Wage and Employment Effects of Right-to-Work Laws in the 2010s

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Abstract

I use administrative data from the Quarterly Census of Employment and Wages to estimate the effects of recent anti-union right-to-work (RTW) laws on labor markets (defined by state-by-industry cells) in affected states. Exploiting the plausibly exogenous timing of such laws in a difference-in-differences design, I find that RTW decreased earnings, with most reductions occurring in industries with high union coverage. For employment and establishment counts, I find no statistically significant effects. I show that wage reductions are closely correlated with RTW-induced declines in union coverage and I rule out alternative explanations for findings such as economic changes in comparison group states and businesses using RTW as a proxy for other pro-management policies.

Introduction

Since the 1947 Taft-Hartley Amendments to the National Labor Relations Act (NRLA), state governments have been able to pass right-to-work (RTW) legislation that prohibits union security clauses in collective bargaining agreements (CBAs). Because union security clauses require that workers covered by a CBA financially contribute to the union either through membership dues or fair share fees, RTW laws effectively allow free-riding and weaken unions. Most RTW laws were quickly adopted in States in the South and West in the first decade after Taft-Hartley's passage, with only four states adopting RTW between 1960 and 2010.

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The period of relative dormancy of RTW adoption ended in 2012, when Indiana passed the first new RTW law in 11 years. Over the next five years, four more states with historically higher levels of union activity, passed and implemented RTW laws. As of writing, with RTW laws on the books in 27 states, a slight simple majority of US workers live in states that prohibit union security clauses. For these two reasons, the recent wave of RTW laws incited controversy and heightened discussion among policymakers, organized labor, and industry groups. On one hand, proponents of RTW laws celebrated the Supreme Court's decision in *Janus v. AFSCME*, which effectively extended RTW through the American public sector. There has been mounting opposition to RTW also. Since 2018, unions mounted a successful campaign to repeal Missouri's RTW law via referendum before it could take effect. Democratic governors in Michigan and Virginia have campaigned on the repeal of RTW. Federally, the Protecting the Right to Organize (PRO) Act – currently stalled in the Senate – would amend the NLRA to rid states of the ability to pass RTW. Generally, this controversy stems from evidence that RTW decreases the frequency of union elections, weakens union bargaining power, and reduces CBA coverage and union membership across labor markets . However, empirical evidence on the second-order effects of RTW on labor market outcomes such as employment and earnings is mixed (Holmes 1998; Farber 2005; Stevans 2009; Eren and Ozbeklik 2016; Bloom et al. 2019; Chava et al. 2020; Fortin et al. 2022).

Using a combination of administrative data from the Quarterly Census of Employment and Wages (QCEW) and survey data from the Current Population Survey's Monthly Outgoing Rotation Groups (CPS MORG) questionnaires, this paper exploits the staggered adoption of RTW across the five aforementioned states in the 2010s to estimate the policy's effect on state labor markets, defined as state-by-industry cells. Specifically, I identify effects on earnings, employment, union coverage, and the union wage premium. To identify causal effects of RTW, I use a difference-in-differences design. This design relies on the assumption that the outcomes in non-RTW states serve as valid counterfactuals to outcomes in the new RTW states had they not implemented the policy. This assumption is plausible in this context. While early RTW states likely "selected into RTW" due to weak unions and economic changes" the RTW-adopting states in the 2010s had historically higher levels of union activity as evidenced by large statehouse protests against RTW before adoption (Stevans 2009; Wade 2019). Further, I show that earnings, employment, union coverage, and union wage premia were not differentially changing in the newly adopting RTW states prior to the laws' effective dates. I use the stacked difference-in-differences estimator of Cengiz et al (2019) and Baker et al (2022) to avoid bias associated with staggered policy adoption and typical two-way fixed effects estimation (Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Sun and Abraham 2021).

Because RTW primarily affects labor market outcomes by affecting unions themselves, I run flexible regressions that allow for heterogeneous effects between industries with high pre-adoption union coverage and those with low pre-adoption union coverage. While low union coverage industries remained unaffected, I find that average weekly earnings declined by about 1.3% in industries with higher than average pre-RTW union coverage (Construction, Education, Manufacturing, Natural Resource Extraction, Transportation and Warehousing, and Utilities). I find that RTW did not significantly affect employment, regardless of pre-RTW industry union coverage, challenging the conventional wisdom that by weakening unions, RTW induces higher labor demand. I then examine the the potential role of two important measures of union strength that correlate with wage outcomes. First, I find that across all industries RTW reduces union coverage itself – as measured by the percentage of workers in an industry whose work is covered by a collective bargaining agreement – by about 11.6%. This estimate does not mask much heterogeneity by pre-RTW union exposure. Second, I find some evidence that in highly unionized industries, RTW actually increases the affects the union wage premium – the earnings gain enjoyed by workers covered by a union contract relative to their nonunion counterparts. These main results do not seem to be biased by changing aggregate outcomes in RTW states prior to the laws' effective dates, a finding that holds for specific state RTW laws as well. Further, these main findings are robust to different inference procedures, definitions of earnings and employment, and alternative comparison groups.

I test for two potential mechanisms commonly invoked to explain why RTW may affect labor markets. First, I examine the role of RTW as a policy signal, finding little evidence that RTW functioned primarily as a signal used by employers to predict future "pro-management" legislation, a hypothesis frequently floated in the literature (Holmes 1998; Bloom et al. 2019). I use Missouri's RTW law, which was overturned by referendum before it could take effect, as a placebo test, finding no significant results compared to Kentucky's RTW that passed and took effect concurrently. Additionally, I find results that while RTW is associated with GOP leadership of a state legislature, GOP leadership trends began before RTW. Instead of the indirect "policy signal" mechanism, I find strong evidence that RTW primarily affected earnings by weakening labor unions' ability to increase aggregate wages. At a state-industry level, RTW's wage effects are negatively correlated with its union wage premium effects. In other words, where RTW widened the earnings gap between union and nonunion workers, it also led to more precipitous earnings declines overall. This finding suggests that some negative earnings effects are borne by nonunion workers, indicating that RTW reduces wages by reducing the union threat effect on nonunion employers. By contrast, there is no clear correlation across state-industry cells between the RTW union coverage effect and the RTW wage effect.

These findings shed light onto the complex relationship between RTW, union bargaining power, firm wage setting ability, and labor market outcomes, a relationship that increasingly relevant given recent policy debates. Additionally, this paper contributes to a few strands of literature. First, this paper builds on research studying the effect of RTW on wages, adding evidence that RTW reduces earnings across labor markets, especially for union dense industries. Of studies of more recent laws, Farber (2005) finds that while Idaho's RTW law significant reduced the wages of non-union workers, Oklahoma's RTW did not noticeably affect wages, a finding supported by Eren & Ozbeklik's (2016) synthetic control analysis of Oklahoma's law. Chava et al (2020) use a difference-in-differences design to examine RTW's effects on firms with collective bargaining agreements in the five states examined in this paper as well as Oklahoma. They find that collectively negotiated raises decreased in magnitude, reducing wage growth in unionized establishments relative to establishments in non-RTW states. Most recently, Fortin et al (2022) use individual level data from the Current Population Survey to find that RTW reduces the wages of both union and nonunion workers, an effect felt more prominently by workers in union dense industries.

Additionally, this paper provides new evidence of RTW's employment effects. Holmes' (1998) seminal study of RTW and employment used a border discontinuity to show that "crossing into" an RTW state from a non-RTW state was associated with higher manufacturing establishment counts and employment. However, he does not attribute this effect entirely to RTW, pointing out that RTW serves as a general proxy for other state-level pro-business policies. More recently, Bloom et al (2019) find that Michigan and Indiana's RTW laws increased employment. However, because their main focus was on firm-level management practices, their regressions do not account for important factors such as state population, state economic growth, and other policies taking effect at the same time. Using a more robust identification strategy geared specifically toward isolating RTW's economic effects, this paper challenges Holmes' (1998) and Bloom et al's (2019) findings, suggesting that despite reducing wages, RTW had no meaningful effect on employment nor the number of establishments within each labor market.

Finally, this paper examines the mechanisms through which RTW affects economic outcomes, reinforcing evidence that RTW primarily alters earnings and employment by weakening unions themselves. Ellwood & Fine (1987) presented the first extant use of panel data to identify causal effects of RTW policies on union organizing, finding that RTW reduced the frequency of union elections and organizing drives in affected states, driving union membership down. More recently Eren & Ozbeklik (2016) find that Oklahoma's 2001 RTW law reduced union membership across the state's economy and specifically in the manufacturing sector, findings reflected in Murphy's (2022) analysis of later laws. Other studies show that RTW reduces union bargaining power, revenues, and elections (Matsa 2010; Zullo 2020, 2021). The findings in this paper suggest that while RTW reduces union coverage, its dampening of the union threat effect especially drives its negative earnings effects.

The rest of this paper is structured as follows. The next section (Section I) provides a brief history of RTW laws and explains how they function. Following this background, in Section II, I discuss the theoretical considerations necessary to make predictions on RTW's effects. In Section III, I discuss data sources and provide descriptive statistics before detailing estimation and identification strategies. Section IV provides empirical results, before Section V examines and mechanisms and Section VI discusses results and concludes.

I History of RTW

The National Labor Relations Act (also known as the Wagner Act), passed by Congress in 1935 as part of the Second New Deal, authorized American companies to legally agree on a fixed mode of management-union relations. A union and management could agree through a CBA that a workplace was an "open shop" (a union cannot require membership or any form

Table 1: Post-2010 State Adoption of RTW Laws By Year

State	Date RTW Passed Into Law	Date RTW Took Effect
Indiana	February 1st, 2012	March 14th, 2012
Michigan	December 12th, 2012	March 27th, 2013
Wisconsin	March 6th, 2015	March 11th, 2015
West Virginia	February 6th, 2016	July 1st, 2016
Kentucky	January 6th, 2017	January 7th 2017

Notes: Data gathered from state RTW statutes and legal notices provided by the National Right to Work Foundation (NRTW 2012, 2013, 2015, 2016, 2017). Effective date is the first day in which CBAs could not legally require membership or financial contribution clauses.

of payment as a stipulation of employment), an "agency shop" (workers can opt out of union membership but must pay an equivalent fee in exchange for CBA protections), a "union shop" (a company can hire new workers who must join the union at a later date), or a "closed shop" (employees must join a union as a stipulation of employment and if they quit the union they must be fired).

This legal environment for labor-management relations only lasted about a decade. In 1947, Congress overrode President Harry Truman's veto, passing the Taft-Hartley amendments to the NLRA. Section 14(b) of Taft-Hartley prohibited the "closed shop" and gave state governments the authority to ban the "union shop" if they saw fit. The passage of Taft-Hartley induced a series of state-level policy changes, as states in the South began to prohibit the "union shop" by passing laws soon known as "right to work" laws.[1] By the late 1950s, RTW had been implemented in nearly every former Confederate state and several states in the West.

Between the 1960 and 2010, only a few additional states passed RTW laws. While the first RTW law passed in the 21st century was passed in Oklahoma in 2001, RTW only began to gain a legislative foothold in the Rust Belt in the 2010s, with the rise of the Tea Party Movement and increasing advocacy on the part of manufacturing industry affiliated lobbyists (Wade 2019).[2] Each of the laws passed over the course of the last decade draw considerably controversy and protest, especially in Indiana, Michigan, and Wisconsin (Wade 2019).[3] West

[1] Right-to-work advocates – often business owners and industrialists – in the south often explicitly justified their support for the laws with racial animus, claiming that strong unions would further racial integration (Devinatz 2015). In the west, industrialists in favor of RTW appealed less to anti-Black racial animosity and more to anticommunism, "American values" and criticisms of "union corruption" (Devinatz 2015).

[2] Notably, Michigan and Indiana are home to substantial portions of the United States' auto manufacturing, with General Motors, Ford, and Chrysler all headquartered in Detroit. Manufacturing is also highly prevalent in Wisconsin. In West Virginia and Kentucky, manufacturing is slightly less prevalent.

[3] As I argue in section III, this fact renders the passage of the five most recent RTW laws more exogenous

Virginia's law was actually passed only after the state legislature overturned the veto of Earl Ray Tomblin, the Democratic governor at the time. This differs from the initial setting in the 1940s through 1960s, when RTW were quickly adopted and organized labor did not sufficiently challenge the laws' adoption.[4]

Table 1 shows the timeline of RTW adoption and implementation since 2010. I list the date on which RTW was passed into law and the date it took effect. Because the more recent RTW laws focus explicitly on allowing workers covered by CBAs to opt out of any financial contribution to the union, RTW effective dates are defined by the point in time when negotiated NLRA-regulated CBAs are no longer legally able to require covered members to pay dues or fair share fees, or other union charges.

II Theoretical Effects and Mechanisms of RTW

Economic effects of RTW follow "first order" effects of RTW on union bargaining power. I f RTW sufficiently induces free-riding on the among CBA-covered workers who now may opt out of paying dues or fair-share fees, union funding streams will decline. With fewer dues paying members, unions may be less able to expand into new shops, leading to further theoretical declines in membership rates and a compounding negative effect on union finances. Notably, even if free-riding does not significantly increase, unions' concerns about hypothetical free-riding may alter their ability to negotiate wages and benefits.[5] In RTW states, unions may exhibit more risk-averse organizing behavior, avoiding new workplaces that may not yield a high concentration of dues paying members given union recognition (Ellwood and Fine 1987). This would lead to fewer new union members and less CBA coverage.

Reductions to union bargaining power can affect aggregate wages in a labor market by altering the magnitude of three effects of unions on earnings: the "union wage premium", the "union threat effect", and the "union spillover effect". The union wage premium captures

than those past immediately following Taft-Hartley.

[4]In one recent case, voters actually overturned Missouri's RTW law in 2018 before it could take effect, demonstrating the extent to which RTW passage in state legislatures does not necessarily correlate to anti-union sentiment among the general public.

[5]Budd and Na (2000) find that free-riders – workers covered by a CBA but not dues-paying union members – receive a 5% lower wages than their coworkers in the union. One explanation is that unions are able to push members into higher paying jobs. If workers at already organized workplaces know this, they may not free ride even with the implementation of RTW.

differences in wages between union and non-union workers, typically associated with wage provisions in CBAs that push wages up above the wages of similar nonunion workers. Usually, estimated union wage wage premia range from 12 to 25%, depending on adjustment for other factors, with most estimates at around 15% (Kulkarni and Hirsch 2021). By contrast, the union threat effect and union spillover effect act on the wages of nonunion workers. The union threat effect kicks in if nonunion establishments increase wages in response to the threat of unionization or because they risk losing workers to higher paid unionized establishments (Naidu and Posner 2022). Increases in the magnitude of union threat effects may decrease union wage premia, as similarly situated nonunion workers experience wage gains and the wage difference between union workers and nonunion narrows.

By weakening unions, RTW could potentially reduce the magnitude of positive union wage premia and union threat effects, while reducing the magnitude of negative union spillover effects. RTW-induced reductions in union membership should theoretically reduce wages of workers in RTW states compared to workers in non-RTW states whose ability to join a union remains unrestricted. Additionally, with reduced funding streams, unions would be less likely to attract skilled arbitrators, organizers, and lawyers, weakening the ability of unions to negotiate and win strong CBAs, thereby reducing wages for union workers in RTW states relative to non-RTW states.

Theoretically, employment effects of RTW follow wage effects. However, the effect of wage floors (such as those established in CBAs) on employment levels depends on labor demand elasticity, long run-short run dynamics and labor market frictions (Manning 2021).[6] This heteroegeneity reflects different effects of unionization itself in industries with different market characteristics (Clark 1984). If labor demand is elastic in the short run and labor markets are relatively frictionless, RTW will bring wage levels closer to competitive equilibrium and induce job creation as establishments hire more labor and enter newly favorable labor markets (Hicks et al. 2016). By contrast, if labor markets are characterized by inelastic demand or monopsonistic wage-setting, then RTW induced wage decreases would lead to either negative or null employment effects. RTW could also lead to employment changes in less-heavily unionized sectors by altering the union spillover effect that occurs when labor demand changes induced

[6]Manning (2021) makes these theoretical observations to explain why empirical studies of minimum wages often yield null or positive employment effect estimates. These observations can be repurposed into a collective bargaining context because CBAs set within-establishment wage floors (Ashenfelter et al. 2010)

by unionization lead to labor supply shocks in initially unaffected labor markets. The canonical example (Kahn 1980a) posits that if unionization reduces labor demand in an affected labor market, workers will move to less unionized labor markets. The resulting positive labor supply shock reduces wages in these nonunion – or low union coverage – labor markets. However, because the employment effect of unionization itself depends on market frictions and labor demand elasticity, the direction of the union spillover effect is ambiguous.

As I discuss in the next section, this paper tests between these alternative theoretical predictions of RTW's effects. By examining wage and employment effect heterogeneity between heavily unionized and less heavily unionized industries, I will be able to check if RTW actually alters the union spillover effect. I also directly test for changes to the union wage premium and union coverage itself, both of which play important roles in mediating RTW's possible effects on wages.

III Data and Econometric Approach

In this section I discuss my data sources and sample construction. Then I explain my econometric approach, detailing my identification, estimation, and inference strategies.

Data Sources

My data come from several sources. Primarily I use the annual "by industry" QCEW files, which provide employment counts and average weekly earnings within each state and NAICS code industry. QCEW data comes from employers' unemployment insurance filings, so it captures an effective census of earnings and employment counts aggregated at different levels (Waddell 2015). Although the QCEW includes public sector workers, I restrict my sample to private sector workers because the vast majority of the private sector is subject to the NLRA and thus affected by RTW laws.[7] By contrast, public sector labor relations are typically governed by state-level labor statutes and the corresponding state public labor relations board or agency.

[7] There are a few private sector industries that are also not governed by the NRLA. Specifically, farmworkers, railroad industry workers, and airline industry workers are exempt from NLRA regulation and thus exempt from RTW laws. However, given that many of the unions that represent farmworkers, airline workers, and rail workers under other statutes also represent NLRA-covered workers it is possible that there can be spillover effects of RTW even in sectors not technically affected by the laws. It is worth nothing that agricultural farmworkers and railroad workers are already excluded from the QCEW (Waddell 2015).

Second, I use the CPS Monthly Outgoing Rotations Group (CPS MORG), which provides microdata on worker demographics, industry, union status, usual weekly hours, and weekly earnings (Flood et al. 2022). The CPS MORG is the only Census survey that asks respondents about weekly earnings and union status, necessitating its use here instead of other alternative sources such as the American Community Survey. I also use the CPS to supplement data on weekly earnings in the QCEW with data on hours worked and hourly wages.[8] This helps me ensure that any detected effect on average weekly wages may pick-up changes in average hours worked per week, as I explain in Section IV.

My reliance on the CPS guides my decision to use the annual "by industry" QCEW files instead of more granular county-level quarterly QCEW data. For all analyses, I examine data within annual "labor markets" – cells defined by state, two-digit NAICS industry classification, and year. Any finer classification reduces my ability to generate valid annual estimates of industry-specific demographics, union coverage, and union wage premia from the CPS because CPS sample sizes are too small. Because the CPS and the QCEW use slightly different industrial categorization schemes, I use the IPUMS crosswalk to connect 1990 Census industry classification to their corresponding two-digit NAICS code.

I compute union coverage within each annual labor market by computing the fraction of workers who report either being a union member or being covered by a union in the CPS MORG, weighting aggregation by CPS earnings weights. To compute union wage premia for a given annual labor market, I run regressions of individual log weekly earnings on an indicator for whether a workers' job is covered by a union contract using data. Running a separate regression for each labor market generates estimates of the raw union wage premium within – the coefficient on the union status indicator – for each group of workers. Notably, these union wage premia are not causally identified. Instead, they capture the raw gap between workers covered by a union contract and other workers, without accounting for worker characteristics aside from state of residence and industry of employment. Equation 1 shows this model

$$log(WeeklyEarnings)_{ist} = \upsilon \cdot CBA_{ist} + \epsilon_{ist} \quad (1)$$

[8] However, I use the QCEW instead of the CPS for main models because as a nearly universal administrative dataset, it it is more comprehensive than the CPS MORG. As a result, it is less prone to measurement error and participant nonresponse bias than the CPS. For example, there are some state-industry cells that report zero union coverage in the CPS due to low sample sizes. I discuss this further in Section IV.

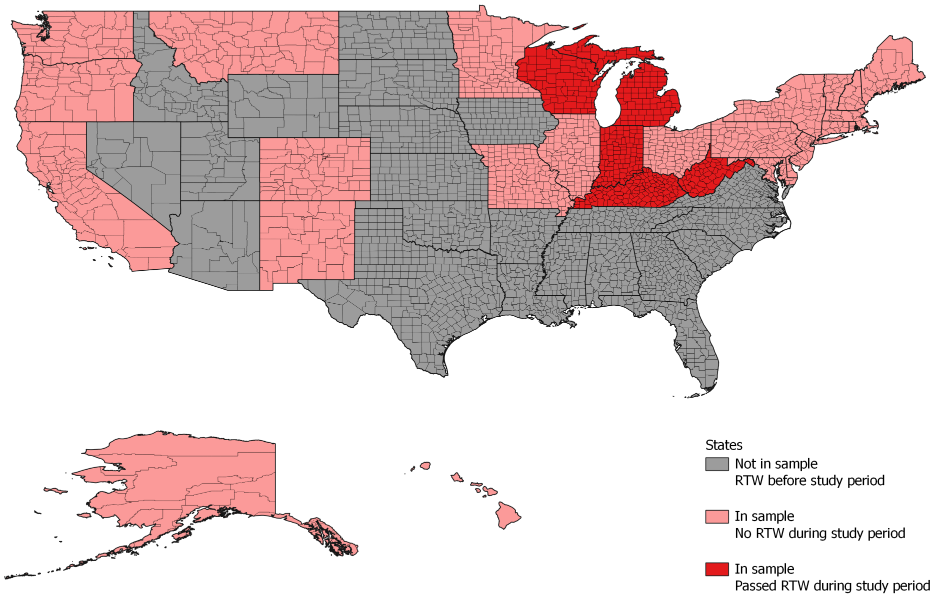


Figure 1: States by Sample Status

where i indexes two-digit NAICS codes, s indexes states, and t indexes years. Because I run this regression using CPS MORG data within each state-industry-year cell, v captures the raw union wage premium within each annual labor market. CBA is a binary indicator for whether a worker is represented by a collective bargaining agreement.[9]

Data on the timing of RTW laws comes from the National Conference of State Legislatures. I consider a state "treated" by RTW beginning the year of implementation. I gather my remaining data from a few sources. I use Vaghul and Zipperer's (2021) data on state minimum wages. I obtain annual state population estimates from the Census Bureau and state GDP numbers from the Bureau of Economic Analysis. Data on state legislature partisan composition come from the National Conference of State Legislatures. I use data from these sources for covariates in the regressions described below.

Table 2 shows summary statistics from 2010, separating data between the five states that passed RTW between 2010 and the present, and the 23 states and the District of Columbia that to date have never had an effective RTW law, a classification shown in Figure 1. Industries in soon-to-be RTW states paid lower weekly wages, employed fewer workers, and included fewer establishments than industries in comparison group states, on average. However, industry union

[9]Notably, the CPS MORG asks participants if they are union members, nonmembers whose work is covered by a CBA, or workers whose workplace is not unionized. I use CBA coverage, rather than union membership to capture the slightly larger number of workers whose workplace is unionized. Generally though, union coverage and union membership exhibit parallel changes over time.

Table 2: 2010 Industry Characteristics in New RTW and Comparison States

	New RTW States (N=89)				Comparison (N=425)			
	Mean	St. Dev	Min	Max	Mean	St. Dev	Min	Max
Average Weekly Wage ($)	832.07	356.35	242.00	1888.00	988.56	498.64	0.00	3874.00
Employment (Thousands)	109.33	125.01	1.66	537.91	129.85	212.22	0.00	1527.86
Establishment Count (Thousands)	7.35	6.76	0.23	30.36	11.04	29.21	0.02	526.89
Union Coverage	0.09	0.11	0.00	0.62	0.09	0.10	0.00	0.50
Union Wage Premium	0.16	0.44	-1.81	1.14	0.17	0.42	-2.21	1.71
State Population (Millions)	5.67	2.66	1.86	9.88	6.61	8.12	0.62	37.60
GDP Per Capita (Thousands)	41.72	0.03	37.29	46.68	57.57	25.41	3.96	176.90
Minimum Wage	7.28	0.06	7.25	7.40	7.65	0.46	7.25	8.67
Democrat-Run Legislature	0.20	0.40	0.00	1.00	0.53	0.50	0.00	1.00
Share College-Educated Workers	0.27	0.18	0.00	0.78	0.32	0.20	0.00	0.89
Share White Workers	0.93	0.05	0.78	1.00	0.84	0.17	0.05	1.00
Share Female Workers	0.42	0.21	0.00	0.89	0.42	0.20	0.00	0.83
Average Age	40.28	4.34	28.86	58.62	40.33	4.19	27.01	55.28

Notes: Data from the QCEW, authors' computations from the CPS MORG, and other sources listed in the text.

coverage and union wage premia were pretty similar across both groups of states.The average union wage premium was around 0.16 for the new RTW states and 0.17 for the the comparison group states, estimates that generally fall into typical ranges in the literature. Average industry demographic composition was similar across both groups of states, with the exception of race and college education. Workers in industries in the new RTW states were whiter and less likely to hold a college degree on average.

Stacked Difference-in-Differences Design

The plausibly exogenous timing of RTW laws across the five states lends itself to a difference-in-differences (DID) approach. However, because the five states had different baseline union coverage, and wage and employment levels, it is likely that treatment effects differ across states. Further, the effect of RTW is not likely to be constant over time. For example, RTW may yield different effects are CBAs expire (Biasi and Sarsons 2021). Unfortunately, with staggered timing of policy changes and likely heterogeneity in effect between states and over time, a twoway fixed-effects regression approach yields causally invalid DD estimates as regression weighting generates improper comparisons between newly treated and already treated units (Goodman-Bacon 2021; Sun and Abraham 2021; Callaway and Sant'Anna 2021). To correct for this, I use the stacked DID estimator first used by Cengiz et al (2019) and further developed by Baker et al (2022) to compare each new RTW state to "clean" control states - states in which RTW was not in effect during my study period. Specifically I obtain subsamples of each new RTW state and all corresponding clean comparison states between 6 years before the RTW effective date and 5 years after. Following Cengiz et al (2019) and Baker et al (2022), I then stack each of these five datasets together, so that my final dataset is balanced in time relative to RTW's effective date.

Using the stacked sample, I estimate Equation 2

$$Y_{istb} = \alpha_{isb} + \theta_{itb} + \beta \cdot RTW_{stb} + \delta \cdot \mathbf{X}_{istb} + \sigma \cdot \mathbf{S}_{stb} + \epsilon_{istb} \tag{2}$$

where i indexes two-digit NAICS industries, s indexes states, t indexes years relative to RTW taking effect, and b indexes data "blocks" defined by each new RTW state and its set of clean controls. Y_{istb} is the outcome of interest, and α_{isb} and θ_{itb} respectively include state-industry-block and year-industry-block fixed effects. The interaction of typical group and time fixed effects – in this case industry-state and industry-year fixed effects – with indicators for each of the five RTW blocks ensures that β – my parameter of interest – captures a weighted average of block-specific treatment effects on each affected state (Cengiz et al. 2019; Baker et al. 2022).[10] Further, by using state-by-industry fixed effects and year-by-industry fixed effects, I respectively average out time-invariant variation between different labor markets and differential time effects that heterogeneously affect each industry. Thus, the parameter of interest β – the coefficient on the RTW indicator – captures the pre-post difference in the outcomes difference between RTW states and comparison states.

To make sure that my difference-in-differences estimates capture the effect of RTW itself, and not other economic or policy factors that may have been changing concurrently, I include a vector $\mathbf{S_{stb}}$ of state level controls, including the state minimum wage, the log of the state's per capita GDP, the log of the state's population, and indicators for whether the state legislature was divided, led by the Republican Party, or led by the Democratic Party. I also include a vector of industry level controls $\mathbf{X_{istb}}$ that includes the average age of workers and shares of college educated workers, nonwhite workers, and female workers within a state-industry-block

[10]These block-specific treatment effects are analogous to the cohort average treatment effects on the treated (CATTs) of Sun and Abraham (2021) and Callaway and Sant'Anna (2021).

cell, computed from the CPS MORG. Failing to control for these variables could invalidate identification because these factors are correlated with political support for unions and thus political opposition to RTW legislation, variables such as education level and industry concentration confound RTW laws and union membership/wage outcomes (Hogler et al. 2004; Jacobs and Dixon 2006; Stevans 2009).

Event Study Design

Although the inclusion of controls in Equation 2 helps ensure that my DID estimates are causally valid, the DID strategy still relies on the assumption that wages and employment would have evolved in parallel between new RTW and comparison states in the absence of RTW. Thus, main DID estimates would be causally invalid if for example lawmakers passed RTW in response to changing earnings or employment trends associated with economic conditions or union activity or strength. If said trends continued after RTW's effective date, than traditional DID estimation may incorrectly attribute the changing trends to RTW. To test for differential pre-RTW trends between the new RTW states and the comparison group, I estimate an event study-based DID regression in Equation 3

$$Y_{istb} = \alpha_{isb} + \theta_{itb} + \sum_{t=-5}^{4} \lambda_t \cdot RTW_{stb} + \delta \cdot \mathbf{X}_{istb} + \sigma \cdot \mathbf{S}_{stb} + \epsilon_{istb} \tag{3}$$

where I replace the single RTW variable in Equation 3 with $\sum_{t=-5}^{4} \lambda_t \cdot RTW_{stb}$, sets of indicators that respectively capture leading and lagging effects of RTW on affected states from 5 years before RTW took effect onward. I omit the indicator for six years before RTW's effective date instead of the traditional year-before omission because of the possibility of anticipation effects. In at least the case of Michigan, RTW was initially signed into law at the end of 2012 – the year before it actually took effect. Further, much of the public debate surrounding RTW took place the year before it took effect. Employers with a reasonable belief that RTW would soon be the law of the land could alter their hiring and wage-setting practices even without the certainty of it taking effect. Unions could also anticipate RTW, responding to an increasingly hostile regulatory and political environment by altering organizing and negotiation practices.

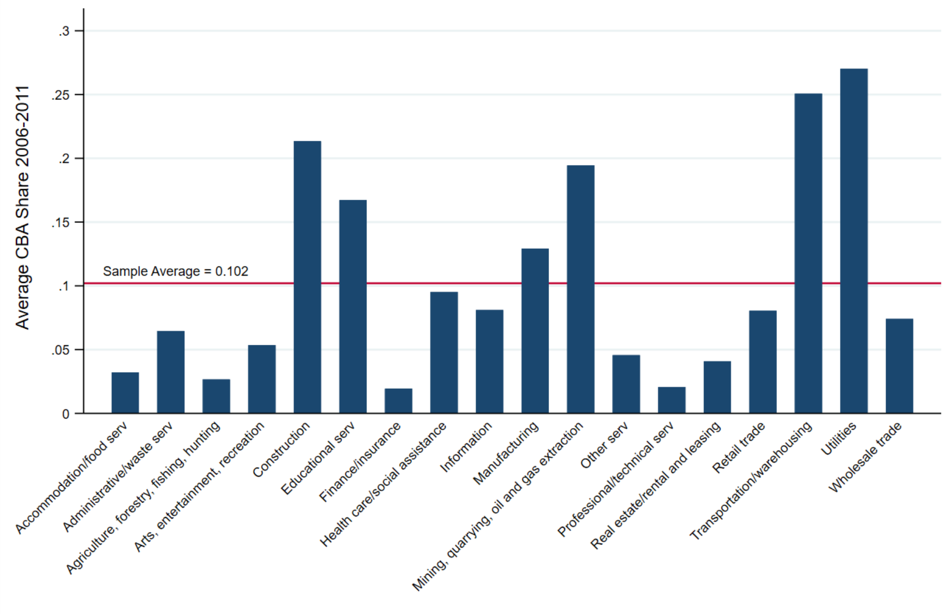


Figure 2: Pre-RTW Union Coverage by Industry

Effect Heterogeneity by Industry

Finally, I test for heterogeneous effects by industry, grouping together industries by whether their union coverage was higher than the sample average between 2006 and 2011. Figure 2 shows the sample average and each industry's union coverage during this time period.[11] To test for heterogeneous effects by industry, I interact all right-hand side variables in equations 2 with indicators for high union coverage and low union coverage industries.

I run these models for two reasons. First, VanHeuvelen (2020) and Fortin et al (2022) find that RTW laws disproportionately affect labor markets that featured initially higher levels of union coverage.[12] Second, by testing for heterogeneous effects of RTW by pre-RTW union coverage, I can test if the laws led to any changes in union spillover effects. For example, if RTW decreases union coverage and increases employment in heavily unionized labor markets as a result of RTW, it could induce a positive wage shock in less heavily unionized labor markets as labor exits, following the inverse of Kahn's (1980b) model.

[11]High union coverage industries include Construction, Educational Services, Manufacturing, Mining/Quarrying and Oil and Gas Extraction, Transportation and Warehousing, and Utilities. Low union coverage industries include Accommodation/Food Services, Administration/Waste Services, Agriculture/Forestry/Fishing/Hunting, Arts/Entertainment/Recreation, Health Care/Social Assistance, Information, Other Services, Professional/technical Services, Real Estate/Rental/Leasing, Retail Trade, and Wholesale Trade.

[12]This makes sense, considering more heavily unionized industries are effectively more "exposed" to RTW laws, since more establishments and more workers would be governed by CBAs subject to the removal of union security clauses.

Table 3: The Effect of RTW on Log Weekly Earnings

	(1)	(2)	(3)
A. Main DID			
RTW	-0.0079**	-0.0049	-0.0048
	(0.0040)	(0.0040)	(0.0040)
R^2	0.9960	0.9963	0.9963
B. DID by 2006-2010 Industry Union Coverage			
RTW*High Union Industry	-0.0106*	-0.0133**	-0.0133**
	(0.0059)	(0.0067)	(0.0067)
RTW*Low Union Industry	-0.0066	-0.0007	-0.0007
	(0.0052)	(0.0049)	(0.0049)
R^2	0.9960	0.9963	0.9964
Observations	24,177	24,177	24,177
State-by-Industry Clusters	518	518	518
State Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: * significant at 90%, ** significant at 95%, *** significant at 99%. Standard errors clustered at the state-industry level are in parentheses. All specifications include State-by-Industry and Year-by-Industry fixed effects. State controls include the natural log of a state's minimum wage, the natural log of a state's per capita GDP, and indicators for legislature partisanship. Industry composition controls include the college educated share of an industry's workforce, the female share of an industry's workforce that is female, and the people of color share of an industry's workforce.

Because the effect of RTW is likely correlated with pre-RTW union coverage within each industry, I cluster standard errors at the state-industry level to ensure clustering mirrors the level of treatment (Bertrand et al. 2004).[13] This also helps account for the fact that some of the same exact units from clean comparison states show up several times across the stacked dataset (Dube et al. 2010)

IV Results

In this section I report results from the main two econometric specifications I start by examining RTW's earnings and employment effects, before turning to RTW's effects on union strength. I then examine result heterogeneity by industry and sumamrize several robustness checks.

[13] Alternatively, Appendix A shows results of main regressions with wild cluster bootstrapped p-values, with clustering at the more conservative state level. The statistical significance of main results does not substantively change.

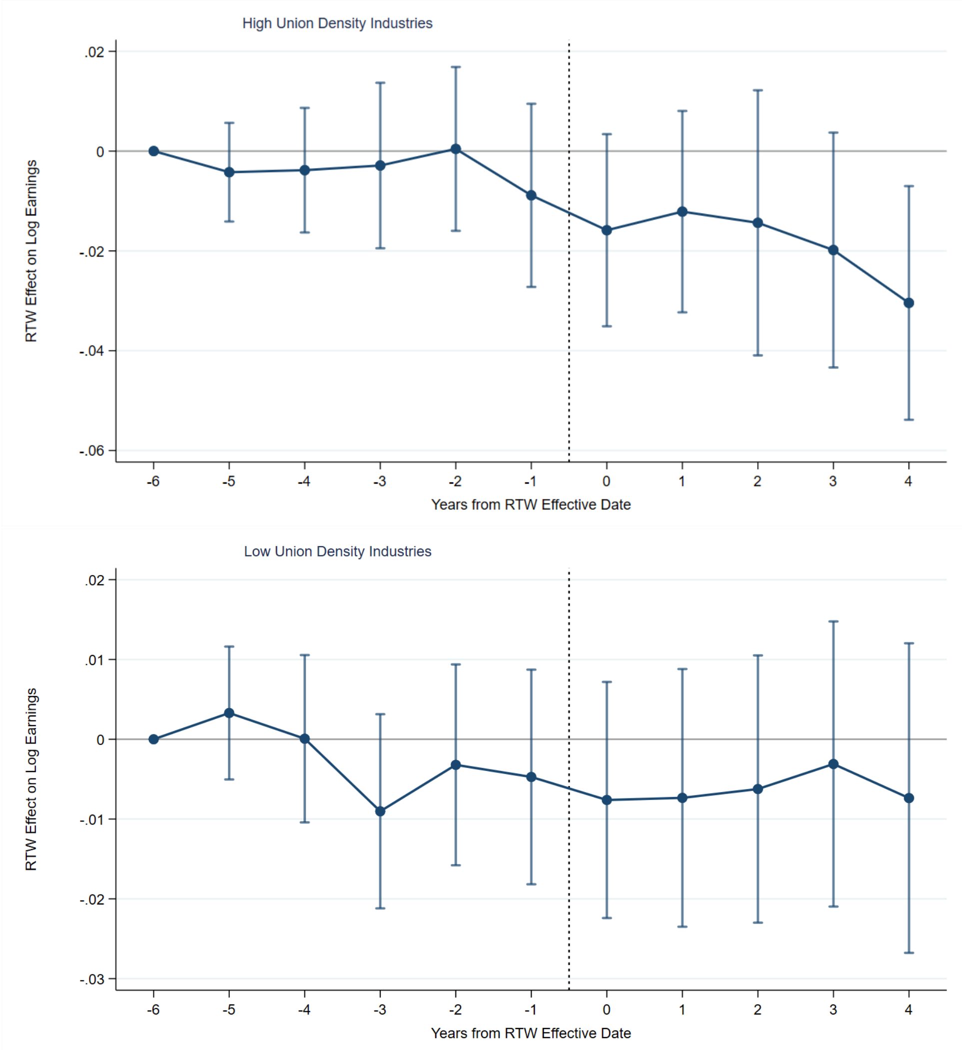


Figure 3: Log Earnings Event Studies

Earnings

Table 3 presents results of regressions specified in Equation 2 with the natural log of weekly earnings in the QCEW as the dependent variable. I start with a basic benchmark specification in Column 1 , with only the cohort-state-industry and cohort-year-industry fixed effects. I then add in state economic and industry demographic controls in Columns 2 and 3 respectively.

Panel A shows estimated effects across all industries. RTW reduced wages by about 0.9% across all industries, controlling for state economic and policy factors and industry demographic composition. Baseline models estimate a slightly lower effect, but coefficients barely change with the addition of industry demographic composition controls after state economic and policy factors are included in regressions. Panel B displays results from regressions that interact the main RTW indicator with indicators for high and low union coverage industries, as described in Section III. The main specification reported in Panel A appears to mask some heterogeneity

Table 4: The Effect of RTW on Log Employment

	(1)	(2)	(3)
A. Main DID			
RTW	-0.0164*	-0.0035	-0.0036
	(0.0094)	(0.0092)	(0.0092)
R^2	0.9991	0.9992	0.9992
B. DID by 2006-2010 Industry Union Coverage			
RTW*High Union Industry	-0.0217	-0.0113	-0.0114
	(0.0229)	(0.0219)	(0.0218)
RTW*Low Union Industry	-0.0138	0.0002	0.0001
	(0.0085)	(0.0077)	(0.0077)
R^2	0.9991	0.9992	0.9992
Observations	24,177	24,177	24,177
State-by-Industry Clusters	518	518	518
State Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: * significant at 90%, ** significant at 95%, *** significant at 99%. Standard errors clustered at the state-industry level are in parentheses. All specifications include State-by-Industry and Year-by-Industry fixed effects. State controls include the natural log of a state's minimum wage, the natural log of a state's per capita GDP, and indicators for legislature partisanship. Industry composition controls include the college educated share of an industry's workforce, the female share of an industry's workforce that is female, and the people of color share of an industry's workforce.

in RTW's wage effect, with the fully controlled regression in Column 3 estimating an RTW-induced 1.3% reduction in weekly earnings in high union coverage industries. By contrast, for low union coverage industries, RTW led to a minimal and statistically insignificant earnings reduction of only about 0.5%.

Figure 3 plots coefficients from separate event study regressions of high and low union coverage industries respectively. There is little evidence of differential pre-RTW trends in earnings in the new RTW states, with the exception of a statistically insignificant dip in earnings in highly unionized industries the year before RTW took effect. This could be due to some anticipation on the part of employers, who expect the likely passage of RTW in a Republican-led state government once legislation is introduced. Post-RTW coefficients reflect the main results, as shown in Table 3.

Employment

Table 4 reports results from regressions specified in Equation 2 with the natural log of state-by-industry employment as the dependent variable. I detect a reduction of employment of about

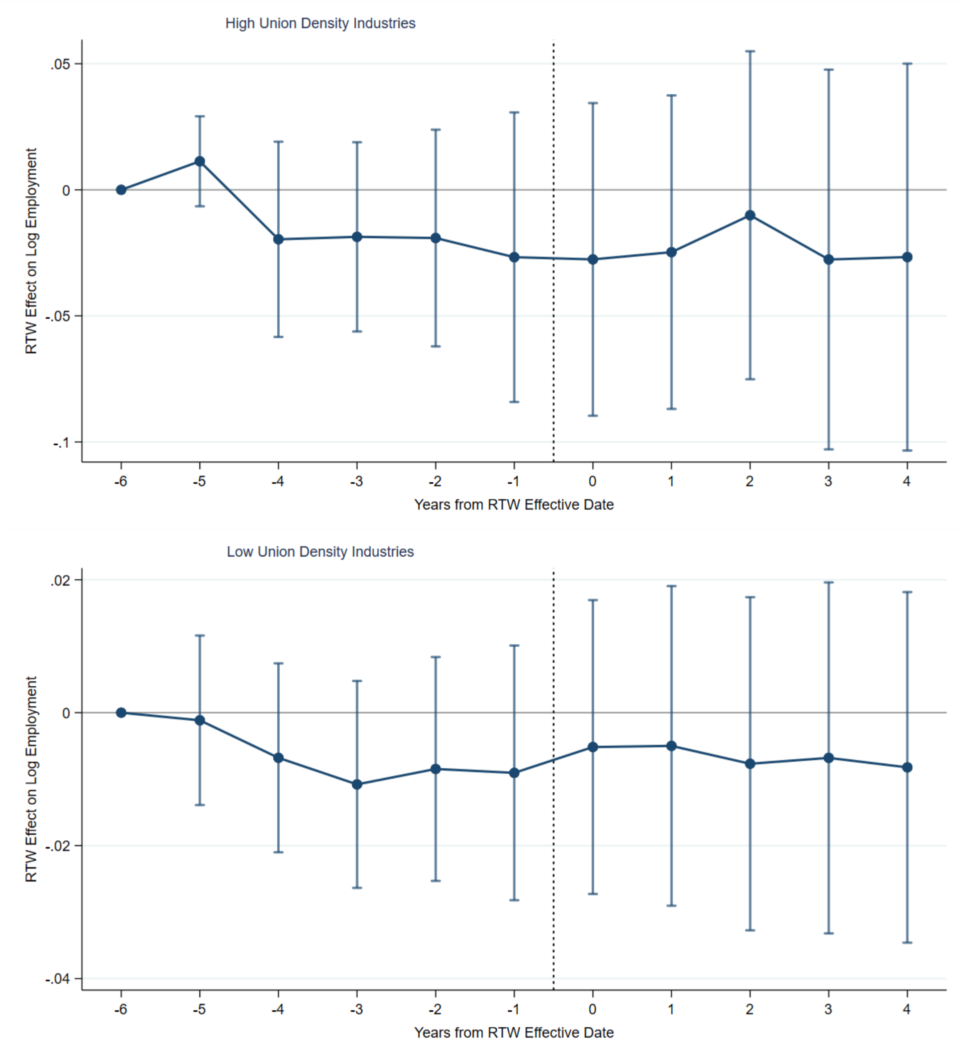


Figure 4: Log Employment Event Studies

1.6% for all industries when the regression includes no controls. However, once I include state controls, the coefficient declines substantially to nearly zero. Adding in industry controls negligibly changes the coefficient. This pattern is reflected in coefficients on interactions between *RTW* and indicators for high union coverage industries. After including state controls, the coefficient's magnitude declines from a statistically imprecise -0.0217 to a slightly more precise -0.0113. This null effect holds with the addition of industry controls. For low union coverage industries, the estimated coefficient on *RTW* is -0.0138 without the inclusion of controls. Adding in state and industry controls yields a precise zero estimate, with a low standard error and a very low (albeit positive) coefficient of 0.0001 in the preferred specification.

Tests for differential pre-RTW trends in employment between the new RTW states and the comparison group states show no evidence of leading or anticipatory effects. Figure 4 plot event study coefficients for separate regressions of high union coverage and low union coverage

Table 5: The Effect of RTW on Log Union Coverage

	(1)	(2)	(3)
A. Main DID			
RTW	-0.1396***	-0.1138**	-0.1155**
	(0.0464)	(0.0515)	(0.0515)
R^2	0.8232	0.8239	0.8250
B. DID by 2006-2010 Industry Union Coverage			
RTW*High Union Industry	-0.1247**	-0.1104*	-0.1167**
	(0.0496)	(0.0563)	(0.0528)
RTW*Low Union Industry	-0.1486**	-0.1196	-0.1142
	(0.0683)	(0.0755)	(0.0759)
R^2	0.8232	0.8238	0.8255
Observations	21,172	21,172	21,172
State-by-Industry Clusters	510	510	510
State Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: * significant at 90%, ** significant at 95%, *** significant at 99%. Standard errors clustered at the state-industry level are in parentheses. All specifications include State-by-Industry and Year-by-Industry fixed effects. State controls include the natural log of a state's minimum wage, the natural log of a state's per capita GDP, and indicators for legislature partisanship. Industry composition controls include the college educated share of an industry's workforce, the female share of an industry's workforce that is female, and the people of color share of an industry's workforce.

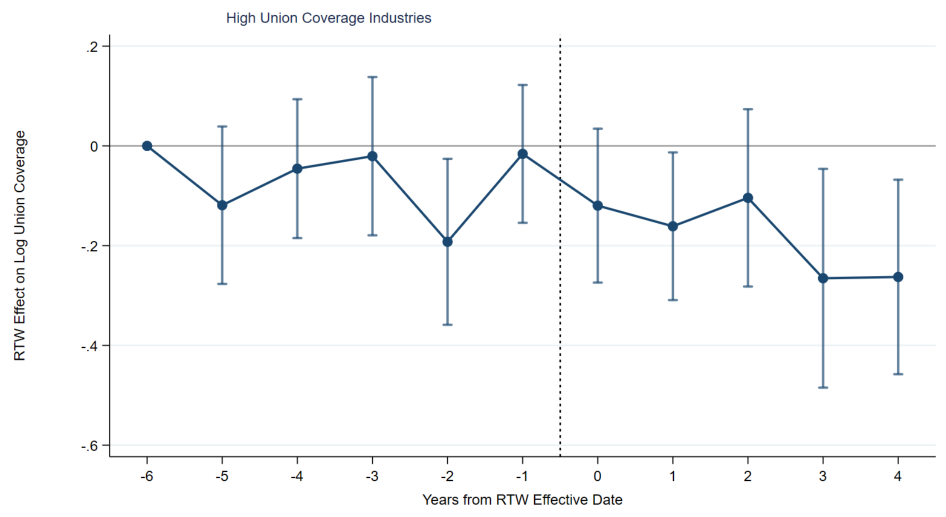
industries. Not only are all pre-RTW coefficients statistically insignificant, they do not display any meaningful trend over time, relative to RTW's effective date. [14]

Together, these main results suggest that while RTW reduced earnings for workers in heavily unionized industries, it did not substantively effect employment across state economies. Effect heterogeneity in earnings between highly unionized industries and less unionized industries suggests that a state-by-industry's pre-RTW "exposure" to RTW matters — sectors with more establishments that will be affected by the policy (because workplace conditions are governed by CBAs) e

Union Coverage and Union Wage Premia

Because RTW's first order effects are on labor unions, I test RTW's effect on the two measures of union power detailed in Section III: each state-by-industry cell's union coverage and the union wage premium. Table 5 reports estimates of Equation 2 using the natural log of union coverage

[14] Given the justification that positive RTW employment effects are associated with an influx of new business establishments, I also run main DID and event study models for the natural log of the number of establishments within each state-by-industry cell. Appendix B presents results from these regressions, showing a similar null effect to the employment models.



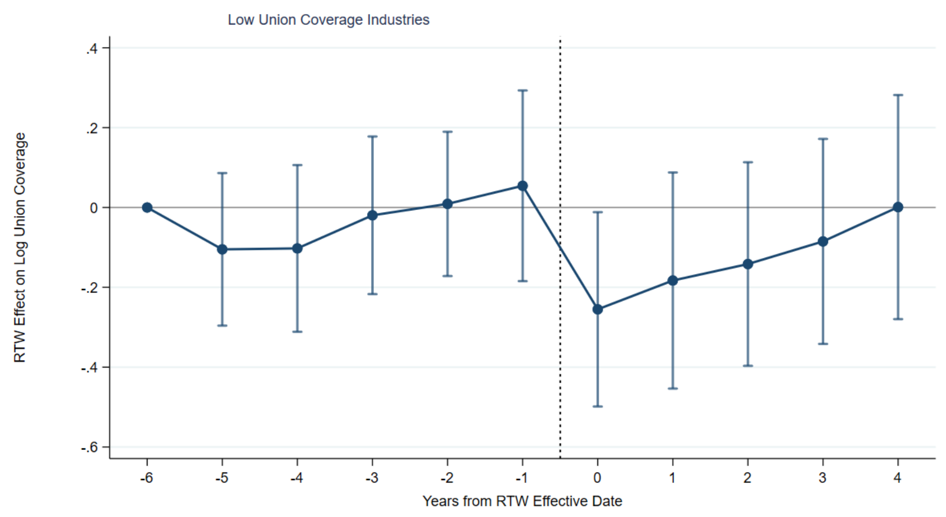


Figure 5: Log Union Coverage Event Studies

as an outcome.[15] Pooled specifications, reported in Panel A, estimate an approximately 11.55% reduction in union coverage averaged across all industries. Interacted specifications — reported in Panel B — show that the pooled specification does not mask much heterogeneity between high and low union coverage industries. RTW reduced union coverage in high union coverage industries and low union coverage industries by 11.67% an 11.42% respectively. However, for low union coverage industries, estimates were less precise and are thus statistically inconclusive.

Figure 5 shows coefficients from event study regressions. Across both types of industry, union coverage displayed no leading trend prior to RTW's effective date, with the exception for one statistically significant negative lead coefficient two years before RTW took effect. However,

[15]Notably, sample sizes are lower in the union power regressions than in the earnings and employment regressions. This is because of the omission of sectors with zero union coverage. I omit these sectors for two reasons. First, it is unlikely that they are truly totally nonunion sectors. Instead, they appear as nonunion because the CPS sample sizes were too small. Second, without full support, I cannot compute union wage premia nor compare union wage premia and union coverage results. Main wage and employment regressions estimate similar coefficients when "zero union" industries are omitted from the sample.

Table 6: The Effect of RTW on Union Wage Premia

	(1)	(2)	(3)
A. Main DID			
RTW	0.0279	0.0441	0.0393
	(0.0282)	(0.0338)	(0.0334)
R^2	0.3392	0.3400	0.3446
B. DID by 2006-2010 Industry Union Coverage			
RTW*High Union Industry	0.0580*	0.0688*	0.0641*
	(0.0328)	(0.0386)	(0.0383)
RTW*Low Union Industry	0.0096	0.0292	0.0257
	(0.0405)	(0.0485)	(0.0483)
R^2	0.3392	0.3402	0.3467
Observations	21,170	21,170	21,170
State-by-Industry Clusters	510	510	510
State Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: * significant at 90%, ** significant at 95%, *** significant at 99%. Standard errors clustered at the state-industry level are in parentheses. All specifications include State-by-Industry and Year-by-Industry fixed effects. State controls include the natural log of a state's minimum wage, the natural log of a state's per capita GDP, and indicators for legislature partisanship. Industry composition controls include the college educated share of an industry's workforce, the female share of an industry's workforce that is female, and the people of color share of an industry's workforce.

the year before RTW took effect, the coefficient on the lead was approximately zero, making the possibility that union coverage was declining relatively in the new RTW states unlikely. This finding is relevant considering previous literature on "endogeneous RTW laws" (Stevans 2009). Unlike the early-adopting RTW states in the 1940s and 1950s, the lack of pre-trend in union coverage means that RTW did not take effect in the 2010s as a result of already weakening unions[16]. This suggests that that detected wage decreases following RTW occurred as a result of the law itself and not in response to pre-RTW reductions in union power that simultaneously depressed wages and allowed state politicians to pass RTW.

Another measure of union power is the union wage premium, defined in Section III. The union wage premium captures the percentage gap in earnings between union and nonunion workers. Table 6 reports DID estimates for the union wage premium. While coefficients on *RTW* for the pooled specification are statistically insignificant, there is heterogeneity by industry. High union coverage industries experienced an increase in the union wage premium by about 6.41% after including all controls, a coefficient that is significant at 90% confidence.

[16]This point is further supported by the fact that in the lead up to the passage of RTW across the five new RTW states, unions mounted substantial opposition campaigns, rallying members and allies to lobby state legislatures and attend political demonstrations, as detailed in Section I.

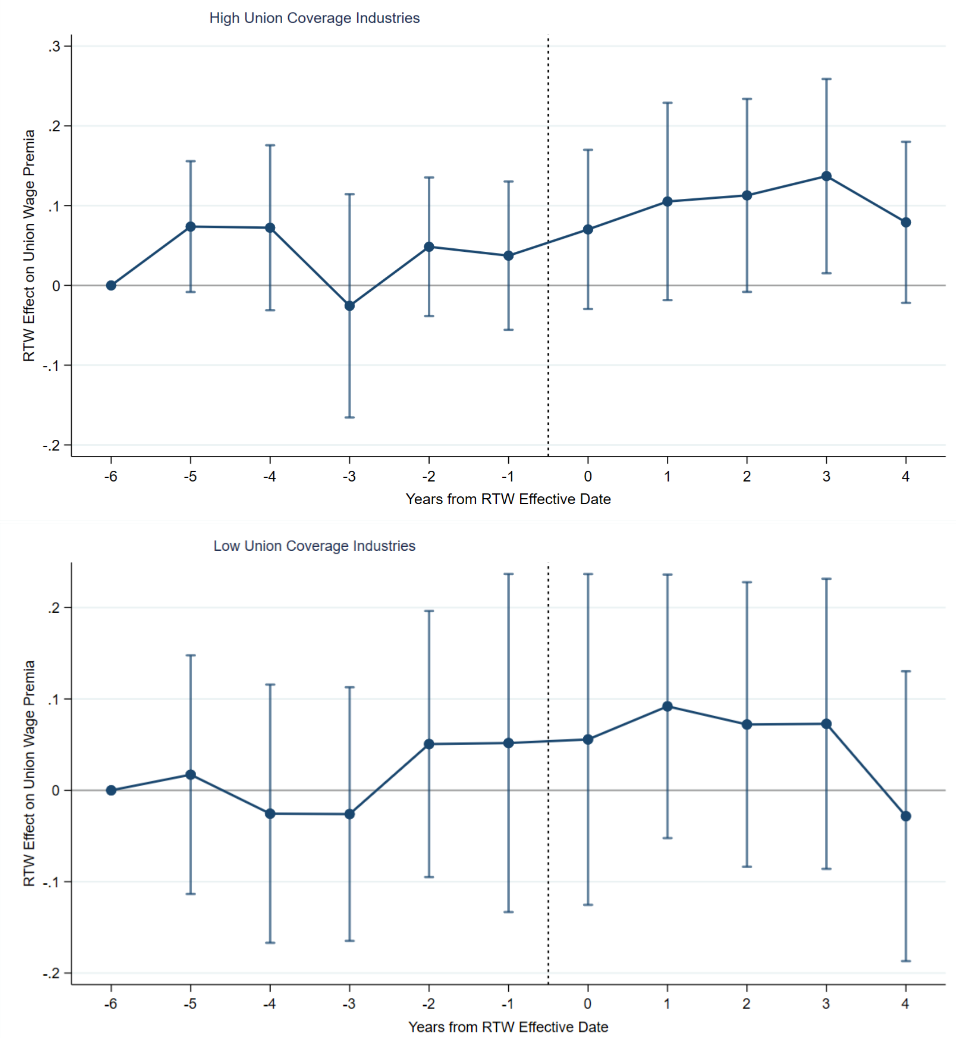
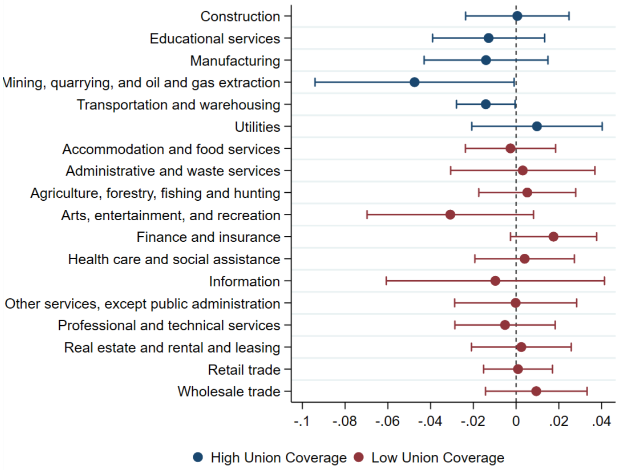


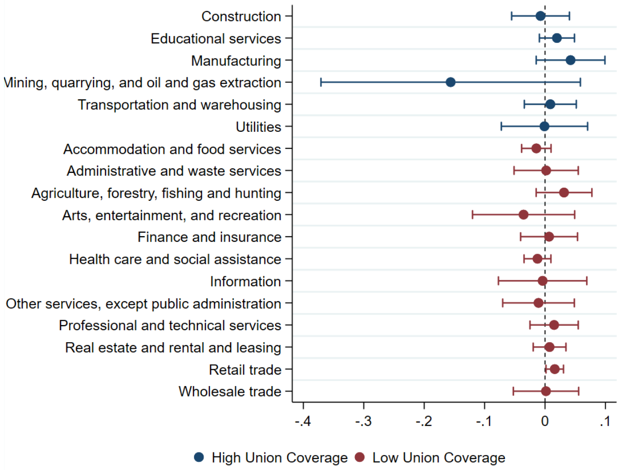
Figure 6: Log Union Wage Premium Event Studies

Event study estimates plotted in Figure 6 reflect this finding, suggesting that union wage premia increased in high union coverage industries after RTW took effect. Coefficients on all post-RTW lags are positive and either significant at 90% or 95% confidence for high union coverage industries. By contrast, estimates are less precise and closer to zero for low union coverage industries. Four years after RTW took effect, the estimated effect on the union wage premium was no only statistically insignificant but negative. There were no leading trends for either type of industry.

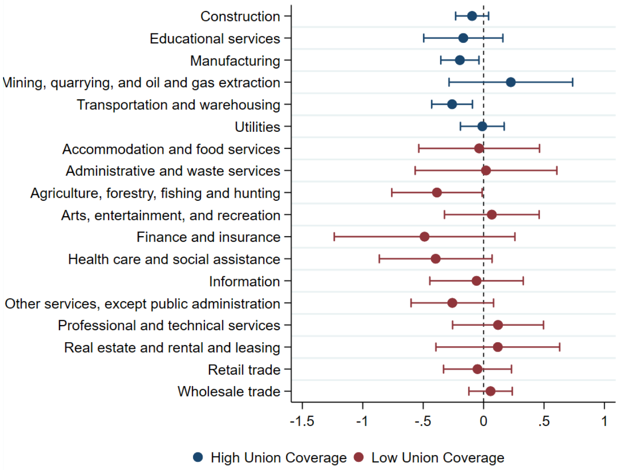
While estimated positive effects on union wage premia but negative effects on CBA coverage may seem contradictory, it is possible that unions shifted resources from new organizing to internal organizing and contract negotiation. The union threat effect presents another explanation. The gap between union covered and nonunion workers could have widened if RTW decreased the union threat effect and estimated wage losses primarily occurred among nonunion workers.



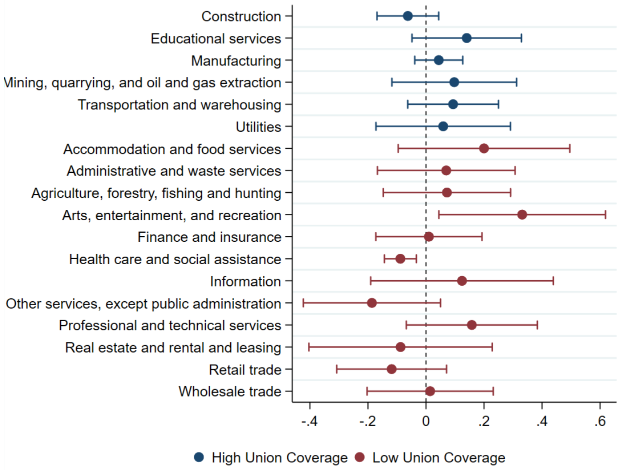
(a) Log Earnings



(b) Log Employment



(c) % Union



(d) UWP

Figure 7: Industry RTW Effect Heterogeneity

In response to reduced union coverage, nonunion employers with wage setting power could have offered lower wages, assuming that the threat from future unionization would reduce. These possibilities will be further explored in Section V.

Heterogeneity by Industry

To further examine why the differences in RTW effect exist between the two different industry groups, I interact the RTW indicator with indicators for each industry and plot coefficients for each outcome in Figure 7. Panel (a) shows that while earnings declined as a result of RTW for most high union density industries, the most prominent decline – about 5% – occurred in mining, quarrying, and oil and gas extraction. Transportation and warehousing also experienced a statistically significant reduction in earnings by about 2%. Educational services and manufacturing also experienced similar magnitude declines, although estimates were less precise. Wages for construction remained unchanged, while utilities experienced a slight but statistically insignificant increase in wages of about 1.5%. Employment outcomes (shown in Panel (b) of Figure 7) were null statistically insignificant and close to zero across most industries, meaning that the results reported in Table 4 and Figure 4 do not mask any meaningful heterogeneity within the two industry groups.

Panel (c) displays coefficients on RTW for each industry's union coverage. Interestingly, coefficients across both types of industries were negative, and only a few were statistically significant due to high standard errors. Union wage premia results in Panel (d) show a more defined pattern. Most highly unionized industries experienced an increase in the earnings gap between union-covered and nonunion workers as a result of RTW. However, results were more mixed in less unionized industries, with some experiencing increases in the union wage premium and others experiencing reductions. Given the heterogeneity in earnings effect and in union power effects between industries, it is possible that by analyzing correlations between a specific sector's union power effects and its earnings effect I can analyze the mechanisms behind RTW's effects on earnings. I further explore this possibility in Section V.

Robustness Checks

Before discussing results and examining potential mechanisms behind the wage and employment effects, I show that the main results are robust to changes in variable definition, dataset construction, and regression specification.

The Intensive Margin of Employment and Alternative Earnings Definitions—Thus far, because the QCEW lacks data on hours worked or hourly wages, regressions tell us little about the intensive margin of employment. It is possible that hours worked within each labor market could have shifted as a result of RTW. For example if CBAs standardized hours or unions traded off hours clauses for other contract items due to reduced bargaining power, then RTW may have shifted weekly hours worked, which could have reduced weekly wages without affecting hourly wages. Additionally, because the QCEW measures employment by number of jobs within each state-by-industry cell, detected null employment effects may mask changes in employment at the intensive margin. Firms may have responded to lower wages by hiring more hours of labor, without hiring more workers at the extensive margin. Another possibility is that the detected decrease in weekly earnings and null effect of employment obscure changes in the number of weeks worked each year. For example, average weekly earnings may fall as a result of RTW but annual earnings may remained unchanged if workers work more throughout the year, leading employers to experience no change to their total labor costs.

To check if either of these effects occurred, I run three regressions, combining CPS data on hours worked and hourly wages with the QCEW administrative data. First, I use CPS data on average hours worked within each labor market as a dependent variable, finding no effect. I then divide each labor markets average weekly earnings from the QCEW by the average hours worked per week from the CPS, finding results similar (although less precise) than my main weekly earnings models. I also run regressions, using average hourly wages in the CPS as my dependent variable, finding a slightly lower effect to the QCEW hourly wages regression, but also with higher standard errors. However, even with the CPS hourly wage regressions, there is a notable differential between the negative coefficient on *RTW* when interacted with an indicator for highly unionized industry and a nearly zero coefficient for low union coverage industries. Finally, I examine effects on the log of total compensation within each sector, using data from the CPS, finding coefficient similar to the main log earnings models, albeit one estimated with less precision. These results are provided in Appendix C. Generally, I find that main results are robust to alternative definitions of employment and earnings.

State Specific Trends— Even though I do not detect any aggregate wage and employment trends in new RTW states before the law took effect, it is still possible that individual states exhibited significant pre-trends, challenging the validity of causal inference. It is useful to check if individual RTW states did not display state-specific trends in wages and employment before RTW took effect I run state specific event studies to show that each individual RTW state did not display clear employment or wage trends prior to the adoption of RTW. As shown in Appendix D, there is little evidence of state-specific trends before RTW was adopted, although there is some evidence of anticipation effects in Kentucky.

Alternative Comparison Groups — To check robustness to different comparison groups, I use two alternative groups of comparison states. First, instead of using all non-RTW states during the time window surrounding RTW's passage, I exclude the states bordering the Pacific Ocean

– the states geographically farthest from the new RTW states. When these states are omitted from the comparison group, the coefficient on RTW for highly unionized industries declines slightly in magnitude. Additionally, I compare the new RTW states to all other states with either split or Republican Legislatures. This allows me to make sure that the states adopting RTW are compared with states adopting similar policies. Similarly to the ”no West Coast” alternative group, restricting the same to only Republican-led states causes the earnings effect estimate to decline slightly in magnitude for earnings, while other results are remain unchanged. Results from these robustness checks are provided in Appendix E.

V Mechanisms

I consider two potential reasons behind the negative RTW earnings effect. First, I look at the possibility that RTW's effects can be primarily explained by more aggressive deregulation, tax code changes, or other policies that are more likely to be passed. Then, I delve into the state-by-industry cell specific relationship between effects on union coverage, union wage premia, and wages.

Policy Changes and Signalling

To further disentangle the effect of RTW itself from changes in tax codes and deregulation often passed concurrently by Republican legislatures, I use the state of Missouri as a placebo test. In early 2017, Missouri's then-governor Eric Greitens signed an RTW policy into law. RTW was slated to take effect in late August of 2017, yet a few days before the effective date, labor unions and activist groups submitted a sufficient number of signatures to trigger the state's referendum process, requesting a statewide vote on the law's repeal. This process stalled the effective date of RTW in Missouri until after the referendum occurred. On August 8th, 2018, almost a year after RTW was initially supposed to take effect, 67% of voters voted “No on Prop A”, repealing the RTW law (Ancel 2018).

Following this logic, I run regressions using a placebo RTW variable that considers Missouri treated starting in 2017 instead of the actual RTW variable that accurately captures historical RTW status. If the coefficient on the placebo Missouri RTW variable is statistically and economically significant, my main findings are likely to be associated with RTW itself and

Table 7: RTW in Kentucky and Fake RTW in Missouri

	(1) Log(Earns)	(2) Log(Emp)	(3) Log(Ests)	(4) Log(%Union)	(5) UWP
A. KY DID by 2006-2010 Industry Union Coverage					
*High Union Industry	-0.0311***	-0.0373	-0.0409	-0.1146	0.0182
	(0.0116)	(0.0767)	(0.0546)	(0.1251)	(0.0968)
*Low Union Industry	-0.0001	-0.0016	-0.0009	0.1470	-0.0453
	(0.0075)	(0.0150)	(0.0211)	(0.1568)	(0.1023)
B. MO DID by 2006-2010 Industry Union Coverage					
Fake *High Union Industry	-0.0148	0.0466	0.0032	-0.0958	0.0544
	(0.0100)	(0.0304)	(0.0143)	(0.1222)	(0.0751)
Fake *Low Union Industry	-0.0108	0.0034	0.0365	-0.1630	0.0524
	(0.0122)	(0.0130)	(0.0377)	(0.1074)	(0.0814)
R^2	0.9960	0.9991	0.9976	0.8133	0.2892
Observations	4,831	4,831	4,867	4,196	4,867
State-by-Industry Clusters	446	446	448	438	448
State Controls	Yes	Yes	Yes	Yes	Yes
Industry Controls	Yes	Yes	Yes	Yes	Yes

Notes: * significant at 90%, ** significant at 95%, *** significant at 99%. Standard errors clustered at the state-industry level are in parentheses. All specifications include State-by-Industry and Year-by-Industry fixed effects. State controls include the natural log of a state's minimum wage, the natural log of a state's per capita GDP, and indicators for legislature partisanship. Industry composition controls include the college educated share of an industry's workforce, the female share of an industry's workforce that is female, and the people of color share of an industry's workforce.

not something spurious. I specifically compare the effects of Missouri's placebo RTW law to the effects of Kentucky's actual RTW because both states are compared to the exact same set of clean control states during the same time window. Table 7 reports results of Missouri placebo DID regressions of several dependent variables on interacted RTW indicators and controls. Notably, none of the coefficients on the main *RTW* variable or the industry-interacted *RTW* variables are statistically significant at the 95% confidence level. Perhaps more importantly, most coefficients are notably closer to zero than their corresponding statistically and economically significant "real RTW" counterparts from Kentucky.

Although I control for legislature partisan composition in main regressions, I additionally check how legislature composition changed around the time of RTW's effective date using an event study specification. The top panel of Figure 8 plots coefficients from these event studies on the probability that either Democrats or Republicans hold a majority in the state legislature. Importantly, the shift in legislative power from Democrats to Republicans began before RTW passed. The bottom panel of Figure 8 compares Missouri and Kentucky's legislature partisanship changes around the time of RTW's passage and implementation. If political shifts were the primary mechanism through which RTW affected labor markets, Missouri should have

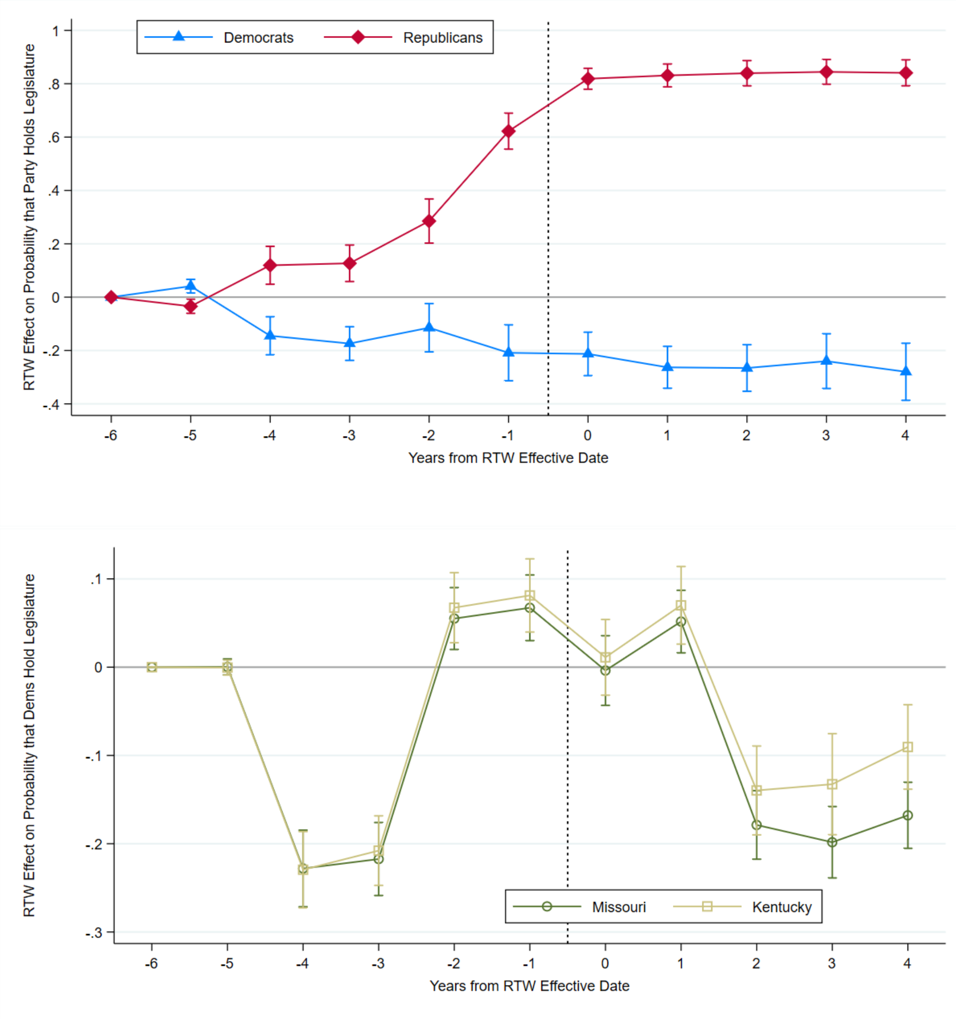


Figure 8: RTW Timing and Changes in Legislature Composition

less noticeable political shifts than Kentucky, given the results presented in Table 7. Instead, legislatures in Kentucky and Missouri had very similar partisan composition during the entire study period (2011-2017), with the exception of 2020 and 2021.

Overall, it appears that RTW had little meaningful effect on policy signals. Missouri did not have a meaningful RTW effect compared to Kentucky, even though politicians in Missouri passed an (ultimately ineffectual) RTW policy into law. Additionally, RTW did not induce any shifts in partisan leadership within affected state legislatures. Democrats were consistently losing seats beforehand, while Republicans were gaining. One remaining possibility is that employers viewed RTW as a signal that state legislatures would remain held by Republicans in the long run, anticipating the staying power of other pro-business policies and reducing earnings in turn. This would make sense given the plateau of Republican leadership that emerges after RTW takes effect.

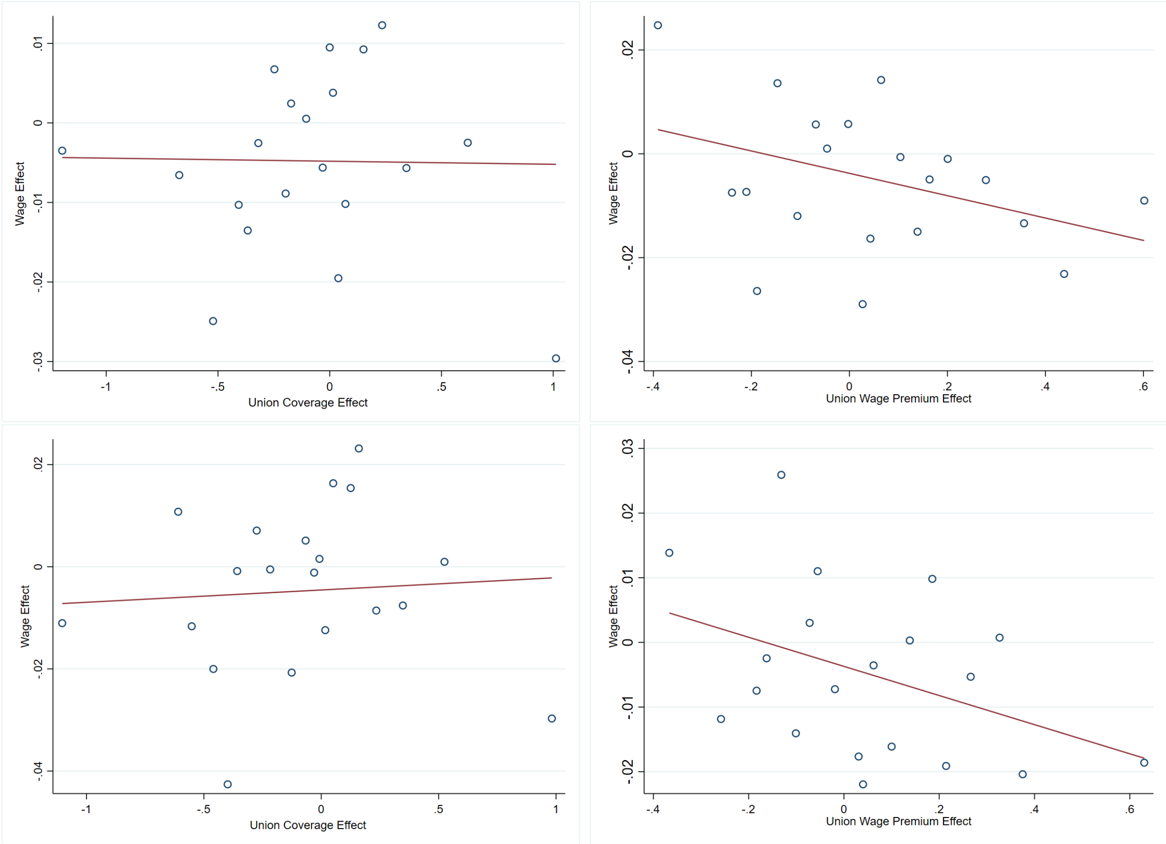
Union Power

To further explore the role of industry union coverage in mediating the effect of RTW on wages, I run a regression following Equation 2 with interactions between RTW and each affected state-by-industry cell. I then run a similar regression using the log of union coverage as the dependent variable. This allows me to check if the state-industry level union coverage effect is correlated with the corresponding wage effect of RTW. Figure 9 shows plots of average wage affects across union coverage effect bins and union wage premium bins. Panel (a) plots data unadjusted for state fixed effects and Panel (b) plots data residualized from regressions on state indicators. There is little correlation between union coverage effects and wage effects, across sectors. As suggested in Figure 2, the same sectors that experience significant earnings reductions are not necessarily the same sectors that experience significant union coverage reductions.

However, there is a negative correlation between union wage premium effects and wage effects, especially within each state. For union wage premia to increase while overall earnings decrease, nonunion earnings declines must drive the overall effect.[17] Thus, one explanation for these correlations is that RTW primarily reduces earnings by weakening the credibility of the union threat. Nonunionized establishments reduce wages, anticipating a lower chance of unionization in the future due to RTW, reducing earnings across each sector and increasing the union wage premium simultaneously. This scenario is possible even if union coverage itself does not change within a sector.

While unable to comprehensively detect the mechanisms through which RTW reduced earnings, these analyses rule out the possibility that RTW's economics effects were driven by employers' use of RTW as an informational signal of future policy changes. Instead, it is more likely that changes to union power – although not necessarily union coverage – associated with RTW led to earnings reductions. The explanation most consistent with data is that in industries where employers anticipated a reduced union threat due to RTW, nonunion establishments reduced wages, causing declines to average earnings across the sector.

[17] It is possible that union earnings could decline as well, this result only requires that the nonunion earnings fall by a larger magnitude.



(a) Without State FEs (b) With State FEs

Figure 9: Correlations between RTW Union Power Effects and Wage Effect

VI Conclusion

Using administrative data from the Quarterly Census of Employment and Wages merged with survey data from the Current Population Surveys, this paper provides further evidence that RTW laws reduce earnings, but only in industries with above average union coverage prior to RTW's implementation. Coefficients on *RTW* across all wage model specifications were negative, and were statistically significant when the *RTW* indicator was interacted with a dummy for industries with high union coverage prior to RTW. This finding aligns generally with the recent findings of Fortin et al (2022) but challenges those of Eren & Ozbeklik (2016). Some of these differences can be explained by different study settings. Eren & Ozbeklik (2016) look at Oklahoma's RTW law, which passed over a decade before Indiana's RTW law, the first RTW legislation I examine. Additionally, they do not separate industries by pre-RTW union coverage.

Evidence regarding RTW's effect on employment is less consistent with much of the previous literature. Estimated effects of RTW on employment were usually close to zero, negative, and statistically significant across industries. This recalls the null findings of much of the minimum wage literature (Dube et al. 2010; Allegretto et al. 2017; Cengiz et al. 2019). However, it challenges the notion established by other minimum wage studies that standardized increases in wages (in this case by CBAs) necessarily lead to disemployment (Neumark et al. 2021). If this were true, than RTW would have unambiguously positive employment effects, by removing inefficient wage pressures. Further, I find no evidence that RTW significantly attracts new manufacturing establishments, disputing the findings of Holmes (1998) among others, but aligning with more recent findings such as Bloom et al (2019).

Examining first order effects on union strength, RTW reduced union coverage and increased the earnings gap between union and nonunion workers, suggesting that earnings reductions are associated with changes in union power. A mechanism analysis supports this suggestion, casting doubt the possibility that RTW's effects were due to induced changes in state legislature partisan composition (which began long before RTW took effect) or employers' use of RTW as a signal for future similar policies (as evidenced by the null effect of Missouri's ineffectual RTW on wages). By contrast, I find a moderate correlation between state-by-industry specific RTW-induced union wage premium increases and state-by-industry specific RTW-induced earnings reductions, suggesting that earnings reductions were primarily felt by nonunion workers, and that RTW affected labor markets by reducing the severity of the union threat.

Analyzed together, results suggest that strong unions do not necessarily reduce economic efficiency. While RTW reduced wages, employment did not increase in response, suggesting that the weakening of unions did not allow firms to hire at a more efficient point. Further, RTW affected earnings in some labor markets without changing the actual extent of unionization, suggesting that non-unionized firms enjoy some wage setting power. This overall finding – that firms did not behave consistently with the predictions of a frictionless and demand elastic labor market – lends support to the point initially made by Robinson (1933) and reinforced more recently by Webber (2015) and Benmelech et al (2022). It appears that unions in RTW states, prior to RTW taking effect, kept wages up. Once RTW weakened union's bargaining power and ability to expand into new workplaces, wages fell and employment did not take a significant hit. Notably, there is room for further and more empirically rigorous research on this topic. A more formal structural analysis could estimate how RTW (and more broadly unionization itself) affected firm-level labor supply elasticities or index based measures of labor market concentration.

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Table 8: Wild Bootstrap P-Values for Selected Coefficients

	(1) Log(Earnings)	(2) Log(%Union)	(3) UWP
RTW*High Union Industry	-0.0133*	-0.1167	0.0641*
	[0.0651]	[0.1071]	[0.0911]
Observations	24,177	24,172	21,170
State-by-Industry Clusters	518	510	510
State Controls	Yes	Yes	Yes
Industry Controls	Yes	Yes	Yes

Notes: * significant at 90%, ** significant at 95%, *** significant at 99%. P-values in brackets generated from wild cluster bootstrap procedure, with clustering at the state levels. All specifications include State-by-Industry and Year-by-Industry fixed effects. State controls include the natural log of a state's minimum wage, the natural log of a state's per capita GDP, and indicators for legislature partisanship. Industry composition controls include the college educated share of an industry's workforce, the female share of an industry's workforce that is female, and the people of color share of an industry's workforce.

A Bootstrap Based Inference

All models throughout the main section of the paper cluster standard errors at the labor market or state-industry level. I cluster at this level because of RTW's heterogeneous effects across labor markets due to different initial exposure to unionization. However, RTW is technically applied across an entire state's private sector, so an alternative approach to inference involves clustering at the state level. Unfortunately, because I only use 29 states in my analysis, traditional clustered errors will be too small Cameron et al. (2008). Thus, in this appendix, I use the wild cluster bootstrap to generate p-values and conduct inference (Cameron et al. 2008; Roodman et al. 2019).

Table 8 reproduces the regression coefficients on RTW interacted with indicators for highly unionized industries and includes p-values from the wild cluster bootstrap procedure, with clustering at the state level.

While the p-values of all three coefficients decline from those generated by the state-industry clustered asymptotic errors of the main results, the coefficients on $*HighUnionIndustry$ for log earnings and the union wage premium remain statistically significant at 90% confident

Table 9: The Effect of RTW on Log Establishment Counts

	(1)	(2)	(3)
A. Main DID			
RTW	-0.0158	-0.0012	-0.0014
	(0.0118)	(0.0121)	(0.0121)
R^2	0.9973	0.9973	0.9974
B. DID by 2006-2010 Industry Union Coverage			
RTW*High Union Industry	-0.0351**	-0.0243	-0.0245
	(0.0162)	(0.0168)	(0.0168)
RTW*Low Union Industry	-0.0064	0.0101	0.0102
	(0.0156)	(0.0159)	(0.0159)
R^2	0.9973	0.9974	0.9974
Observations	24,350	24,350	24,350
State-by-Industry Clusters	521	521	521
State Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: * significant at 90%, ** significant at 95%, *** significant at 99%. Standard errors clustered at the state-industry level are in parentheses. All specifications include State-by-Industry and Year-by-Industry fixed effects. State controls include the natural log of a state's minimum wage, the natural log of a state's per capita GDP, and indicators for legislature partisanship. Industry composition controls include the college educated share of an industry's workforce, the female share of an industry's workforce that is female, and the people of color share of an industry's workforce.

B Establishment Counts

Holmes (1998) finds that counties in RTW states that border non-RTW states have higher numbers of manufacturing establishments, a finding he uses to explain his findings of positive manufacturing employment. To test for these effects, I rerun the main DID regressions specified in Equation 2, this time using two new independent variables. First I examine how RTW affected the number of establishments in each sector normalized by state population. Then, I examine how RTW affected the average level of employment at each establishment instead of industry-wide employment normalized by state population. Results from these models are presented in Table 9, with Panel A displaying main DID results, and Panel B displaying results of the RTW indicator interacted with indicators for high and low union coverage industries. Estimated effects on the number of establishments are statistically insignificant across all industries, and while interacting the RTW variable with indicators for high and low union coverage industries uncovers some heterogeneity by coefficient sign, estimates remain statistically insignificant.

Figure 10 plots coefficients for regressions of the log establishment count on leads and lags

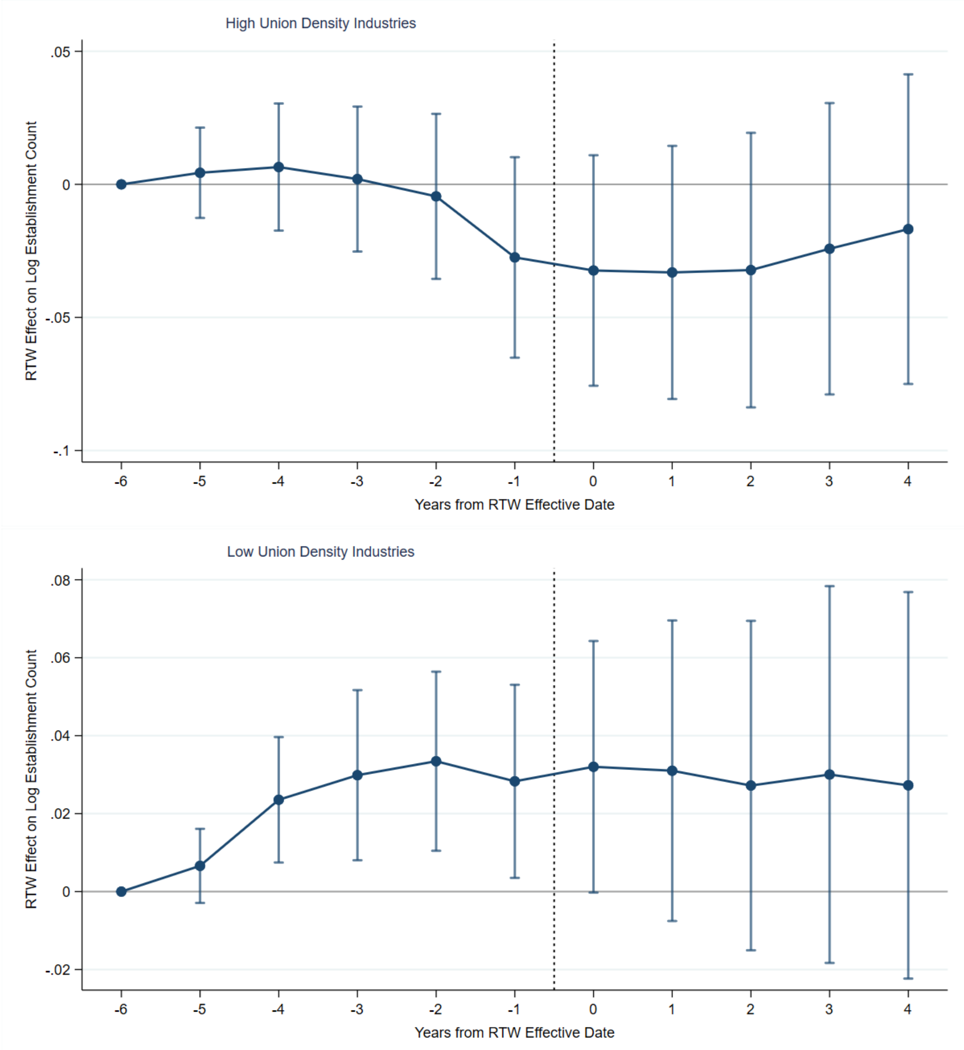

Figure 10: Log Establishment Count Event Studies

of RTW for each type of industry. The event studies generally reflect the findings displayed in Table 9. While post-RTW coefficients are negative for high union coverage industries, none are statistically significant. For low union coverage industries, establishment counts appear to have been increasing prior to RTW took effect, relative to levels in comparison group states. Lead coefficients are positive and statistically significant. While lags remain positive, they are estimated less precisely. Regardless there is no substantial change between the pre-RTW and post-RTW periods, so event study regressions provide no evidence that RTW affected establishment counts in low union coverage industries.

Table 10: Alternative Wage and Employment Definitions

	(1) Hours	(2) Log(Hourly Wage)	(3) CPS Log(Hourly Wage)	(4) Total Sector Labor Cost
RTW*High Union Industry	0.0024	-0.0159	-0.0067	-0.0247
	(0.0096)	(0.0135)	(0.0170)	(0.0215)
RTW*Low Union Industry	0.0035	-0.0043	0.0027	-0.0007
	(0.0048)	(0.0082)	(0.0058)	(0.0113)
R^2	0.8149	0.9863	0.8976	0.9989
N	24,350	24,177	24,350	24,177
State-by-Industry Clusters	518	518	518	518
State Controls	Yes	Yes	Yes	Yes
Industry Controls	Yes	Yes	Yes	Yes

Notes: * significant at 90%, ** significant at 95%, *** significant at 99%. Standard errors clustered at the state-industry level are in parentheses. All specifications include State-by-Industry and Year-by-Industry fixed effects. State controls include the natural log of a state's minimum wage, the natural log of a state's per capita GDP, and indicators for legislature partisanship. Industry composition controls include the college educated share of an industry's workforce, the female share of an industry's workforce that is female, and the people of color share of an industry's workforce.

C Alternative Earnings and Employment Definitions

Table 11 reports results from regressions of alternative earnings and employment measures, specifically paying attention to the role of hours worked. Notably, using data from the CPS, hours worked did not substantively change.

D State Pre-Trends Tests

Figures 11 and 12 shows plots of state-specific event studies plotted against the main event study regressions for each variable. This appendix presents coefficients of state-specific event studies. For each state-specific event study, I used data from each respective data block.

E Alternative Comparison Groups

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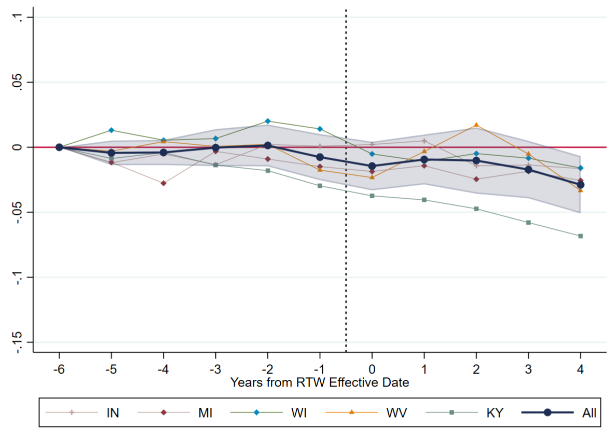
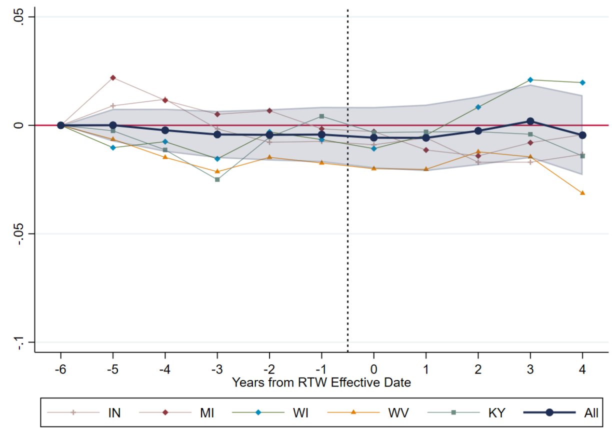
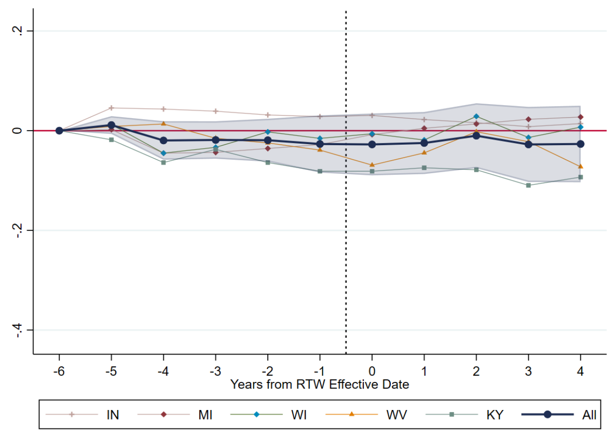


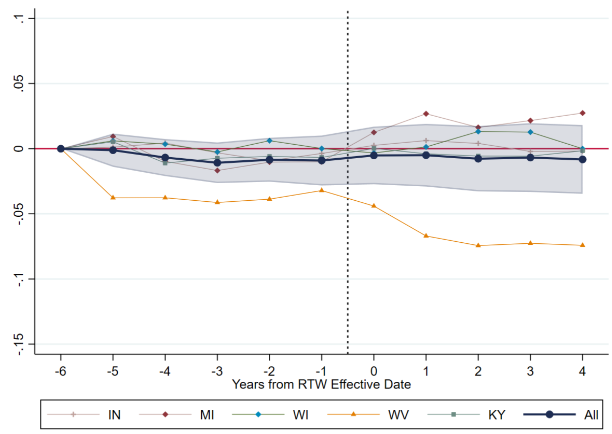
(a) Earnings - High Union



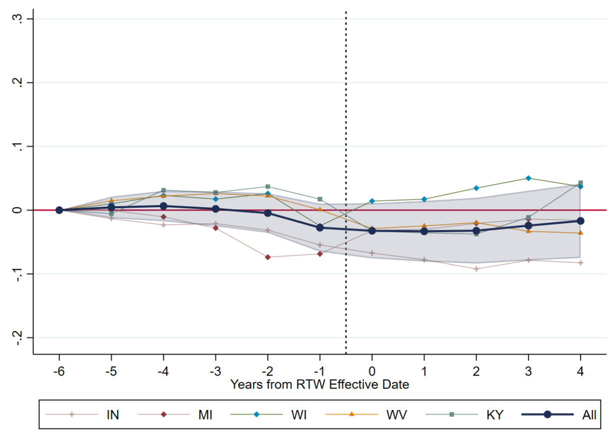
(b) Earnings - Low Union



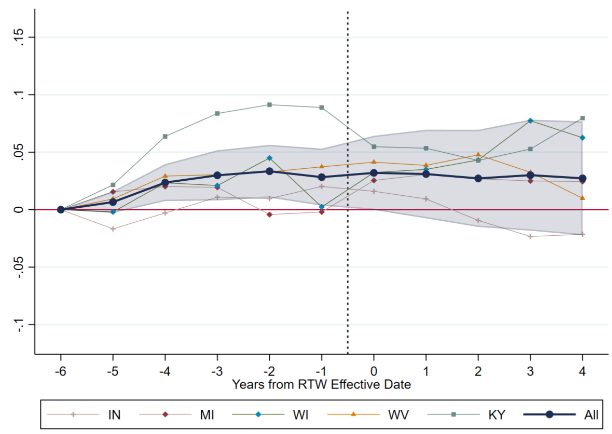
(c) Employment - High Union



(d) Employment - Low Union

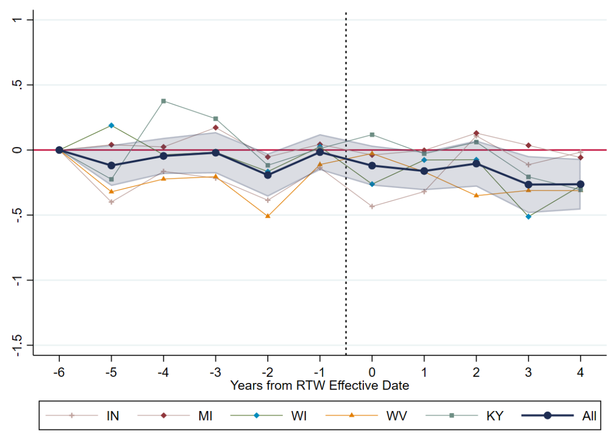


(e) Establishments - High Union

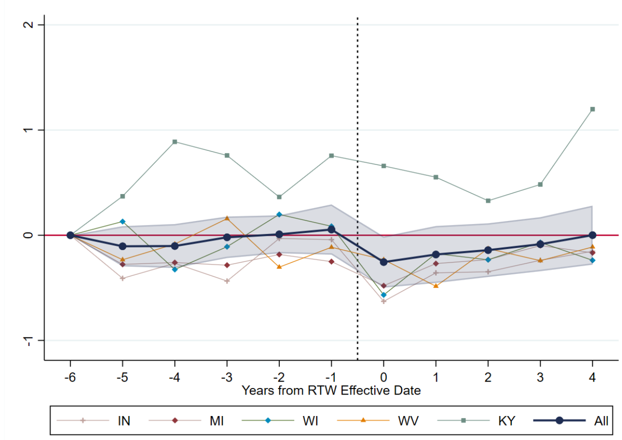


(f) Establishments - Low Union

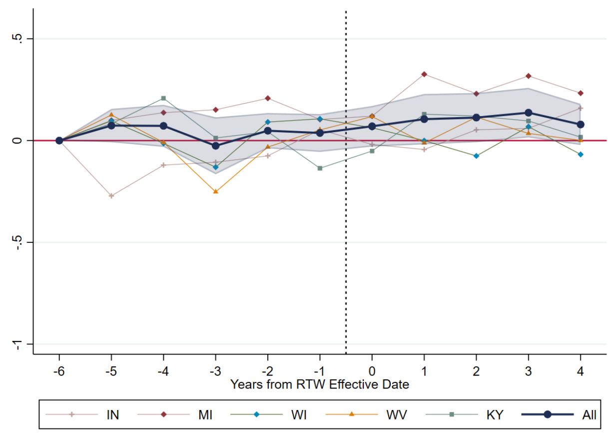
Figure 11: State Specific Trends - Economic Outcomes



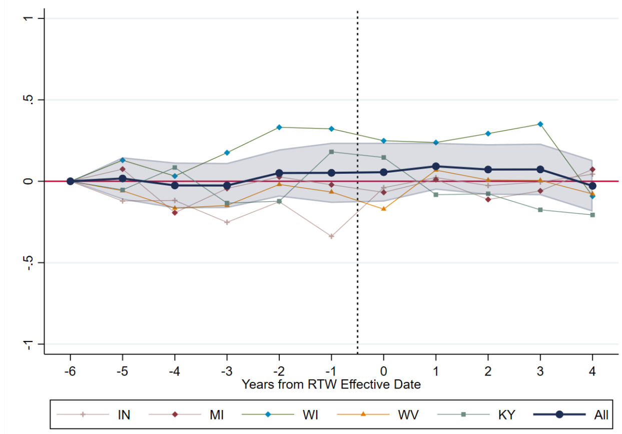
(a) % Union - High Union



(b) % Union - Low Union



(c) UWP - High Union



(d) UWP - Low Union

Figure 12: State Specific Trends - Union Outcomes

Table 11: Alternative Wage and Employment Definitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		No Pacific States				No Democratic States		
DID by 2006-2010 Industry Union Density	Log(Earns)	Log(Emp)	Log(%Union)	UWP	Log(Earns)	Log(Emp)	Log(%Union)	UWP
RTW*Post*High Union Industry	-0.0080	-0.0106	-0.1203**	0.0650*	-0.0108	0.0016	-0.0634	0.0748*
	(0.0067)	(0.0221)	(0.0537)	(0.0387)	(0.0080)	(0.0217)	(0.0538)	(0.0408)
RTW*Post*Low Union Industry	0.0009	0.0017	-0.1174	0.0413	0.0015	0.0090	-0.0916	0.0302
	(0.0048)	(0.0076)	(0.0765)	(0.0415)	(0.0041)	(0.0076)	(0.0723)	(0.0448)
R^2	0.9972	0.9993	0.8288	0.2925	0.9978	0.9994	0.8599	0.3951
Observations	19,300	19,300	16,672	19,467	9,114	9,114	7,753	9,128

Notes: * significant at 90%, ** significant at 95%, *** significant at 99%. Standard errors clustered at the state-industry level are in parentheses. All specifications include State-by-Industry and Year-by-Industry fixed effects. State controls include the natural log of a state's minimum wage, the natural log of a state's per capita GDP, and indicators for legislature partisanship. Industry composition controls include the college educated share of an industry's workforce, the female share of an industry's workforce that is female, and the people of color share of an industry's workforce.