al. 2018). This framework has been used in studies of the effects of restrictive labor laws (Farber 2006; Baron 2018; Feigenbaum, Hertel-Fernandez, and Williamson 2018; Finger and Hartney 2019) and laws facilitating unionization (Hoxby 1996; Frandsen 2016; Lovenheim and Willén 2019, 2018; Paglayan 2019). The most basic empirical specification for this model (Model 1) is

$$Y_{st} = \beta_1 Agency\ Fees\ Prohibited_{st} + \gamma X_{st} + \delta_s + \mu_t + \varepsilon_{st}$$

Where Y is one of the outcomes I examine in state *s* year *t*. The variable $Agency\ Fees\ Prohibited_{st}$ is a dummy variable indicating if a state *s* has a policy prohibiting agency fees for teachers' unions in year *t*. I include a vector of time varying, state-level covariates X_{st} as explained below. Additionally, in analyses of NEA Membership, I control for AFT/NEA Mergers, AFT strongholds, and total student enrollment in that state. Total student enrollment (from the CCD) allows my analysis to estimate the effect of a RTW policy on membership holding the number of students in that state constant. I also include state (δ_s) and year (μ_t) fixed effects that control for fixed differences across time between states and for any year specific events. The inclusion of these fixed effects allows me to focus on within-state changes in legislation and relate

those changes to within-state changes in outcomes. The time-varying nature of the RTW policies across states allows me to partial out policy effects from calendar year effects. This model therefore removes omitted variables fixed across states and over time and averages across those two dimensions to pull out state by year variation in outcomes.

The identification of a causal effect using this DD strategy rests on two interrelated assumptions, for which I perform an extensive set of robustness checks. The first assumption is that the passage of RTW policies is not correlated with underlying, unobserved trends in teacher and student outcomes at the state level (parallel trends). The second assumption is that there are no other state-level policies that tend to occur in the same year of passage as these specific RTW laws that independently cause changes in teacher and student outcomes. If this were the case, then I could be picking up variation in outcomes due to other policy changes and attributing it to RTW laws.

To address these assumptions, I take a number of steps. For one, I include controls for time varying, underlying political ideologies in the states using mass social and economic political liberalism indicators from Caughey and Warshaw (2018). These are ideal control variables for underlying political conditions because public opinion is unlikely to be affected by state policy changes, particularly in the short term. Furthermore, to control for the added concern that conservative politicians may institute both RTW policies and education reform policies when coming into office, I also include a dummy variable indicating if the state is governed by a unified Republican government from Jordan and Grossmann (2017) and updated by my own compilation from the National Conference of State Legislators and Ballotpedia.[7] I also include baseline models

[7] This is defined as a state that has Republican control of the house(s) of legislature as well as a Republican governor. I used this measure as opposed to other measures of Republican dominance for a number of reasons: (1) it represents a level of domination of state policymaking that reflects the ability of the Republican party to most easily determine the rules governing political institutions; (2) I tested other measures for the years in which they were available and results always produced comparable results. However, because these data end in the year 2015, the sample size is slightly

without these controls, and results are consistent across model specifications suggesting that the omission of these or other, unmeasured variables has not altered the results (Altonji, Elder, and Taber 2005).

Second, I examine results from a three-parameter model, mirroring the approach of Lafortune, Rothstein, and Schanzenbach (2018)[8] and Kraft, Brunner, Dougherty, and Schwegman (2020). This approach (Model 2) adds two trend terms to the baseline DD equation and takes the form,

$$Y_{st} = \beta_1 Agency\ Fees\ Prohibited_{st} + \beta_2 Ever\ Prohibited\ Agency\ Fees * Year_{st} + \beta_3 Years\ Since\ Agency\ Fees\ Prohibited_{st} + \gamma X_{st} + \delta_s + \mu_t + \varepsilon_{st}$$

where $Ever\ Prohibited\ Agency\ Fees * Year_{st}$ is a linear time trend for treated states and $Years\ Since\ Agency\ Fees\ Prohibited_{st}$ is a time trend for treated states only in the post period. The coefficient, β_2, therefore represents a falsification test for the timing of policies prohibiting agency fees. If the estimate of $\beta_2$is significant it suggests that I have not met the assumption of parallel trends. I estimate the effect *n* years after policy enactment by adding $\beta_1 + n^*\beta_3$. I present these results in tables and also display results graphically by plotting $n^*\beta_2$ in the pre-period and then adding $\beta_1 + n^*\beta_3$ in the post period.

Third, I test a non-parametric, event study approach that builds on the models described above but does not constrain potential pre- and post-trends to be linear. This model (Model 3) is specified as

$$Y_{st} = \pi_{-6} I\,(year_t - t_0 \leq -6)_s + \sum_{r=-5}^{10} \pi_r I\,(year_t - t_0 = r)_s + \pi_{11} I\,(year_t - t_0 \geq 11)_s + \gamma X_{st} + \delta_s + \mu_t + \varepsilon_{st},$$

reduced for models that include controls. Baseline models without controls include the full sample; (3) there is some basis for this particular control variable in the extant literature on restrictive labor policies (e.g., VanHeuvelen 2020).

where t_0 is the year before enactment of the RTW policy. In this model, π_r represents the effect of the event *r* years later (or before if *r* is less than one) relative to the year before enactment, *r=0*, which is excluded. The π_{-5} to π_{-1} estimates therefore also allow me to non-parametrically test the assumption that there is no selection on trends across states. Because political contributions and NAEP scores are available every other year, I bin the treatment dummies in two year intervals in models that estimate the impact of RTW on these outcomes.

I also examine the robustness of the results to a variety of different model specifications. Because there are seven states with policy changes in the time range that I analyze, some may be concerned that a single state might drive the results; therefore, I examine a series of models that drop each treated state. I also examine models that drop never treated states, include a time trend for each region, limit the number of years before and after the treatment in each treated state, and consider Texas and Arizona as always treated states since they had prior RTW laws that did not explicitly apply to teachers but may have functionally prevented the negotiation of agency fee provisions nonetheless. Finally, because the two-way fixed effects estimate (Model 1) is the weighted average of the estimated effects, I also graph the estimates against their weight to examine the distribution of weights across estimates and check for the existence of negative weights that could be skewing the results (Goodman-Bacon 2018).

Results

I first examine the effects of laws prohibiting agency fees on teachers' unions using two indicators of teachers' union power at the state level: NEA membership and political contributions to candidates. The findings presented in Table 3 suggest that the enactment of a law prohibiting agency fees leads to a decrease in teachers' union membership of roughly 24,000 members, which is equivalent to about 40% of the average membership in these states prior to prohibiting agency fees. This finding is quite robust to the addition of controls, and the more flexible semi-parametric

(Model 2) and event study (Model 3) models displayed in Figure 2 also suggest a large, negative effect. These results also show that the trend in NEA membership in treated states prior to enactment is not statistically different from untreated states.

I also examine the relationship between the political contributions of teachers' unions and prohibitions on agency fees. Although results do not reach standard levels of statistical significance, this may be due to the limited number of years for which there are available data. There is, however, descriptive evidence to suggest that there may be important differences in contributions in states with and without RTW policies. Figure A2 in the Appendix shows that in the year 2000, states that prohibit agency fees had roughly 40% more relative contributions from teachers' unions than states not prohibiting agency fees, but this trend had reversed by 2018 when states not prohibiting agency fees had a greater proportion of contributions from teachers' unions. This reversal mirrors the level differences already in existence in the comparisons of states with and without prohibitions on collective bargaining and strikes, and Figure A3 shows that no such reversal exists for non-teachers' union education groups. This suggests that it is the institutional framework that may be making a difference, as opposed to other underlying trends in education advocacy.

Next, I examine the effects of laws prohibiting agency fees on second order outcomes that could result from the weakening of teachers' unions via RTW policies. I begin by looking at the distribution of resources to teachers and schools. First, As shown in Table 4 and Figure 3, I find no evidence of differences in trends in teacher salaries prior to policy adoption and no effect of laws prohibiting agency fees in the ten years after enactment. Although the post-trend is statistically significant and positive, estimated effects are not significantly different from zero in the ten years after policy enactment.

Second, although the results of Model 1 suggest that laws prohibiting agency fees may cause declines in per pupil expenditures, results from the three-parameter model, also displayed in Figure

3, demonstrate that the trends in expenditures before the enactment of the law were already declining. Similarly, I find that the ratio of students per teacher increased after RTW policy enactment by about one student, though event study estimates suggest that five years prior to treatment pupil-to-teacher ratios were lower in states that would later adopt RTW policies. This contributes to the appearance of a small upward trend in the non-parametric estimates of the effect on the ratio of students per teacher that is not clearly modified by the enactment of the RTW policy. Thus, although per pupil expenditures and pupil-to-teacher ratios tend to be lower in states prohibiting agency fees, this is potentially due to underlying, unobserved conditions in these states rather than the policies themselves. Although this suggests that working conditions are lower in RTW states, this does not prove that it was prohibitions on agency fees that caused these declines.

I next turn to an examination of the effect of RTW policies on education policies. Under the rent seeking framework, I would expect to find that RTW policies lead to new education policies that ultimately increase student achievement. If so, then it could be these policies generate a productive reorganization of schools around markets that increases student performance, even as expenditures and other working conditions remain unaffected. However, I find no effect of prohibiting agency fees on the likelihood of states adopting new education reforms aimed at increasing educational efficiency (Teach For America, teacher performance pay, private school choice programs, or a charter school law).[9] Thus, I find no evidence that RTW policies have opened up opportunities for education reform that were not possible under political regimes more dominated by the teachers' unions.

Finally, I turn to student achievement, operationalized first by pooled fourth and eighth grade, state averages in reading and math on the National Assessment of Education Progress

[9] In Appendix Table A1, I include estimated effects of prohibiting agency fees on each of these policies separately and continue to find no effect of RTW policies. I also find that in the years prior to prohibiting agency fees, states that would prohibit agency fees were *less* likely to adopt charter school laws.

(NAEP). As shown in Figure 4 and Table 5, I find no evidence of differences prior to policy adoption and no overall effect of RTW policies on student achievement. The estimated coefficients on policy adoption are all negative but non-significant, and the upper bounds of the 95% confidence interval on the baseline estimate (column 1) rules out a positive effect of greater than 0.015 SD. Additionally, to address the concern that estimated effects on test scores might be biased to observed changes in per pupil expenditures, pupil-teacher ratios, or charter policies prior to policy enactment or unobserved factors causing those observed changes (see above), I include controls for these second order outcomes in columns 4 and 5. Results are nearly identical, suggesting that these pre-treatment differences do not affect the results.

I also examine student achievement results separately by grade and subject. Results in columns 1 and 2 of Table 6 suggest that prohibiting agency fees is associated with a decline of 0.09 to 0.1 SD on the NAEP fourth grade reading test. In the three parameter and event study models, I do not find evidence of a pre-trend, and the effect is similarly negative; however, as shown in Figure 5, the estimated effect does not meet standard levels of statistical significance. Results are nearly identical with the added second order controls (columns 4-5). As shown in columns 6-10 of Table 6 and Table 7, I find no effect on math scores or eighth grade scores in either subject. In Appendix Table A2, I also examine the effect of RTW within gender, income, and racial subgroups, and estimations are very similar across these subgroups.

Robustness

In Appendix Table A3, I show that results are not driven by one single state through a series of models that drop each of the treated states. Further, to lessen the impact of serial correlation (Bertrand, Duflo, and Mullainathan 2004), I have tested models that limit the number of years before and after treatment to five. The magnitude of the effect on NEA membership decreases, but the estimate retains directionality and statistical significance. I also show that results are quite robust to dropping never treated states, suggesting that results are not driven by underlying differences in

the types of states with and without RTW policies. Moreover, I demonstrate that results are very robust to different definitions of RTW policies by considering Texas and Arizona always treated; notably, estimated effects on student achievement retain directionality and increase in magnitude and precision in these models. More specifically, in these models, I find a negative effect of RTW policies on pooled, fourth grade reading, and fourth grade math NAEP scores. Results are also quite robust to the addition of region time trends to capture place-specific, time varying trends, suggesting that the effects are not derived from underlying unobserved trends within education labor markets.

Some may also be concerned about the possibility that unit or cohort-specific treatment effect heterogeneity may bias results (Goodman-Bacon 2018). I therefore plot each of 2x2 differences-in-differences components from Goodman-Bacon's (2018) decomposition theorem against their weight from the parametric analysis (Model 1). Figures in Appendix B display the average differences-in-differences estimate and total weight on each of the components, separated by four general types of comparisons. These graphs show that none of the estimates receive negative weights.

Discussion

The *Janus v. AFCSME* (2018) decision fundamentally changed the institutional context for teachers' unions in the United States by generating new public sector labor restrictions across the country. This decision came at a time when public and private sector labor movements in the United States were already struggling to retain membership and combat new labor restrictions at both the state and federal levels. Despite their importance for both labor and education politics, up to this point little evidence has existed to contextualize and understand the consequences of these institutional changes across the nation.

The empirical literature on teacher unionization has focused primarily on policies *facilitating* teacher unionization using two theoretical frameworks: rent seeking and teacher voice. In short, the

rent seeking perspective argues that teachers' unions pursue teachers' job interests in ways that make schools less efficient, whereas the teacher voice view argues that stronger unions enable teachers and their representatives to share valuable information with school leadership. Although not entirely mutually exclusive, these two theories have tended to produce a simplified, yet still unresolved question about whether teachers' unions are the heroes or the villains in the story of American education.

This study contributes new knowledge on the effects of Right to Work policies across the United States. I find that prohibitions on agency fees for teachers' unions led to large declines in teachers' union power. Moreover, I find that efforts to weaken unions did not result in new political opportunities for education reforms that teachers' unions have opposed, nor did they increase student achievement. My results rule out the possibility of a meaningful positive effect on student outcomes, and where efforts to weaken unions have affected student achievement, the effect appears to have been a negative one.

Any negative effects are concentrated in fourth grade reading, and I find essentially no effect on fourth grade math scores or eighth grade scores. One possible explanation for the estimated difference between reading and math scores in fourth grade is that there is really no difference in the effects, and the apparent decline in reading scores from the parametric DD model (Model 1) just reflects idiosyncratic error. On the other hand, there may be real differences in the effects on math and reading achievement in fourth grade, and if so, the effect of restrictive labor policies appears larger for reading than math. This is somewhat surprising given that many educational program evaluations have found larger effects on math scores than on reading scores. However, we may not expect RTW policies to act like typical education programs, because prohibiting agency fees for teachers' unions is, quite simply, not a typical educational program. If *math* scores are more associated with curricular interventions, then it might be that *reading* scores are more associated with

non-pedagogical teacher activities that are typically harder to identify but may nevertheless be affected by a loss of teacher voice. This notion aligns with prior class size research that has found larger effects in reading than math (e.g., Angrist and Lavy 1999).

The lack of an effect on education reforms is also somewhat surprising given the general acceptance that teachers' unions play an outsized role in education politics as well as their strong opposition to policies and programs like school choice, charter schools, performance pay systems, and Teach For America (Moe 2006b, 2011, 2015; Goldstein 2014). If teachers' unions are the well-organized, powerful vested interests blocking much needed education reform that parents and students urgently want, then we would expect that education policymaking and student achievement would be dramatically changed when teachers' unions are weakened (Moe 2011, 2015). However, it seems that efforts to muzzle teachers' unions have not opened up windows of opportunity for the enactment of education reforms to increase school efficiency nor have they improved (or dramatically reduced) test scores.

This signals the possibility that the mechanisms of teacher union strength are less dependent on formal governance arrangements than previously thought. This could be because some efforts to contain union voice spark backlash and mobilization of both union members and other teacher allies that counteract the effects (Freeman and Han 2012). The somewhat small and inconsistent teacher voice effects that I have uncovered may, therefore, be less reliant on the formal organization of teachers' unions and more reflective of additional factors that have not yet been seriously considered by leading theoretical frameworks.

Implications and Future Research

Although debates on teachers' unions are often boiled down to a search for a clean and decisive take on teachers' unions as the heroes or villains in the story of American education, it is plausible that some of the divergence in findings on the effects of teacher unionization is not

because some have gotten it wrong and others have gotten it right, but rather because some actions that teachers' unions take are rent seeking and others are productive expressions of teacher voice. This notion reflects what labor economists, Freeman and Medoff (1984), argued over 30 years ago,

> In our view, there is some truth to both sides of the debate. The central question is not "Who in principle is right?" but rather, "Which face is quantitatively more important in particular economic outcomes?" and, given the diversity of experiences with collective bargaining, "What factors lead to the predominance of one face over the other in different settings?" (p. 246)

Accepting that the information sharing and rent seeking activities of teachers' unions can complement and counteract each other in complex ways may produce a more realistic conceptualization of the effects of teachers' unions. Teachers' unions advocate for teachers' job interests, which may sometimes align with student interests, but at other times they may conflict. Researchers may gain more valuable insights if they can move past the search for a simple answer on the effects of teachers' unions and instead examine the conditions under which teachers' unions engage in rent seeking or teacher voice activities.

In that pursuit, my findings combine with prior literature to suggest that in the context of restrictive labor policies, the information sharing role of teachers' unions appears to be quantitatively more important in the determination of student test scores. My results align with other literature finding that Wisconsin's Act 10 decreased student achievement in that state by lowering teacher salaries and increasing teacher turnover (Baron 2018; Han and Maloney 2019) and results of the positive effect of teacher unionization in states that prohibit collective bargaining (Han and Keefe 2020). This contributes to a growing body of research finding that unions do not necessarily impede student learning and that additional school spending can affect education production (Frandsen 2016; Jackson, Johnson, and Persico 2016; Matsudaira and Patterson 2017; Baron 2018; Lafortune, Rothstein, and Schanzenbach 2018; Brunner, Hyman, and Ju 2019). This does not imply that teachers' unions are the heroes of American education. Instead of characterizing unions as wholly and decisively bad or good, policymakers and researchers should focus on determining productive

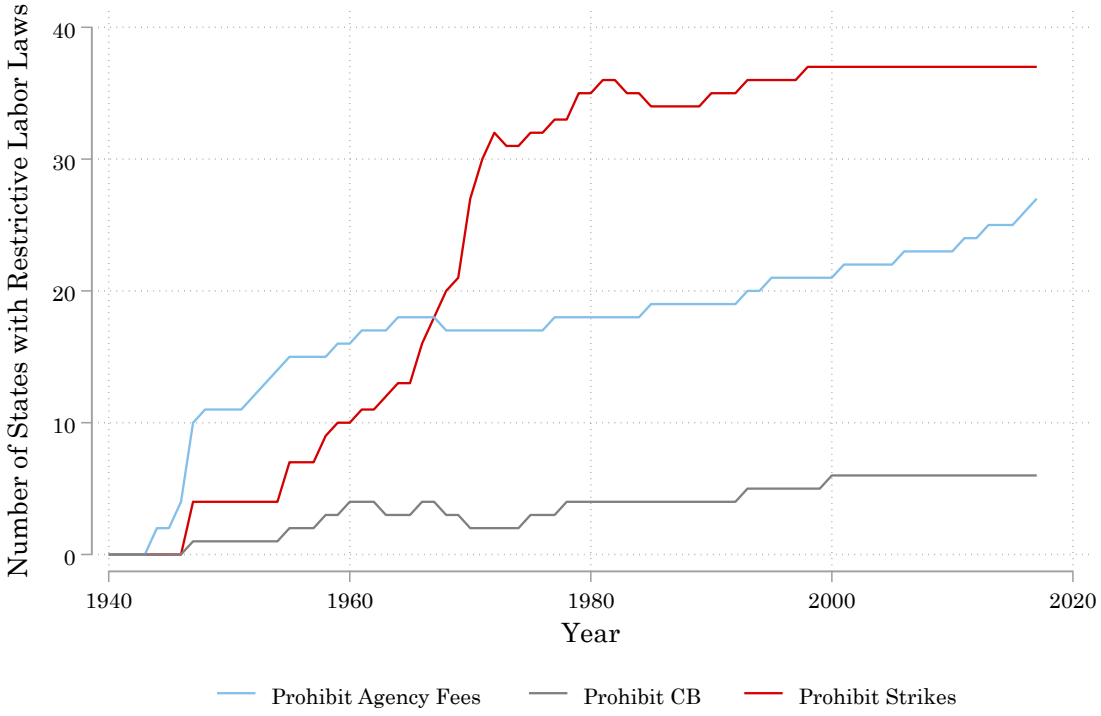
and realistic ways to improve educational systems that do not destroy nor depend on teachers' unions. In this endeavor, I find that weakening teachers' unions through new prohibitions on agency fees is unlikely to improve educational outcomes for students and may harm students.

If *Janus* (2018) behaves like recent RTW policies, then my results suggest that it is likely to result in large decreases in teacher union membership. If the *Janus* decision has a relative effect of similar magnitude to prior RTW policies, then it would reduce teacher union membership by roughly 40% in states that did not previously prohibit agency fees, resulting in a total loss of over 800,000 members. There is no need to assume that these declines would happen overnight, particularly since some losses may result from the failure of new teachers to join rather than current members actively leaving union ranks. Additionally, for those hoping that the *Janus* decision will open up political opportunities for new education reforms or increase student academic achievement, my results suggest that it is unlikely to reach those goals. Future research can, however, examine the effects of *Janus* (2018) empirically using an approach building off the one I have described and prior work examining national policies shifts that had previously been enacted in certain states (e.g., Dee and Jacob 2011).

As noted above, future research in this field should also pay special attention to broader effects beyond the typical education-specific outcomes. Teachers' union strength may matter for more than the outcomes examined in this study, and this may be due to their political advocacy, alliances with other labor unions, and other activities that represent teachers' interests while simultaneously supporting or conflicting with the broader public good. Our understanding of the political economy of education could benefit from considering teachers' unions as one piece of a larger educational ecosystem and labor movement.

Teachers and their unions do not just bargain with local school boards; they are involved in complex community organizations, social networks, and other coalitions that advocate for a broad

array of policies across multiple levels of governance (Ashby and Bruno 2016; Henig et al. 1999; Lyon and Henig 2017). Teachers are critical members of local labor forces, and in many areas education systems represent some of the largest employers (Henig et al. 1999). Teachers are parents, community members, and local employees, and so weakening the organizational capacity of teachers' unions may generate consequences that reach into and beyond education systems in unseen and indirect ways. Future research that acknowledges these complexities can help connect the growing education policy literature with longstanding political science and labor economics knowledge bases by situating the research on teachers' unions within a more comprehensive conceptual framework.



Notes: This is the number of states with restrictive labor policies that target teachers' unions but not necessarily other private *or public sector* unions. See Freeman and Han (2012) for further discussion of teachers' unions as specific targets of these policies.
Source: Author's compilation from the NBER Collective Bargaining Law Data Set including the Kim Reuben extension (Valletta and Freeman 1988), the U.S. Department of Labor (1972, 1979, 1981), Sanes and Schmitt (2014), and Feigenbaum, Hertel-Fernandez, and Williamson (2018).

Figure 1. The Number of State Restrictive Labor Policies for Teachers' Unions over Time

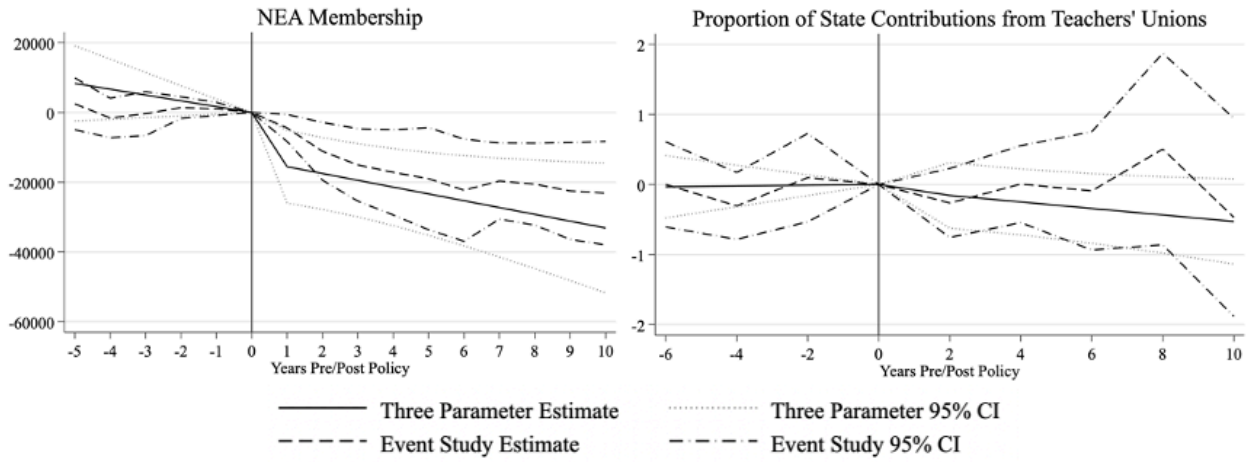
Table 1. Descriptive Information on Rent Seeking and Teacher Voice Indicators

Variable	Observations	Years (Fall)	Mean	Standard Deviation	Data Source
Agency Fee Prohibition	1,450	1990-2018	0.449	0.498	Freeman & Valetta (1988); Sanes and Schmitt (2014); Feigenbaum, Hertel-Fernandez, & Williamson (2018)
NEA Membership	1,350	1990-2016	53,285	63,594	NEA Handbooks (1990-1993); the74 (1994-2017)
Percent of Statewide Political Contributions from Teachers' Unions	410	2000-2018	1.044	1.040	National Institute of Money in Politics (NIMP)
Average Teacher Salary 2016$	1,300	1990-2015	$55,868	$8,744	Common Core of Data (CCD); Paglayan, 2018
Per Pupil Expenditures 2016$	1,350	1990-2016	$10,160	$2,713	Common Core of Data (CCD); Paglayan, 2018
Pupil/Teacher Ratio	1,350	1990-2016	15.90	2.540	Common Core of Data (CCD)
Education Reform Policy	1,450	1990-2018	0.703	0.457	Finger (2018); Author's compilation
Pooled NAEP Scores	645	1991-2016	0.194	0.233	National Assessment of Educational Progress (NAEP)
Reading 4th: Overall Average Score	562	1991-2016	0.057	0.195	National Assessment of Educational Progress (NAEP)
Math 4th: Overall Average Score	524	1991-2016	0.475	0.299	National Assessment of Educational Progress (NAEP)
Reading 8th: Overall Average Score	477	1997-2016	0.108	0.166	National Assessment of Educational Progress (NAEP)
Math 8th: Overall Average Score	520	1991-2016	0.304	0.256	National Assessment of Educational Progress (NAEP)

Table 2. Policies Prohibiting Agency Fees for Teachers' Unions Prior to Janus (2018)

State	Year(s) of Enactment
Alabama	1953
Alaska	
Arizona	2006
Arkansas	1944
California	
Colorado	
Connecticut	
Delaware	
Florida	1944
Georgia	1947
Hawaii	
Idaho	1985
Illinois	
Indiana	1995
Iowa	1947
Kansas	1959
Kentucky	
Louisiana	1977
Maine	
Maryland	
Massachusetts	
Michigan	2013
Minnesota	
Mississippi	1961
Missouri	
Montana	
Nebraska	1946
Nevada	1952
New Hampshire	
New Jersey	
New Mexico	
New York	
North Carolina	1947
North Dakota	1948
Ohio	
Oklahoma	2001
Oregon	
Pennsylvania	
Rhode Island	
South Carolina	1954
South Dakota	1946
Tennessee	1947
Texas	1947-1968, 1993
Utah	1955
Vermont	
Virginia	1947
Washington	
West Virginia	2016
Wisconsin	2011

Notes: Grey shading indicates policy instances examined in this paper.



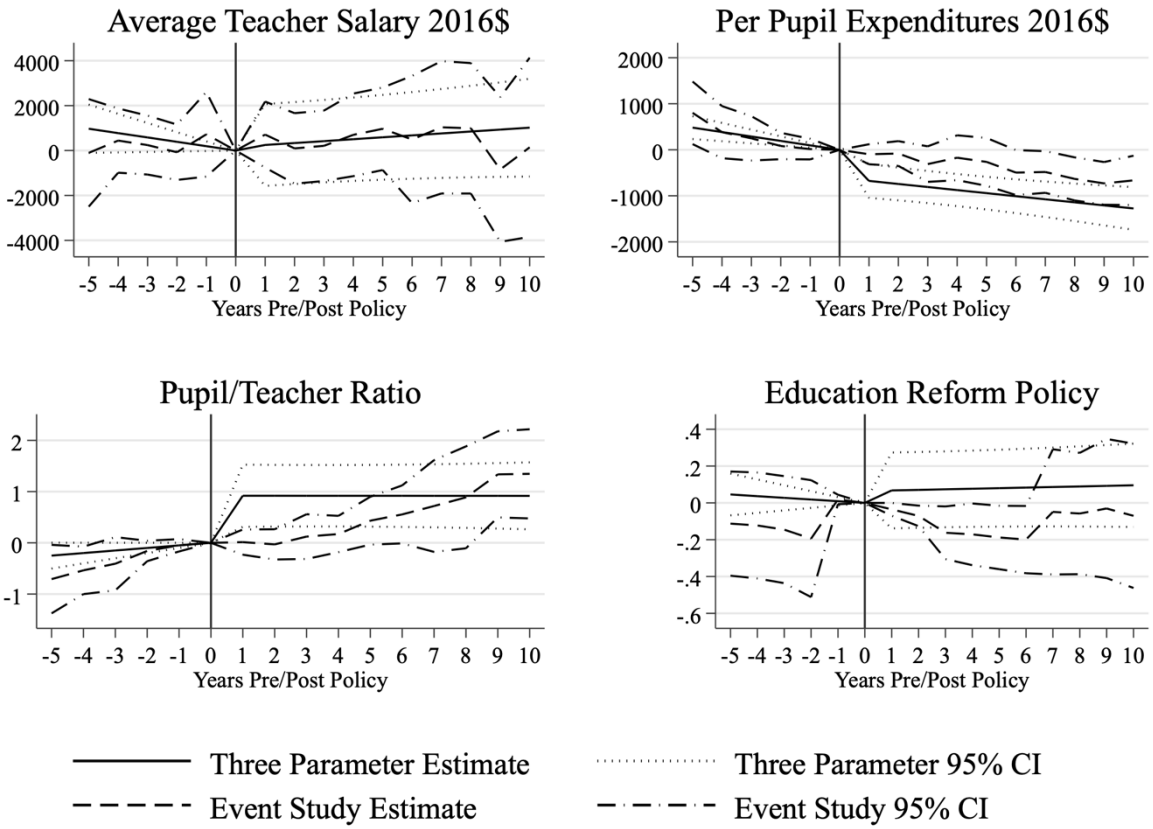
Notes: Three Parameter Estimates are derived from semi-parametric models (Model 2) that estimate an immediate jump as well as a post-trend, while also checking for the existence of linear pre-trends. Event Study Estimates are derived from a non-parametric specification (Model 3). Estimates include state and year fixed effects, as well as controls for underlying dynamics of social and political ideologies and Republican control of state government. The NEA Membership results also control for NEA/AFT merger years, AFT stronghold status, and total student enrollment.

Figure 2. Effects of Agency Fee Prohibitions on Teachers' Union Strength

Table 3. The Effects of Laws Prohibiting Agency Fees on Teachers' Union Strength

	NEA Membership			Teachers' Union Political Contributions		
	(1)	(2)	(3)	(4)	(5)	(6)
Agency Fees Prohibited	-24238***	-22110***	-13599*	0.035	0.032	-0.064
	(6333)	(5776)	(5570)	(0.160)	(0.187)	(0.249)
Ever Prohibited Agency Fees* Year			-1665			0.006
			(1100)			(0.038)
Years Since Agency Fees Prohibited			-286			-0.052
			(329)			(0.031)
Effect After 5 Years			-15030*			-0.324
			(6725)			(0.349)
Effect After 10 Years			-16460*			-0.583
			(8053)			(0.481)
% Change Relative to Pre-Treatment	-40%	-37%		3%	2%	
State and Year Fixed Effects	X	X	X	X	X	X
Social and Political Controls		X	X		X	X
Number of Observations	1,350	1,350	1,350	505	505	505
Number of States	50	50	50	50	50	50
Years	1990-2018	1990-2018	1990-2018	2000-2018	2000-2018	2000-2018

Notes: Each column in the table comes from a separate regression. Columns 1, 2, 4 and 5 show results from models using a single dummy indicator of treatment (Model 1), and columns 3 and 6 show results from semi-parametric models (Model 2) that estimate an immediate jump (Agency Fees Prohibited) as well as a post-trend (Years Since Agency Fees Prohibited). Model 2 also checks for the existence of linear pre-trends (Ever Prohibited Agency Fees*Year). All estimates include state and year fixed effects. Controls include underlying dynamics of social and political ideologies and Republican control of state government. Models examining NEA Membership also control for NEA/AFT merger years and total student enrollment. Robust standard errors clustered at the state level are in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.



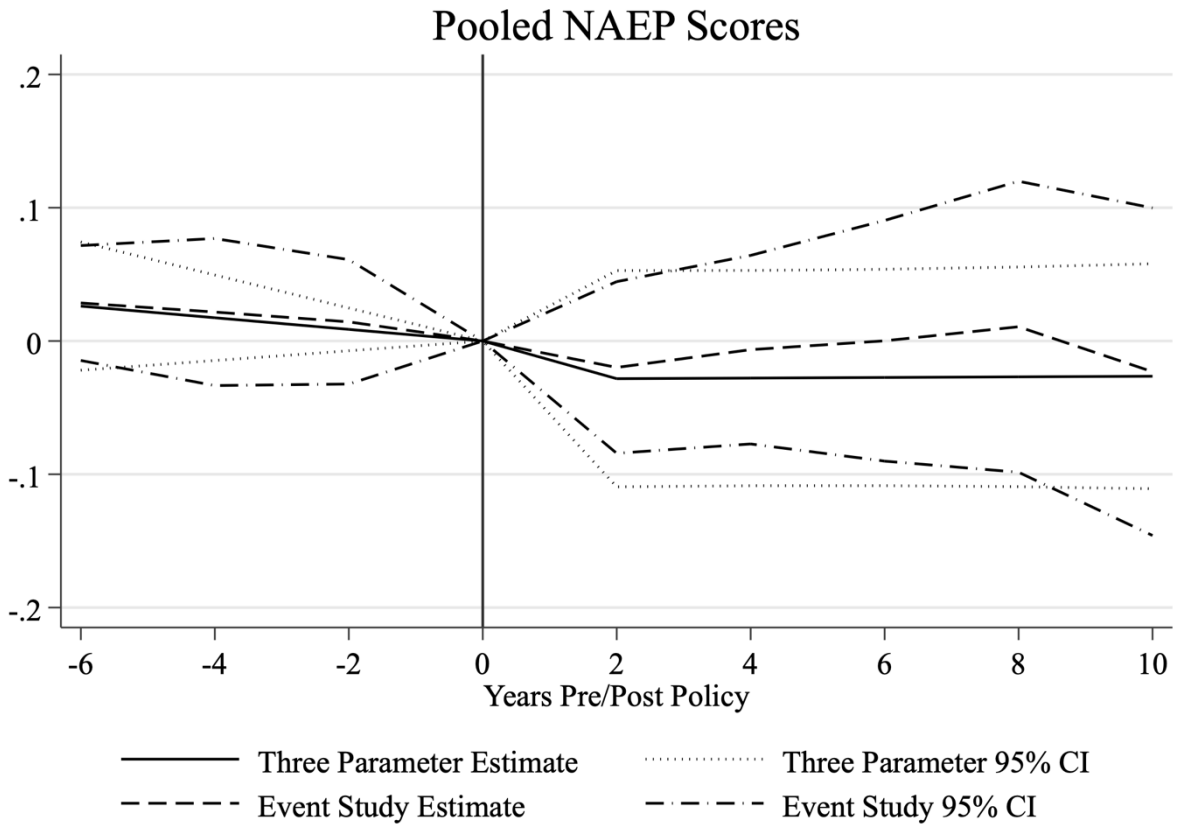
Notes: Three Parameter Estimates are derived from semi-parametric models (Model 2) that estimate an immediate jump as well as a post-trend, while also checking for the existence of linear pre-trends. Event Study Estimates are derived from a non-parametric specification (Model 3). Estimates include state and year fixed effects, as well as controls for underlying dynamics of social and political ideologies and Republican control of state government.

Figure 3. The Effect of Prohibiting Agency Fees on Education Resources and Policies

Table 4. The Effects of Laws Prohibiting Agency Fees on Education Resources and Policies

	Average Teacher Salary 2016$			Per Pupil Expenditures 2016$			Pupil/Teacher Ratio			Education Reform Policy		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Agency Fees Prohibited	-984	-854	442	-1345***	-1139***	-579**	1.234**	1.205**	0.920**	0.022	-0.001	0.065
	(991)	(942)	(973)	(243)	(207)	(199)	(0.407)	(0.412)	(0.314)	(0.108)	(0.112)	(0.106)
Ever Prohibited Agency Fees* Year			-194			-96***			0.050			-0.009
			(110)			(25)			(0.026)			(0.012)
Years Since Agency Fees Prohibited			280**			30			-0.050			0.012
			(101)			(25)			(0.025)			(0.012)
Effect After 5 Years			1841			-430			0.669**			0.126
			(1275)			(248)			(0.229)			(0.136)
Effect After 10 Years			3240			-282			0.419*			0.187
			(1677)			(338)			(0.198)			(0.180)
% Change Relative to Pre-Treatment	-2%	-2%		-11%	-9%		7%	7%		5%	0%	
State and Year Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X
Social and Political Controls		X	X		X	X		X	X		X	X
Number of Observations	1,300	1,300	1,300	1,350	1,350	1,350	1,350	1,350	1,350	1,450	1,450	1,450
Number of States	50	50	50	50	50	50	50	50	50	50	50	50
Years	1990-2015	1990-2015	1990-2015	1990-2016	1990-2016	1990-2016	1990-2016	1990-2016	1990-2016	1990-2018	1990-2018	1990-2018

Notes: Each column in the table comes from a separate regression. Columns 1, 2, 4, 5, 7, 8, 10, and 11 show results from models using a single dummy indicator of treatment (Model 1), and columns 3, 6, 9, and 12 show results from semi-parametric models (Model 2) that estimate an immediate jump (Agency Fees Prohibited) as well as a post-trend (Years Since Agency Fees Prohibited). Model 2 also checks for the existence of linear pre-trends (Ever Prohibited Agency Fees*Year). All estimates include state and year fixed effects. Controls include underlying dynamics of social and political ideologies and Republican control of state government. Robust standard errors clustered at the state level are in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.



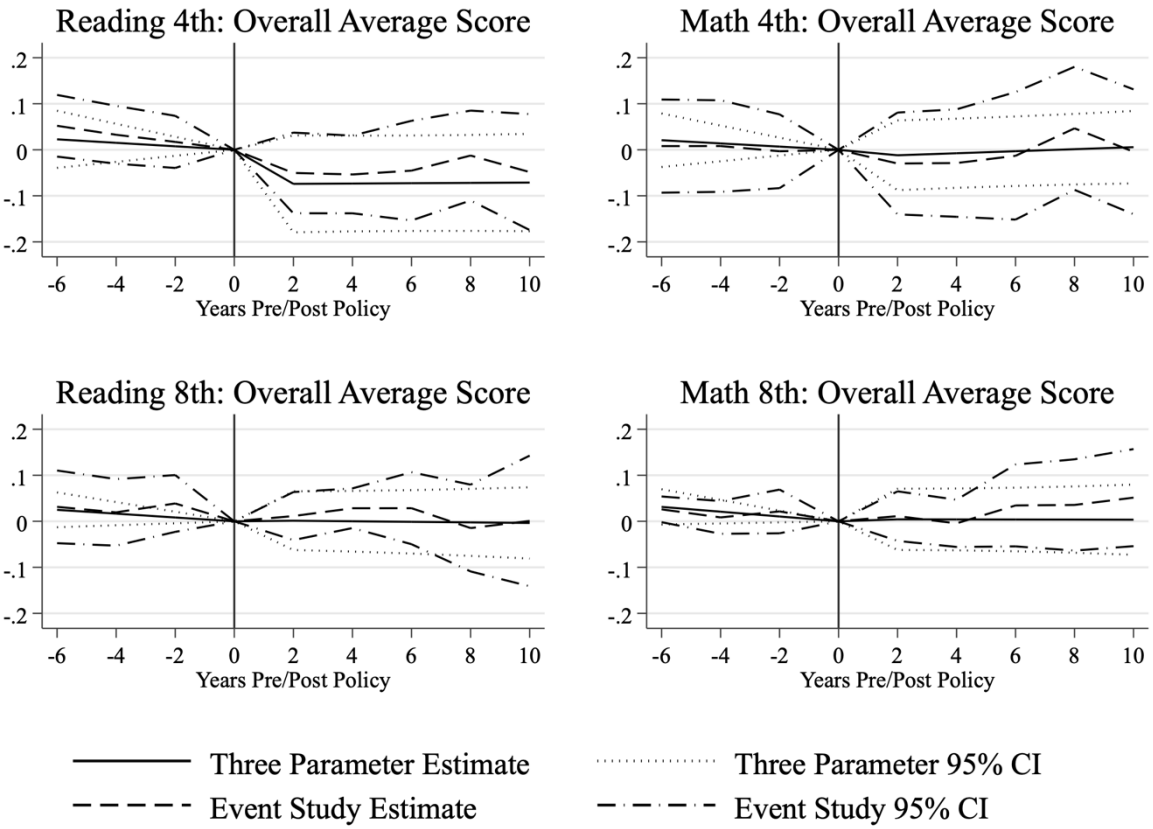
Notes: Three Parameter Estimates are derived from semi-parametric models (Model 2) that estimate an immediate jump as well as a post-trend, while also checking for the existence of linear pre-trends. Event Study Estimates are derived from a non-parametric specification (Model 3). Estimates include state and year fixed effects, as well as controls for underlying dynamics of social and political ideologies and Republican control of state government.

Figure 4. The Effect of Prohibiting Agency Fees on Student Achievement

Table 5. The Effects of Laws Prohibiting Agency Fees on NAEP Pooled Scores

	(1)	(2)	(3)	(4)	(5)
Agency Fees Prohibited	-0.047	-0.054	-0.029	-0.051	-0.031
	(0.032)	(0.035)	(0.042)	(0.034)	(0.041)
Ever Prohibited Agency Fees* Year			-0.004		-0.004
			(0.004)		(0.004)
Years Since Agency Fees Prohibited			0.005		0.004
			(0.004)		(0.004)
Effect After 5 Years			-0.006		-0.010
			(0.054)		(0.054)
Effect After 10 Years			0.017		0.011
			(0.069)		(0.069)
State and Year Fixed Effects	X	X	X	X	X
Social and Political Controls		X	X	X	X
Second Order Outcome Controls				X	X
Number of Observations	645	645	645	645	645
Number of States	50	50	50	50	50
Years	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016

Notes: Each column in the table comes from a separate regression. Columns 1, 2, and 4 show results from models using a single dummy indicator of treatment (Model 1), and Columns 3 and 5 show results from semi-parametric models (Model 2) that estimate an immediate jump (Agency Fees Prohibited) as well as a post-trend (Years Since Agency Fees Prohibited). Model 2 also checks for the existence of linear pre-trends (Ever Prohibited Agency Fees*Year). All estimates include state and year fixed effects. Social and political controls include underlying dynamics of social and political ideologies and Republican control of state government. Second order outcome controls include charter policies, the ratio of students per teacher, and per pupil expenditures. Robust standard errors clustered at the state level are in parentheses. * $p<.05$, ** $p<.01$, *** $p<.001$.



Notes: Three Parameter Estimates are derived from semi-parametric models (Model 2) that estimate an immediate jump as well as a post-trend, while also checking for the existence of linear pre-trends. Event Study Estimates are derived from a non-parametric specification (Model 3). Estimates include state and year fixed effects, as well as controls for underlying dynamics of social and political ideologies and Republican control of state government.

Figure 5. The Effect of Prohibiting Agency Fees on Student Achievement

Table 6. The Effects of Laws Prohibiting Agency Fees on Fourth Grade Student Achievement

	Reading 4th: Overall Average Score					Math 4th: Overall Average Score				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Agency Fees Prohibited	-0.085*	-0.097*	-0.075	-0.101*	-0.082	-0.027	-0.039	-0.016	-0.036	-0.016
	(0.042)	(0.044)	(0.055)	(0.045)	(0.054)	(0.032)	(0.035)	(0.039)	(0.035)	(0.039)
Ever Prohibited Agency Fees* Year			-0.004		-0.004			-0.003		-0.003
			(0.005)		(0.005)			(0.005)		(0.005)
Years Since Agency Fees Prohibited			0.004		0.004			0.006		0.005
			(0.005)		(0.005)			(0.005)		(0.005)
Effect After 5 Years			-0.054		-0.063			0.012		0.010
			(0.070)		(0.070)			(0.055)		(0.056)
Effect After 10 Years			-0.033		-0.044			0.040		0.036
			(0.089)		(0.090)			(0.075)		(0.077)
State and Year Fixed Effects	X	X	X	X	X	X	X	X	X	X
Social and Political Controls		X	X	X	X		X	X	X	X
Second Order Outcome Controls				X	X				X	X
Number of Observations	562	562	562	462	462	524	524	524	424	424
Number of States	50	50	50	50	50	50	50	50	50	50
Years	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016

Notes: Each column in the table comes from a separate regression. Columns 1, 2, 4, 6, 7, and 9 show results from models using a single dummy indicator of treatment (Model 1), and Columns 3, 5, 8, and 10 show results from semi-parametric models (Model 2) that estimate an immediate jump (Agency Fees Prohibited) as well as a post-trend (Years Since Agency Fees Prohibited). Model 2 also checks for the existence of linear pre-trends (Ever Prohibited Agency Fees*Year). All estimates include state and year fixed effects. Social and political controls include underlying dynamics of social and political ideologies and Republican control of state government. Second order outcome controls include charter policies and per pupil expenditures. Robust standard errors clustered at the state level are in parentheses. * p<.05, ** p<.01, *** p<.001.

Table 7. The Effects of Laws Prohibiting Agency Fees on Eighth Grade Student Achievement

	Reading 8th: Overall Average Score					Math 8th: Overall Average Score				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Agency Fees Prohibited	-0.015	-0.027	0.003	-0.014	0.003	-0.013	-0.029	0.004	-0.015	0.006
	(0.026)	(0.030)	(0.032)	(0.024)	(0.031)	(0.031)	(0.036)	(0.034)	(0.030)	(0.034)
Ever Prohibited Agency Fees* Year			-0.004		-0.004			-0.005		-0.004
			(0.003)		(0.003)			(0.003)		(0.003)
Years Since Agency Fees Prohibited			0.004		0.004			0.005		0.005
			(0.003)		(0.003)			(0.003)		(0.003)
Effect After 5 Years			0.020		0.021			0.030		0.029
			(0.045)		(0.044)			(0.042)		(0.042)
Effect After 10 Years			0.038		0.039			0.056		0.051
			(0.060)		(0.059)			(0.054)		(0.055)
State and Year Fixed Effects	X	X	X	X	X	X	X	X	X	X
Social and Political Controls		X	X	X	X		X	X	X	X
Second Order Outcome Controls				X	X				X	X
Number of Observations	477	477	477	377	377	520	521	522	420	420
Number of States	50	50	50	50	50	50	50	50	50	50
Years	1997-2016	1997-2016	1997-2016	1997-2016	1997-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016

Notes: Each column in the table comes from a separate regression. Columns 1, 2, 4, 6, 7, and 9 show results from models using a single dummy indicator of treatment (Model 1), and Columns 3, 5, 8, and 10 show results from semi-parametric models (Model 2) that estimate an immediate jump (Agency Fees Prohibited) as well as a post-trend (Years Since Agency Fees Prohibited). Model 2 also checks for the existence of linear pre-trends (Ever Prohibited Agency Fees*Year). All estimates include state and year fixed effects. Social and political controls include underlying dynamics of social and political ideologies and Republican control of state government. Second order outcome controls include charter policies and per pupil expenditures. Robust standard errors clustered at the state level are in parentheses. * p<.05, ** p<.01, *** p<.001.

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Appendix A.

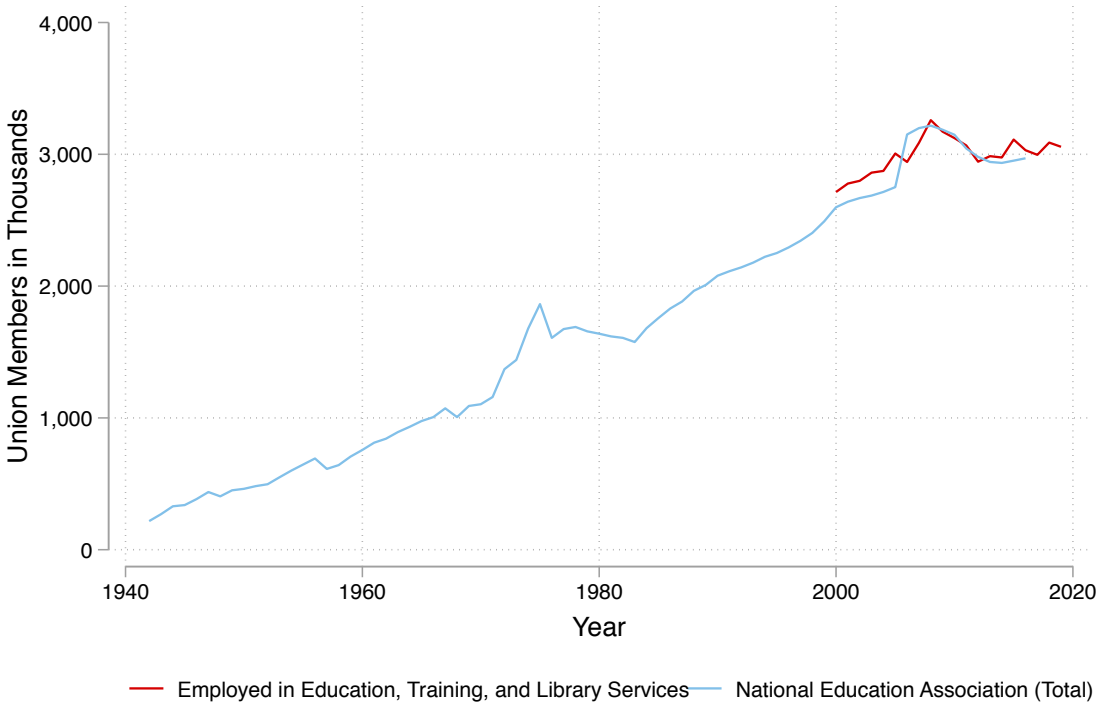
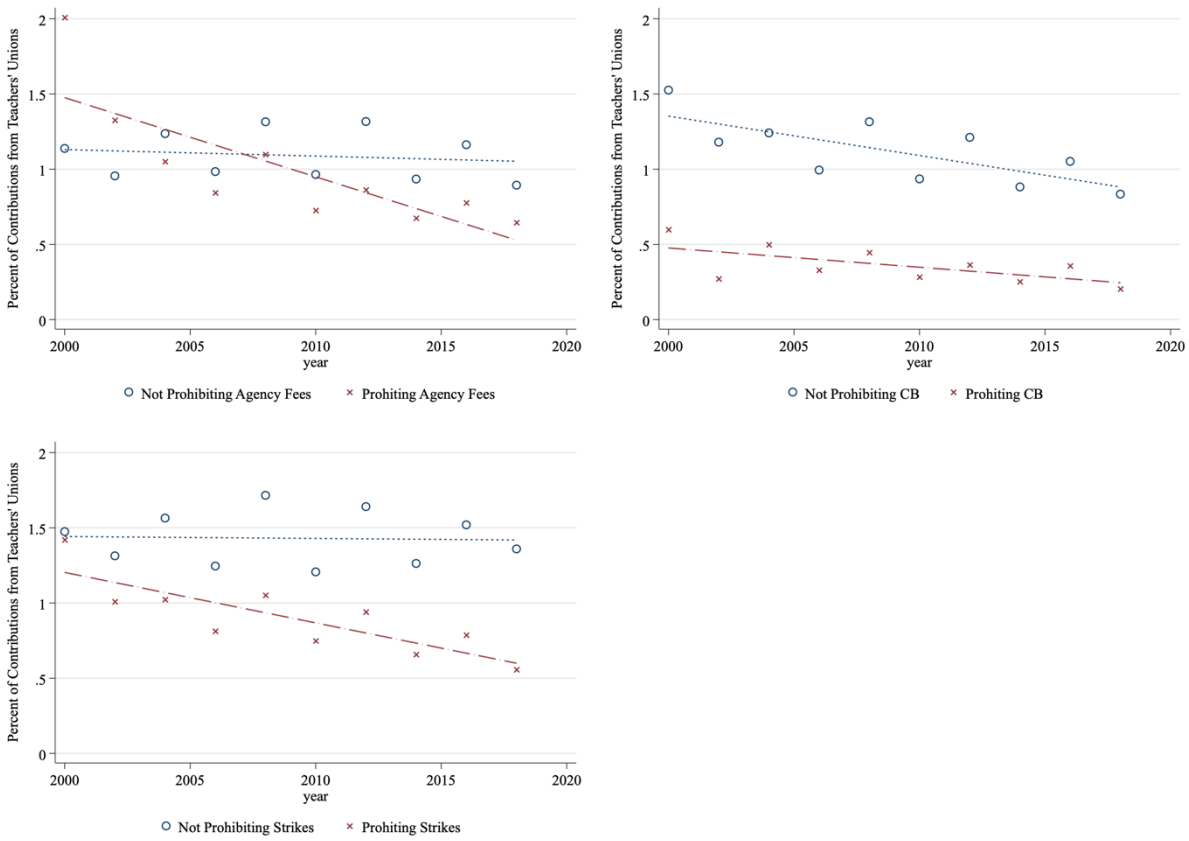


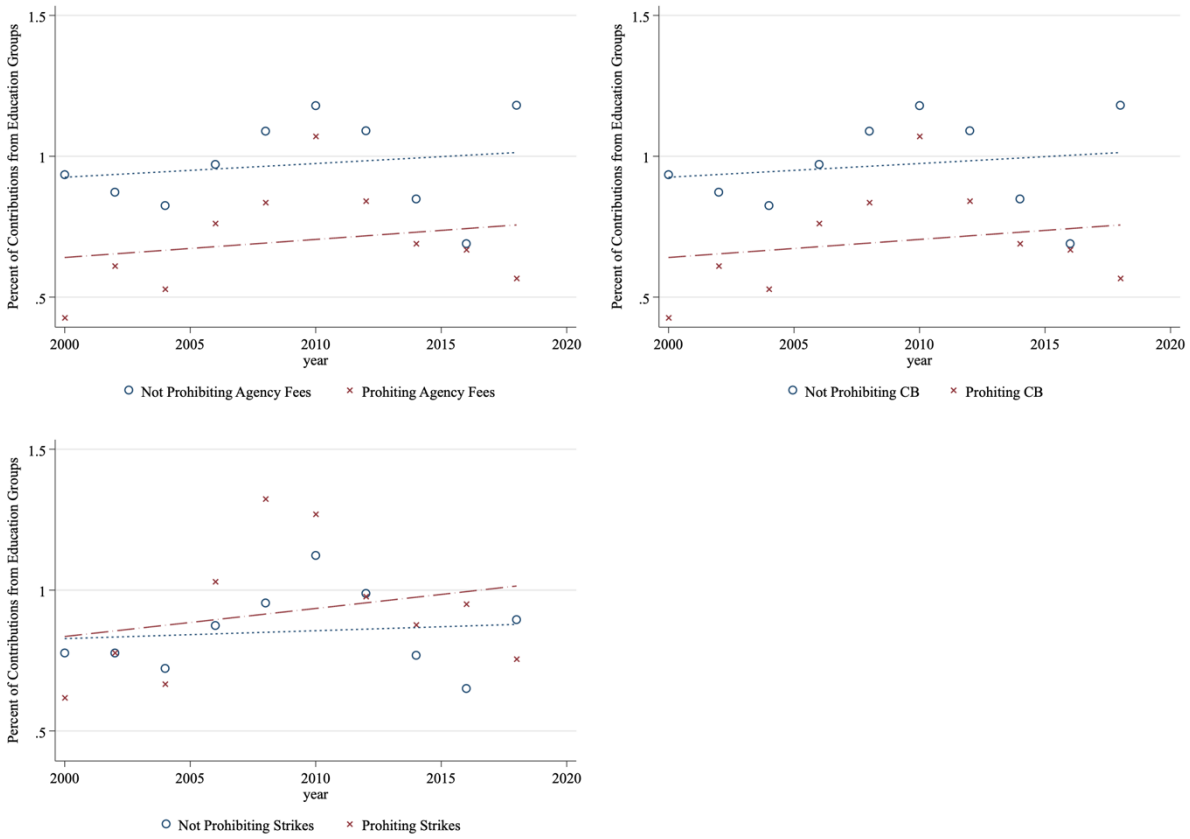
Figure A1. National Trends in Union Membership in the National Education Association and All Employed Education, Training, and Library Workers

Note: NEA membership exceeds employment in Education, Training and Library workers in some years. This is likely for two reasons (1)The National Education Association organizes workers like nurses that are outside of this employment categorization (2) Total NEA membership also includes retired teachers that are not actively employed.



Notes: Data are derived from the National Institute on Money in Politics (NIMP). Nine states (Alabama, Louisiana, Maryland, Mississippi, Nebraska, New Jersey, North Dakota, Virginia, and Wyoming) are missing data between 2000 and 2018. None of these states experience policy changes in the time period of the present study. Because of the substantial level of missingness, often for multiple, consecutive years, I removed these states from analysis to provide the clearest picture of change over time.

Figure A2. Percent of Contributions from Teachers' Unions in States with and without Restrictive Labor Policies



Notes: Data are derived from the National Institute on Money in Politics (NIMP). Nine states (Alabama, Louisiana, Maryland, Mississippi, Nebraska, New Jersey, North Dakota, Virginia, and Wyoming) are missing data between 2000 and 2018. None of these states experience policy changes in the time period of the present study. Because of the substantial level of missingness, often for multiple, consecutive years, I removed these states from analysis to provide the clearest picture of change over time.

Figure A3. Percent of Contributions from non-Teachers' Union Education Groups in States with and without Restrictive Labor Policies

Table A1. The Effects of Laws Prohibiting Agency Fees on Education Reform Policies

	Charter Law			Teach For America			Private School Choice (Vouchers or Tax Credits)			Teacher Performance Pay		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Agency Fees Prohibited	0.061	0.046	0.225*	0.150	0.123	0.093	-0.058	-0.117	-0.035	0.076	0.035	0.011
	(0.099)	(0.102)	(0.108)	(0.109)	(0.099)	(0.106)	(0.105)	(0.114)	(0.077)	(0.123)	(0.132)	(0.095)
Ever Prohibited Agency Fees* Year			-0.023***			0.002			-0.006			0.005
			(0.006)			(0.010)			(0.008)			(0.012)
Years Since Agency Fees Prohibited			0.023***			-0.008			0.021*			0.002
			(0.006)			(0.011)			(0.008)			(0.011)
Effect After 5 Years			0.338**			0.054			0.068			0.023
			(0.125)			(0.134)			(0.083)			(0.102)
Effect After 10 Years			0.451**			0.015			0.172			0.034
			(0.146)			(0.174)			(0.105)			(0.135)
% Change Relative to Pre-Treatment	15%	12%		38%	31%		-15%	-30%		19%	9%	
State and Year Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X
Social and Political Controls		X	X		X	X		X	X		X	X
Number of Observations	1,450	1,450	1,450	1,450	1,450	1,450	1,450	1,450	1,450	1,350	1,350	1,350
Number of States	50	50	50	50	50	50	50	50	50	50	50	50
Years	1990-2018	1990-2018	1990-2018	1990-2018	1990-2018	1990-2018	1990-2018	1990-2018	1990-2018	1990-2017	1990-2017	1990-2017

Notes: Each column in the table comes from a separate regression. Columns 1, 2, 4, 5, 7, 8, 10, and 11 show results from models using a single dummy indicator of treatment (Model 1), and columns 3, 6, 9, and 12 show results from semi-parametric models (Model 2) that estimate an immediate jump (Agency Fees Prohibited) as well as a post-trend (Years Since Agency Fees Prohibited). Model 2 also checks for the existence of linear pre-trends (Ever Prohibited Agency Fees*Year). All estimates include state and year fixed effects. Controls include underlying dynamics of social and political ideologies and Republican control of state government. Robust standard errors clustered at the state level are in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table A2. The Effects of Laws Prohibiting Agency Fees on Student Achievement Subgroups

	All		Boys		Girls		Free/Reduced Lunch Eligible		Not Free/Reduced Lunch Eligible		White Students		Black Students		Hispanic Students	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Agency Fees Prohibited	-0.047	-0.015	-0.052	-0.023	-0.044	-0.010	-0.027	0.008	-0.051	-0.018	-0.038	0.000	-0.047	0.010	-0.062	0.034
	(0.032)	(0.043)	(0.039)	(0.047)	(0.031)	(0.038)	(0.046)	(0.059)	(0.032)	(0.042)	(0.036)	(0.043)	(0.045)	(0.046)	(0.064)	(0.073)
Ever Prohibited Agency Fees* Year		-0.005		-0.005		-0.006		-0.005		-0.006		-0.006		-0.009*		-0.013**
		(0.004)		(0.004)		(0.004)		(0.004)		(0.004)		(0.004)		(0.003)		(0.005)
Years Since Agency Fees Prohibited		0.005		0.005		0.006		0.006		0.006		0.005		0.009**		0.008
		(0.004)		(0.004)		(0.004)		(0.004)		(0.004)		(0.004)		(0.003)		(0.005)
Effect After 5 Years		0.012		0.004		0.020		0.040		0.014		0.026		0.054		0.072
		(0.056)		(0.060)		(0.050)		(0.073)		(0.056)		(0.055)		(0.050)		(0.085)
Effect After 10 Years		0.039		0.032		0.050		0.072		0.045		0.052		0.098		0.110
		(0.072)		(0.077)		(0.064)		(0.090)		(0.073)		(0.071)		(0.058)		(0.103)
State and Year Fixed Effects	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Social and Political Controls																
Number of Observations	645	645	595	595	595	595	565	565	515	515	645	645	554	554	501	501
Number of States	50	50	50	50	50	50	50	50	50	50	50	50	50	50	50	50
Years	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016	1991-2016

Notes: Each column in the table comes from a separate regression. The outcome is state average, standardized NAEP scores across fourth and eighth grade math and reading. Odd numbered columns use a single dummy indicator of treatment (Model 1), and even numbered columns show results from semi-parametric models (Model 2) that estimate an immediate jump (Agency Fees Prohibited) as well as a post-trend (Years Since Agency Fees Prohibited). Model 2 also checks for the existence of linear pre-trends (Ever Prohibited Agency Fees*Year). All estimates include state and year fixed effects. Robust standard errors clustered at the state level are in parentheses. * p<.05, ** p<.01, *** p<.001.

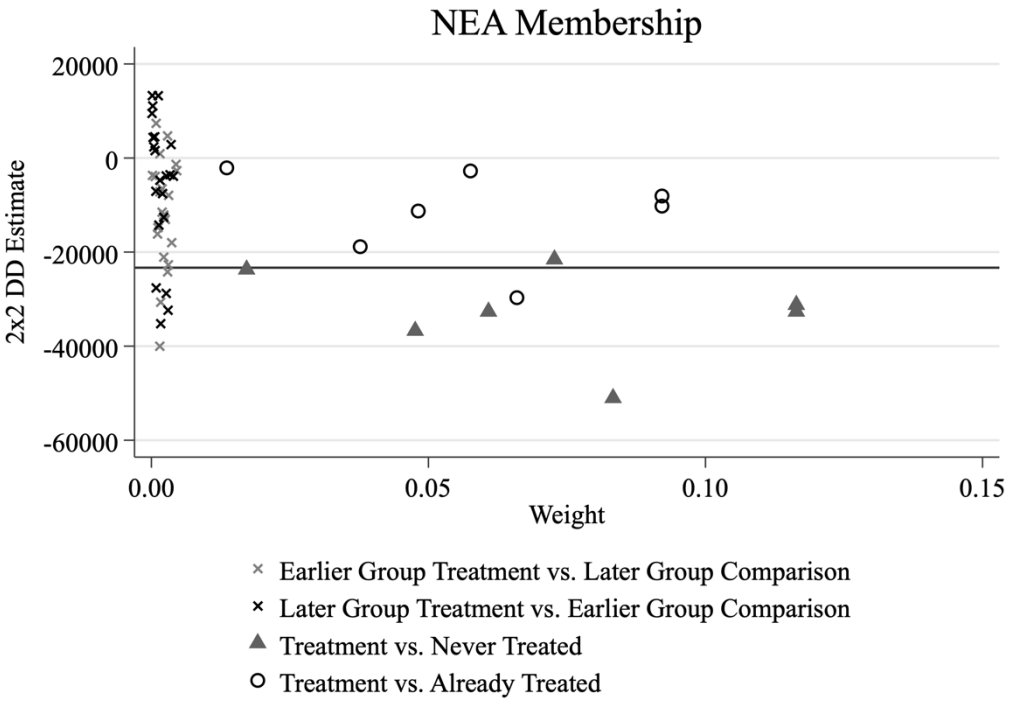
Table A3. Robustness Checks

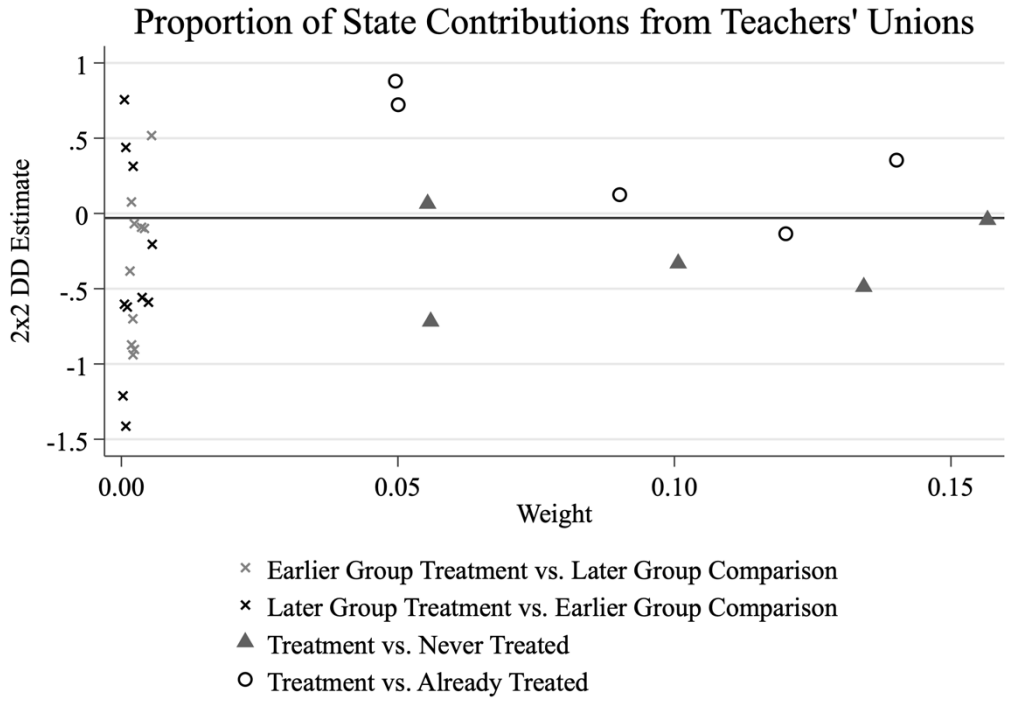
	Baseline Model	Drop AZ	Drop IN	Drop KY	Drop MI	Drop OK	Drop TX	Drop WV	Drop WI	Drop +5 Years Pre/Post	Drop Never Treated	TX and AZ Always Treated	Region Time Trends
Effect of Agency Fee Prohibitions on…													
NEA Membership	-24238***	-24811***	-26563***	-24396***	-24948***	-25506***	-23170**	-24920***	-21163***	-16565**	-12867**	-22411*	-18494**
	(6333)	(6817)	(6473)	(6423)	(6628)	(6808)	(7381)	(6452)	(5556)	(5001)	(4588)	(8623)	(5332)
Contributions from Teachers' Unions	0.037	-0.037	0.014	0.042	0.134	0.007	0.038	0.076	0.010	-0.175	0.330	-0.041	0.151
	(0.162)	(0.167)	(0.162)	(0.166)	(0.149)	(0.166)	(0.164)	(0.180)	(0.190)	(0.172)	(0.227)	(0.555)	(0.189)
Teacher Salary, 2016$	-1104	-845	-917	-1103	-708	-2089**	-1302	-1131	-1113	154	-1497	-1032	-1046
	(989)	(1200)	(1122)	(990)	(959)	(696)	(1102)	(991)	(1142)	(595)	(958)	(624)	(835)
Expenditures/Pupil, 2016$	-1034***	-988***	-1198***	-1045***	-1028***	-922***	-1132***	-1032***	-1017***	-557	-650*	-1053***	-740**
	(241)	(273)	(219)	(244)	(251)	(259)	(252)	(245)	(258)	(304)	(241)	(212)	(227)
Pupil/Teacher Ratio	1.234**	0.822**	1.277**	1.243**	1.383**	1.207*	1.307**	1.274**	1.394**	0.191	1.203**	0.774***	1.205***
	(0.407)	(0.244)	(0.470)	(0.407)	(0.420)	(0.524)	(0.436)	(0.419)	(0.435)	(0.154)	(0.414)	(0.178)	(0.318)
Education Reform Policy	0.022	0.044	0.007	-0.004	0.043	-0.075	0.042	0.035	0.075	-0.026	0.006	0.078	0.017
	(0.108)	(0.131)	(0.122)	(0.110)	(0.121)	(0.074)	(0.114)	(0.115)	(0.111)	(0.111)	(0.111)	(0.053)	(0.113)
Pooled NAEP Scores	-0.047	-0.073*	-0.051	-0.046	-0.043	-0.026	-0.057	-0.050	-0.040	0.002	-0.050	-0.088***	-0.032
	(0.032)	(0.029)	(0.036)	(0.032)	(0.036)	(0.027)	(0.033)	(0.034)	(0.036)	(0.028)	(0.035)	(0.024)	(0.038)
4th Grade Reading Scores	-0.085*	-0.120**	-0.081	-0.083	-0.081	-0.060	-0.095*	-0.092*	-0.076	-0.023	-0.087	-0.136***	-0.066
	(0.042)	(0.035)	(0.047)	(0.042)	(0.047)	(0.035)	(0.044)	(0.045)	(0.048)	(0.040)	(0.045)	(0.027)	(0.045)
4th Grade Math Scores	-0.027	-0.038	-0.038	-0.024	-0.013	-0.015	-0.041	-0.032	-0.012	0.029	-0.046	-0.059*	-0.022
	(0.032)	(0.038)	(0.032)	(0.032)	(0.034)	(0.034)	(0.030)	(0.033)	(0.035)	(0.036)	(0.036)	(0.029)	(0.030)
8th Grade Reading Scores	-0.015	-0.034	-0.013	-0.015	-0.018	0.005	-0.016	-0.013	-0.021	-0.003	-0.011	-0.035	-0.006
	(0.026)	(0.032)	(0.026)	(0.026)	(0.032)	(0.013)	(0.026)	(0.029)	(0.035)	(0.025)	(0.028)	(0.025)	(0.027)
8th Grade Math Scores	-0.013	-0.026	-0.019	-0.013	-0.007	0.010	-0.028	-0.017	-0.011	0.020	-0.015	-0.050	0.006
	(0.031)	(0.037)	(0.033)	(0.031)	(0.036)	(0.025)	(0.030)	(0.034)	(0.038)	(0.018)	(0.034)	(0.029)	(0.039)

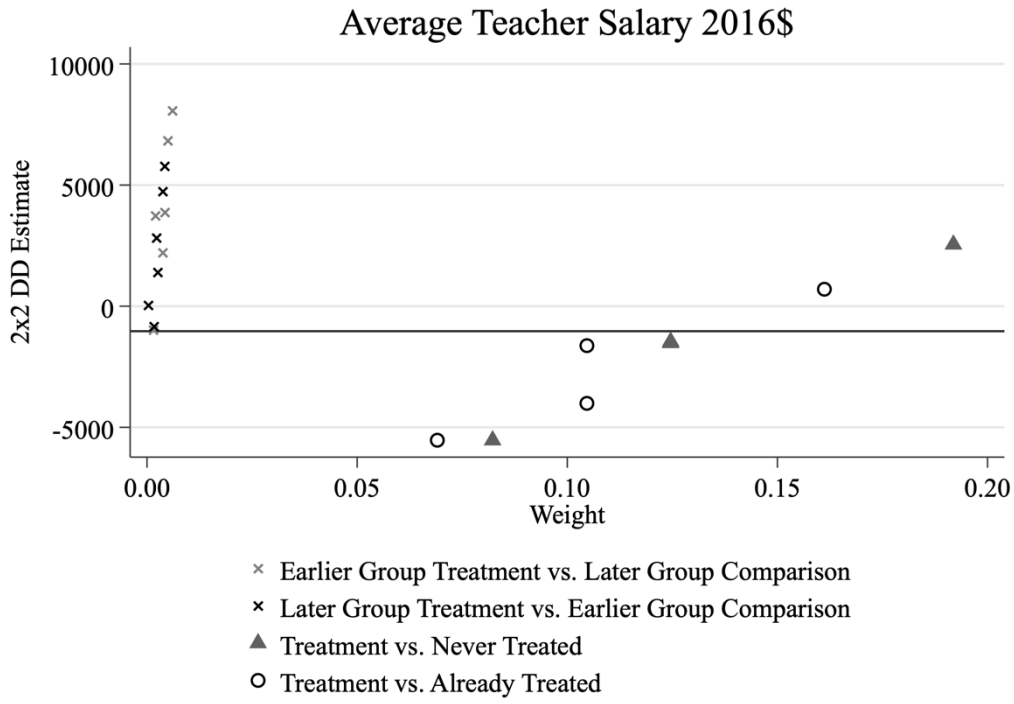
Notes: Each column in the table comes from a separate regression. Models use a single dummy indicator of treatment (Model 1), All estimates include state and year fixed effects. Models include no controls, except those examining NEA Membership control for NEA/AFT merger years, AFT stronghold status, and total student enrollment. See Table 1 for years examined for each outcome. Robust standard errors clustered at the state level are in parentheses. * p<.05, ** p<.01, *** p<.001.

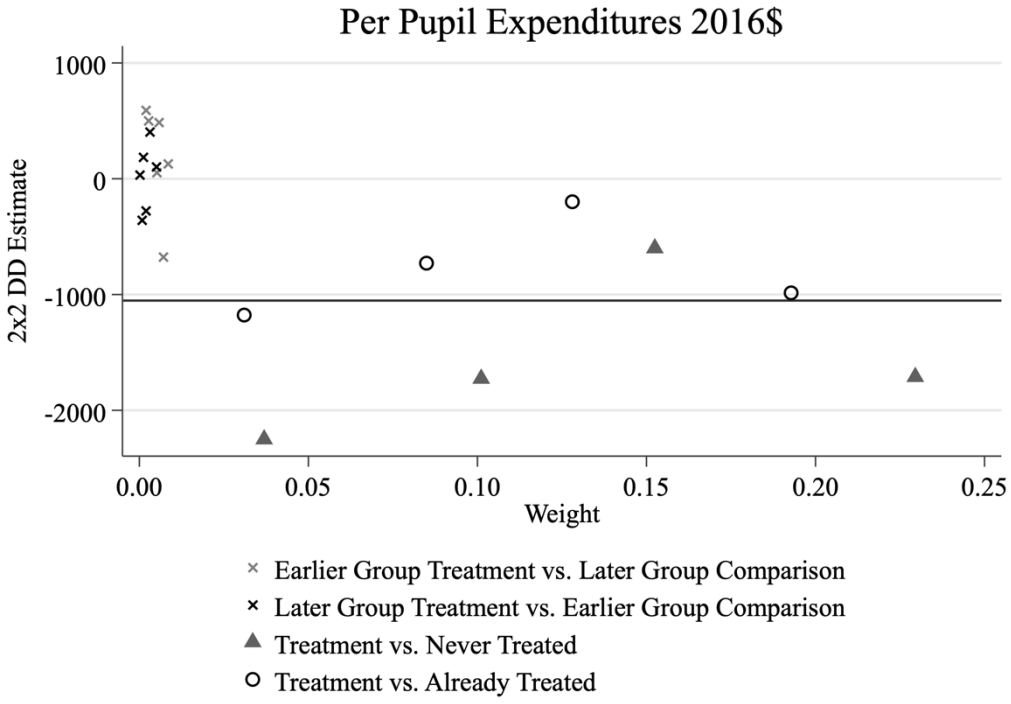
Appendix B. Graphs of 2x2 Differences-in-Differences Estimates of the Effects of Prohibiting Agency Fees Against Their Weights

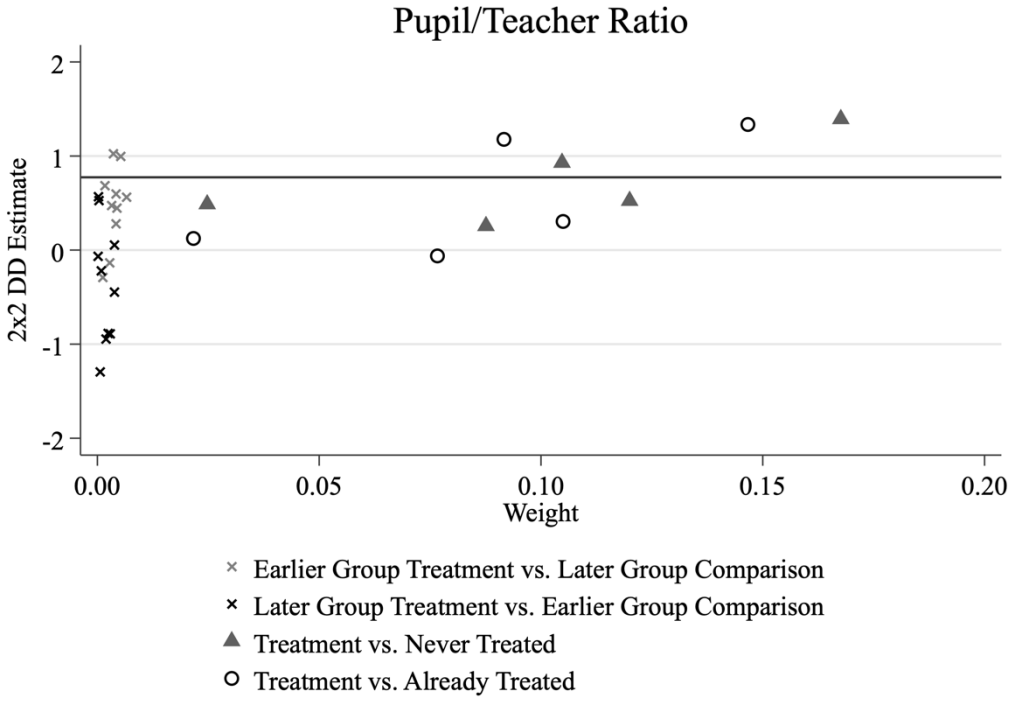
The following figures help to address concerns about unit-specific treatment effect heterogeneity biasing results due to the weighting of 2x2 components. The figures plot each of 2x2 differences-in-differences (DD) components from Goodman-Bacon's (2018) decomposition theorem against their weight from the fully parametric analysis above. The figures display the average DD estimate and total weight on each of the four general types of comparisons, as noted below. The black line shows the two-way fixed effects estimate, which is the average of the *y*-axis values weighted by their *x*-axis value.

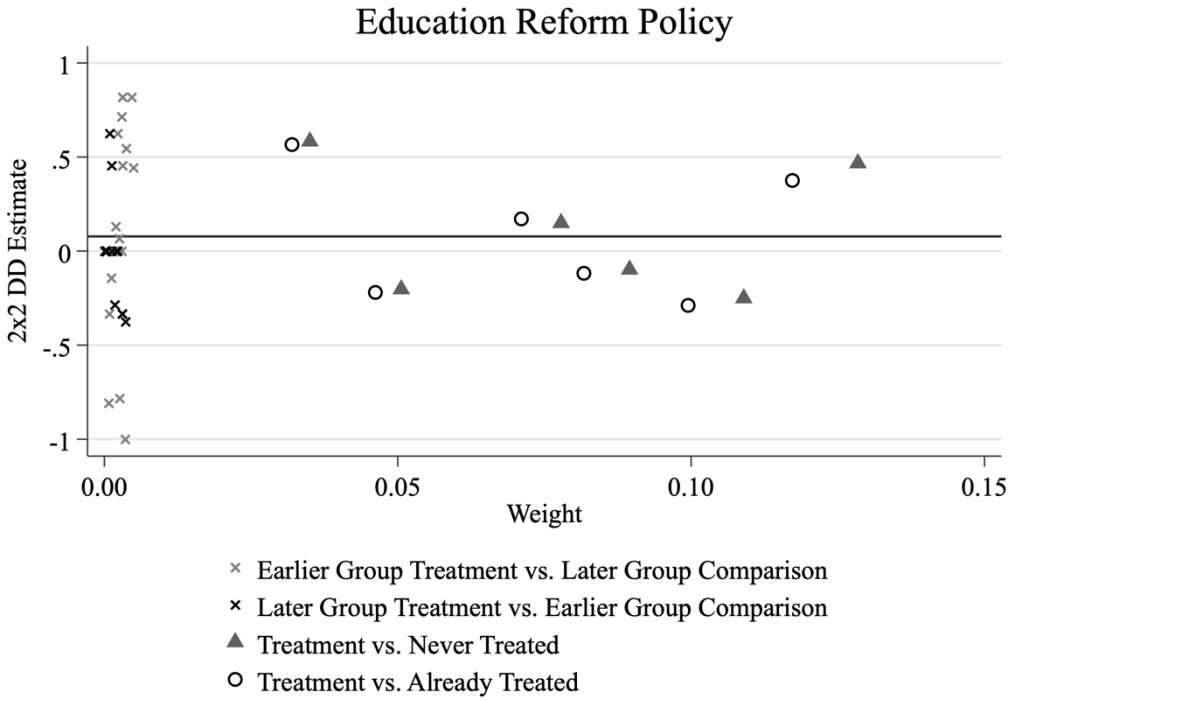


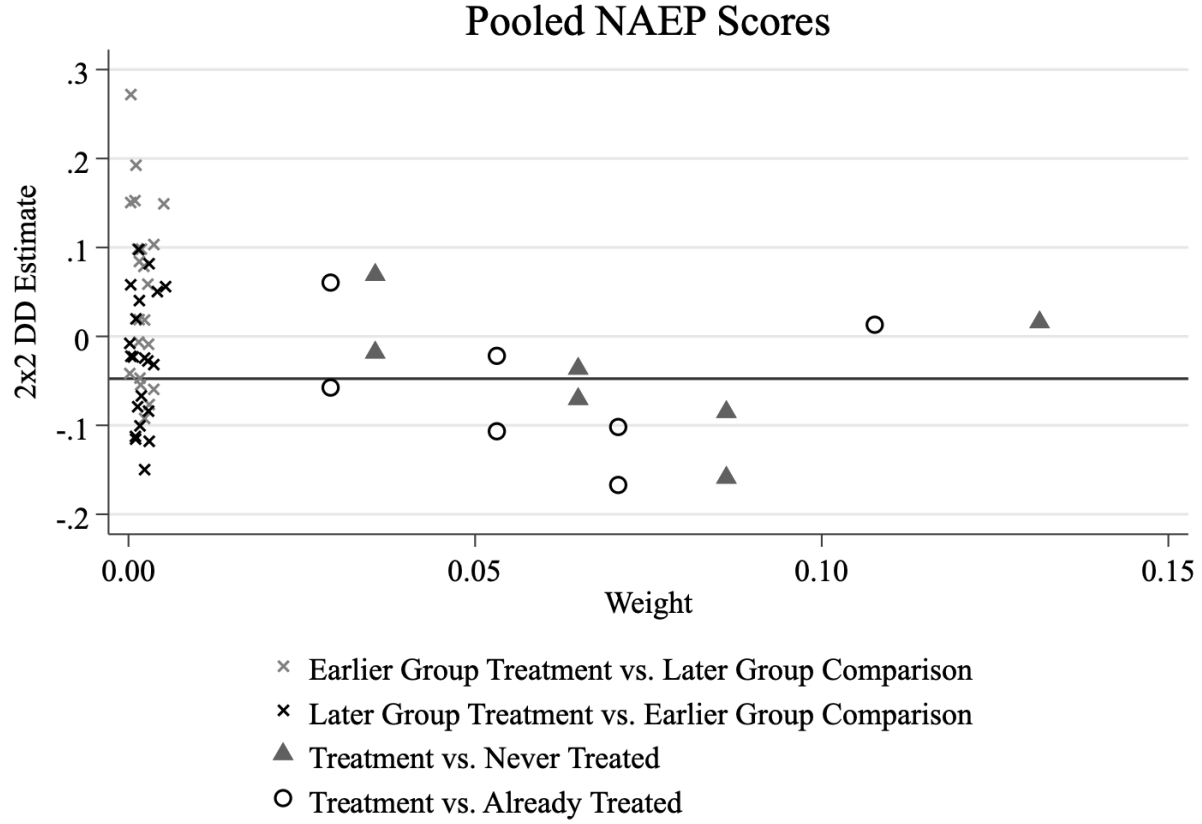

Proportion of State Contributions from Teachers' Unions
2x2 DD Estimate
1
.5
0
-.5
-1
-1.5
0.00
0.05
0.10
0.15
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated

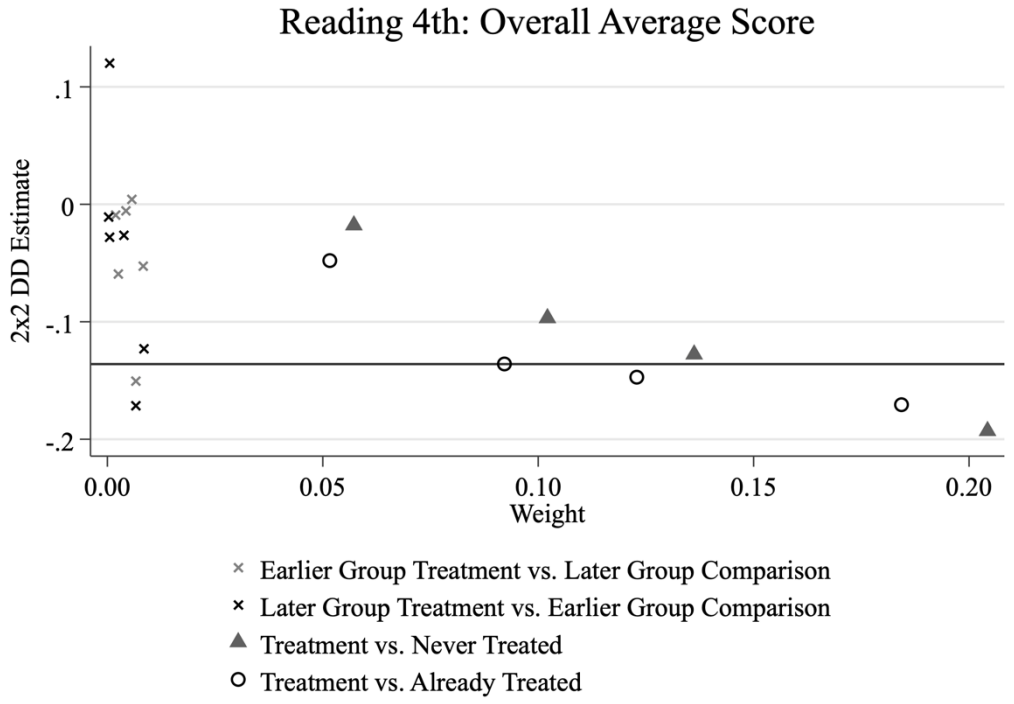

Average Teacher Salary 2016$
2x2 DD Estimate
10000
5000
0
-5000
0.00
0.05
0.10
0.15
0.20
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated

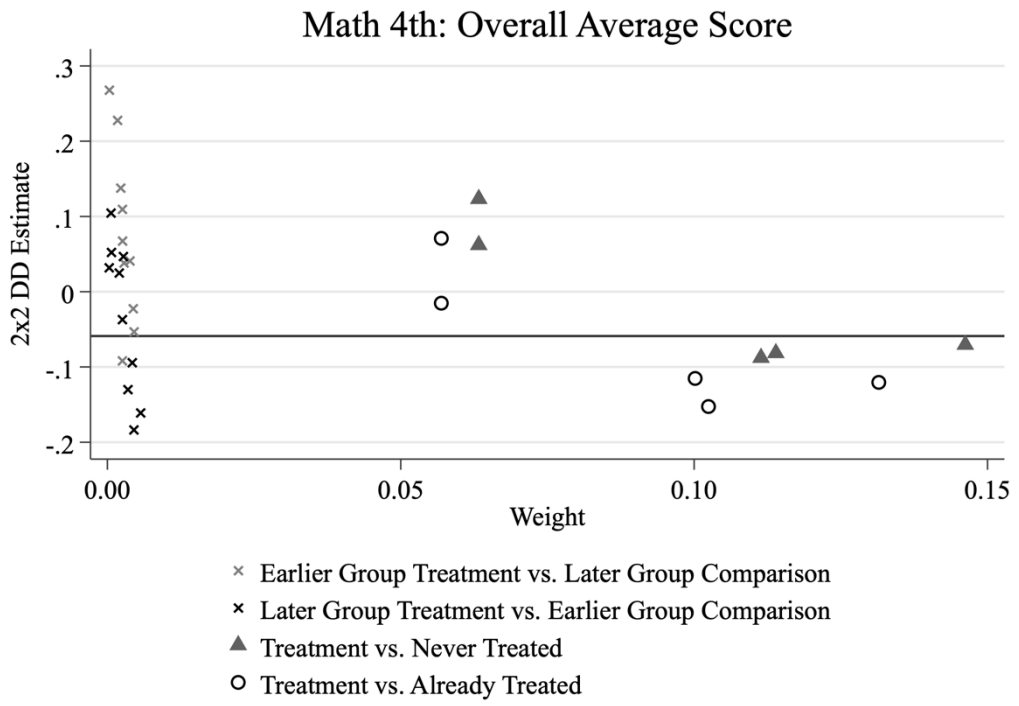

Per Pupil Expenditures 2016$
2x2 DD Estimate
1000
0
-1000
-2000
0.00
0.05
0.10
0.15
0.20
0.25
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated

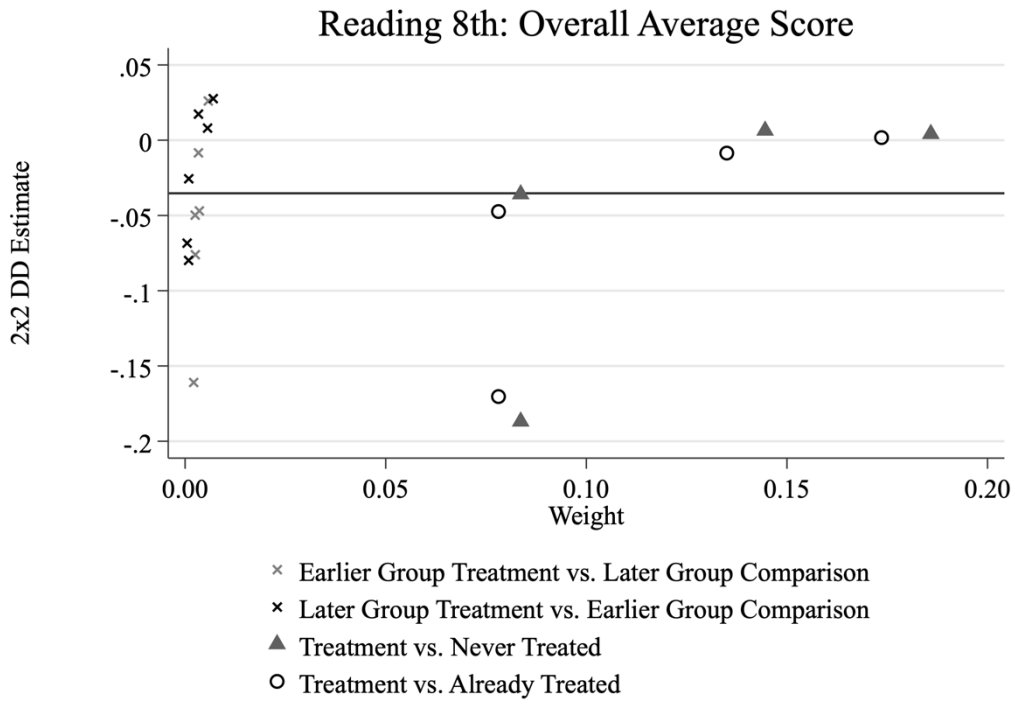

Pupil/Teacher Ratio
2x2 DD Estimate
2
1
0
-1
-2
0.00
0.05
0.10
0.15
0.20
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated

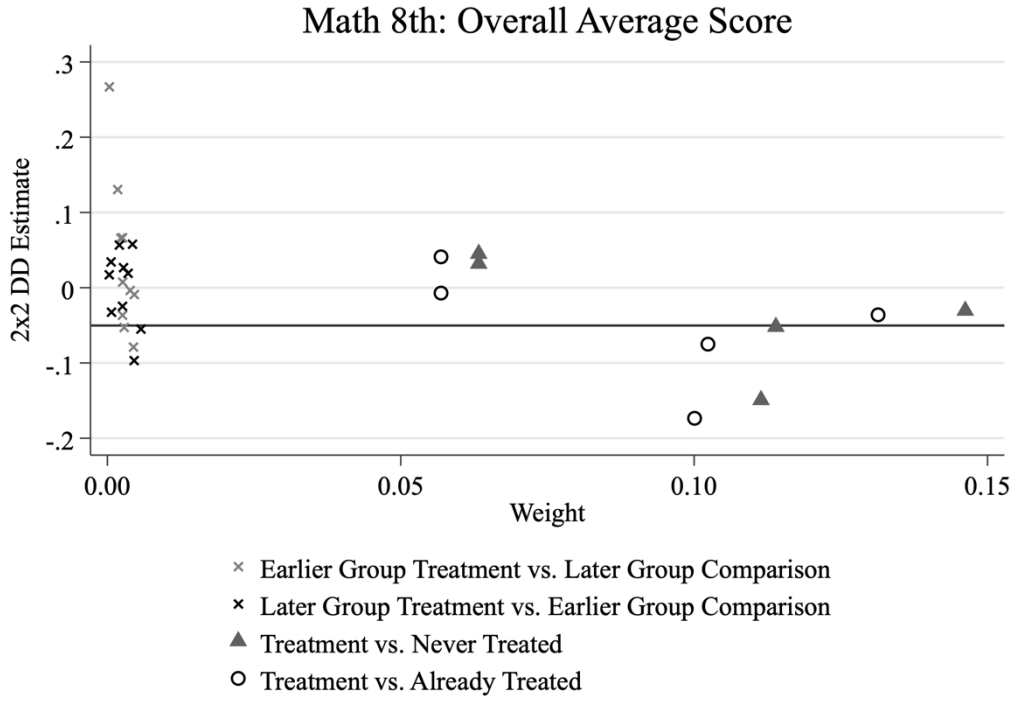

Education Reform Policy
2x2 DD Estimate
1
.5
0
-.5
-1
0.00
0.05
0.10
0.15
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated


Pooled NAEP Scores
2x2 DD Estimate
.3
.2
.1
0
-.1
-.2
0.00
0.05
0.10
0.15
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated


Reading 4th: Overall Average Score
2x2 DD Estimate
.1
0
-.1
-.2
0.00
0.05
0.10
0.15
0.20
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated


Math 4th: Overall Average Score
2x2 DD Estimate
.3
.2
.1
0
-.1
-.2
0.00
0.05
0.10
0.15
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated


Reading 8th: Overall Average Score
2x2 DD Estimate
.05
0
-.05
-.1
-.15
-.2
0.00
0.05
0.10
0.15
0.20
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated


Math 8th: Overall Average Score
2x2 DD Estimate
.3
.2
.1
0
-.1
-.2
0.00
0.05
0.10
0.15
Weight
Earlier Group Treatment vs. Later Group Comparison
Later Group Treatment vs. Earlier Group Comparison
Treatment vs. Never Treated
Treatment vs. Already Treated