

The Impact of Right-to-Work Laws on Educational Outcomes*

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Abstract

In this paper, I examine how Right-to-Work (RTW) laws, which prohibit mandatory union fees, affect student achievement by leveraging variation in policy adoption across states between 2011 and 2017. Using a border-county research design and newly available data from the Stanford Education Data Archive, I compare achievement and school spending changes in counties where RTW laws were implemented to changes in neighboring counties in non-RTW states. To address staggered adoption and limited treatment units, I use modern difference-in-differences methods—regression adjustment, synthetic difference-in-differences—and wild cluster bootstrap inference. I find that RTW laws increased student achievement by 0.06 standard deviations in math and reading, with effects growing to a peak of 0.15 – 0.19 standard deviations several years after adoption. These achievement gains occurred despite substantial reductions in school resources: by the fourth year after RTW adoption, per-pupil spending fell by over \$600 (total current spending) in the short run, with long-run reductions reaching up to \$1,436 per pupil. The results are robust across various specifications and estimation approaches, though pre-trend analyses suggest more caution is warranted for reading estimates. These findings challenge the view that reducing union authority necessarily harms educational quality, while highlighting the complex relationship between labor laws, school resources, and student outcomes.

*Preliminary and incomplete. Please do not cite or distribute without permission. All errors are my own. Correspondence: strayerm@purdue.edu.

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1 Introduction

Right-to-work (RTW) laws fundamentally alter how teachers' unions operate in public education by prohibiting mandatory union fees. Between 2011 and 2017, several states enacted such legislation, marking a significant shift in public sector labor relations that presaged the Supreme Court's 2018 *Janus v. AFSCME* decision that ruled that mandatory fees among even non-union members (generally called "agency fees") violated the First Amendment right to free speech for public-sector workers. When *Janus* was decided, the New York Times reported that teachers' unions could lose up to a third of their members and funding, potentially weakening their ability to advocate for educational resources and improved working conditions that they argued benefit students (Goldstein and Green 2018). These predictions about union decline following RTW laws and *Janus* set up a crucial empirical question about how such changes would affect student outcomes. While recent studies have examined these changes, methodological challenges in isolating causal effects persist.

This paper leverages variation in RTW adoption across neighboring counties and employs modern difference-in-differences methods to provide new evidence on how restricting union power affects educational outcomes. Understanding these effects is crucial given that teachers' unions are among the largest public sector organizations, and their actions affect educational opportunities for millions of public school students.

Proponents of RTW laws argue that weakening unions could improve student outcomes by giving administrators more flexibility to implement reforms and allocate resources efficiently, while critics contend that diminishing teachers' collective voice could reduce their ability to advocate for smaller class sizes, better working conditions, and other factors that support effective instruction. With public expenditures on K-12 education exceeding \$950 billion annually, in constant 2022-2023 dollars (National Center for Education Statistics 2024), understanding how institutional changes like RTW laws affect student achievement has important implications for both educational policy and public finance.

Prior to *Janus*, 27 states had implemented RTW laws (Gihleb, Giuntella and Tan 2024). The ruling primarily affected public sector workers in the remaining 23 states and the District of Columbia, where approximately 5.6 million public sector employees were covered by collective bargaining agreements in 2018 (Hirsch, Macpherson and Even 2024). In these states, unions had been permitted to negotiate contracts requiring non-members to pay agency fees to cover collective bargaining costs, though the Supreme Court ruling *Abood v. Detroit* narrowly defined the allowable uses of dues collected by non-members.

In this paper, I examine how Right-to-Work (RTW) laws enacted between 2011 and 2017 affected educational outcomes in four states: Wisconsin (2011), Michigan (2013), West Virginia (2016), and Kentucky (2017). Following Gihleb, Giuntella and Tan (2024), I use a border-county research design that compares changes in educational outcomes between counties in RTW-adopting states and their neighboring counties in states that never adopted RTW laws. This approach exploits the discrete policy change at state borders while leveraging the spatial continuity of underlying economic and demographic factors that typically vary smoothly across geographic boundaries.

The border-county design offers several advantages for causal identification. Counties adjacent to state borders often share similar local labor markets, face common regional economic shocks, and display comparable baseline characteristics and pre-treatment trajectories ((Gihleb,

Giuntella and Tan 2024)). These features make neighboring counties more plausible counterfactuals than arbitrary non-RTW states, even though—as documented in Tables 1 and 2—meaningful differences exist between treatment and control counties prior to RTW adoption. The key identifying assumption is not that border counties are identical, but rather that they would have experienced parallel trends in educational outcomes absent the policy change.

I make three main contributions to our understanding of labor laws and educational outcomes. First, while most previous work focuses primarily on the educational effects of policies that facilitate unionization, I provide new evidence on the effects of laws that restrict union authority. This distinction is important because the impacts of strengthening versus weakening union power may be asymmetric. For example, while stronger unions might improve student outcomes by negotiating for smaller class sizes and better working conditions that attract and retain effective teachers, weakening unions through RTW laws could either enhance achievement by giving administrators more flexibility to implement reforms or harm it by reducing teachers’ ability to advocate for resources they believe support learning. Second, I leverage newly available data from the Stanford Education Data Archive (SEDA) that enables analysis of student achievement across states and over time. The SEDA data addresses a fundamental challenge in cross-state education research by providing comparable achievement measures despite states’ use of different assessments. Third, I employ modern difference-in-differences methods that account for treatment effect heterogeneity and staggered policy adoption, addressing recent methodological critiques of two-way fixed effects estimators. I additionally implement synthetic difference-in-differences estimation as a robustness check, which further addresses potential violations of the parallel trends assumption by optimally weighting control units to better match pre-treatment trends.

The remainder of the paper is organized as follows. In Section 2, I provide institutional background on RTW laws and teachers’ unions. In Section 3, I present a theoretical framework for understanding how changes in union power could affect educational resources. I review the evolution of research on union impacts in education in Section 4, followed by Section 5, where I describe the data. In Section 6, I describe the empirical strategy, and I present the main results in Section 7. I conclude with Section 8.

2 Institutional Background

2.1 Right-to-Work Laws: Origins and Development

The 1947 Taft-Hartley Act established the legal framework for RTW laws, which amended the Wagner Act to address concerns about union power. Early research examining these laws’ effects began with Meyers (1955), who studied their impact on union security in Texas. Subsequent decades of research consistently show that RTW laws reduce unionization rates and wages, with particularly strong effects in heavily unionized sectors (Ellwood and Fine 1987; Fortin, Lemieux and Lloyd 2023).

Before Taft-Hartley, unions could negotiate “closed-shop” agreements, where only union members could be hired, and “union-shop” agreements, which required workers to join the union after being hired. Taft-Hartley outlawed closed shops nationwide and allowed states to prohibit union-shop agreements through RTW laws, ensuring workers could not be required to join or financially

support a union as a condition of employment.

RTW laws emerged in three waves. Figure 1 maps RTW adoption across the United States, with states color-coded by RTW status: gray-blue for states that never adopted RTW laws, light blue for states that implemented RTW before 2011, and darker blue for states that adopted RTW between 2011 and 2017. Each state displays its year of RTW implementation. The first wave began shortly after Taft-Hartley, with southern states leading adoption by 1947. A second wave followed in the 1950s and 1960s, expanding RTW's reach into western and plains states. By 1963, nineteen states had RTW laws, most passed in response to union gains during WWII and growing business opposition to organized labor. A third wave began in the 1990s and continued through the 2018 *Janus* ruling.

2.2 Recent Right-to-Work Laws: 2011-2017

The 2010s saw a significant wave of RTW laws, with states implementing measures that reshaped labor policy and union influence in diverse ways. Wisconsin's 2011 Act 10 was particularly far-reaching, restricting unions of certain public-sector employees, including K-12 teachers, to bargaining solely over basic wage increases capped at the rate of inflation. The law also prohibited mandatory union fees, required annual union recertification elections, and ended automatic dues collection.^{1,2} Existing labor contracts were allowed to remain in effect until their expiration, after which the new restrictions would apply, creating variation that Baron (2018) and others have leveraged to identify the law's causal effects on educational outcomes.

Michigan's 2013 RTW law followed a similar framework to Wisconsin's. Signed by Republican Governor Rick Snyder and effective March 28, 2013, the law barred unions from requiring workers to pay dues or fees as a condition of employment in both public and private sectors, though existing labor contracts remained in effect until expiration.³ Like Wisconsin, Michigan notably exempted police and firefighter unions, continuing a pattern of treating public safety workers differently in labor legislation.⁴

West Virginia enacted its RTW law in February 2016 when a Republican-led legislature overrode the Democratic governor's veto. The law, which prohibits requiring union membership or payment of dues as conditions of employment, applies broadly to both public and private sector workers, including K-12 teachers. Initially set to take effect on July 1, 2016, the law faced immediate and prolonged legal challenges. A preliminary injunction by Circuit Judge Jennifer Bailey in February 2017 prevented its enforcement until the state Supreme Court reversed this decision

¹In 2015, Wisconsin extended RTW provisions to private sector workers.

²In December 2024, a Dane County Circuit Court judge declared significant portions of Wisconsin's Act 10 unconstitutional under the state's equal protection clause, thereby reinstating comprehensive collective bargaining rights for public sector employees. This decision nullified over 60 sections of the 2011 law, including those that capped wage increases and mandated annual union recertification votes. However, the ruling did not address Act 10's prohibitions on mandatory union fees or automatic dues collection, leaving those provisions intact. The implementation of this decision is pending appeal, which may delay its effect.

³In that month (March 2013), 58 collective bargaining agreements were finalized in Michigan, accounting for a quarter of all agreements signed in the first year after the RTW law's passage and exceeding the monthly average for the period by 35% (Spalding 2014).

⁴Michigan made history on March 24, 2023, by becoming the first state in almost six decades to overturn its right-to-work law. Democratic Governor Gretchen Whitmer signed legislation that reinstated unions' authority to mandate financial contributions from the workers they represent. The repeal took effect on February 13, 2024.

in September 2017, allowing the law to be enforced from that point onward. Further complications arose in February 2019 when the circuit court ruled that the ban on mandatory fees violated unions' constitutional rights of association, property, and liberty. While the state secured a stay on this ruling, legal uncertainty persisted until April 2020, when the Supreme Court of Appeals of West Virginia ultimately upheld the law's constitutionality in *Morrisey v AFL-CIO*.

Kentucky became the 27th state to adopt an RTW law on January 9, 2017. The legislation ensured that employees, including K-12 teachers, could not be required to join a union or pay dues as a condition of employment. Additionally, the state repealed its prevailing wage law, which mandated specific wage rates for state and local government construction projects. Senate Bill 6 also passed, requiring public employees to provide annual written consent for union dues deductions. Together, these measures represented a significant shift in Kentucky's labor policy framework, with implications for both public and private sector employees.

2.3 Teachers' Unions: Evolution and Contemporary Dynamics

The evolution of teachers' unions reflects broader changes in American labor relations and education policy. While the National Education Association (NEA) was established in 1857, it initially operated as a professional organization led primarily by administrators rather than teachers (Moe 2001). The American Federation of Teachers (AFT), founded in 1916, emerged with explicit union principles through affiliation with the American Federation of Labor, creating an early divide in teacher organizing approaches (Cowen and Strunk 2015).

The modern era of teacher unionism began in 1961 when the United Federation of Teachers (AFT's New York City affiliate) won collective bargaining rights through a successful strike. This victory catalyzed organizing efforts nationwide and prompted the NEA to embrace collective bargaining despite internal resistance from administrator members (Moe 2001). The union movement rapidly gained strength with the NEA and AFT growing to nearly five million members between them, making the NEA the largest labor union in the United States (DiSalvo and Hartney 2020).

After Wisconsin enacted RTW laws in 2011, NEA membership there dropped from approximately 85,000 members to 35,000 members by 2016. In Michigan, after its 2013 RTW law, membership declined from roughly 115,000 to 95,000 members by 2016 (Marianno and Strunk 2018). Despite these recent challenges, teachers' unions remain influential players in local school politics and continue to adapt their organizing strategies to maintain their voice in education policy (Marianno and Strunk 2018).

Teachers' unions now face mounting challenges from expanding school choice programs (Ingraham 2024), growing wage gaps between teachers and comparable professionals (Allegretto 2024), and intensifying political opposition to union influence. The 2018 *Janus* decision marked a watershed moment by prohibiting mandatory agency fees nationwide, effectively extending right-to-work conditions to all public sector employees. This ruling intensified debates about union power and representation in public education that had been playing out at the state level through earlier right-to-work legislation.

2.4 RTW Laws in the Educational Context

The effects of RTW laws in education may differ from those in the private sector. While Fortin, Lemieux and Lloyd (2023) find that private sector RTW effects operate partly through reduced “union threat effects”—the pressure non-union employers feel to match union wages—public school districts typically operate as local monopolies with limited competition. This distinct institutional structure shapes how RTW laws affect educational outcomes, creating a need to examine these policies specifically in the education context.

The intersection of right-to-work laws and teacher unionism provides a unique opportunity to study how changes in union power shape educational outcomes. Research shows these laws significantly affect union resources and bargaining power. Lyon (2021) finds RTW laws decrease teachers’ union membership by approximately 40% on average across states, broadly consistent with the state-specific patterns noted above, substantially reducing unions’ financial resources and capacity for collective bargaining.⁵ This weakening extends beyond simple membership effects—unions often must redirect resources toward member recruitment and retention under RTW conditions, potentially influencing their involvement in educational initiatives and policy advocacy.

These institutional changes make RTW laws particularly valuable for studying union effects on education. Unlike studies of unionization itself, which face challenging selection and endogeneity issues, RTW laws represent plausibly exogenous shocks to union power. Moreover, the staggered adoption of these laws across states provides variation in timing that can help identify causal effects while accounting for local context and institutional factors. This setting offers an opportunity to advance our understanding of how union power shapes educational outcomes, a question that has challenged researchers since the earliest studies of teacher unions.

3 Theoretical Framework: How RTW Laws May Affect Student Achievement

Understanding how RTW laws might affect student achievement requires examining two competing theoretical perspectives about the relationship between union strength and educational outcomes. Each framework suggests different mechanisms through which reducing union power via RTW laws could affect student performance (Hoxby 1996; Freeman and Medoff 1984).

The rent-seeking model posits that teachers’ unions pursue policies that benefit teachers, potentially at the expense of student achievement (Hoxby 1996). This perspective suggests that weakening unions through RTW laws should enhance educational efficiency and student outcomes. By reducing unions’ financial resources and bargaining leverage, RTW laws should limit their ability to secure costly contract provisions that constrain administrative flexibility and increase costs without improving instruction. With diminished union resistance, school leaders could more easily implement performance-based personnel policies, allocate resources more efficiently, and remove ineffective teachers. Additionally, reduced job protections might incentivize greater teacher effort since employment security would depend more on performance than seniority. Under this model, RTW laws’ constraints on union power should allow districts to operate more efficiently and improve student achievement.

⁵Teachers’ union membership is proxied by membership in the NEA, the largest teachers’ union in the U.S.

In contrast, the teacher voice model suggests unions can enhance school productivity by allowing teachers to provide valuable input on educational practices (Lyon 2021; Freeman and Medoff 1984). This perspective emphasizes educators' specialized knowledge about effective classroom practices and argues that unions help transmit this expertise into policy. RTW laws could harm student achievement by diminishing this knowledge-sharing function. Reduced dues revenue and membership would constrain unions' capacity to systematically gather and convey teacher insights about curriculum, assessment, and working conditions that affect instruction. Without strong union advocacy, administrators might implement policies that appear efficient on paper but ignore classroom realities that teachers understand through direct experience. Additionally, weaker unions may have less ability to negotiate for working conditions that experienced teachers know support effective instruction. The resulting deterioration in teaching conditions could increase turnover among skilled educators and make the profession less attractive to talented candidates.

These contrasting frameworks suggest RTW laws could affect student achievement through several channels: resource allocation, personnel policies, working conditions, teacher effort, and knowledge flow between classroom educators and administrators. The rent-seeking model predicts RTW laws will enhance achievement by reducing union-imposed inefficiencies, while the teacher expertise framework suggests they will harm learning by constraining teachers' collective voice in educational decision-making. Which effect dominates likely depends on institutional context—the benefits of reduced rent-seeking may outweigh voice losses in some settings but not others. This theoretical ambiguity highlights the importance of empirical evidence about RTW laws' actual impacts on student outcomes.

My identification strategy examines these effects by comparing achievement in border counties where one state adopted RTW laws while its neighbor did not. This approach helps isolate RTW laws' causal effects from other factors affecting educational outcomes. The analysis focuses particularly on how resource allocation, working conditions, and student achievement change when RTW laws reduce union power through restrictions on mandatory dues.

4 Literature Review

4.1 Early Cross-Sectional Evidence

Initial research on teachers' unions relied primarily on cross-sectional comparisons. Eberts and Stone (1987) analyzed student-level data from the Sustaining Effects Study, examining approximately 14,000 fourth-grade students across 328 elementary schools. They found that students in unionized districts showed 3% higher average gains than those in non-union districts, with larger differences (7%) for students scoring at the mean.

Other early studies produced varying results. Kleiner and Petree (1988), analyzing state-level data between 1972 and 1982, found that higher teacher unionization rates were associated with increased educational spending and modestly improved student achievement scores. However, Peltzman (1996), examining the performance of non-college-bound students using military entrance exam data from 1970 to 1991, found that teacher unionization had negative effects on student achievement, particularly in states where unions achieved early organizational success. These early cross-sectional studies faced fundamental methodological challenges for establish-

ing causation—they could not account for unobserved differences between unionized and non-unionized districts, reverse causality between unionization and student outcomes, or the endogenous selection of districts into unionization. These limitations highlighted the need for more rigorous identification strategies.

4.2 Methodological Advances and Causal Identification

Hoxby (1996)'s influential study was a major step forward in understanding the impact of teacher unions on education. In contrast, Hoxby analyzed panel data at the school district level, exploiting variation in the timing of state laws that facilitated unionization. She employed a combination of difference-in-differences and instrumental variables, using the passage of these laws as instruments to address the endogeneity of unionization. This approach allowed her to isolate the causal effects of union power while accounting for unobservable, time-invariant district characteristics and time trends.

Hoxby (1996) found that unionization increased per-pupil spending by 12.3% and teacher salaries by 5.0%, while also raising high school dropout rates by 2.3 percentage points. These findings, based on school district-level data, support the “rent-seeking” view of unions, which posits that unions prioritize teachers’ interests—such as higher salaries and reduced workloads—over improvements in student outcomes and school efficiency.

Lovenheim (2009) investigated the impact of teachers’ unions on school resources and student outcomes in three Midwestern states, using a novel dataset of union election certifications to precisely identify unionization timing. His difference-in-differences analysis, incorporating nonparametric leads and lags, revealed that, contrary to expectations, unions had no effect on teacher pay or per-student expenditures. While unions did increase teacher employment by 5%, this did not result in reduced class sizes, as student enrollment grew concurrently. Moreover, no significant impact on high school dropout rates was observed. These findings challenged prevailing views on teachers’ union effects and highlighted how measurement improvements could lead to substantially different conclusions about union impacts.

Lovenheim and Willén (2019) examined the long-term impact of teacher collective bargaining on student outcomes. Using longitudinal data from the 2005–2012 American Community Survey matched to individuals’ birth states, they leveraged the staggered adoption of state duty-to-bargain laws between 1960 and 1987. This approach enabled them to compare cohorts within states who experienced varying levels of exposure to these laws during their schooling years. Their findings revealed that exposure to duty-to-bargain laws led to a nearly 4% decline in annual earnings and a 1 percentage point decrease in employment rates for male students. Notably, these adverse effects were not observed among female students.

Rambachan and Roth (2023) reanalyzed Lovenheim and Willén’s (2019) study, applying sensitivity analyses that relaxed the parallel trends assumption. Their findings indicated that the employment effects were highly sensitive to even slight departures from parallel trends. Allowing for minimal deviations from linear trends—approximately one-fortieth of a standard deviation in teacher value-added between consecutive periods—led the confidence intervals to encompass zero for both men and women. This analysis highlights the critical role of sensitivity testing in evaluating the robustness of results in difference-in-differences designs with staggered treatment adoption, particularly when the parallel trends assumption is uncertain.

4.3 Research on Restrictive Policies

More recent work shifted focus from studying unionization's effects to examining impacts of policies restricting union power. Baron (2018) analyzed Wisconsin's Act 10 using variation in districts' exposure based on contract expiration timing. Through this natural timing variation and an event study approach, Baron found that restricting collective bargaining substantially lowered test scores by 0.20 standard deviations—an effect size larger than having a highly effective versus average teacher (Chetty, Friedman and Rockoff 2014)—with the largest impacts in math and science and among lower-performing students.

Lyon (2021) analyzed the effects of RTW laws on teachers, students, and education policy-making across the United States from 1990 to 2018, using a difference-in-differences framework. The study found that RTW policies reduced teachers' union membership by approximately 40% and decreased unions' political contributions. Despite these reductions, RTW policies did not lead to significant changes in education reform activity or improvements in student outcomes. The analysis identified a small, negative effect on student achievement, particularly in fourth-grade reading scores, with no significant effects on other tested outcomes.

Both studies relied on traditional difference-in-differences methods, which methodological work shows produce biased estimates in settings with staggered treatment adoption and heterogeneous effects (Goodman-Bacon 2021). Advances in difference-in-differences techniques now address these concerns by improving the selection of comparison groups and accounting for variation in treatment effects over time. In addition to regression adjustment, I implement the Synthetic Difference-in-Differences estimator developed by Arkhangelsky et al. (2021), which improves pre-treatment trend matching by applying unit and time weights. This method is particularly useful given the small number of treated states and helps address concerns about violation of parallel trends.

This paper builds on these developments by studying RTW laws using modern difference-in-differences methods designed to account for treatment effect heterogeneity and staggered policy adoption. The analysis employs both parametric and non-parametric approaches to examine dynamic treatment effects while testing and relaxing assumptions about pre-trends and effect homogeneity.

5 Data

5.1 Stanford Education Data Archive

I use the Stanford Education Data Archive, Version 5.0 (SEDA) for standardized measures of educational achievement across U.S. school districts from the 2009-2018 school years (Fahle et al. 2024). SEDA addresses a fundamental challenge in cross-state education research by providing comparable achievement measures despite states' use of different assessments. The database combines approximately 300 million state accountability test scores from roughly 45 million public school students in grades 3 through 8.

The primary outcome variables are derived from state standardized test scores in mathematics and Reading Language Arts (RLA) for grades 3 through 8, which states report to the U.S. Department of Education's EDFacts system. SEDA researchers transform these proficiency counts into comparable test score distributions and link them to the National Assessment of Educational

Progress (NAEP) scale. This process yields test scores that are comparable both across states and over time within subjects. Scores are converted to effect sizes relative to a reference cohort, measured in student-level standard deviations.

SEDA combines achievement data with contextual information from multiple federal sources including the Common Core of Data (CCD) and American Community Survey (ACS). These provide county-level measures such as enrollment, racial composition, free/reduced-price lunch eligibility, and socioeconomic indicators.

5.2 School Finance Data

I leverage detailed financial data from the U.S. Census Bureau's Annual Survey of School System Finances (F-33) for fiscal years 2009 through 2018. This survey provides comprehensive data on school district revenues and expenditures, covering approximately 13,000 regular public school districts annually. The fiscal year generally runs from July 1 to June 30, though some states differ.⁶

The survey captures detailed spending categories, with particular focus on current expenditures for instruction and support services. I focus primarily on two key measures: total current spending for elementary-secondary programs (which includes salaries, benefits, purchased services, and supplies) and current spending specifically directly related to classroom instruction (such as regular education programs, special education, vocational programs, teacher salaries, and instructional materials). I convert all financial variables to per-pupil measures using fall enrollment counts.

To align the district-level finance data with my county-based analysis framework, I aggregate district spending to the county level using enrollment-weighted averages. This approach ensures that per-pupil spending measures appropriately reflect the relative size of districts within each county. The aggregation process maintains the core spending categories while accounting for the nested structure of districts within counties.

5.3 Sample Characteristics

My analysis focuses on counties along state borders where one state adopted RTW laws between 2011-2017 while its neighbor did not. Tables 1-4 present key characteristics of these counties as of 2010, one year before Wisconsin became the first state in my sample to adopt RTW legislation. These tables allow us to assess both the validity of the border-county research design and the representativeness of the border-county sample.

Table 1 reveals substantial differences between border counties in RTW-adopting and never-RTW states prior to any policy changes. Border counties in eventual RTW states had significantly lower student achievement, with mathematics scores 0.125 standard deviations below their never-RTW neighbors ($p < 0.01$) and reading scores 0.066 standard deviations lower ($p < 0.05$). The demographic composition of these counties also differed markedly. Border counties in RTW states served significantly lower proportions of Asian (1.4% vs 3.9%), Black (6.4% vs 18.5%), and Hispanic (5.5% vs 17.7%) students, with correspondingly higher proportions of white students (85.7% vs 59.6%). These demographic differences were accompanied by socioeconomic disparities—RTW

⁶Alabama and DC operate on an October 1 to September 30 cycle, while Nebraska, Texas, and Washington use September 1 to August 31.

border counties had lower college degree attainment (20.8% vs 30.1%) and lower median household income (about 18% lower based on the difference in log median income).

The school finance data in Table 2 shows that these demographic differences were matched by resource disparities. Border counties in RTW states spent \$1,265 less per pupil on current expenditures compared to their never-RTW neighbors (\$10,299 vs \$11,563), with instructional spending showing a gap of \$747 per student. The composition of school funding also differed substantially—RTW border counties received significantly less federal revenue (\$1,609 less per pupil) and local revenue (\$1,202 less per pupil), partially offset by higher state funding (\$1,473 more per pupil).

Tables 3 and 4 help assess whether the differences between RTW and never-RTW counties reflect broader state-level patterns or are specific to border areas. In never-RTW states, border counties diverge from non-border counties on several key dimensions. Border counties have higher Hispanic enrollment (17.7% vs 7.9%, $p < 0.10$), lower rural population shares (16.5% vs 25.6%, $p < 0.10$), and notably different financial characteristics. Border counties spend \$522 more per pupil on current expenditures ($p < 0.05$) and receive substantially more federal revenue (\$1,434 more per pupil, $p < 0.01$) compared to non-border counties, while receiving less state and local funding. In contrast, border counties in RTW states show fewer significant differences from their non-border counterparts, with disparities primarily in demographic composition—border counties have lower Asian and Black enrollment shares and higher white enrollment shares. The overall pattern suggests that while border counties in RTW states are largely representative of their states' characteristics, border counties in never-RTW states have distinct demographic and financial profiles that should be considered when interpreting the results.

The analysis of sample characteristics reveals important patterns. While RTW and never-RTW states show substantial differences, border and non-border counties are largely similar across most dimensions, with the notable exception of school finance variables in never-RTW states. The similarity between border and non-border counties suggests the border sample is broadly representative of state averages in terms of student composition and most school characteristics. However, the substantial pre-existing differences between RTW and never-RTW states warrant careful attention to the parallel trends assumption underlying difference-in-differences analysis. These differences represent an important caveat when interpreting the results that follow.

6 Empirical Strategy

6.1 Border-County Research Design

To address these substantial differences between RTW and non-RTW states, I employ a border-county research design following Gihleb, Giuntella and Tan (2024) and others (e.g., Holmes 1998; Dube, Lester and Reich 2010). This approach restricts attention to counties along state borders where one state adopted RTW laws while its neighbor did not. The intuition is that neighboring counties, despite lying in different states, operate in similar local labor markets and face common regional economic shocks. By restricting comparisons to geographically proximate counties, the design leverages the fact that while RTW laws change discretely at state borders, most underlying economic and demographic factors vary smoothly across space.

As shown in Tables 1 and 2, even among border counties, meaningful differences exist between treatment and control groups prior to RTW adoption. However, the border-county design does not require neighboring counties to be identical in pre-treatment characteristics. Rather, the key identifying assumption is that these adjacent counties would have experienced parallel trends in educational outcomes absent RTW laws. This assumption is more plausible between geographic neighbors than between arbitrary RTW and non-RTW states, as neighboring counties face similar regional economic conditions and labor market shocks.

The persistence of significant differences even between border counties warrants particularly careful examination of pre-trends, which I present in Section 6.7. This examination helps validate whether the design's key identifying assumption—that paired counties would have followed parallel achievement trajectories absent RTW laws—is plausible in my setting despite these baseline differences in characteristics.

6.2 Potential Spillover Effects

A key threat to the border-county design's validity is the potential for spillover effects across state boundaries. If RTW laws in one state affect educational outcomes in neighboring non-RTW states, this would violate the stable unit treatment value assumption necessary for causal interpretation. In the context of education, spillovers could occur through two main channels: teacher mobility and district policy responses.

Teachers who strongly value union protections might relocate from RTW to non-RTW counties, while those who prefer environments with weaker union influence might move in the opposite direction. If more effective teachers are more likely to have the resources and opportunities to relocate, this selective mobility could affect achievement in both treated and control counties. Similarly, school districts near borders might adjust their policies in response to neighboring states' RTW status, perhaps altering compensation or working conditions to remain competitive in teacher labor markets.

These spillover effects would likely magnify the measured effects of RTW laws. If more effective teachers are more likely to oppose RTW laws and therefore relocate to control counties, this would reduce achievement in treated areas (through the loss of high-quality teachers) while improving achievement in control areas (through positive selection of incoming teachers). The net effect would be larger achievement differences between RTW and non-RTW counties in border areas compared to interior counties. Similarly, if control districts near borders improve working conditions to retain high-quality teachers, this would further increase achievement differences between border counties. Under these assumptions, the border county design might overstate the true effect of RTW laws on educational outcomes. However, the fact that I find positive effects on achievement suggests either that RTW laws improved educational efficiency enough to overcome any negative effects of teacher sorting, or that the assumptions about more effective teachers being more likely to relocate may not hold in this setting.

6.3 Sample Construction

Following Gihleb, Giuntella and Tan (2024), I construct a sample of county pairs that share a border, with one county located in an RTW-adopting state (Wisconsin, Michigan, West Virginia, or

Kentucky) and the other in a state that never adopted RTW laws (Minnesota, Illinois, Ohio, Pennsylvania, Maryland, or Missouri). The analysis focuses on states that enacted RTW laws between 2011 and 2017 and those that never implemented such laws, addressing concerns highlighted by recent advancements in difference-in-differences methodology (Goodman-Bacon 2021).

For counties in states that never implement an RTW law, I assign treatment timing based on their paired treatment county's adoption year. Following Cengiz et al. (2019), I create a stacked dataset where each county appears as many times as it has relevant cross-border neighbors. The final analysis sample includes 113 unique county pairs across 9 state borders, composed of 62 unique counties in RTW-adopting states and 51 unique counties in never-RTW states.^{7,8}

6.4 Baseline Specification

I employ two complementary approaches to estimate the effects of RTW laws. My primary specification uses the regression adjustment (RA) estimator developed by Callaway and Sant'Anna (2021) (CS), which is particularly well-suited to settings with staggered treatment adoption and potential treatment effect heterogeneity. Recent methodological work highlights potential biases in traditional two-way fixed effects (TWFE) estimates when treatment effects are heterogeneous across units or over time (Goodman-Bacon 2021). The CS estimator addresses these concerns while naturally accommodating my border-county design's use of never-treated counties as controls.

As a complement to the CS analysis, I also estimate a traditional TWFE model to assess the robustness of my results. I describe each approach in turn.

Let i index counties, t index years, and g index the “cohort” of counties that first become treated in year g .⁹ Let $y_{it}(g)$ denote the potential outcome for county i in year t if it first received treatment in year g . Let $y_{it}(0)$ denote the potential outcome for county i in year t if it is never treated. The parameter of interest is the group-time average treatment effect on the treated (ATT), $\theta(g, t)$, which represents the average treatment effect for counties in cohort g in year t .

$$\theta(g, t) = E[y_t(g) - y_t(0)|G_g = 1] \quad (1)$$

where G_{ig} is an indicator equal to one if county i is first treated in year g , and zero otherwise. The RA estimand is defined as:

$$\theta_{RA}(g, t) = E\left[\frac{G_g}{E(G_g)}\{y_t - y_{g-1} - m_{g,t}(x)\}\right] \quad (2)$$

where $m_{g,t}(x) = E[y_t - y_{g-1}|x, C_{g,t}^* = 1]$ is the conditional expectation of the change in outcomes for the control group, given covariates x , $C_{g,t}^*$ is an indicator equal to one if a county belongs to the control group (never-treated) for cohort g at time t .

The covariates included in the estimation are pre-treatment means of racial composition (percent Asian, Black, Hispanic, Native American, and white students), urbanicity measures (urban,

⁷Keweenaw County, MI, which is adjacent to Cook County, MN, does not have any test score data, and so the Keweenaw County, MI-Cook County, MN border pair is dropped from the file.

⁸The nine border pairs are WI-IL, WI-MN, MI-OH, KY-IL, KY-MO, KY-OH, WV-OH, WV-PA, and WV-MD.

⁹For the following exposition, I generally follow StataCorp (2024).

suburban, town, and rural percentages), achievement in both mathematics and reading, log median income, unemployment rate, poverty rate, SNAP receipt rate, free/reduced lunch eligibility, economically disadvantaged percentage, single mother household rate, and bachelor's degree attainment rate. For the TWFE specification described below, I use these same pre-treatment means as controls.

Standard errors for the estimated $\theta_{RA}(g, t)$ are clustered at the county-pair level and computed using the influence function approach outlined by Callaway and Sant'Anna (2021).¹⁰ This method, which avoids the need to compute the covariance matrix for nuisance parameters, is computationally efficient while producing results equivalent to a generalized method-of-moments approach. To account for the relatively small number of clusters in my sample, I also apply wild cluster bootstrap procedures, as recommended by Cameron and Miller (2015).¹¹ I report both the standard errors based on the influence function and the wild bootstrap results in Section 7.

6.5 Two-Way Fixed Effects Specification

I also estimate a traditional two-way fixed effects model:

$$Y_{gcpt} = \alpha_p + \psi_{s(c)} + \delta_t + \gamma RTW_{st} + X'_{cpt}\beta + \epsilon_{gcpt} \quad (3)$$

where Y_{gcpt} represents the outcome (student achievement in math or reading) for grade g , county c (within pair p), and time t . County-pair fixed effects (α_p) control for any time-invariant factors specific to geographically adjacent counties across state borders. State fixed effects ($\psi_{s(c)}$) capture time-invariant characteristics of the state to which county c belongs, while year fixed effects (δ_t) account for common shocks affecting all counties in a given year. The treatment variable (RTW_{st}) equals 1 if state s has implemented an RTW law by time t and 0 otherwise. The coefficient of interest, γ , represents the average treatment effect of RTW laws on the outcome. The school finance outcomes are estimated similarly, without a grade level.

6.6 Synthetic Difference-in-Differences (SDID)

As a complementary robustness check, I implement the Synthetic Difference-in-Differences (SDID) estimator proposed by Arkhangelsky et al. (2021). SDID combines elements of traditional difference-in-differences (DiD) and synthetic control methods to address potential violations of the parallel trends assumption. The method is particularly valuable given the pre-existing differences between RTW and non-RTW border counties documented in Tables 1 and 2.

Like traditional DiD, SDID eliminates time-invariant unobserved heterogeneity through differencing. However, SDID improves upon DiD by optimally weighting control units to better match pre-treatment outcome trends in treated units, rather than assuming all control units are

¹⁰I cluster at the county-pair level because the key identifying variation comes from within-pair comparisons across state borders. My sample contains 62 county pairs formed from 62 counties in RTW-adopting states matched to 51 counties in never-RTW states. As a robustness check in future work, I will also present results with state-level clustering, though this further reduces the number of clusters.

¹¹The wild cluster bootstrap provides more reliable inference with few clusters by resampling residuals while preserving within-cluster correlation structure. Following Cameron and Miller (2015), I use a six-point distribution for the bootstrap weights rather than the Rademacher distribution, as it provides better finite sample properties when the number of clusters is small.

equally valid counterfactuals. This weighted approach helps address concerns about non-parallel pre-treatment trends without requiring them to be exactly parallel. I implement SDID using school district-level data with complete observations across the entire sample period. Unlike the border county design, which focuses only on counties adjacent to state borders, the SDID analysis includes all school districts with complete data in the selected states. For reading achievement analysis (2009-2018), I include school districts from Wisconsin, Michigan, Kentucky, and West Virginia as treatment units, with districts from Illinois, Minnesota, Missouri, and Pennsylvania as control units. Maryland and Ohio are excluded from the control group due to gaps in their data. For mathematics achievement, I include the same treatment states but in order to include West Virginia have to drop 2014 and 2016. The control group consists of Illinois, Minnesota, Missouri, Ohio, and Pennsylvania. Again, Maryland is dropped.

To ensure comparability with the border county DiD analysis, I use the same set of time-varying exogenous control variables in both specifications: racial composition (percent Asian, Black, Hispanic, Native American, and White) and urbanicity measures (urban, suburban, town, and rural percentages). Following best practices for SDID inference with few treated units, I employ the placebo method with 50 replications to compute standard errors. This approach creates synthetic placebo treatments by randomly reassigning treatment status and generates a distribution of placebo effects against which to compare the actual estimated effect.

6.7 Event Study Analysis

To examine how RTW effects evolve over time, I estimate event study specifications using the CS estimator. This allows me to examine treatment effects separately for each year relative to RTW adoption while maintaining the same identifying assumptions as the main analysis. Figure 2 presents these dynamic effects visually for student achievement and school finance outcomes, revealing how impacts emerge and potentially change in the years following RTW adoption.

The key assumption underlying the difference-in-differences design is that treatment and control group outcomes would have evolved similarly absent treatment. While this counterfactual assumption cannot be directly tested, examining pre-treatment patterns provides evidence about its plausibility.

Generally speaking, no clear trends emerge in either achievement or school finance outcomes prior to RTW adoption. For mathematics achievement, point estimates in pre-RTW periods range from -0.042 to 0.063 standard deviations, with no clear systematic pattern. While a joint test of all pre-treatment coefficients yields a chi-square statistic of 16.53 ($p=0.021$), the individual pre-treatment estimates oscillate between positive and negative values without showing consistent directional trends.

The evidence raises somewhat greater concerns for reading achievement, where several pre-treatment coefficients differ significantly from zero, particularly six years prior to RTW adoption (estimate=-0.057, SE=0.017) and three years prior (estimate=-0.064, SE=0.014). This suggests the reading results should be interpreted with appropriate caution. Similarly, formal tests indicate some deviation from parallel pre-trends for both total current spending (chi-square=26.57, $p < 0.001$) and instructional spending (chi-square=22.23, $p=0.002$). However, as with mathematics achievement, the pre-treatment point estimates show no clear systematic pattern.

These patterns motivate my preference for the regression adjustment estimator, which helps address potential non-parallel trends by modeling how outcomes evolve in the control group

conditional on pre-treatment characteristics. Rather than assuming parallel trends hold unconditionally, the RA estimator allows the evolution of outcomes to vary with observable characteristics like baseline achievement, demographics, and economic conditions. By comparing treated counties to control counties with similar pre-treatment characteristics, this approach can account for systematic differences in outcome trajectories related to these observables. The event study plots in Figure 2 provide visual evidence that, after this adjustment, any remaining pre-treatment differences do not follow consistent patterns that would obviously bias the treatment effect estimates.

7 Results

7.1 Effects on Student Achievement

Using the regression adjustment estimator developed by Callaway and Sant'Anna (2021), I find that RTW laws had positive, statistically significant effects on both mathematics and reading achievement (Table 5). For mathematics, my preferred specification with full controls indicates that RTW laws increased test scores by 0.062 standard deviations (conventional SE = 0.018, $p < 0.01$). This effect remains significant when using wild cluster bootstrap standard errors (SE = 0.018). To contextualize this magnitude: Kraft (2020) finds that by 5th grade, student achievement typically improves by about 0.40 standard deviations over an academic year. Thus, the RTW effect represents about 16% of typical annual learning gains. For perspective, Kraft (2020) finds that broad-scale education interventions evaluated in U.S. Department of Education studies show a median effect of just 0.03 standard deviations, making the RTW effects relatively large for a state-level policy change.

The effects on reading achievement are similarly positive and precisely estimated. With full controls, I find that RTW laws increased reading scores by 0.058 standard deviations (conventional SE = 0.017, $p < 0.01$), an effect that remains significant with wild cluster bootstrap standard errors (SE = 0.018). The similar magnitude of effects across subjects is notable, as previous research typically finds that school-based interventions have stronger effects on mathematics than reading achievement.

The inclusion of control variables meaningfully affects the estimated effects, highlighting the importance of accounting for observable differences across counties. For mathematics, the effect increases from 0.036 (SE = 0.009) with no controls to 0.062 (SE = 0.018) with full controls. Similarly for reading, the effect grows from 0.017 (SE = 0.009) to 0.058 (SE = 0.017). This pattern suggests that the counties that adopted RTW laws had characteristics associated with lower achievement growth, causing the unadjusted estimates to underestimate the true policy effects.

To assess robustness, I also estimate traditional two-way fixed effects (TWFE) models with various combinations of fixed effects and controls (Table 6). For mathematics, these estimates range from 0.014 (SE = 0.018) in the base specification to 0.046 (SE = 0.009) with full controls and all fixed effects. The reading estimates follow a similar pattern, ranging from 0.029 (SE = 0.014) to 0.038 (SE = 0.008). The consistently smaller TWFE estimates align with recent methodological work showing that these models can underestimate effects when treatment timing varies and effects are heterogeneous.

7.2 Effects on School Resources and Potential Mechanisms

The achievement gains occurred despite significant reductions in school spending (Table 7). Using the same Callaway and Sant'Anna estimator, I find that RTW laws reduces total current spending by \$606.51 per pupil (conventional SE = 186.37, $p < 0.01$) in the specification with full controls. This represents about a 5.6% reduction relative to the sample mean of \$10,849 per pupil. Nearly all of this reduction (\$567.57 per pupil, SE = 150.42, $p < 0.01$) came through cuts to instructional spending, which includes expenditures for regular education programs, special education, vocational programs, teacher salaries, and instructional materials. The instructional spending reduction represents an 8.8% decline relative to its mean of \$6,471.

These spending effects are robust across specifications. The TWFE estimates show slightly larger reductions once fixed effects are included, with total current spending falling by \$709.18 per pupil (SE = 121.99) and instructional spending declining by \$624.38 per pupil (SE = 84.13) in the fully specified models. The similarity between the CS and TWFE estimates suggests less treatment effect heterogeneity in financial outcomes compared to achievement.

The combination of positive achievement effects and reduced spending presents an interesting puzzle. One potential explanation is that RTW laws improved the efficiency of education spending by weakening union work rules that may have constrained resource allocation. The fact that instructional spending absorbed nearly all of the total spending reduction suggests districts may have found ways to maintain educational quality with fewer resources devoted to classroom instruction.

Another possibility is that changes in teacher workforce composition—induced by reduced union protection—led to improved effectiveness despite lower spending. While I cannot directly test these mechanisms with my data, the patterns are consistent with RTW laws increasing managerial flexibility in ways that enhanced educational productivity.

The achievement gains are particularly notable given their occurrence in an environment of declining resources. The effect sizes—increases of 0.058 to 0.062 standard deviations—suggest meaningful improvements in student learning despite per-pupil spending reductions of \$567–\$607. This implies substantial increases in the productivity of educational spending following RTW adoption, though further research is needed to fully understand the mechanisms driving these efficiency gains.

7.3 Robustness Check: Synthetic Difference-in-Differences Estimates

To assess robustness using an alternative identification strategy, I estimate treatment effects using the Synthetic Difference-in-Differences estimator. These models use school district-level data with complete observations across all sample years, encompassing all districts with complete data in the selected states rather than only those in border counties.

For mathematics achievement, the SDID estimates show positive and statistically significant effects of RTW laws. The point estimate without controls is 0.016 standard deviations (SE = 0.005), while the estimate with basic race and location controls is 0.017 standard deviations (SE = 0.005). These effects are smaller in magnitude than those produced by the regression adjustment approach but remain positive and statistically significant, supporting the main findings.

Similarly, for reading achievement, SDID produces positive and significant estimates. Without controls, RTW laws increased reading scores by 0.014 standard deviations (SE = 0.009), while the

estimate with basic controls is 0.016 standard deviations ($SE = 0.006$). The consistency in direction across different methodological approaches strengthens confidence in the finding that RTW laws positively affected student achievement.

[ADD RESULTS FOR SCHOOL FINANCE OUTCOMES]

The smaller magnitude of SDID estimates compared to the regression adjustment approach may reflect differences in the level of analysis (state versus county), the construction of balanced panels, or the weighting scheme that places greater emphasis on units with well-matched pre-treatment trends. Despite these differences, the consistent direction of effects across methodologies reinforces the paper's primary conclusions that RTW laws improved student achievement despite reducing educational resources.

[DISCUSS RESULTS FROM MODELS EXCLUDING WV FROM TREATMENT GROUP]

7.4 Dynamic Effects and Effect Size Interpretation

The event study estimates reveal how RTW effects evolved over time (Figure 2). For mathematics, effects become positive around the second year after RTW adoption and grow larger over time, reaching about 0.15-0.18 standard deviations 3-4 years after adoption before declining somewhat. Reading shows a similar pattern, with effects becoming positive the third year, but with even larger long-run effects, reaching about 0.16-0.19 standard deviations 4-6 years post-adoption. These peak effects of 0.15-0.19 SD are quite notable, equivalent to nearly half of a student's typical annual learning gains.

The timing of spending changes relative to RTW adoption provides important insights into potential mechanisms. Rather than immediate sharp declines, school finance effects emerge gradually over time. Total current spending shows no significant changes in the first few years after RTW adoption, but begins declining meaningfully by the fourth year post-adoption, falling by approximately \$1,177 per pupil (significant at the 5% level). These reductions continue and grow larger in subsequent years, reaching about \$1,436 per pupil by year 5 post-adoption.

The pattern for instructional spending follows a similar trajectory, with gradual declines becoming statistically significant around year 4 post-adoption (a reduction of \$976 per pupil) and continuing to grow to approximately \$1,048 per pupil by year 5. The delayed and gradual nature of these spending reductions suggests they may have occurred through natural attrition and slower growth rather than immediate cuts, possibly reflecting reduced union bargaining power in contract negotiations over time. This interpretation aligns with the institutional features of public education, where many expenditures are determined by multi-year contracts and changing spending patterns often require sustained policy pressure rather than sudden shifts.

7.5 Reconciling Achievement Gains with Resource Reductions

The combination of significant achievement gains despite substantial resource reductions presents an important puzzle for understanding how RTW laws affected educational production. The \$567-624 reduction in per-pupil instructional spending represents nearly 9% of baseline levels, yet test scores improved by 0.058-0.062 standard deviations on average, with even larger long-run gains. Three potential mechanisms could explain this apparent paradox:

1. Improved allocative efficiency: RTW laws may have given administrators more flexibility

to direct reduced resources toward their most productive uses by weakening union work rules and contract provisions that constrained resource allocation.

2. Enhanced workforce productivity: Reduced union protection could have shifted the composition of the teaching workforce through selective attrition or changes in recruitment, potentially leading to a more effective (though less expensive) teacher pool.
3. Increased organizational effectiveness: The weakening of union constraints may have allowed schools to implement productivity-enhancing organizational changes that were previously blocked by collective bargaining agreements.

While my data cannot definitively distinguish between these channels, the gradual emergence of achievement gains and spending reductions suggests mechanisms operating through cumulative organizational changes rather than simple resource reallocation. The similar proportional reductions in total and instructional spending (5.6% vs 8.8%) indicate that districts maintained their relative prioritization of classroom expenditures even as overall resources declined.

These findings contribute to our understanding of the education production function and the role of labor market institutions in affecting school productivity. The substantial achievement gains despite reduced spending suggest that the constraints imposed by teacher collective bargaining may create meaningful inefficiencies in public education. However, the gradual emergence of these gains highlights the importance of organizational adaptation in realizing the potential benefits of increased managerial flexibility.

8 Conclusion

This paper provides new evidence on how Right-to-Work laws affect student achievement and school resources using modern methods that address key empirical challenges. Focusing on four states that adopted RTW laws between 2011-2017, I find that these laws led to modest improvements in both mathematics and reading achievement despite significant reductions in school spending. My preferred estimates using the Callaway and Sant'Anna (2021) regression adjustment estimator indicate that RTW laws increased math scores by 0.062 standard deviations and reading scores by 0.058 standard deviations. The event study analysis reveals that these effects grew over time, reaching 0.15-0.19 standard deviations 3-6 years after adoption.

These achievement gains occurred alongside substantial reductions in school resources. RTW laws reduced total current spending by about \$607 per pupil (5.6%) and instructional spending by \$568 per pupil (8.8%). The combination of improved achievement despite reduced spending points to significant productivity gains following RTW adoption.

These findings provide an interesting contrast with Lyon (2021), who found null to negative effects of RTW laws on student achievement. Several methodological differences likely explain our divergent results. First, I employ newer econometric methods that better account for treatment effect heterogeneity and staggered policy adoption—an important advancement given recent work showing that traditional two-way fixed effects estimates can be biased when treatment effects vary across units or over time. The inclusion of SDID as a complementary estimator reinforces the robustness of my findings, as it addresses potential violations of parallel trends by improving pre-treatment balance through weighted matching. Second, my approach of using

only pre-treatment controls helps ensure that tests of parallel trends reflect genuine similarities in achievement trajectories rather than being influenced by post-treatment factors that could themselves be affected by RTW laws. Third, my border county research design comparing adjacent counties across state lines provides a stronger control for unobserved geographic factors that could confound the relationship between RTW laws and achievement. Finally, the Stanford Education Data Archive provides richer achievement data that enables more precise estimation of policy effects. The positive achievement effects coupled with reduced spending suggest that reducing union authority through RTW laws may enable schools to operate more efficiently over time. This finding challenges the view that teachers' unions primarily act as a "voice" mechanism that improves educational quality by advocating for better working conditions and school resources. While unions appear to play an important role in maintaining higher levels of school spending, the fact that student achievement improved despite eventual spending reductions suggests that some union-negotiated provisions may constrain productivity-enhancing changes in resource allocation or personnel management.

My results highlight the value of revisiting earlier findings using modern empirical methods and newly available data sources. The differences between my estimates and prior work underscore how methodological choices and data quality can substantively affect our understanding of education policies' impacts. Future research could further explore the mechanisms driving these achievement gains, perhaps by examining changes in teacher workforce composition, resource allocation, or school management practices following RTW adoption.

The gradual nature of both spending reductions and achievement improvements suggests these effects likely emerged through institutional changes that took time to materialize. The spending reductions become significant around the fourth year after RTW adoption, with achievement gains following a similar gradual pattern. This suggests these effects may have come through cumulative changes in organizational practices rather than immediate policy shifts. Potential channels include changes in teacher recruitment and retention patterns, adjustments in resource allocation as administrators gain more flexibility, or the adoption of new management practices that were previously constrained by collective bargaining agreements.

More broadly, this analysis demonstrates the importance of carefully considering both identification strategies and the dynamic nature of policy effects when studying state-level policy changes. The border county design, regression adjustment estimator, and wild bootstrap standard errors each play important roles in generating credible estimates despite the challenges of few treated states and staggered policy adoption. The inclusion of SDID as a complementary estimator reinforces the robustness of my findings, as it addresses potential violations of parallel trends by improving pre-treatment balance through weighted matching. Moreover, the examination of dynamic effects reveals important patterns in how institutional changes affect both educational resources and productivity over time. These methodological and substantive insights may prove valuable for evaluating other state-level education reforms.

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Table 1: Student and County Characteristics in Border Counties, 2010

	RTW (2011-2017)	Never-RTW	Difference
Number of Counties	62	51	
Mathematics Achievement	-0.068 (0.175)	0.057 (0.227)	-0.125*** (0.039)
Reading Achievement	-0.047 (0.170)	0.020 (0.181)	-0.066** (0.033)
Demographics			
Percent Asian	0.014 (0.016)	0.039 (0.026)	-0.024*** (0.008)
Percent Black	0.064 (0.067)	0.185 (0.144)	-0.120** (0.053)
Percent Hispanic	0.055 (0.065)	0.177 (0.151)	-0.121** (0.060)
Percent Native American	0.007 (0.021)	0.003 (0.007)	0.004* (0.002)
Percent White	0.857 (0.109)	0.596 (0.255)	0.261*** (0.099)
Percent Free/Reduced Lunch	0.476 (0.094)	0.493 (0.164)	-0.017 (0.061)
Percent English Language Learners	0.023 (0.027)	0.067 (0.053)	-0.044** (0.020)
Percent Special Education	0.147 (0.024)	0.147 (0.015)	0.000 (0.005)
Socioeconomic Characteristics			
Socioeconomic Status	0.095 (0.416)	0.294 (0.616)	-0.199 (0.195)
Log Median Income	10.789 (0.176)	10.973 (0.249)	-0.184** (0.076)
Percent with College Degree	0.208 (0.056)	0.301 (0.090)	-0.093*** (0.025)
Poverty Rate	0.138 (0.036)	0.125 (0.046)	0.013 (0.015)
Geographic Characteristics			
Percent Rural	0.385 (0.278)	0.165 (0.198)	0.220*** (0.066)
Percent Urban	0.148 (0.224)	0.256 (0.263)	-0.108 (0.111)

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Stanford Education Data Archive, Version 5.0 (2009-2018) (Reardon et al. 2024). Sample includes counties in states that adopted RTW laws between 2011-2017 (WI, MI, WV, KY) and their never-RTW neighbors (MN, IL, OH, PA, MD, MO) that share a physical border with a state from the opposite treatment group. Student achievement measured in student-level standard deviations. Student and county characteristics represent county averages for 2010, one year before the first RTW law was adopted in the first focus state. Standard deviations shown in parentheses for means, standard errors shown in parentheses for differences.

Table 2: School Finance Outcomes in Border Counties by Treatment Status, 2010

	RTW (2011-2017)	Never-RTW	Difference
Number of Counties	62	51	
Per-Pupil Total Current Spending (\$)	10,299 (1,509)	11,563 (409)	-1,265*** (391)
Per-Pupil Instruction Spending (\$)	6,359 (1,025)	7,106 (234)	-747** (297)
Per-Pupil Total Revenue (\$)	11,255 (1,437)	12,594 (713)	-1,339*** (343)
Per-Pupil Federal Revenue (\$)	1,284 (377)	2,893 (337)	-1,609*** (107)
Per-Pupil State Revenue (\$)	5,836 (961)	4,364 (585)	1,473*** (268)
Per-Pupil Local Revenue (\$)	4,135 (1,247)	5,337 (554)	-1,202*** (201)
Instruction Share of Spending (%)	61.6 (2.1)	60.6 (0.9)	0.9* (0.5)

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Annual Survey of School System Finances (2009-2018). Sample includes counties in states that adopted RTW laws between 2011-2017 (WI, MI, WV, KY) and their never-RTW neighbors (MN, IL, OH, PA, MD, MO) that share a physical border with a state from the opposite treatment group. School district averages, aggregated to county level using fall enrollment as weights, for 2010, one year before the first RTW law was adopted in the first focus state. Financial variables measured in current dollars. Standard deviations shown in parentheses for means, standard errors shown in parentheses for differences.

Table 3: Student and County Characteristics by Border Status, 2010

	Never-RTW States			RTW States (2011-2017)		
	Border	Non-Border	Difference	Border	Non-Border	Difference
Number of Counties	51	432		62	267	
Mathematics Achievement	0.057 (0.227)	0.094 (0.194)	-0.037 (0.033)	-0.068 (0.175)	-0.076 (0.215)	0.008 (0.026)
Reading Achievement	0.020 (0.181)	0.058 (0.152)	-0.038 (0.026)	-0.047 (0.170)	-0.035 (0.189)	-0.012 (0.024)
Demographics						
Percent Asian	0.039 (0.026)	0.037 (0.040)	0.002 (0.009)	0.014 (0.016)	0.030 (0.025)	-0.016*** (0.005)
Percent Black	0.185 (0.144)	0.175 (0.187)	0.009 (0.055)	0.064 (0.067)	0.137 (0.127)	-0.072*** (0.027)
Percent Hispanic	0.177 (0.151)	0.079 (0.081)	0.098* (0.058)	0.055 (0.065)	0.058 (0.051)	-0.002 (0.016)
Percent Native American	0.003 (0.007)	0.005 (0.020)	-0.002** (0.001)	0.007 (0.021)	0.008 (0.031)	-0.001 (0.003)
Percent White	0.596 (0.255)	0.703 (0.229)	-0.107 (0.099)	0.857 (0.109)	0.767 (0.165)	0.090** (0.036)
Percent Free/Reduced Lunch	0.493 (0.164)	0.429 (0.152)	0.063 (0.061)	0.476 (0.094)	0.487 (0.135)	-0.012 (0.024)
Percent English Language Learners	0.067 (0.053)	0.038 (0.043)	0.029 (0.020)	0.023 (0.027)	0.034 (0.030)	-0.011 (0.007)
Percent Special Education	0.147 (0.015)	0.148 (0.023)	-0.001 (0.004)	0.147 (0.024)	0.142 (0.021)	0.005 (0.004)
Socioeconomic Characteristics						
Socioeconomic Status	0.294 (0.616)	0.354 (0.662)	-0.060 (0.199)	0.095 (0.416)	-0.062 (0.700)	0.157 (0.139)
Log Median Income	10.973 (0.249)	10.940 (0.260)	0.033 (0.075)	10.789 (0.176)	10.804 (0.223)	-0.014 (0.042)
Percent with College Degree	0.301 (0.090)	0.278 (0.106)	0.024 (0.026)	0.208 (0.056)	0.241 (0.101)	-0.033* (0.018)
Poverty Rate	0.125 (0.046)	0.119 (0.050)	0.007 (0.015)	0.138 (0.036)	0.149 (0.055)	-0.010 (0.010)
Geographic Characteristics						
Percent Rural	0.165 (0.198)	0.256 (0.231)	-0.091* (0.054)	0.385 (0.278)	0.296 (0.273)	0.089 (0.054)
Percent Urban	0.256 (0.263)	0.184 (0.253)	0.072 (0.106)	0.148 (0.224)	0.231 (0.240)	-0.083 (0.055)

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Stanford Education Data Archive, Version 5.0 (2009-2018) (Reardon et al. 2024). Sample includes counties in states that adopted RTW laws between 2011-2017 (WI, MI, WV, KY) and their never-RTW neighbors (MN, IL, OH, PA, MD, MO). Border counties share a physical border with a state from the opposite treatment group. Student achievement measured in student-level standard deviations. Student and county characteristics represent county averages for 2010, one year before the first RTW law was adopted in the first focus state. Standard deviations shown in parentheses for means, standard errors shown in parentheses for differences.

Table 4: School Finance Outcomes by Treatment and Border Location, 2010

	Never-RTW States			RTW States (2011-2017)		
	Border	Non-Border	Difference	Border	Non-Border	Difference
Number of Counties	51	432		62	268	
Per-Pupil Total Current Spending (\$)	11,563 (409)	11,041 (1,964)	522** (264)	10,299 (1,509)	11,165 (1,689)	-866 (600)
Per-Pupil Instruction Spending (\$)	7,106 (234)	6,774 (1,223)	332** (160)	6,359 (1,025)	6,616 (831)	-257 (351)
Per-Pupil Total Revenue (\$)	12,594 (713)	13,038 (2,686)	-444 (368)	11,255 (1,437)	12,206 (1,742)	-951* (562)
Per-Pupil Federal Revenue (\$)	2,893 (337)	1,459 (684)	1,434*** (121)	1,284 (377)	1,792 (866)	-508* (285)
Per-Pupil State Revenue (\$)	4,364 (585)	5,337 (2,026)	-973*** (320)	5,836 (961)	6,286 (1,199)	-450 (377)
Per-Pupil Local Revenue (\$)	5,337 (554)	6,242 (2,536)	-905*** (283)	4,135 (1,247)	4,128 (1,667)	6 (335)
Instruction Share of Spending (%)	(0.9)	(2.9)	(0.3)	(2.1)	(2.5)	(0.7)

Notes: * p < 0.1, ** p < 0.05, *** p < 0.01. Annual Survey of School System Finances. Sample includes counties in states that adopted RTW laws between 2011-2017 (WI, MI, WV, KY) and their never-RTW neighbors (MN, IL, OH, PA, MD, MO). Border counties share a physical border with a state from the opposite treatment group. School district averages, aggregated to county level using fall enrollment as weights, for 2010, one year before the first RTW law was adopted in the first focus state. Financial variables measured in current dollars. Standard deviations shown in parentheses for means, standard errors shown in parentheses for differences.

Table 5: Effect of Right-to-Work Laws on Student Achievement

	(1) No Controls	(2) Basic	(3) Full Controls
Panel A: Mathematics Achievement			
RTW Law	0.036*** (0.009)	0.050*** (0.013)	0.062*** (0.018)
Conventional SE	[0.009]	[0.014]	[0.018]
Wild Bootstrap SE			
Observations	12,537	12,537	12,537
Panel B: Reading Achievement			
RTW Law	0.017* (0.009)	0.042*** (0.015)	0.058*** (0.017)
Conventional SE	[0.009]	[0.015]	[0.018]
Wild Bootstrap SE			
Observations	13,115	13,115	13,115

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Stanford Education Data Archive, Version 5.0 (2009-2018) (Reardon et al. 2024). Results from Callaway and Sant'Anna (2021) regression adjustment estimator. Conventional standard errors clustered at county-pair level shown in parentheses. Wild bootstrap standard errors [999 replications] shown in brackets. Basic controls include racial composition (percent Asian, Black, Hispanic, Native American, and White) and urbanicity measures (percent rural, suburban, town, and urban). Full controls add pre-treatment means of student achievement, economic conditions (unemployment rate, poverty rate, median household income, SNAP receipt rate), and education variables (percent college degree, percent single mother households). Sample includes border-county pairs where one county is in a state that adopted RTW between 2011-2017 (WI, MI, WV, KY) and the other is in a never-RTW neighbor state (MN, IL, OH, PA, MD, MO). Student achievement measured in student-level standard deviations.

Table 6: Effect of Right-to-Work Laws on Student Achievement

	(1)	(2)	(3)	(4)	(5)
Panel A: Mathematics Achievement (Grades 3-8)					
Right-to-Work Law	0.014 (0.018)	0.045*** (0.009)	0.045*** (0.009)	0.045*** (0.009)	0.046*** (0.009)
Observations	12,537	12,537	12,537	12,537	12,537
Adjusted R-squared	0.000	0.370	0.588	0.645	0.740
Mean of Dep. Var.	0.011	0.011	0.011	0.011	0.011
Std. Dev. of Dep. Var.	0.262	0.262	0.262	0.262	0.262
Controls	None	None	None	Basic	Full
County-pair FE	No	No	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
Panel B: Reading Achievement (Grades 3-8)					
Right-to-Work Law	0.029** (0.014)	0.037*** (0.008)	0.037*** (0.008)	0.038*** (0.008)	0.038*** (0.008)
Observations	13,115	13,115	13,115	13,115	13,115
Adjusted R-squared	0.003	0.233	0.485	0.563	0.685
Mean of Dep. Var.	0.016	0.016	0.016	0.016	0.016
Std. Dev. of Dep. Var.	0.212	0.212	0.212	0.212	0.212
Controls	None	None	None	Basic	Full
County-pair FE	No	No	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Stanford Education Data Archive, Version 5.0 (2009-2018) (Reardon et al. 2024). Sample and control variables defined as in Table 5. All specifications include county and grade fixed effects. Student achievement measured in student-level standard deviations. County-pair fixed effects group counties that share a physical border across state lines. State fixed effects control for time-invariant state characteristics. Year fixed effects control for common shocks affecting all counties in a given year.

Table 7: Effect of Right-to-Work Laws on School Spending

	(1) No Controls	(2) Basic	(3) Full Controls
Panel A: Total Current Spending (Per Pupil)			
RTW Law (\$)	-619.06***	-561.15***	-606.51***
Conventional SE	(98.98)	(156.40)	(186.37)
Wild Bootstrap SE	[102.03]	[156.36]	[185.91]
Observations	1,109	1,109	1,109
Panel B: Total Current Spending on Instruction (Per Pupil)			
RTW Law (\$)	-618.93***	-529.80***	-567.57***
Conventional SE	(71.40)	(109.80)	(150.42)
Wild Bootstrap SE	[73.94]	[115.43]	[147.01]
Observations	1,109	1,109	1,109

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Annual Survey of School System Finances (2009-2018). Results from Callaway and Sant'Anna (2021) regression adjustment estimator. Conventional standard errors clustered at county-pair level shown in parentheses. Wild bootstrap standard errors [999 replications] shown in brackets. Sample and control variables defined as in Table 5. Financial variables measured in current dollars per pupil. Total current spending includes all current operating expenditures. Instructional spending includes only direct classroom expenditures like teacher salaries and instructional materials. School district averages aggregated to county level using fall enrollment as weights.

Table 8: Effect of Right-to-Work Laws on School District Spending

	(1)	(2)	(3)	(4)	(5)
Panel A: Total Current Spending (Per Pupil)					
Right-to-Work Law (\$)	292.87 (199.05)	-717.56*** (122.79)	-719.02*** (122.70)	-719.02*** (123.21)	-709.18*** (121.99)
Observations	1,109	1,109	1,109	1,109	1,109
Adjusted R-squared	0.005	0.514	0.761	0.832	0.853
Mean of Dep. Var.	10,849.05	10,849.05	10,849.05	10,849.05	10,849.05
Std. Dev. of Dep. Var.	1,700.61	1,700.61	1,700.61	1,700.61	1,700.61
Controls	None	None	None	Basic	Full
County-pair FE	No	No	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
Panel B: Total Current Spending on Instruction (Per Pupil)					
Right-to-Work Law (\$)	-45.94 (118.77)	-628.48*** (83.29)	-628.57*** (83.25)	-628.57*** (83.60)	-624.38*** (84.13)
Observations	1,109	1,109	1,109	1,109	1,109
Adjusted R-squared	-0.001	0.529	0.750	0.800	0.813
Mean of Dep. Var.	6,471.16	6,471.16	6,471.16	6,471.16	6,471.16
Std. Dev. of Dep. Var.	1,088.00	1,088.00	1,088.00	1,088.00	1,088.00
Controls	None	None	None	Basic	Full
County-pair FE	No	No	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Annual Survey of School System Finances (2009-2018). Sample and control variables defined as in Tables 5 and 7. All specifications include county fixed effects. Financial variables measured in current dollars per pupil. County-pair fixed effects group counties that share a physical border across state lines. State fixed effects control for time-invariant state characteristics. Year fixed effects control for common shocks affecting all counties in a given year. School district averages aggregated to county level using fall enrollment as weights.

Figures

Figure 1: Implementation of Right-To-Work Laws in the United States

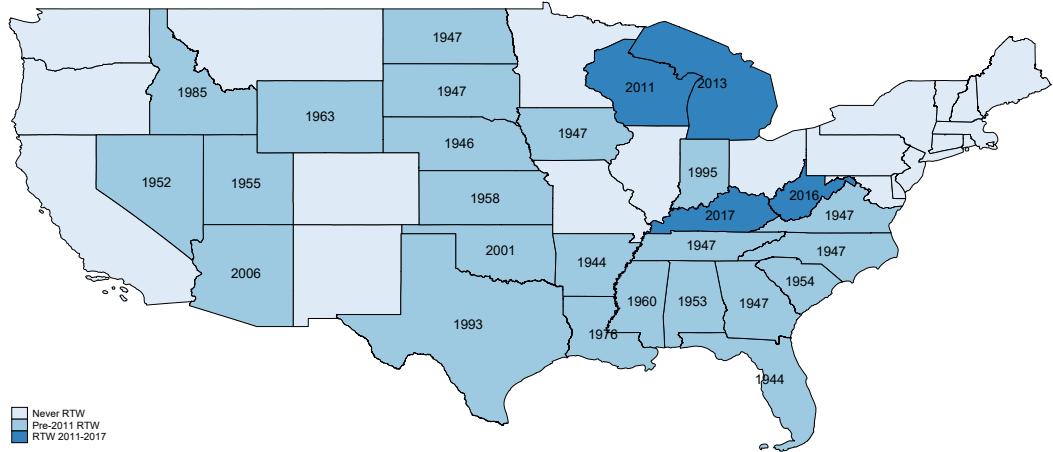
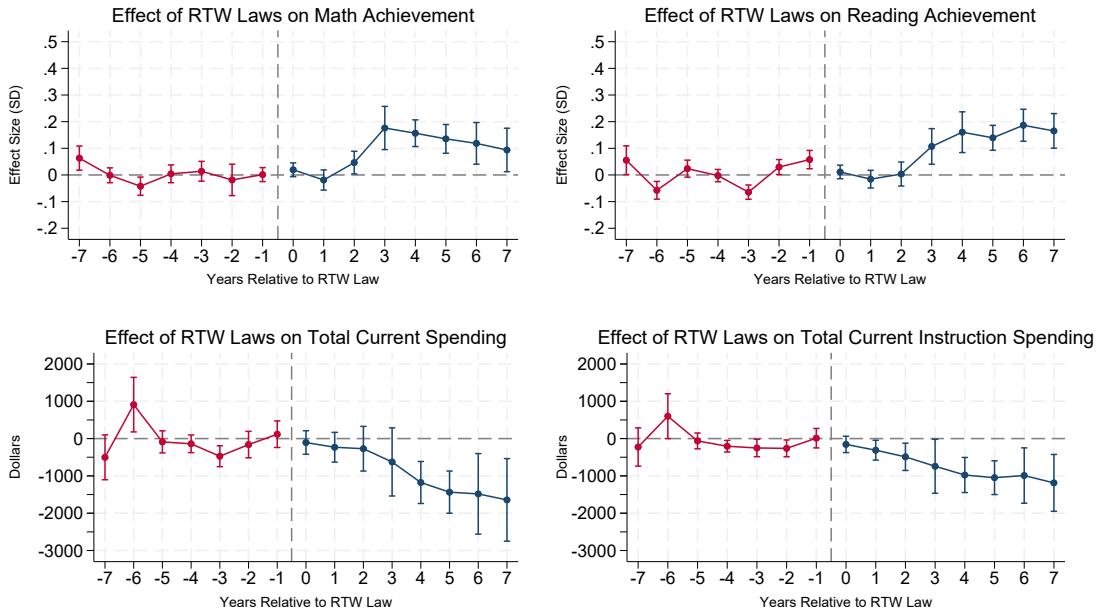


Figure 2: Event Study Plots



Notes: Stanford Education Data Archive, Version 5.0 (2009-2018) (Reardon et al. 2024) (Top left and top right). Annual Survey of School System Finances (2009-2018) (Bottom left and bottom right). Results from Callaway and Sant'Anna (2021) regression adjustment estimator. Tick marks show 95% confidence intervals using wild cluster bootstrapped standard errors (999 replications). Includes controls for racial composition (percent Asian, Black, Hispanic, Native American, and White) and urbanicity measures (percent rural, suburban, town, and urban), and pre-treatment means of student achievement, economic conditions (unemployment rate, poverty rate, median household income, SNAP receipt rate), and education variables (percent college degree, percent single mother households). Sample includes border-county pairs where one county is in a state that adopted RTW between 2011-2017 (WI, MI, WV, KY) and the other is in a never-RTW neighbor state (MN, IL, OH, PA, MD, MO). Student achievement measured in student-level standard deviations.