Right-to-Work Laws, Unionization, and Wage Setting*

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ABSTRACT:

This paper uses two complementary approaches to estimate the effect of right-to-work (RTW) laws on wages and unionization rates. The first approach uses an event study design to analyze the impact of the adoption of RTW laws by five states over the last ten years. The second approach is based on the idea that RTW laws have a disproportionally large impact on industries with high unionization rates compared to industries with low unionization rates regardless of RTW. Both approaches indicate that RTW laws lower wages and unionization rates. In our preferred specification, we find that using RTW as an instrumental variable for union status yields an estimate of the effect of unions on wages of 0.35, which substantially exceeds the OLS estimate of 0.16. We use a potential outcomes framework to suggest that these results likely indicate that RTW has both direct and indirect effects on wages. A likely explanation for the direct effect is the union threat effect, while the indirect effect reflects how the negative impact of RTW on the unionization rate depresses wages through the causal effect of unions on wages.

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1. Introduction

A vast literature has documented large differences between the wages of union and non-union workers in different countries and time periods.[1] Cross-sectional studies that control for observed characteristics show that unionization is associated with higher and less dispersed wages. Panel data studies that rely on changes in union status to estimate the impact of unionization reach broadly similar conclusions.[2] Building on these results, several studies have shown that de-unionization in some countries, including the United States, was an important factor in the secular increase in wage inequality.[3]

However, it remains unclear whether the strong association between union status and wages represents a causal effect of unions on wages. A significant concern is that jobs covered by collective bargaining agreements may be systemically different from uncovered jobs. For instance, unions may be targeting high-rent jobs that would pay higher wages even in the absence of unionization. In that setting, cross-sectional and panel data studies may not provide valid estimates of how unionization affects the wages paid on a given job. Indeed, DiNardo and Lee (2004) find that new unionization in a given firm has little impact on the average wage paid on the given set of jobs available in that firm. While the finding may have limited external validity as it only applies to firms where a union narrowly won a union election, it suggests that the research design used in most of the literature may not yield causal estimates of unionization on wages.

In this paper, we leverage state differences in right-to-work (RTW) laws in the United States to take a new look at how unionization and union power more generally affect the wage distribution. Workers covered under a collective bargaining agreement in an RTW state cannot be legally compelled to pay their union dues even though they benefit just as much from the collective agreement as those who pay their dues. This exemption generates a "free-rider"

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[1] See Blanchflower and Bryson (2003) and Card, Lemieux, and Riddell (2003) for cross-country and cross-time evidence on the magnitude of the union wage premium.

[2] Examples of studies that use panel data to estimate union wage effects include Freeman (1984), Card (1996), and Lemieux (1998).

[3] Freeman (1993), Card (1992), and DiNardo, Fortin, and Lemieux (1996) find that de-unionization has contributed to the decline in wage inequality among U.S. males. Card, Lemieux, and Riddell (2004) reports similar findings for Canada and the United Kingdom.

problem that harms union finances and, consequently, on the ability of unions to organize new firms and provide services to their members. Lower rates of unionization in RTW states suggest that these laws greatly reduce the power of unions and their ability to organize workplaces.

One crucial challenge with studying the impact of RTW laws is that most RTW states adopted these laws in the late 1940s, which makes it hard to distinguish the effect of RTW from other underlying state differences using recent data. In this paper, we use two identification strategies to overcome this challenge. First, we exploit the fact that several states, including some large Midwestern states, have introduced RTW laws since 2011. We use an Event Study Design (ESD) and a Difference-in-differences (DD) approach to estimate the impact of RTW on wages and unionization rates in states "treated" with RTW.

The second identification strategy exploits the fact that the impact of RTW varies based on the underlying unionization rates of different industries. For example, financial and personal services industries have very low rates of unionization regardless of RTW laws. By contrast, educational services, public administration, and construction have high unionization rates in non-RTW states and substantially lower unionization rates in RTW states. These contrasts suggest using a "differential exposure design" where the impact of RTW can be estimated by relying on its differential impact in different industries. Unlike the ESD that relies on the parallel trend assumption, the fundamental identifying assumption in the exposure design is that, conditional on observables, the inter-industry wage structure would be the same across states in the absence of RTW laws. In other words, if industries where RTW reduces unionization rates the most also pay relatively lower wages in RTW states, we would interpret this wage impact as a causal effect of RTW.

Although our two research designs can be used to estimate the effect of RTW on wages and the rate of unionization, RTW is not necessarily a valid instrument for unionization. While we show that RTW is a relevant instrument for unionization, the exclusion restriction may be violated if RTW depresses union and non-union wages due to various spillover effects, including union threat effects (Rosen, 1969). Recent work by Fortin, Lemieux, and Lloyd (2021) and Farber et al. (2021) suggest that spillover effects of unions may be just as large as the direct effect of unions. We show that, in this setting, IV estimates of the effect of unions on wages can be decomposed into a conventional local average treatment effect (LATE) and a "bias" term reflecting the direct effect of RTW on wages linked to spillover effects. Although this means that

RTW laws may affect wages through a different set of channels, this does not affect the validity of our two identification strategies for estimating the causal effect of RTW on wages and unionization rates.

Our paper is related to several strands of the literature. First, it contributes to the literature on RTW laws by documenting the recent impact of these laws on unionization rates. Earlier studies sought to assess how much of the cross-state impact of RTW laws on unionization was due to the direct effect of the laws as opposed to other underlying differences in attitudes towards unions.[4] A critical contribution of our paper is to take advantage of the introduction of RTW laws in several states in recent years to isolate the effect of RTW laws after controlling for state effects and other characteristics of workers.

The paper also adds to the small literature looking at how labor laws may affect the wage distribution through their impact on unionization. Using historical data, Farber et al. (2021) look at how the Wagner Act and National War Labor Board affected wages by increasing unionization. More recently, labor reforms have typically reduced the power of unions. For instance, Biasi and Sarsons (2021) studied the consequences of Act 10 in Wisconsin that removed the ability of unions to bargain over wages. They find that reduced union power contributed to an increase in the gender wage gap among teachers.

Our paper is also related to previous attempts to find instrumental variables for unionization. DiNardo and Lee (2004) use a regression discontinuity design to examine whether wages increase at firms where unions narrowly won an organizing election relative to firms where they narrowly lost. Surprisingly, they fail to find a significant wage impact of new unionization.[5] Farber et al. (2021) argue that the introduction of the Wagner Act and National

[4] For example, Farber (1984) argues that state differences in unionization between RTW and non-RTW states largely reflect attitudes towards unions. Ellwood and Fine (1987) use union elections data to go back in time and estimate the effect of the introduction of RTW laws between 1951 and 1977. They find that RTW laws sharply reduce union organizing activities, and lead to a 5-10 percentage point decline in the rate of unionization in the long run. Unlike Ellwood and Fine, Farber et al. (2021) fail to find a significant impact of RTW laws in their re-examination of the evidence based on a more modern event-study approach.

[5] Subsequent work suggests that this finding may be linked to limitations of the research design used by DiNardo and Lee (2004). A possible explanation for the null finding is that the causal effect of unionization on wages is small in the case of close elections. If workers do not believe a union can extract wage concessions from the firm, perhaps because of limited rents to be shared, they will be less likely to support unionization. Lee and Mas (2012) find support for this view in their analysis of the impact of union election victories on the stock market return of firms. They find a small impact of unionization on stock market returns for close elections but a larger impact when unions win the election by a wider margin. In terms of internal validity, Frandsen (2021) shows evidence of manipulation in the RD design and non-random selection in the composition of workers who stay with the firm after unionization.

War Labor Board can be used as instrumental variables for unionization. Accordingly, we consider RTW laws as potential instrumental variables and discuss under what conditions the approach is valid.

The remainder of the paper proceeds as follows. In Section 2, we provide background on RTW laws and present descriptive statistics from the Current Population Survey (CPS) used for the empirical analysis. Section 3 proposes a potential outcomes framework to analyze the effect of RTW laws on wages and the rate of unionization and discuss the conditions under which RTW is a valid instrument for unionization. Section 4 presents the results based on the ESD approach, while Section 5 reports the findings that rely on the differential exposure design. Finally, we conclude in Section 6.

2. Data and RTW laws

2.1 Overview of RTW laws

Under the 1935 National Labor Relations Act, all U.S. workers covered by collective bargaining agreements receive the same benefits from unionization, including compensation, benefits, and access to grievance procedures regardless of whether they are members of the union. In most states, workers covered by a collective agreement have to pay union dues (typically withheld from paychecks by employers) regardless of whether they decide to become members of their union.

However, following the passage of the Taft-Hartley Act in 1947, it became possible for States to introduce so-called “right-to-work” (RTW) laws making it no longer compulsory for workers covered under a collective bargaining agreement to pay their union dues. As shown in Figure 1, several (mostly Southern) states quickly adopted RTW around that time. A few states then adopted RTW laws in the 1950s, 1960s, and 1970s. The impact of these RTW adoptions cannot be studied using microdata on union status and wages that only became available with a full set of state indicators in the late 1970s. Idaho’s adoption of RTW in 1985 is the first case where micro data from the CPS can be used to study the impact of RTW on wages and unionization rates. Unfortunately, the evidence is inconclusive due to small samples (Farber, 2005).

The twenty-first century has seen a renewed push for RTW laws at the local, state, and federal level. Nine states have introduced state-wide RTW legislation, of which six retain newly adopted RTW laws. These include (year of adoption shown in parentheses) Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), West Virginia (2016), and Kentucky (2017). In Missouri, RTW was voted down in a 2018 referendum after having initially passed both houses in 2017. In New Hampshire, RTW legislation was introduced in 2017 and 2021 but in both instances failed to pass the state House vote. Finally, in 2019 New Mexico passed legislation to ban RTW laws at the local level shortly after ten counties introduced local RTW laws. More details of each of these reforms is provided in the notes of Table 1. At the federal level, the 2019 ruling by the Supreme Court on *Janus v AFSCME* has extended RTW to all public sector employees – local, state, or federal - within the United States.

As a profoundly partisan policy, it is important to consider the political and economic context of this recent push for RTW. Since the 2001 adoption of RTW in Oklahoma, there have been two waves of RTW expansion. The first takes place in the mid-West in the immediate aftermath of the Great Recession, shortly after the 2010 mid-term elections. This election included 37 gubernatorial races, in which Republican candidates turned 12 governorships.[6] Republicans also gained the crucial trifecta of state governance – governor, house, and senate – needed to push through RTW legislation in 12 additional states after the 2010 election. These included Indiana, Wisconsin, and Michigan, which adopted RTW laws under their new Republican governments. Of the nine other states, six already had RTW laws, and the three states of Maine, Ohio, and Pennsylvania have remained non-RTW.

The second wave of RTW laws follows a similar pattern of Republican victories. Between the 2010 and 2018 mid-term elections, Republicans gain trifectas in 11 additional states – 4 in non-RTW states. Crucially, these included Iowa, Kentucky, Missouri, and New Hampshire in the 2016 mid-term elections. Iowa adopted RTW in 1947, while the remaining three states all attempted to pass RTW laws in 2017, but only Kentucky succeeded.[7] A Republican government is therefore not a guarantee of RTW adoption. That said, the reverse relationship holds for

[6] Republican candidates won 11 governorships in previously Democrat states and one with an independent incumbent (Florida). In contrast, Democratic candidates won only five races with a Republican incumbent, netting Republicans six states in the 2010 mid-term elections.
[7] Alaska was the fourth non-RTW to see a Republican government during this period. However, the Republican government lasted only one year, and since 2015 the Alaskan state government has been divided.

Democratic governments. For example, New Mexico's ban on local RTW laws was introduced shortly after a 2019 Democratic trifecta in the state.

The adoption of RTW in West Virginia and Oklahoma followed a different sequence of political events. West Virginia's adoption of RTW in 2016 took place under a divided government (Democratic governor and Republican house and senate) shortly before the 2016 gubernatorial election. However, this election ended up with a Republican government.[8] The same sequence of events took place in Oklahoma, which adopted RTW laws in 2001 under a divided government with an impending gubernatorial election. Therefore, one may interpret the adoption of RTW laws in these two settings as an appeal to right-leaning voters.

Table 1 documents the introduction of all state-wide RTW legislation since 2000, as well as the outcomes of the most recent gubernatorial race and the year in which Republicans gained a trifecta in the state government. An important point mentioned in Table 1 is that RTW was introduced in the public sector in Wisconsin when Act 10 went into effect on June 29, 2011. Act 10 also introduced other provisions limiting the power of public sector unions, including an annual vote to maintain union certification.

Section 4 discusses how the staggered adoption of these RTW laws might be studied within an event-study research design. In our main specifications, we use a different date for RTW adoption among public (2011) and private (2015) sector employees in Wisconsin. In doing so, we may be overstating the effect of RTW per se since Act 10 included other anti-union measures besides RTW.

2.2 CPS Data

Most of the empirical analysis relies on data from the merged outgoing rotation group of the Current Population Survey (MORG CPS) from 2003 to 2019. In addition, we present some complementary evidence using earlier CPS data starting in 1983 and exclude more recent data for 2020-21 due to the impact of COVID-19 on the labor market.[9] We use union coverage, as opposed to union membership, as our measure of unionization throughout. We only focus on

[8] The winning candidate was a Democrat but switched to the Republican party shortly after his victory.
[9] 1983 is the first year for which information on union status is available in the MORG CPS.

observations with unallocated wages to avoid the significant attenuation bias linked to the fact that union status is omitted in the CPS wage imputation (Hirsch and Schumacher, 2003).

Other sample selection criteria and variable definitions are similar to those used in Fortin, Lemieux, and Lloyd (2021). In the case of workers paid by the hour, our wage measure is the hourly wage directly reported by the worker. The wage measure is average hourly earnings (usual earnings divided by usual hours of work) for workers not paid by the hour. Wages are deflated into constant dollars of 2019 using the CPI-U. Top coded wage observations are adjusted by multiplying the wage by a factor of 1.4. See Lemieux (2006) for more information about data processing.

As discussed earlier, RTW laws are likely to have a larger impact on high-unionization industries than on industries with low unionization rates with or without RTW. We study these industry patterns using ten board industry aggregates: construction, manufacturing, wholesale and retail trade, transportation and utilities, FIRE (finance, insurance, and real estate), business and professional services, health and welfare, education, personal services, and public administration. Agricultural workers are excluded from the analysis, and the small primary sector (mostly mining and oil and gas) is combined with manufacturing.

Figure 2 compares the unionization rates by industrial sector for RTW and non-RTW states over the 2000-19 period. As is well known (Curme, Hirsch, and Macpherson, 1990), there are large differences in the rate of unionization across industries. Consistent with prior evidence (Lumsden and Petersen, 1975; Farber, 1984), unionization rates are substantially lower in RTW than in non-RTW states. The gap is particularly large in education and public administration. The unionization rate in these two sectors is around 50 percent in non-RTW states but less than 25 percent in RTW states. The construction sector and transportation and utilities have unionization rates of around 25 percent in non-RTW states, followed by manufacturing and health and welfare at around 15 percent. The unionization rate is substantially less than 10 percent in both RTW and non-RTW states in all other sectors.

While unionization rates in RTW and non-RTW states are strongly correlated, notable differences suggest that RTW may have larger effects in some sectors than others. For instance, construction and transportation, and utilities have similar unionization rates in non-RTW states. However, in RTW states, the unionization rate in the construction sector is twice as low as in the transportation and utilities sector. A possible explanation for this difference is that construction workers tend to be more loosely connected to employers, making it particularly hard for unions to organize when the legal environment is unfavorable due to RTW laws (Allen, 1988; Belman and Voos, 2006). On the other hand, the situation is arguably different in sectors like manufacturing and transportation, and utilities where the workforce is more stable, and unions often represent workers at the national level (e.g., in the airline industry), making it less likely for state-level RTW to depress the rate of unionization.

Existing evidence suggests that RTW laws and other aspects of the legal environment significantly impact public-sector unionization (mainly education and public administration). For instance, Freeman and Valletta (1988) and Ichniowski and Zax (1991) show that public sector unionization grew much less in the 1970s and 1980s in states with RTW laws and other union unfriendly measures than in other states.

In light of the patterns highlighted in Figure 2, we further group the ten industries into three broad groups to show the evolution of unionization rates in states that never adopted RTW laws (the "never RTW"), states that adopted RTW since 2000 (the "RTW adopters"), and states that already had RTW laws in the year 2000 (the "always RTW"). The "high unionization" industry group consists of industries (construction, education, and public administration) where the unionization rate is high, and RTW laws substantially reduce unionization. The "mid-unionization" group comprises industries (manufacturing, health, transportation & utilities) with an average unionization rate and RTW impact level. Finally, the "low-unionization" group consists of remaining industries where the unionization rate tends to be low both in RTW and non-RTW states.

Figure 3a shows the evolution of the unionization rate in high-unionization industries in the three sets of states. As most workers in this group are in the public sector, the unionization rate is relatively stable relative to the well-documented decline in private sector unionization rate (Freeman, 1988; Farber, 2005). However, an important exception is a substantial decline in unionization in the RTW adopter states after 2010. This decline closely matches the adoption of RTW in large mid-Western states that started around the same time (see Figure 1). Thus, figure 3a provides some early indication that the introduction of RTW laws has substantially reduced unionization in high-unionization industries, for which Figure 2 suggests the impact of RTW laws is likely to be the largest.

By contrast, Figures 3b (mid-unionization industries) and 3c (low-unionization industries) do not show an unusual drop in unionization among RTW adopters relative to states where RTW laws remained stable over time. In most cases, unionization rates in these (mostly) private sector industries slowly decline over time. Taken together, the evidence in Figure 3 suggests that, except for high-unionization industries in RTW-adopter states, unionization rates in the three sets of states have followed fairly parallel trends over time. Thus, the heterogeneous impact of RTW in different industries motivates the differential exposure design, while the evidence on parallel trends provides support for the ESD/DD design.

3. The effect of RTW on wages and unionization rates: a potential outcomes approach

This section introduces a potential outcomes framework to clarify how RTW laws can have direct effects on wages or indirect effects through their impact on unionization rates. When there is no direct effect, RTW laws can be used as an instrumental variable in an individual-level regression of wages on union status. This IV strategy can yield an estimate of the causal effect of union status on wages.

However, there are reasons to believe that RTW laws may be directly affecting wages regardless of union status. This implies a violation of the stable unit treatment value assumption (SUTVA) commonly used in the treatment effect literature. Union threat effects (Rosen, 1969) are a leading reason why SUTVA may fail. The idea behind threat effects is that non-union employers may seek to emulate the union wage structure to discourage workers from supporting unionization. Since RTW laws make it more challenging for unions to represent workers due to the free-rider problem, non-union employers in RTW states no longer need to pay their workers quite as much as a way of dissuading them from joining a union. Recent studies (Farber et al., 2021; Fortin, Lemieux, and Lloyd, 2021) suggest substantial threat effects. For instance, Fortin, Lemieux, and Lloyd (2021) conclude that the contribution of declining union threat effects to the growth in male wage inequality is as large as the conventional (shift-share) effect of de-unionization on wages.

We capture these concepts using a potential outcomes framework where earnings Y depend on the union status, U, and RTW laws. To fix ideas, consider the conventional approach where potential wages solely depend on the union status. Observed earnings are given by:

$$\mathrm{Y} = \mathrm{Y}(0) + \mathrm{U}[\mathrm{Y}(1) - \mathrm{Y}(0)],$$

where $\mathrm{Y}(1)$ and $\mathrm{Y}(0)$ represent potential earnings when workers are unionized and not unionized, respectively.

Consider the indicator variable D, where D=1 when a worker is in a state subject to RTW, and D=0 otherwise. U(D) indicates how the union status of workers depends on RTW laws. The observed union status is given by:

$$\mathrm{U} = \mathrm{U}(0) + \mathrm{D}[\mathrm{U}(1) - \mathrm{U}(0)]$$

RTW is a relevant instrument for the union status as long as $\mathrm{U}(1) \neq \mathrm{U}(0)$. As is well known (Imbens and Angrist, 1994), using D as an instrumental variable for U yields an estimate of the local average treatment effect (LATE) among compliers as long as the monotonicity assumption is also satisfied. In our setting, compliers are the workers induced out of unionization under RTW. That is workers for whom $\mathrm{U}(1) < \mathrm{U}(0)$. The monotonicity assumption rules out defiers who, counterintuitively, would be induced to take up unionization under RTW ($\mathrm{U}(1) > \mathrm{U}(0)$). Under these assumptions, it follows from Imbens and Angrist (1994) that the IV estimate β^{IV} is the LATE for workers induced out of unionization by RTW:

$$\beta^{IV} = \frac{E[Y|D=1] - E[Y|D=0]}{E[U|D=1] - E[U|D=0]} = E[\mathrm{Y}(1) - \mathrm{Y}(0)|\mathrm{U}(1) < \mathrm{U}(0)].$$

We next allow earnings to depend directly on RTW laws by writing potential outcomes Y(U,D) as a function of both union status and RTW laws. Earnings for a unionized worker can now be written as:

$$\mathrm{Y}(1) = \mathrm{Y}(1,0) + \mathrm{D}[\mathrm{Y}(1,1) - \mathrm{Y}(1,0)],$$

while earnings for a non-union worker are given by:

$$\mathrm{Y}(0) = \mathrm{Y}(0,0) + \mathrm{D}[\mathrm{Y}(0,1) - \mathrm{Y}(0,0)]$$

The second term in both equations captures the direct effect of RTW on earnings not mediated through the impact of RTW on the union status of workers. This direct effect leads to a violation SUTVA, since the treatment effect $\mathrm{Y}(1) - \mathrm{Y}(0)$ now directly depends on D. To illustrate the nature of the resulting bias in the IV estimator, let's combine the equations for Y(0) and Y(1) using the union status dummy. This yields the following expression for observed earnings Y:

$$
\begin{aligned}
&\mathrm{Y} \;=\; \mathrm{Y}(0,0) + \mathrm{U}(0)[\mathrm{Y}(1,0) - \mathrm{Y}(0,0)] \\
&+ \mathrm{D}[\mathrm{U}(1) - \mathrm{U}(0)] * [\mathrm{Y}(1,1) - \mathrm{Y}(0,1)] \\
&+ \mathrm{D}\Big[\mathrm{Y}(0,1) - \mathrm{Y}(0,0) + \mathrm{U}(0)\big[\big(\mathrm{Y}(1,1) - \mathrm{Y}(1,0)\big) - \big(\mathrm{Y}(0,1) - \mathrm{Y}(0,0)\big)\big]\Big]
\end{aligned}
$$

The first term on the right-hand side of the equation represents earnings in the absence of RTW laws. The term on the second line of the equation is the "complier" effect that captures the wage impact Y(1,1) – Y(0,1) induced by a change in unionization when a state switches to RTW. The last term captures the direct effect of RTW on wages due, for instance, to a reduced union threat effect. Although the direct effect can be different for union and non-union workers, we simplify the expression by constraining the two effects to be the same:

$$\mathrm{Y}(1,1) - \mathrm{Y}(1,0) = \mathrm{Y}(0,1) - \mathrm{Y}(0,0).$$

Under this assumption, the direct effect of RTW simplifies to $\mathrm{D}[\mathrm{Y}(0,1) - \mathrm{Y}(0,0)]$. It follows that the IV estimate β^{IV} now consists of the causal effect of union status on earnings (the LATE) plus a bias term linked to the direct effect of RTW:

$$\beta^{IV} = \gamma + \frac{\lambda}{\phi}\ ,$$

where $\gamma = E[\mathrm{Y}(1,1) - \mathrm{Y}(0,1) | \mathrm{U}(1) < \mathrm{U}(0)]$ is the LATE. The bias term λ/ϕ is the ratio of the direct effect of RTW on non-union wages for all workers, $\lambda = E[Y(0,1)|D = 1] - E[Y(0,0)|D = 0]$, over the first-stage effect of RTW on the unionization rate, $\phi = E[U|D = 1] - E[U|D = 0]$.

Fortin, Lemieux, and Lloyd (2021) find that threat effects of unionization more or less double the effect of unionization on earnings in a local labor market (defined by state and industry in their case). This finding suggests that the bias term could be as large as the causal effect of union on earnings, γ. Finding a large IV estimate when using RTW as an instrumental variable likely suggests that RTW directly affects wages which biases up the IV estimate of the causal effect.

While it would be helpful to estimate the magnitude of the bias linked to the direct effect of RTW on wage, it is not possible to do since the potential outcome $Y(0,1)$ cannot be observed for all workers when $D = 1$. Likewise, $Y(0,0)$ cannot be observed for all workers when $D = 0$. While we can estimate the average value of the potential outcomes among non-union workers and compute the difference $\tilde{\lambda}$ when $D = 1$ and $D = 0$:

$$\begin{aligned}\tilde{\lambda} &= E[Y(0,1)|U = 0, D = 1] - E[Y(0,0)|U = 0, D = 0] \\ &= E[Y|U = 0, D = 1] - E[Y|U = 0, D = 0],\end{aligned}$$

the difference is not the same as λ due to the non-random selection of workers into union status. For instance, if union workers induced out of unionization by RTW are more skilled than non-union workers, we would expect $\tilde{\lambda}$ to be positive even if the true direct effect, $Y(0,1) - Y(0,0)$, is equal to zero.

Note that even if RTW is not a valid instrumental variable for the union status, it is still interesting to estimate the reduced form effect of RTW on wages, $E[Y|D = 1] - E[Y|D = 0]$. The reduced form effect can be written as the sum of the direct effect and the indirect effect of RTW mediated through its impact on the unionization rate. The latter is given by the product of the LATE and the impact of RTW on the unionization rate (the first-stage effect ϕ):

$$E[Y|D = 1] - E[Y|D = 0] = \gamma\phi + \lambda$$

We can always estimate this total effect of RTW on wages even if RTW is not a valid instrument for unionization. What we cannot do in this setting is decompose the effect of RTW into a direct and indirect effect. Accordingly, we will always report the reduced form effects along with IV estimates in the results sections, keeping in mind that the latter is only interpreted as a LATE when the direct effect of RTW is equal to zero.

4. Event study estimates of the effect of adopting RTW laws

This section reports the results obtained using a first identification strategy based on RTW adoption in five states between 2011 (introduction of Act 10 in Wisconsin) and 2017 (adoption of RTW in Kentucky). We focus on the 2007-2019 period to keep the analysis sample relatively balanced in terms of the number of years in the pre- and post-RTW adoption periods.

In terms of notation, we define E_s as the date (in year and month) RTW was adopted in state s. Time relative to the adoption is $K_{st} = t - E_s$. We use the following specification to conduct an event-study analysis at the individual level for the outcome variable Y_{ist}:

$$Y_{ist} = \alpha_s + \delta_t + \sum_{k=-5}^{k=5} \pi_k \mathbf{1}\{K_{st} = k\} + X_{it}\psi + \varepsilon_{ist}.$$

where α_s is a set of state fixed effects; δ_t is a set of year fixed effects; $\mathbf{1}\{\cdot\}$ is the indicator function; Y_{ist} either represents log wages or the union status of individual i; and X_{it} is a set of individual-level covariates. In addition to the 10 industry categories discussed in Section 3, the covariates include a rich set of individual and job characteristics. These covariates consist of years of education, a quartic in potential experience, experience-education interactions (16 categories plus experience times education), eight occupation categories, and dummy variables for race, marital status, public sector, part-time, and MSA status. We also fully interact industry dummies with year dummies to control for industry trends that could confound the impact of RTW adoption. For example, if wages and unionization rates are trending down in manufacturing, failing to control for industry trends could bias up (in absolute terms) the estimated RTW impacts in states like Michigan and Indiana that have a higher share of manufacturing jobs. In the main specifications, standard errors are clustered at the state-year level.

We use five years of data in the pre-period to detect pre-trends that could represent a threat to identification. We also use five years in the post-period. Since we do not want to extend the analysis beyond 2019 due to the Covid-19 pandemic, we would need to rely heavily on the few states that adopted RTW early on to go beyond five years. The sample of states is limited to those that had not adopted RTW laws prior to 2007. Thus, the effect of RTW is estimated by comparing the five RTW-adopter states to the "never adopters".

We also estimate the difference-in-differences (DD) version of the model where the effect of RTW is constant over the post-period ($\pi_k = \pi$ when $k \geq 0$), and constrained to zero in the pre-period ($\pi_k = 0$ when $k < 0$). The DD specification is given by:

$$Y_{ist} = \alpha_s + \delta_t + \pi \cdot RTW_{st} + X_{it}\psi + \varepsilon_{ist},$$

where the treatment dummy RTW_{st} is equal to one when $t \geq E_s$, and zero otherwise.

Figure 4 presents the results for men and women pooled together. Figure 4a shows the "first-stage" effect of RTW on the unionization rate. There is no evidence of pre-trends as none of the estimates of π_k for $k < 0$ are significantly different from zero. By contrast, all of the estimated coefficients after RTW adoption are negative and significantly different from zero. The negative effect of RTW on the unionization rate gradually increases from -0.017 in the initial year of adoption to -0.040 after five years.

Figure 4b shows that adopting RTW also leads to lower wages, though the estimates are substantially noisier than in the case of the unionization rate. There is again little evidence of pre-trends, while wages gradually decline after the adoption of RTW. Although the wage impact ranges from about -0.010 to -0.015 in 2 to 5 years after adopting RTW, most of the wage effects are not statistically significant.

The corresponding DD estimates reported in the first column of Table 2 are more precisely estimated since all the post-coefficients are constrained to be the same, while the pre-coefficients are constrained to zero. The first-stage estimates indicate that RTW reduces the unionization rate by close to two percentage points (0.0185), while wages drop by a bit more than one percentage point (0.0123 effect on log wages). The ratio of the wage and unionization coefficients yields an IV estimate of the effect of unionization of wages of 0.66. It is quite large compared to conventional OLS estimates of the union wage gap. For instance, the last row of Table 2 shows that the OLS estimate in this sample is equal to 0.16. Based on the discussion in Section 3, this suggests that RTW may also have a direct effect on wages, which violates the exclusion restriction.

Note, however, that the standard error on the IV estimate is quite large. The standard error gets even larger when we cluster standard errors at the state level in Appendix Table A1. The IV estimates are only significant at the 90 percent level, and the first stage is weak. We prefer to cluster at the state-year level in our main specification since we only have five treatment states, which causes small sample issues when trying to cluster at the state level. The lack of precision when clustering at the state level suggests, nonetheless, that the results reported in Table 2 should be interpreted with caution.

Figure 5 reports the results for men and women separately. The main takeaway of the figure is that the results for all workers pooled together primarily reflect the larger impact on RTW on the wages and unionization rates of women. For instance, Figure 5b indicates that the rate of unionization among female workers gradually declines after the introduction of RTW. The impact is above four percentage points 5 years after the introduction of RTW. Likewise, there is a clear decline in wages after the introduction of RTW for women, but little evidence of such an effect for men. The DD estimates in columns 2 and 3 of Table 2 show a similar pattern of results where the wage effect is almost three times larger for women (0.018) than men (0.007).

The larger impact of RTW on women relative to men suggests that the public sector (mainly education and public administration), where female workers are over-represented, may be more affected by the introduction of RTW. We explore this possibility by estimating the impact of RTW on the three “high-unionization” industries introduced in Section 2 — construction, education, and public administration—.[10] More generally, dividing the sample by high vs. low unionization rates industries may help uncover substantial heterogeneity in the effect of RTW. As discussed earlier, and more extensively in the next section, RTW is unlikely to have much impact in industries where the unionization rate is low regardless of RTW laws,

Figure 6 shows the event-study estimates for the three high-unionization and the seven other industries with lower unionization rates. The results show that the overall effect of RTW is almost entirely driven by its impact on the three high-unionization industries. Likewise, the DD estimates reported in Table 2 are much larger in the three high-unionization industries than in the

[10] The results are similar when we only use education and public administration. Although the construction industry employs relatively few women, we group that sector with education and public administration for the sake of consistency with the industry breakdowns introduced in Section 2.

other sectors. The IV estimate in the three high-unionization industries is equal to 0.54, slightly smaller than the corresponding estimate for all sectors pooled together.

The last column of Table 2 shows a "triple-differences" version of the IV estimator. The estimator is obtained by relaxing the assumption of parallel trends in states adopting and not adopting RTW laws:

$$Y_{ist} = \alpha_s + \delta_t + \pi_1 \cdot H_{it} \cdot RTW_{st} + \pi_2 \cdot RTW_{st} + X_{it}\psi + \varepsilon_{ist},$$

where H_{it} is an indicator variable for individual *i* working in a high-unionization industry at time *t*. The coefficient π_2 captures the main effect of RTW on wages and unionization. For example, it could account for the fact that the Midwestern states adopting RTW were hit harder by the Great Recession relative to control states. As long as this departure from the parallel trend assumption is similar in different industries, π_1 will capture the differential impact of RTW in high-unionization industries. The triple-differences IV estimate is the ratio of the effect of $H_{it} \cdot RTW_{st}$ on wages and unionization. It can also be obtained by running a two-stage least-squares regression of wages on unionization using the interaction $H_{it} \cdot RTW_{st}$ as an instrumental variable for unionization while controlling for the main effect of RTW and all the other covariates.

The triple-differences IV estimator is smaller (0.51) than most of the DD estimators reported in the other columns of Table 2. This result suggests that departures from the parallel trend assumptions are slightly inflating the DD estimates and overstating the direct effect of RTW on wages. The IV estimate remains, nonetheless, substantially above OLS estimates of the effect of unions on wages, suggesting that RTW has a direct effect on wages. However, the results are inconclusive due to the substantial size of the standard errors, especially when we cluster at the state level (Appendix Table 1). Nevertheless, as we will see in the next section, it provides a rationale for our second identification strategy that provides more precise estimates of the key parameters of interest.

A second reason for switching to the second identification strategy is that the ESD/DD estimates are sensitive to the treatment of RTW in the public sector in Wisconsin. As mentioned above, RTW was one of several other anti-union measures introduced in the Wisconsin public sector as part of Act 10 in 2011. As such, the effect of RTW we are estimating may also be capturing the impact of other measures limiting the scope of collective bargaining. Appendix

Table A2 shows that the effect of RTW becomes substantially smaller and often insignificant when the Wisconsin public sector is excluded from the analysis. Thus, while we think it is important to include the Wisconsin public sector as part of the analysis, our main findings likely represent an upper bound to the pure impact of RTW.

Another potential concern is that some of the control states, for instance California, may be poor controls for the treated states that are all in the East North Central part of the Midwest (Michigan, Wisconsin, and Indiana), or just south of this area (West Virginia and Kentucky). We address this issue in Appendix Table A3 by reporting estimates based on a narrower set of "Rust-Belt'' control states.[11] The main estimates are slightly smaller, but otherwise similar to those based on the broader set of control states. For example, the first-stage effect for all workers drops from -0.0185 to -0.0154 when we only use the Rust-Belt states as controls, while the IV estimate declines from 0.664 to 0.574. These results indicate that the choice of control states has little impact on the main findings.

5. Results based on the differential exposure design

Unlike the first identification strategy based on the introduction of RTW laws in adopter states, the second strategy relies on the differential impact of RTW across industries in states with and without RTW. The fundamental identifying assumption of this exposure design is that, conditional on observables, the inter-industry wage structure would be the same across states in absence of RTW laws. In other words, if industries in which RTW has the largest impact on unionization rates also pay relatively lower wages in RTW states, we would interpret this wage impact as a causal effect of RTW.

We implement the approach using the following regression model:

$$Y_{ist} = \alpha_s + \delta_t + \sum_{j=1}^{j=10} \mathbf{1}\{I_{it} = j\} \cdot (\theta_j + \pi_j RTW_s) + X_{it}\psi + \varepsilon_{ist},$$

Where I_{it} is a categorical variable indicating industry affiliation and, as before, the outcome variable Y_{ist} is either wages or union status. θ_j is a set of main industry effects, while π_j captures the differential impact of RTW in industry j. The covariates X_{it} include the job and individual

[11] The Rust-Belt control states are New York (with NYC excluded due to its very different industry composition), Pennsylvania, Ohio, Illinois, Minnesota, and Missouri.

characteristics defined in Section 4 and a full set of interactions between industry and year dummies. We also include a set of three-digit industry effects to control for differences in industry composition within the ten broad industry groups. For instance, within the manufacturing sector durable goods industries tend to be over-represented in non-RTW states, while the oil and gas sector is over-represented in RTW states.[12]

Note that RTW is assumed to be time-invariant in the estimating equation as the subscript *s* indicates dependence on state and not time. We do so to make it clear that the estimation approach does not require any time variation in the RTW. In practice, however, we let RTW vary over time when some states introduce RTW laws.

Under this research design, the effect of unions on wages can be estimated by using the interaction between industry dummies and RTW status as instruments for unionization. The equation being estimated is:

$$W_{ist} = \alpha_s + \delta_t + \beta U_{ist} + \sum_{j=1}^{j=10} \mathbf{1}\{I_{it} = j\} \cdot \theta_j + X_{it}\psi + \varepsilon_{ist}.$$

where W_{ist} is the wage and U_{ist} is the union status of worker *i* at time *t*.

Unlike the models considered in Section 4, we are now in a case where the model is overidentified since we have nine instruments (interactions between the RTW dummy and nine industry dummies relative to a base industry) to estimate a single parameter β. If the model is true, the effect of the instruments (the π_j's) in the reduced form and first-stage equations should be proportional to each other, and their ratio should be equal to β. This stringent restriction is unlikely to hold when the effect of unions on wages is heterogeneous. It is, nonetheless, useful to visualize the relationship between the impact of RTW on wages and unionization rates to see to what extent they line up with each other.

Figure 7 plots the effect of RTW on industry wages against the effect on unionization rates obtained after controlling for individual covariates and a set of state, year, and three-digit industry dummies. To simplify the exposition, we plot the opposite of the coefficients π_j, that is

[12] An alternative approach would be to interact the three-digit industry dummies with RTW in the estimating equation. However, doing so would yield noisier estimates and may lead to an overfitting problem in the first-stage equation when running IV. So we instead use a more parsimonious approach where we control for the main effect of three-digit industries in an unrestricted way, but constrain the difference in industry effects between RTW and non-RTW states to only depend on the ten broader industry groups.

the impact on wages and unionization rates of not being in an RTW state. The regressions are estimated over the 2003-19 period.

Although the two sets of estimated effects do not perfectly line up, they are closely correlated, as evidenced by an R-square of 0.66 when we run a regression of the ten industry wage effects on the ten unionization rate effects, weighting for the relative size of the industries. (The regression line is plotted using a dashed line). The implied IV estimator is the slope of the relationship, 0.347 with a standard error of 0.088 in this simple specification. Consistent with the previous discussion, RTW appears to have a particularly large impact on the three "high-unionization" industries –construction, education, and public administration—that are all in the upper right part of Figure 7.

The main results are reported in Table 3. Since the exposure approach does not rely on changes in RTW laws, we show results over three separate periods (1983-1992, 1993-2002, 2003-2019) that are relatively similar to those considered by Fortin, Lemieux, and Lloyd (2021).[13] In each period, we report separate results for men and women as well as pooled results for all workers. To make the first-stage and reduced form results easier to interpret, we present the (weighted) average estimates of the π_j's for the high- and mid-unionization industries relative to the low-unionization ones.

Consistent with Figure 7, the IV results reported in Panel A of Table 3 are precisely estimated and primarily lie in the 0.3-0.4 range. The estimated effects tend to be slightly larger for men than women, and do not exhibit a systematic trend over time. As expected, the first-stage (Panel C) and reduced-form estimates are substantially larger for high-union relative to mid-union industries. Looking across all nine specifications, the average first-stage effect of high-union industries is -0.196 compared to -0.076 for mid-union industries. The corresponding reduced form effects are -0.078 and -0.015, respectively. Thus, the ratio of the reduced-form to the first-stage is 0.40 for high-union compared to 0.22 for mid-union industries. This ratio is consistent with results reported in Figure 7, where the reduced form (wage) effects are above the regression line for high-union industries (construction, education, and public administration) but below the line for the mid-union industries (manufacturing, transportation & utilities, and health). Although these differences lead to a failure of the overidentification test, the range of

[13] The choice of analysis periods period is in part driven by a major change in three-digit industry codes between 2002 and 2003.

estimate (0.22 to 0.40) is plausible in the sense that it lies slightly above the OLS estimates of the effect of unions on wages.

6. Discussion and concluding comments

Results based on our two research designs provide evidence that RTW laws reduce unionization rates and wages. The first design (ESD/DD) relies on the arguably weaker parallel assumption. Although the ESD estimates provide clear visual evidence of these effects, the DD estimates are somehow imprecise, especially when we cluster at the state level. The findings are also sensitive to the inclusion of the Wisconsin public sector, where RTW was introduced along with other measures restricting collective bargaining as part of Act 10 in 2011.

Interestingly, previous work on the impact of the earlier introduction of RTW on unionization rates has also reached some ambiguous conclusions. Using union election data for 1951 and 1977, Ellwood and Fine (1987) find that RTW laws sharply reduce union organizing activities and lead to a 5-10 percentage point decline in the rate of unionization in the long run. Unlike Ellwood and Fine, Farber et al. (2021) fail to find a significant impact of RTW laws in their re-examination of the evidence based on a more modern event-study approach. Specification issues aside, RTW laws may be operating gradually, making it challenging to detect their impact using an ES design.

Our second approach based on a differential exposure design does not suffer from these limitations and can be used to estimate the long-term impact of RTW laws on wages and unionization rates. An important advantage of this cross-sectional approach is that it yields much more precise estimates than those obtained using the ESD/DD approach. However, a potential limitation is that it relies on the arguably stronger assumption that inter-industry wage differentials would be similar in different states if not for RTW that disproportionally affects high-unionization wage industries.

To the best of our knowledge, the systematic differences in industry wage premia between RTW and non-RTW states have not been documented previously. The fact that industry wages in RTW states tend to be lower in high-union industries where RTW laws have the largest potential impact suggests that RTW is a parsimonious and credible explanation for the observed differences in the inter-industry wage structure. So, although the identifying assumption underlying the differential exposure is ultimately untestable, we think that, on balance, the evidence is consistent with RTW having a causal effect on unionization and wages. Although the evidence from the ES/DD design in not as conclusive, the similarity of the results obtained using the two approaches is re-assuring.

Having established that RTW affects unionization rates and wages, an important question is whether RTW can be used as an IV for union status in a wage equation. As discussed in Section 3, IV estimates can be interpreted as the LATE of unions on wages provides that RTW does not have a direct effect on wages. In other words, the effect of RTW on wages has to be entirely mediated through its impact on the rate of unionization. Our preferred specification yields an IV estimate of 0.354 compared to an OLS estimate of 0.161, using the estimates for all workers in 2003-2019 based on the exposure design (column1 of Table 3). The large size of the effect suggests that the impact of RTW is not entirely mediated through its effect on unionization rates. RTW may also have a direct impact due, for example, to lower union threat effects. In that sense, the findings are consistent with recent studies (Farber et al., 2021, and Fortin, Lemieux, and Lloyd, 2021) that suggest threat effects of the same order of magnitude as the direct impact of unions on wages.

Importantly, even if we do not think that RTW is a valid instrument for unionization, the reduced-form evidence shows that it has a large impact on wages. This evidence indicates that the legal environment in which firms and workers operate impacts wage setting significantly. Given that unionization rates are also affected, the evidence suggests that, at a minimum, unionization is an important channel through which RTW leads to lower wages. More generally, our findings are consistent with the view that public policies which make it harder for unions to organize and represent workers lead to lower bargaining power and wages. It would be interesting to see in future work whether anti-union policies like RTW also have an impact on the labor share and wage inequality.

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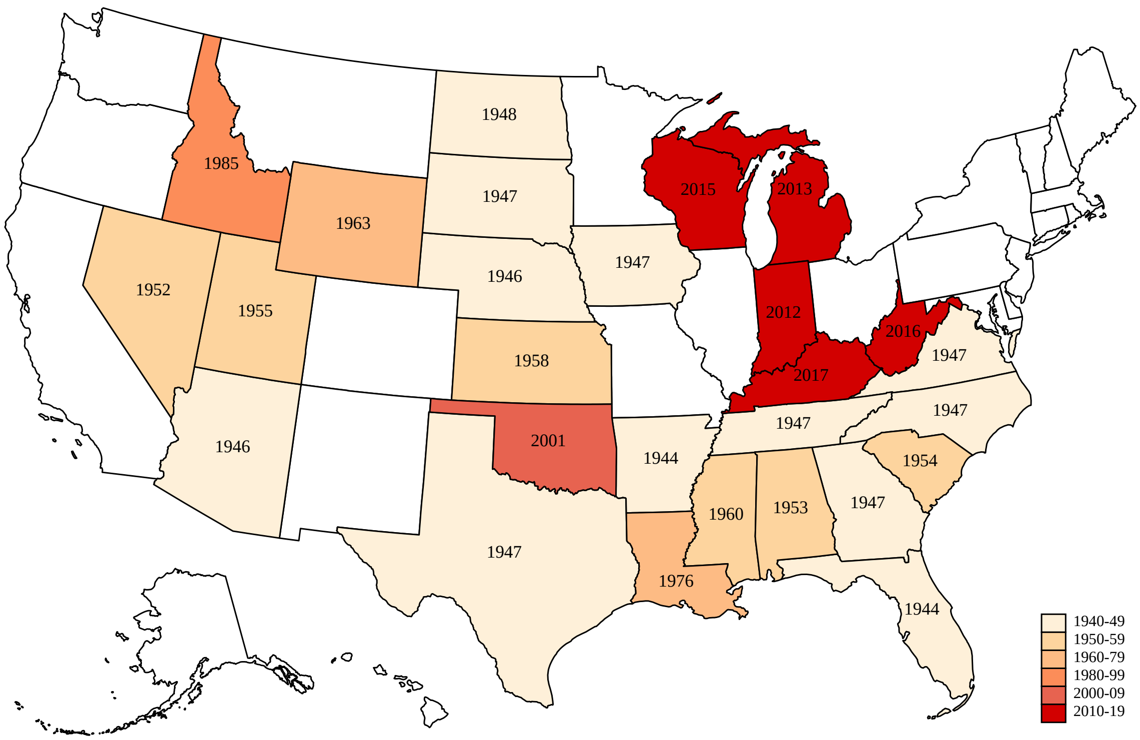

Figure 1: The Expansion of Right-to-Work Coverage

Note: The map demonstrates the adoption of RTW laws across US states beginning in 1944. The map does not include RTW laws brought to vote in Missouri (2017) and New Hampshire (2017, 2021), but not implemented. For more details of recent reforms see Table 1.

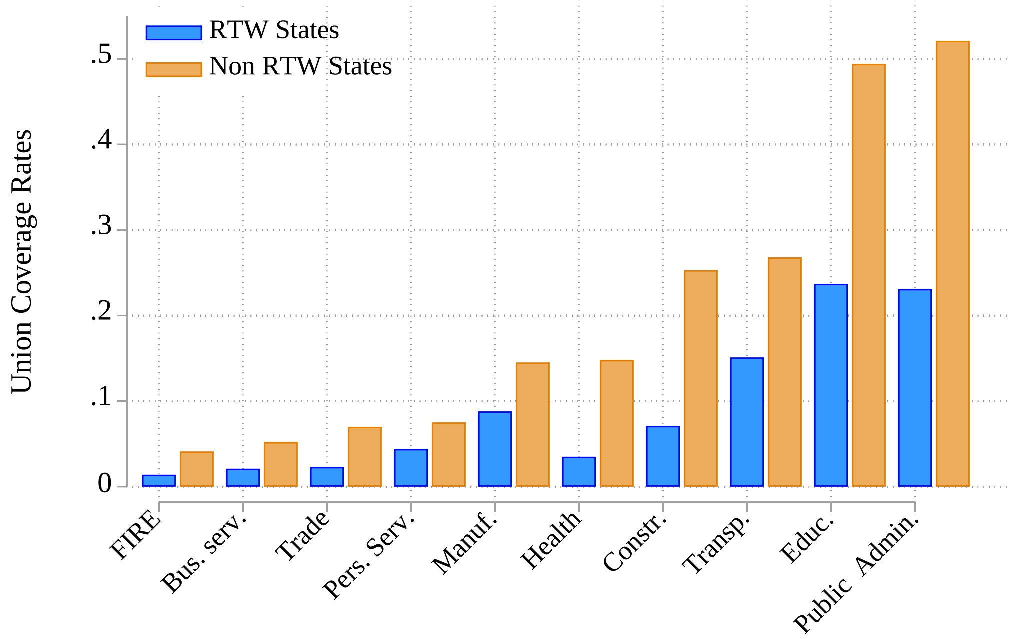

Figure 2: Differences in Union Coverage Rates across RTW and non-RTW States

Note: The union coverage rates for RTW and non-RTW states are computed using MORG CPS data for the 2000-2019 period.

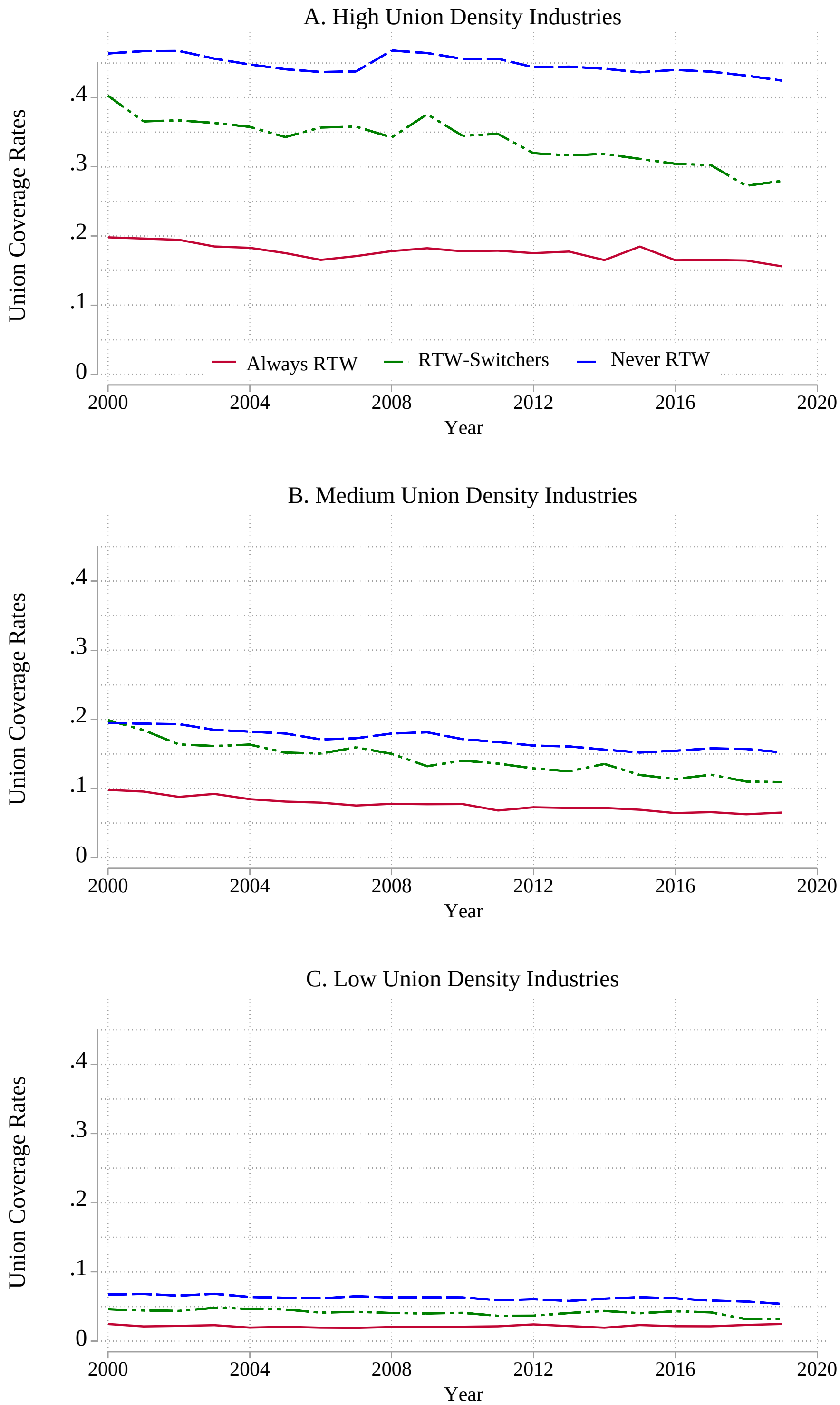

Figure 3: Trends in Union Coverage Rates across Groups of Industries

Note: Computed using MORG CPS data for the 2000-2019 period. High union density industries include construction, education, and public administration; medium union density industries are manufacturing, health, and transportation & utilities; low union density industries are personal and business services, trade, and finance, insurance, and real estate (FIRE).

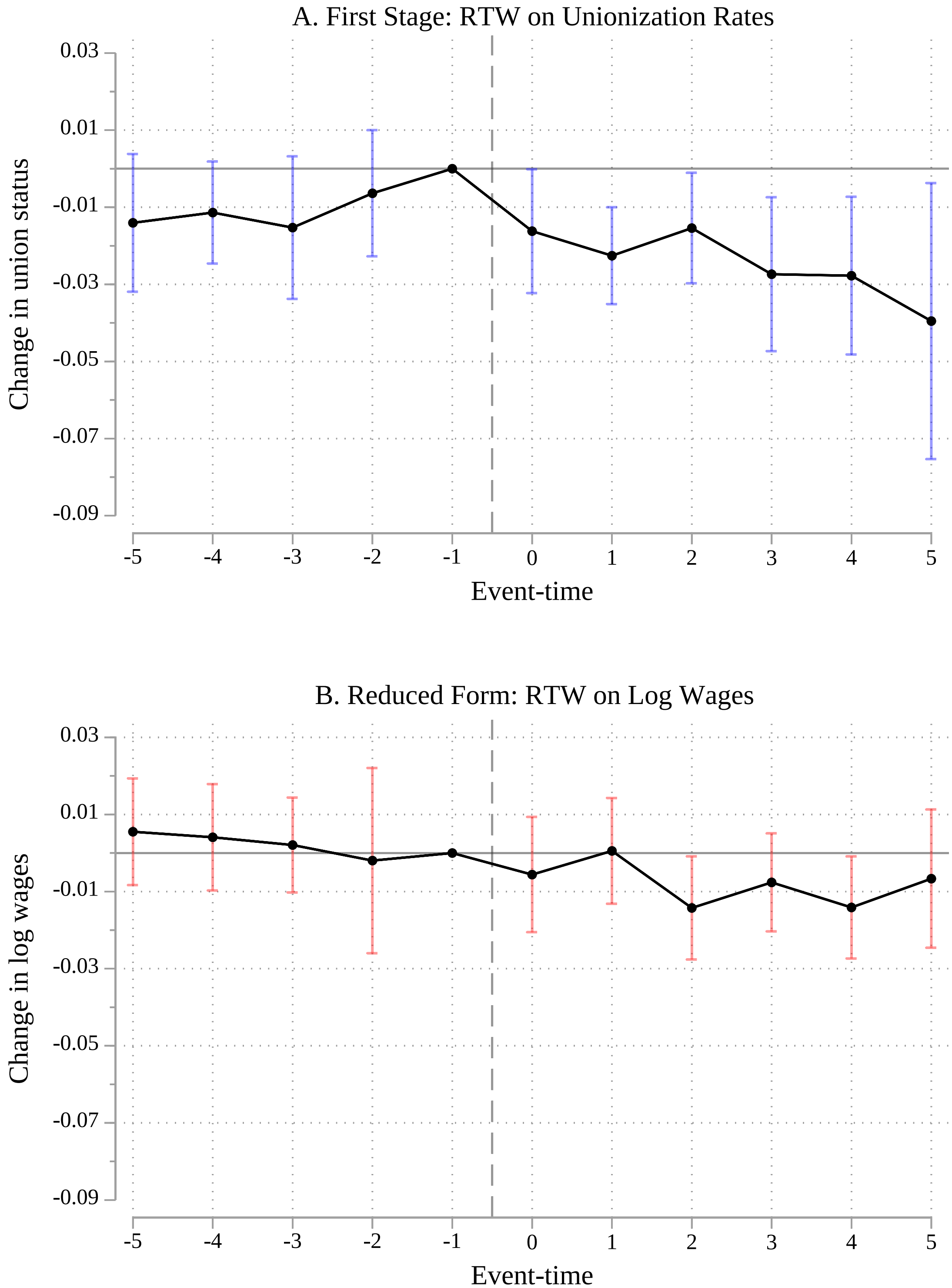

Figure 4: Event-study Estimates of the Impact of Adopting RTW Laws

Note: The figures use 2007-19 MORG CPS data to compare the evolution of log wages and unionization rates in the five states that adopted RTW laws between 2011 and 2017 relative to states that never adopted RTW laws. Confidence bands (95 percent confidence level) are provided around the point estimates. See text for details.

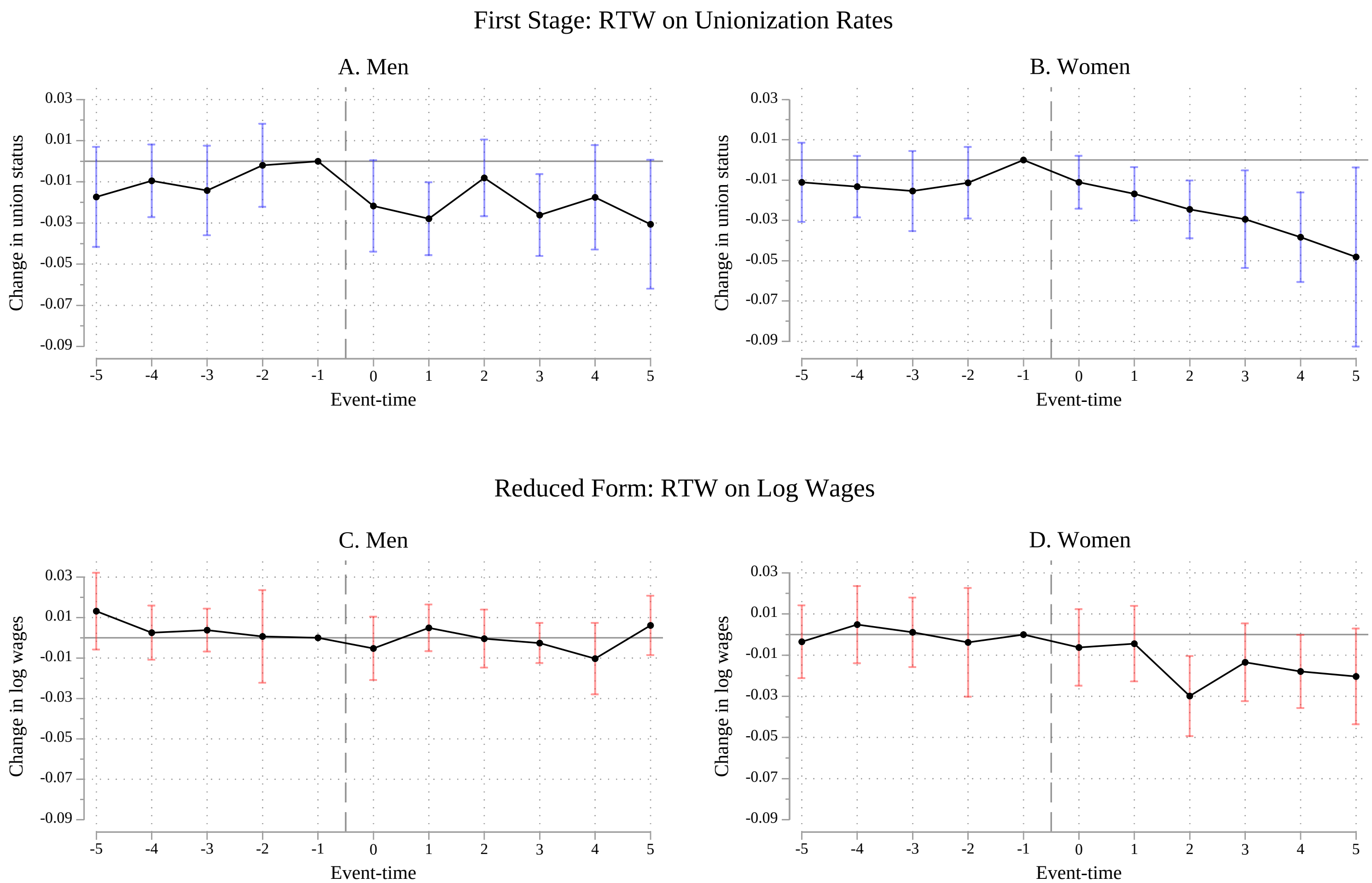

Figure 5: Event-study Estimates of the Impact of Adopting RTW Laws by Gender

Note: The figures use 2007-19 MORG CPS data to compare the evolution of log wages and unionization rates in the five states that adopted RTW laws between 2011 and 2017 relative to states that never adopted RTW laws. Confidence bands are provided around the point estimates. See text for details.

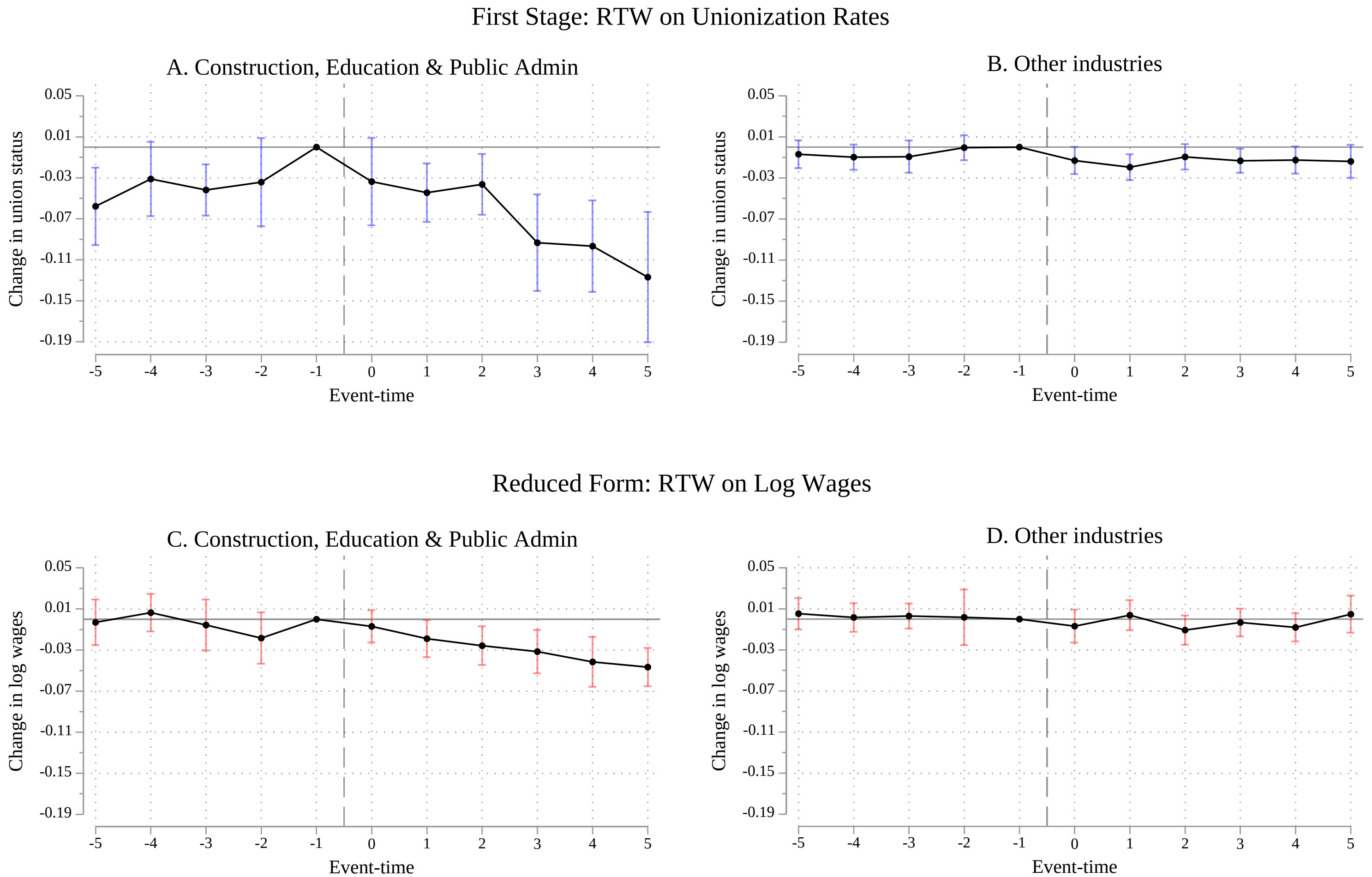

Figure 6: Event-study Estimates of the Impact of Adopting RTW Laws by Industry

Note: The figures use 2007-19 MORG CPS data to compare the evolution of log wages and unionization rates in the five states that adopted RTW laws between 2011 and 2017 relative to states that never adopted RTW laws. Confidence bands are provided around the point estimates. See text for details.

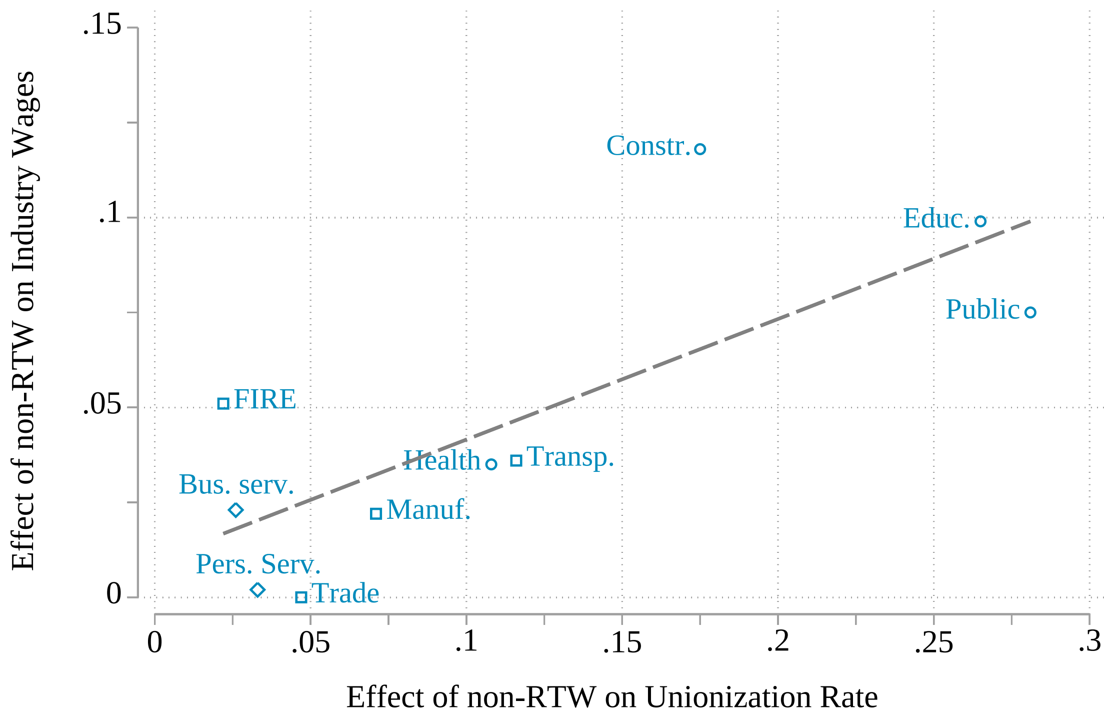

Figure 7: Relative Effect of RTW on Industry Wages and Unionization Rates

Note: The figures plot the estimated effect of RTW interacted with industry affiliation in regression models that also control for for individual covariates and a set of state, year, and three-digit industry dummies. The regression models are estimated using MORG CPS data for 2003-2019. See text for details.

Table 1: The Role of Political Events in the Adoption of RTW Laws

State	Date Effective	Previous gubernatorial election year	Incumbent	Winner	Trifecta Status State government control	Year changed
Oklahoma-[1]	2 September 2001	1998	R	R	Divided	1995
Indiana[2]	1 February 2012	2008	R	R	Republican	2011
Michigan	8 March 2013 (filed December 2011)	2010	D	R	Republican	2011
Wisconsin[3]	9 March 2015	2010	D	R	Republican	2011
West Virginia	12 February 2016	2012	D	D	Divided	2015
Kentucky[4]	7 January 2017	2015	D	R	Republican	2017
Missouri[5]	28 August 2017 (passed 6 February 2017)	2016	D	R	Republican	2017
New Hampshire[6]	Defeated in 2017 and again in 2021.	2016	D	R	Republican	2017
New Mexico[7]	Banned 2019	2018	R	D	Democratic	2019

Notes:

[1] The incumbent Republican governor was replaced Democrat the following year.

[2] Previously passed in 1957, but repealed by a Democratic governor in 1965.

[3] Act 10 went into effect 29 June 2011, bringing RTW to public sector employees in Wisconsin. This Act also brought about a significant reduction in public sector employment.

[4] Kentucky had 12 local RTW laws prior to the enactment of state-wide legislation. These were upheld in the Sixth Circuit Court of Appeals on 18 November 2016.

[5] On 18 august 2017 sufficient signatures were filed to put the bill to referendum. Republicans moved the vote forward from the November 2018 election to the 7 August 2018 primary. RTW was previously defeated in Missouri in 1978.

[6] New Hampshire introduced a RTW law in 1947 but it was shortly repealed in 1949. In 2017 a proposed RTW bill was defeated in the NH House of Representatives. And again in 2021.

[7] Following a sequence of local RTW laws in New Mexico, a state-wide ban on right-to-work laws at the local county level was introduced under House Bill 85. This invalidated 10 local resolutions that had been passed in the previous 14 months. https://nmpoliticalreport.com/2019/03/29/lujan-grisham-signs-bill-invalidating-counties-right-to-work-laws/

Table 2: Difference-in-differences Estimates of the Effect of RTW
on Unionization Rates and Wages

	All workers	Men	Women	High union industries	Other industries	All workers (triple-diff)
	(1)	(2)	(3)	(4)	(5)	(6)
A. First-stage: Effect of RTW on unionization rates						
	-0.0185	-0.0147	-0.0226	-0.0467	-0.0099	-0.034
	(0.0048)	(0.0047)	(0.0061)	(0.0127)	(0.0033)	(0.0111)
B. Reduced Form: Effect of RTW on log wages						
	-0.0123	-0.007	-0.0184	-0.0253	-0.0084	-0.0172
	(0.0035)	(0.0040)	(0.0043)	(0.0058)	(0.00386)	(0.0069)
C. IV estimates of the effect of unions on wages (RTW as IV for union status)						
	0.664	0.473	0.812	0.541	0.850	0.505
	(0.227)	(0.281)	(0.253)	(0.180)	(0.441)	(0.254)
D. OLS estimates of the effect of unions on wages						
	0.162	0.185	0.128	0.171	0.144	
	(0.0031)	(0.0036)	(0.0035)	(0.0041)	(0.0041)	
Obs.	772283	384392	387891	175095	597188	772283

Notes: Models estimated using the 2007-2019 CPS data. The five treatment states where RTW was adopted are Wisconsin (2011-2015), Indiana (2012), Michigan (2013), West Virginia (2016) and Kentucky (2017). Control states are those that never adopted RTW laws. In addition to RTW, the models control for education, experience, gender, marital status, race, MSA status, part-time status, occupation, public sector affiliation, state dummies, and a full set of industry-year dummies. The high unionization industries are construction, education, and public administration.

The triple-difference specification includes an interaction between RTW and high-union industries. The estimated coefficient on that variable is shown in panels A and B, while the variable is used as IV (with RTW as a control) in panel C.

Standard errors are clustered at the state-year level.

Table 3: Estimates of the Effect of RTW on Wages Based on a Differential Exposure Design

	2003-2019			1993-2002			1983-1992		
	All	Men	Women	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. IV estimates of the effect of unions on wages (RTW x industry as IV)									
	0.354	0.420	0.291	0.421	0.503	0.359	0.306	0.399	0.248
	(0.046)	(0.051)	(0.047)	(0.048)	(0.057)	(0.049)	(0.059)	(0.076)	(0.056)
B. OLS estimates of the effect of unions on wages									
	0.161	0.175	0.134	0.175	0.177	0.160	0.168	0.168	0.152
	(0.007)	(0.009)	(0.007)	(0.007)	(0.009)	(0.007)	(0.007)	(0.009)	(0.006)
C. First-stage: Effect of RTW on unionization rates relative to low-union industries									
High-union	-0.215	-0.189	-0.242	-0.204	-0.186	-0.220	-0.169	-0.163	-0.171
	(0.014)	(0.013)	(0.017)	(0.016)	(0.014)	(0.019)	(0.016)	(0.014)	(0.020)
Mid-union	-0.058	-0.051	-0.064	-0.070	-0.073	-0.067	-0.074	-0.067	-0.080
	(0.009)	(0.007)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
D. Reduced Form: Effect of RTW on log wages relative to low-union industries									
High-union	-0.085	-0.093	-0.073	-0.094	-0.103	-0.083	-0.056	-0.068	-0.043
	(0.011)	(0.012)	(0.012)	(0.011)	(0.012)	(0.013)	(0.010)	(0.011)	(0.012)
Mid-union	-0.011	-0.002	-0.020	-0.020	-0.010	-0.029	-0.013	0.001	-0.027
	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)
Obs.	1,737,180	874,050	863,130	940,321	470,350	469,971	1,450,509	746,715	703,793

Notes: Estimated using the 1983-2019 MORG CPS data. All models control for education, experience, gender, marital status, race, MSA status, part-time status, occupation, public sectoraffiliation, state dummies, 3-digit industry dummies, a full set of industry-year dummies, and the 10 broad industry dummies fully interacted with year dummies.

The set of instrumental variables consist of RTW status interacted with the 10 broad industry dummies. In the case of the first-stage and reduced form models, we reduce the dimensionality of the results by reporting the weighted difference between the high-union (construction, education, public administration) and mid-union (manufacturing, transportation&utilities, and health) industries relative to low-union industries. Standard errors are clustered at the state-industry level.

Appendix Table 1: Difference-in-differences Estimates of the Effect of RTW on Unionization and Wages with Standard Errors Clusters at the State Level

	All workers	Men	Women	High union industries	Other industries	All workers (triple-diff)
	(1)	(2)	(3)	(4)	(5)	(6)
A. First-stage: Effect of RTW on unionization rates						
	-0.0185	-0.0147	-0.0226	-0.0467	-0.00992	-0.0340
	(0.0131)	(0.00925)	(0.0177)	(0.0352)	(0.00619)	(0.0268)
B. Reduced Form: Effect of RTW on log wages						
	-0.0123***	-0.00695**	-0.0184***	-0.0253***	-0.00843	-0.0172
	(0.00372)	(0.00327)	(0.00538)	(0.00794)	(0.00498)	(0.0119)
C. IV estimates of the effect of unions on wages (RTW as IV for union status)						
	0.664*	0.473	0.812	0.541	0.850	0.505
	(0.402)	(0.329)	(0.546)	(0.452)	(0.517)	(0.606)
D. OLS estimates of the effect of unions on wages						
	0.162***	0.185***	0.128***	0.171***	0.144***	
	(0.00819)	(0.00941)	(0.00782)	(0.0116)	(0.0114)	
Obs.	772283	384392	387891	175095	597188	772283

Notes: Models estimated using the 2007-2019 CPS data. The five treatment states where RTW was adopted are Wisconsin (2011-2015), Indiana (2012), Michigan (2013), West Virginia (2016) and Kentucky (2017). Control states are those that never adopted RTW laws. In addition to RTW, the models control for education, experience, gender, marital status, race, MSA status, part-time status, occupation, public sector affiliation, state dummies, and a full set of industry-year dummies. The high unionization industries are construction, education, and public administration.

The triple-difference specification includes an interaction between RTW and high-union industries. The estimated coefficient on that variable is shown in panels union industries. The estimated coefficient on that variable is shown in panels A and B, while the variable is used as IV (with RTW as a control) in panel C.

Standard errors are clustered at the state level. The stars indicate statistical significance at the 90% (*), 95% (**), and 99% (***) levels.

Appendix Table 2: Difference-in-differences Estimates of the Effect of RTW on and Wages with the Wisconsin Public Sector Excluded

	All workers	Men	Women	High union industries	Other industries	All workers (triple-diff)
	(1)	(2)	(3)	(4)	(5)	(6)
A. First-stage: Effect of RTW on unionization rates						
	-0.00680**	-0.00659	-0.00767*	-0.0104	-0.00663**	-0.00353
	(0.00316)	(0.00405)	(0.00396)	(0.00877)	(0.00300)	(0.00859)
B. Reduced Form: Effect of RTW on log wages						
	-0.0109***	-0.00634	-0.0165***	-0.0247***	-0.00816**	-0.0178**
	(0.00356)	(0.00406)	(0.00442)	(0.00589)	(0.00388)	(0.00704)
C. IV estimates of the effect of unions on wages (RTW as IV for union status)						
	1.605*	0.961	2.155*	2.381	1.231	5.046
	(0.843)	(0.768)	(1.149)	(1.853)	(0.752)	(11.58)
D. OLS estimates of the effect of unions on wages						
	0.163***	0.186***	0.128***	0.173***	0.144***	
	(0.00314)	(0.00360)	(0.00355)	(0.00421)	(0.00414)	
Obs.	768031	382680	385351	171635	596396	768031

Notes: Models estimated using the 2007-2019 CPS data. The five treatment states where RTW was adopted are Wisconsin (2011-2015), Indiana (2012), Michigan (2013), West Virginia (2016) and Kentucky (2017). Control states are those that never adopted RTW laws. In addition to RTW, the models control for education, experience, gender, marital status, race, MSA status, part-time status, occupation, public sector affiliation, state dummies, and a full set of industry-year dummies. The high unionization industries are construction, education, and public administration.

The triple-difference specification includes an interaction between RTW and high-union industries. The estimated coefficient on that variable is shown in panels union industries. The estimated coefficient on that variable is shown in panels A and B, while the variable is used as IV (with RTW as a control) in panel C.

Standard errors are clustered at the state level. The stars indicate statistical significance at the 90% (*), 95% (**), and 99% (***) levels.

Appendix Table 3: Difference-in-differences Estimates of the Effect of RTW on and Wages with only Rust-Belt States as Controls

	All workers	Men	Women	High union industries	Other industries	All workers (triple-diff)
	(1)	(2)	(3)	(4)	(5)	(6)
A. First-stage: Effect of RTW on unionization rates						
	-0.0154***	-0.0124***	-0.0190***	-0.0404***	-0.00791**	-0.0298***
	(0.00437)	(0.00460)	(0.00575)	(0.0119)	(0.00325)	(0.0110)
B. Reduced Form: Effect of RTW on log wages						
	-0.00883**	-0.00245	-0.0159***	-0.0247***	-0.00438	-0.0209***
	(0.00380)	(0.00436)	(0.00456)	(0.00627)	(0.00420)	(0.00732)
C. IV estimates of the effect of unions on wages (RTW as IV for union status)						
	0.574**	0.197	0.838***	0.612***	0.553	0.703**
	(0.280)	(0.342)	(0.322)	(0.229)	(0.539)	(0.332)
D. OLS estimates of the effect of unions on wages						
	0.171***	0.191***	0.137***	0.190***	0.146***	
	(0.00303)	(0.00395)	(0.00380)	(0.00464)	(0.00367)	
Obs.	280389	139644	140745	56930	223459	280389

Notes: Models estimated using the 2007-2019 CPS data. The five treatment states where RTW was adopted are Wisconsin (2011-2015), Indiana (2012), Michigan (2013), West Virginia (2016) and Kentucky (2017). Control states are those that never adopted RTW laws. In addition to RTW, the models control for education, experience, gender, marital status, race, MSA status, part-time status, occupation, public sector affiliation, state dummies, and a full set of industry-year dummies. The high unionization industries are construction, education, and public administration.

The triple-difference specification includes an interaction between RTW and high-union industries. The estimated coefficient on that variable is shown in panels union industries. The estimated coefficient on that variable is shown in panels A and B, while the variable is used as IV (with RTW as a control) in panel C.

Standard errors are clustered at the state level. The stars indicate statistical significance at the 90% (*), 95% (**), and 99% (***) levels.