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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 in five U.S. states since 2011. The second approach relies on a differential exposure design that exploits the differential impact of RTW laws on industries with high unionization rates relative to industries with low unionization rates. Both approaches indicate that RTW laws lower wages and unionization rates. Under the assumption that RTW laws only affect wages by lowering the unionization rate, RTW can be used as an instrumental variable (IV) to estimate the causal effect of unions on wages. In our preferred specification based on the differential exposure design, the IV estimate of the effect of unions on wages is 0.35, which substantially exceeds the corresponding OLS estimate of 0.16. This large wage effect suggests that RTW may also directly affect wages due to a reduced union threat effect.

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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 wages. 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 conventional estimates of the union wage gap 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 the effect of unionization and union power, more generally, on wages. 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 workers who pay their dues. This exemption generates a "free-rider" problem that harms union finances and, consequently, the ability of unions to organize new firms

[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.

and provide services to their members. Lower rates of unionization in RTW states suggest that these laws reduce the power of unions and their ability to organize workplaces.

One challenge with studying the impact of RTW laws is that most RTW states adopted these laws in the late 1940s, making it hard to distinguish the effect of RTW from other underlying state differences when 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 low unionization rates 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, which 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. Under this assumption, 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 impact of RTW on wages and unionization rates, RTW is not necessarily a valid instrument for unionization in a conventional sense. 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; Taschereau-Dumouchel, 2020). Recent work by Fortin, Lemieux, and Lloyd (2021) and Farber et al. (2021) suggest that spillover effects may be just as large as the conventional effect of unions on wages (the union wage gap). We show that, in this setting, IV estimates of the effect of unions on wages can be decomposed into a local average treatment effect (LATE) and an additional term reflecting the direct effect of RTW on wages linked to spillover effects. These interpretation issues aside, the two identification strategies provide valid estimates of 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 wages through their impact on unionization. Using historical data, Farber et al. (2021) look at how the Wagner Act and National War Labor Board raised wages by increasing unionization. More recently, labor reforms have typically reduced the power of unions. For instance, Biasi and Sarsons (2022) study the consequences of Act 10 in Wisconsin, which removed the ability of unions to bargain over wages. They find that reduced union power contributed to increasing 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 on wages.[5] Farber et al. (2021) argue that the introduction of the Wagner Act and National War Labor Board can be used as instrumental variables for unionization. Accordingly,

[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 when re-examining 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) provides evidence of manipulation in the RD design and non-random selection in the composition of workers who stay with the firm after unionization.

we consider RTW laws as a potential instrumental variable and discuss under what conditions the approach is valid.

The remainder of the paper proceeds as follows. Section 2 provides background on RTW laws and presents descriptive statistics from the Current Population Survey (CPS) data used for the empirical analysis. Section 3 proposes an econometric framework to analyze the effect of RTW laws on wages and unionization rates and discusses 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 union membership. In most states, all workers covered by a collective agreement --including those who are not union members-- have to pay union dues typically withheld from paychecks by employers.

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 the same 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 microdata 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, 2005a).

The twenty-first century has seen a renewed push for RTW laws at the local, state, and federal levels. 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 are 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 crucial 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 took place in the Midwest 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 expansion 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 election. 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 Democratic governments. For example, New Mexico's ban on local RTW laws was introduced shortly after a 2019 Democratic trifecta in the state.

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[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.

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. Our main specifications 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 files of the Current Population Survey (MORG CPS) from 2003 to 2019. We also present complementary evidence using earlier CPS data starting in 1983.[9] More recent data for 2020-21 are excluded due to the impact of COVID-19 on the labor market. We use union coverage, instead of union membership, as our measure of unionization throughout. We only focus on observations with

[8] The winning candidate was a Democrat but switched to the Republican party shortly after his victory.
[9] 1983 is the first year in which information on union status is available in the MORG CPS. Questions on the union status of workers were also asked in smaller May CPS supplements between 1973 and 1982. We do not use these data here, given our focus on the more recent period when several states introduced RTW laws.

unallocated wages to avoid the significant attenuation bias linked to the fact that the union status is not used to impute wages in the CPS (Hirsch and Schumacher, 2003).[10]

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, the 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 more considerable impact on high-unionization than low-unionization industries. We study these industry patterns using ten broad 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; Moore, 1998), unionization rates are substantially lower in RTW states than in non-RTW states. The gap is very substantial 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. In all other sectors, the unionization rate is substantially less than 10 percent in both RTW and non-RTW states.

[10] Union coverage is also imputed for individuals who do not answer the union coverage question in the CPS. Allocation flags for non response are available from 1989 on. In our main analysis sample limited to the years 1989 to 2019, the union coverage variable is imputed for 6.1 percent of individuals. This is a substantially lower fraction than in the case of wages where 32.3 percent of observations are imputed between 1989 and 2019. Furthermore, wages are also imputed in most cases (5.6 out of 6.1 percent of observations) where union coverage is imputed. Since we already remove all observations with imputed wages from the analysis sample, removing the additional 0.5 percent of observations with imputed union coverage has no meaningful impact on the findings.

Overall, unionization rates in RTW and non-RTW states are roughly proportional. Running a regression of the unionization rate in non-RTW states on the unionization rate in RTW states, at the industry level, yields a slope coefficient very close to 2 with a statistically insignificant intercept. This indicates that, on average, unionization rates a roughly twice as large in non-RTW than in RTW states. Some notable differences suggest that RTW may have a relatively larger effect in some sectors than others. For instance, the construction and transportation & utilities sectors 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 & utilities where the workforce is more stable. Unions often represent workers at the national level (e.g., in the airline industry), making it less likely for state-level RTW laws 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 show the evolution of unionization rates into three groups of states: 1) states that never adopted RTW laws ("never RTW"), 2) states that adopted RTW since 2000 ("RTW adopters"), and 3) states that already had RTW laws in the year 2000 ("always RTW"). We further group the ten industries into three broad industry groups. 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) that are close to average in terms of unionization rates and RTW impacts. Finally, the "low-unionization" group consists of the remaining industries where the unionization rate tends to be low 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 quite stable relative to the well-documented decline in private sector unionization rate (Freeman, 1988; Farber, 2005b). 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 Midwestern states that started around the same time (see Figure 1). Thus, Figure 3a provides some early indication that, as expected from the patterns documented in Figure 2, the introduction of RTW laws has substantially reduced unionization in high-unionization industries.

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. 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. Interpreting the effect of RTW on wages and unionization rates

This section aims to clarify how RTW affects wages and unionization rates, and provide conditions under which RTW is a valid instrument for unionization.

3.1 Fixed coefficient linear model

To fix ideas, consider a simple linear model for Y_i, the wage of worker *i*:

$$Y_i = \gamma \cdot U_i + \lambda \cdot R_i + \varepsilon_{iy}, \tag{1}$$

where U_i indicates the union status of worker *i*, R_i is an indicator variable for RTW status, and ε_{iy} is the error term. Although we refer to wages for the sake of simplicity, log wages are used in the empirical analysis. The parameter γ is a conventional union wage gap that represents the impact of switching the union status of worker i, holding everything else constant. λ represents the direct effect of RTW regardless of union status. In this simple model the effect of unions and RTW is homogenous across workers. This strong assumption is relaxed in Section 3.2 below.

As mentioned earlier, union threat effects are a leading reason why RTW may have a direct effect on wages. Threat effects appear when non-union employers 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.

In additional to their direct effect, RTW laws may indirectly affect wages by increasing the cost of unionization and reducing the unionization rate. The effect of RTW on unionization can be captured by the parameter ϕ in a linear model for the union status of workers:

$$U_i = \phi \cdot R_i + \varepsilon_{iu}. \tag{2}$$

The reduced form wage equation is obtained by substituting equation (2) into equation (1):

$$Y_i = (\gamma\phi + \lambda) \cdot R_i + \varepsilon'_{iy} = \pi \cdot R_i + \varepsilon'_{iy}, \tag{3}$$

where $\pi = \gamma\phi + \lambda$ is the reduced form effect of RTW on wages. The error term is given by $\varepsilon'_{iy} = \varepsilon_{iy} + \gamma\varepsilon_{iu}$. The indirect effect of RTW on wages, $\gamma\phi$, is the product of the effect of RTW on the unionization rate, ϕ, and the union wage gap, γ. In this simple setting, the reduced form effect of RTW on wages can be given a causal interpretation under the assumption that the error term is uncorrelated with R_i. We maintain this assumption for simplicity here and discuss in Section 4 two research designs where causal estimation relies on weaker assumptions.

In this general model, the IV estimate β^{IV} of the effect of unions on wages, where RTW is used as the instrument, can be written as the ratio of the reduced form equation (3) over the "first-stage" equation (2):

$$\beta^{IV} = \frac{\pi}{\phi} = \gamma + \frac{\lambda}{\phi}\ . \tag{4}$$

There are two possible interpretation of the term λ/ϕ in equation (4). When seeking to estimate the union wage gap, γ , λ/ϕ is a bias term linked to the failure of the exclusion restriction ($\lambda \neq 0$). Under some conditions, however, we could instead interpret β^{IV} as a "full" effect of unions on wages that incorporates both the traditional union wage gap and the additional effect of unionization coming from the threat effect.

For example, one popular approach is to proxy the threat effect, or other general equilibrium effects of unions, using the average unionization rate $\overline{U}$ in a relevant labor market (e.g. by state and industry). Since RTW laws affect the whole local labor market, aggregating up equation (4) implies that the effect of RTW on $\overline{U}$ is also equal to ϕ. In that setting, we can think of the underlying wage setting model as

$$Y_i = \gamma \cdot U_i + \gamma_T \cdot \overline{U}_i + \varepsilon_{iy}. \tag{5}$$

The union wage gap γ still represents the impact of switching the union status of worker *i* holding constant the average unionization rate, while γ_T captures the additional impact of market-level unionization linked to the threat effect. Since RTW increases the average unionization rate by ϕ, we can interpret the direct effect of RTW on wages, λ, as the product of γ_T and ϕ. Substituting this expression in equation (4) now yields:

$$\beta^{IV} = \gamma + \frac{\gamma_T \cdot \phi}{\phi} = \gamma + \gamma_T\ . \tag{6}$$

So instead of representing a biased estimate of the union wage gap, β^{IV} can now be interpreted as the "full" effect of unions that captures both the traditional union wage gap, γ, and the threat effect, γ_T. Although this result only holds when the threat effect is captured by the average unionization rate, equation (6) provides a useful alternative interpretation of the IV estimates reported below.

3.2 Potential outcomes framework

The simple linear setup with homogenous effects helps illustrate the direct and indirect effect of RTW on wages. Homogeneity is a strong assumption, however. While little is known about possible heterogeneity in the direct effect of RTW on wages, a vast literature has shown that the union wage gap is highly heterogeneous across workers. For example, Card (1996) and Lemieux (1998) show that the union wage gap declines as a function of observed and unobserved skills. There are also well-documented differences in union wage effects by industry (e.g., Macpherson and Hirsch, 2021). These differences are particularly problematic since Figure 2 suggests that RTW laws have a larger impact in high-unionization industries. As such, IV estimates that rely on RTW as an instrument may not capture the union wage gap for the broader population even when the exclusion restriction $\lambda = 0$ is satisfied.

We clarify the interpretation of the estimates using a potential outcomes framework. First, consider the simple case where the exclusion restriction is satisfied, and there is no direct effect of RTW on wages. In that setting, $\mathrm{Y}_i(1)$ and $\mathrm{Y}_i(0)$ represent potential wages when worker *i* is unionized and not unionized, respectively. Observed wages are given by:

$$\mathrm{Y}_i = \mathrm{Y}_i(0) + \mathrm{U}_i * [\mathrm{Y}_i(1) - \mathrm{Y}_i(0)], \tag{7}$$

where the difference $\gamma_i = \mathrm{Y}_i(1) - \mathrm{Y}_i(0)$ is the union wage gap –the treatment effect—for worker i.

Unlike equation (1) where the treatment effect of unions on wages is fixed at γ for all workers, the potential outcomes framework does not impose any restriction on how the treatment effect γ_i varies in the population. Likewise, we want to let RTW affect the probability of unionization in an unrestricted way instead of imposing a constant effect as in equation (2). In addition to differences in the impact of RTW on unionization by industry (Figure 2), RTW impacts may also be different based on observed and unobserved individual characteristics of workers. Let $\mathrm{U}_i(1)$ and $\mathrm{U}_i(0)$ indicate the union status of workers with and without RTW, respectively. The observed union status is given by:

$$\mathrm{U}_i \ = \ \mathrm{U}_i(0) + \ \mathrm{R}_i * [\mathrm{U}_i(1) - \mathrm{U}_i(0)] \qquad (8)$$

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

$$\beta^{IV} = \frac{E[\mathrm{Y}_i|\mathrm{R}_i = 1] - E[\mathrm{Y}_i|\mathrm{R}_i = 0]}{E[U_i|\mathrm{R}_i = 1] - E[\mathrm{U}_i|\mathrm{R}_i = 0]} = E[\gamma_i \mid \mathrm{U}_i(1) < \mathrm{U}_i(0)]. \qquad (9)$$

The important insight here is that we should not expect the IV estimate (the LATE) to be the same as $E[\gamma_i]$, the average treatment effect over the whole population. The IV estimate should instead be interpreted as the effect of union among workers in high-unionization industries in RTW states who are most likely to be induced out of unionization by RTW.

Next, we discuss how to interpret the IV estimate and the reduced form effect of RTW when direct effects are allowed. Unlike equation (1) where the effect of U_i and R_i is constant, consider a flexible specification $\mathrm{Y}_i(\mathrm{U}_i, \mathrm{R}_i)$ where Y_i depends on U_i and R_i in an unrestricted way. The potential outcomes $\mathrm{Y}_i(1)$ and $\mathrm{Y}_i(0)$ can now be written as

$$\mathrm{Y}_i(1) = \ \mathrm{Y}_i(1,0) + \ \mathrm{R}_i[\mathrm{Y}_i(1,1) - \mathrm{Y}_i(1,0)], \qquad (10)$$

and

$$\mathrm{Y}_i(0) = \ \mathrm{Y}_i(0,0) + \ \mathrm{R}_i[\mathrm{Y}_i(0,1) - \mathrm{Y}_i(0,0)]. \qquad (11)$$

Substituting equations (8), (10), and (11) into equation (7) yields the following expression for observed wages:

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

$$+ \mathrm{R}_i * [\mathrm{U}_i(1) - \mathrm{U}_i(0)] * [\mathrm{Y}_i(1,1) - \mathrm{Y}_i(0,1)]$$
$$+ \mathrm{R}_i * \left[\left(1 - \mathrm{U}_i(0)\right) * \left(\mathrm{Y}_i(0,1) - \mathrm{Y}_i(0,0)\right) + \mathrm{U}_i(0) * \left(\mathrm{Y}_i(1,1) - \mathrm{Y}_i(1,0)\right)\right] \quad (12)$$

The first term on the right-hand side of equation (12) represents wages in the absence of RTW laws. The term on the second line of the equation is the "complier" effect that captures the wage impact, $\mathrm{Y}_i(1,1) - \mathrm{Y}_i(0,1)$, for the group of workers induced out of unionization ($\mathrm{U}_i(1) - \mathrm{U}_i(0) = -1$) by RTW. The term on the third line of equation (12) captures the direct effect of RTW on wages due, for instance, to a reduced union threat effect. The first component of the last term indicates the wage impact of RTW among workers who would not be unionized in absence of RTW. Likewise, the second component represents the direct effect of RTW among workers who would be unionized in absence of RTW.

The reduced form equation for the effect of RTW on wages is obtained by taking the conditional expectation of equation (12) with respect to R_i:

$$\mathrm{E}[\mathrm{Y}_i \mid R_i] = \alpha + \phi E[\gamma_i \mid \mathrm{U}_i(1) < \mathrm{U}_i(0)] * R_i$$
$$+ \left[\left(1 - \bar{U}(0)\right) * E[\lambda_i^n | \mathrm{U}_i(0) = 0] + \bar{U}(0) * E[\lambda_i^u | \mathrm{U}_i(0) = 1]\right] * R_i, \quad (13)$$

where $\alpha = E(\mathrm{Y}_i(0,0) + \mathrm{U}_i(0) * [\mathrm{Y}_i(1,0) - \mathrm{Y}_i(0,0)])$, the constant in the model, represents average wages in absence of RTW; $\phi = E[\mathrm{U}_i(1) - \mathrm{U}_i(0)]$ is the fraction of workers induced out of unionization by RTW; $\lambda_i^n = \mathrm{Y}_i(0,1) - \mathrm{Y}_i(0,0)$ is the direct effect of RTW for non-union workers; $\lambda_i^u = \mathrm{Y}_i(1,1) - \mathrm{Y}_i(1,0)$ is the direct effect of RTW for union workers; and $\bar{U}(0)$ is the fraction of unionized workers in absence of RTW.

Comparing equation (13) to equation (3) illustrates how the potential outcomes framework helps interpret the direct and indirect effect of RTW on earnings. The constant effect of unions on wages in equation (3), γ, is replaced by the LATE in equation (13). Likewise, the direct effect of RTW in equation (3), λ, is now a weighted average of the direct effect among union, $E[\lambda_i^u | \mathrm{U}_i(0) = 1]$, and non-union workers, $E[\lambda_i^n | \mathrm{U}_i(0) = 0]$. The direct RTW effect among non-union workers can be thought as a traditional threat effect. As it becomes more challenging to unionize workers under RTW, non-union firms can now pay lower wages to their workers. The direct RTW effect among union workers may be a mix of threat and bargaining effects. As the option value (non-union wages) faced by union workers declines, so does the bargained union wage when the bargaining power is fixed. Furthermore, bargaining power itself may decline as unions take a financial hit under RTW. While it is hard to know how the magnitude of the direct effect compares for union and non-union workers, we expect the effect to be negative in both cases.

As in the case with constant direct and indirect effect of RTW (equation 4), the IV estimate β^{IV} does not provide a consistent estimate of the union wage gap (LATE in this case) due to the bias linked to the direct effect. How large may the bias be? Fortin, Lemieux, and Lloyd (2021) find that threat effects of unionization more or less double the effect of unionization on wages in a local labor market defined by state and industry. This finding suggests that the direct effect may be as large as the traditional union wage gap. Therefore, finding a large IV estimate when using RTW as an instrumental variable would likely indicate that RTW has a direct effect on wages linked to threat effects, or other general equilibrium effects of unionization on wages.

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, let E_s represent the date (in year and month) RTW was adopted in state *s*. Time relative to adoption is $K_{st} = t - E_s$. The following specification is used to conduct an event-study analysis at the individual level for log wages Y_{ist} (a similar event-study analysis is performed for the union status U_{ist}):

$$Y_{ist} = \alpha_s + \delta_t + \sum_{k=-5}^{k=5} \pi_k \, \mathbf{1}\{K_{st} = k\} + X_{it}\psi + \varepsilon_{ist}, \qquad (14)$$

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; π_k is the estimated effect of RTW *k* years after adopting RTW, with π_k for $k < 0$ capturing possible pre-trends; and X_{it} is a set of individual-level covariates. In addition to the ten 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 (indicators for 16 categories based on four education and four experience groups, plus years of experience times years of 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.

As is well known, the validity of event study designs relies on the parallel trend assumption. That is, wages and unionization rates in the RTW adopter states would have followed the same trend as the "never adopters" if not for the adoption of RTW laws. We probe the validity of the parallel trends assumption by using five years of data in the pre-period to estimate pre-trends in the outcome variables. We also estimate treatment effects for 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 assumed to be constant over the post-period ($\pi_k = \pi$ for $k \geq 0$), and constrained to zero in the pre-period ($\pi_k = 0$ when $k < 0$). The DD specification is given by:

$$Y_{ist} = \alpha_s + \delta_t + \pi \cdot R_{st} + X_{it}\psi + \varepsilon_{ist}, \tag{15}$$

where the treatment dummy R_{st} is 1 when $t \geq E_s$, and 0 otherwise. As in the case of the event study design, we also estimate the DD model for the union status of workers:

$$U_{ist} = \alpha_s^u + \delta_t^u + \phi \cdot R_{st} + X_{it}\psi^u + \varepsilon_{ist}^u, \tag{16}$$

Recall from Section 3 that under the assumption that RTW has no direct effect on wages ($\lambda = 0$), RTW can be used as instrument for union status, and the IV estimate of the union wage gap is the ratio of π over ϕ.

Relative to the simplified model presented in equations (2) and (3) where the RTW treatment has to be "as good as random", the DD design relies on the weaker parallel trend assumption. One potential complication with heterogeneous treatment effects discussed in Section 3 is that DD estimates may not be interpretable as local average treatment effects in a "staggered" DD design like ours. As pointed out in a series of recent papers (Borusyak, Jaravel, and Spiess, 2021; Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021), DD estimates may not yield a weighted average of individual-level treatment effects due to "forbidden" comparisons between states that adopt RTW at different points in times.

Event-study estimates of dynamic treatment effects are not as affected by this problem since they allow for treatment effect heterogeneity linked to the time elapsed since the adoption of RTW laws. In our setting, it is natural to expect the impact of RTW laws to only gradually reduce the unionization rate as it takes time to certify or decertify unions. Furthermore, as in the case of Wisconsin studied by Biasi and Sarsons (2022), RTW may only start to apply at the expiration of existing contracts. That said, Borusyak, Jaravel, and Spiess (2021) and Sun and Abraham (2021) show that event-study estimates may not be interpretable as average treatment effects in the presence of heterogeneous treatment effects across states.

These challenges in interpreting event-study and DD estimates are unlikely to be a major issue in our setting where most observations are in "never treated" states, which provides a large number of controls throughout the 2007-19 period. We also have several years of pre-treatment data for all states adopting RTW. Thanks to these features of the research design, none of the treated observations have negative weights that would invalidate the interpretation of the estimates as average treated effects.[11] We discuss these issues further in the case of the DD

[11] A challenge with interpreting event study or DD estimates in staggered designs with treatment effect heterogeneity is that these estimates are not always interpretable as a weighted average of state-year specific treatment effects. The critical issue is that double-differencing can lead to some negative weights. It is most likely to happen when most units are eventually treated, leading to early-adopting states having negative weights in late periods (Borusyak, Jaravel, and Spiess, 2021). Fortunately, the distribution of weights can be estimated empirically.

estimates reported below, and we conclude that using alternative estimators robust to treatment effect heterogeneity has little impact on our key findings.

4.1 Main results

Figure 4 presents the event-study estimates for men and women pooled together, the next figure will show separate results by gender. 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 RTW adoption. 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. When looking at all workers in column 1, 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 wage and unionization coefficients ratio yields an IV estimate of the effect of unionization on wages of 0.66. The estimate 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, which is similar to union wage effects typically reported in the literature.[12] Based on the discussion in Section 3, this finding suggests that RTW may also have a direct effect on wages, which violates the exclusion restriction.

We find that, in our design, all the weights are positive. Our estimates can, thus, be interpreted as a weighted average of state-year treatment effects, although the weights do not necessarily coincide with those used to compute the ATT among the treated (De Chaisemartin and d'Haultfoeuille, 2020).

[12] Lewis (1986) summarizes findings from many studies that yield a mean union wage gap estimate of about 15 percent. Likewise, Macpherson and Hirsch (2021) show that union wage gap estimates have remained between 10 to

Note, however, that the standard error on the IV estimate is relatively 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 results in 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 ES estimates 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 of RTW on the wages and unionization rates of women. For instance, Figure 5b indicates that the unionization rate among female workers gradually declines after the introduction of RTW. The impact is above 4 percentage points five years after the introduction of RTW. Likewise, there is a evident 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 sectors such as 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.[13] 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 impact 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 indicate 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

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20 percent over the last 50 years. Similar conclusions are reached in historical studies by Callaway and Collins (2018) and Farber et al. (2021). Recent evidence by Kulkarni and Hirsch (2021) based on displaced workers where the change in union status is arguable exogenous also suggests a union wage gap of about 15 percent.

[13] 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.

in the 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 next 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. The estimator is implemented by adding an interaction between RTW and a dummy variable H_{it} indicating that individual *i* is working in a high-unionization industry at time *t*. The wage equation becomes

$$Y_{ist} = \alpha_s + \delta_t + \pi_1 \cdot H_{it} \cdot R_{st} + \pi_2 \cdot R_{st} + X_{it}\psi + \varepsilon_{ist}. \quad (17)$$

A similar specification is used for the union status of workers. 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 can then be obtained as the ratio of the effect of $H_{it} \cdot R_{st}$ for wages and unionization, or by running a two-stage least-squares regression of wages on unionization using the interaction $H_{it} \cdot R_{st}$ as an instrumental variable for unionization while controlling for the main effect of RTW and all the other covariates.

Column 6 shows that 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).

The last two columns of Table 2 report estimates for public and private sector workers separately. As noted above, the larger effect for women may reflect differences in the jobs held by men and women. In addition to industry differences, women are more likely to work in the public sector, where unionization rates are much higher than in the private sector (Card, Lemieux, and Riddell, 2020). As expected, the impact of RTW on public sector unionization is much larger (0.062) than in the private sector (0.008). The effect of RTW on wages is also larger in the public sector, though the public-private difference is not as large as in the case of unionization. As a result, the IV estimate is smaller in the public (0.328) than in the private sector (1.169).

The large impact of RTW on public sector unionization is mainly driven by the introduction of RTW in Wisconsin in 2011. 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 essential 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.

4.2 Robustness checks

So far, we have been using all states that never adopted RTW as controls for the RTW adopters. A potential concern is that some of these control states, for instance, California, may not be suitable comparisons for the treated states. The latter 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.[14] The main estimates are slightly smaller, but 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.

As discussed above, DD and event study estimates may not yield interpretable average treatment effects in a staggered design with treatment effect heterogeneity. Fortunately, several new approaches are now available for estimating ATEs in that setting. In particular, Borusyak,

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

Jaravel, and Spiess (2021) propose a computationally simple imputation estimator where state and year effects (and the effect of the covariates) are first estimated for non-treated observations. Then, the regression estimates are used to impute outcomes without treatment. Treatment effects can then be computed by subtracting these imputed outcomes from observed outcomes under treatment and computing the average difference over all treated observations.

Intuitively, the imputation estimator should yield results similar to conventional DD/ES estimates when the control group is large and observed over the whole sample period. In these circumstances, the estimated year effects in the DD (equation 15) or ES (equation 14) models are mostly "driven" by the controls. They are similar to the year effects estimated in the first step of the imputation estimator. In the case of all workers, Table 3 shows that the estimates obtained using the imputation estimator are only slightly smaller than the "benchmark" DD estimates (same as those reported in column 1 of Table 2).

A second possible approach suggested by Sun and Abraham (2021) is to compute DD or ES estimates for each treatment "cohort" (a single state in our setting) and average out the estimates to obtain the ATE. Note that the private and public sectors in Wisconsin are treated as separate treatment cohorts since RTW was adopted at different times in these two cases. To improve precision, we estimate a pooled model where the treatment variable (RTW) is interacted with dummies for each treated state, but where the estimated effects of covariates (and state and year effects) are constrained to be the same across treatment cohorts.[15]

The state-specific estimates are reported in the middle part of Table 3. Although the estimates are pretty noisy, a couple of key messages emerge from that part of the table. First, in most cases, the estimated effect of RTW on wages and unionization rates is negative. The only exceptions are the late adopters, West Virginia and Kentucky. Although we use these RTW adopters in our main (pooled) analysis, the state-specific estimates have to be interpreted with caution since RTW was only adopted towards the end of the sample period in these two states (February 2016 for West Virginia, January 2017 for Kentucky). Based on the evidence reported in Figure 4, these short-term effects may not capture the full impact of RTW. Second, and as discussed earlier, the effect of RTW on unionization is much larger in the Wisconsin public sector relative to all the other cases.

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[15] The results obtained estimating each treatment cohort separately are similar but less precisely estimated.

The average of the state-specific estimates is reported in the last row of Table 3.[16] As in the case of the imputation estimator, the estimates are only slightly smaller than the benchmark DD estimates reported in Table 2. It suggests that the DD estimate can be interpreted as an average treatment effect of RTW even though we have a staggered design where treatment effect heterogeneity can result in interpretation problems. We also report the standard errors obtained when clustering at the state level in square brackets. Although standard errors are typically larger when clustering at the state level, this is not always the case. In particular, standard errors for the state averages (row 4) decrease when clustering at the state level. Although it is theoretically possible for standard errors to decline when clustering at the state level, the results most likely reflect the challenges of obtaining valid standard errors when only a small number of states are being treated.

Overall, while the DD and ES estimates suggest that RTW negatively impact wages and unionization, the results are inconclusive due to the (typically) larger standard errors when clustering at the state level and the sensitivity of the findings to the Wisconsin public sector. The latter raises questions about the external validity of the findings. We address these two issues by switching to our second research design that potentially improves precision and external validity by using all RTW states in the estimation.

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 in all states without RTW laws. In other words, if industries where RTW has the largest impact on unionization rates also pay relatively lower wages in RTW states, we would interpret this wage impact as a consequence of RTW.

[16] The average state effect is computed by weighting the six state-specific estimates using the (CPS sample weighted) number of treated observations.

To implement the approach, it is helpful to first introduce the following model for the union status of workers:[17]

$$U_{ist} = \alpha_s^u + \delta_t^u + \sum_{j=1}^{j=10} \mathbf{1}\{I_{it} = j\} \cdot (\theta_j^u + \phi_j R_s) + X_{it}\psi^u + \varepsilon_{ist}^u, \qquad (18)$$

where I_{it} is a categorical variable indicating industry affiliation; θ_j^u is a set of main industry effects that represent a baseline rate of unionization common to all states; ϕ_j captures the impact of RTW on the unionization rate in industry *j*, as documented (without controls) in Figure 2. Just like we need $\phi \neq 0$ for RTW to be a relevant instrument in Section 3, here we need the ϕ_j's to be systematically different in different industries. Again, the evidence reported in Figure 2 strongly suggests this is the case.

The covariates X_{it} include the job and individual characteristics defined in Section 4 and a full set of interactions between industry and year dummies. We also include three-digit industry fixed 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.[18]

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

Next, consider the following wage equation where, as discussed above, the industry wage effects $\tilde{\theta}_j$ are constrained to be the same in all states:

[17] Note that the research design would be valid even if the rate of unionization in RTW and non-RTW states were proportional to each other, as is roughly the case in Figure 2. The difference in the level of unionization rather than the ratio of unionization rates is what matters here. This point is easy to see in extreme cases where the industry unionization rate is zero in both RTW and non-RTW states. Intuitively, industries with these near-zero unionization rates can be used as controls for those where RTW has a large impact regardless of the precise relationship between unionization rates in the two sets of states.

[18] 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 leads 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.

$$Y_{ist} = \tilde{\alpha}_s + \tilde{\delta}_t + \gamma U_{ist} + \sum_{j=1}^{j=10} \mathbf{1}\{I_{it} = j\} \cdot \tilde{\theta}_j + X_{it}\tilde{\psi} + \tilde{\varepsilon}_{ist}. \tag{19}$$

This equation is a generalization of the simple wage equation first introduced in equation (1), except that we ignore the direct effect of RTW to simplify the exposition (more on this below). Substituting equation (18) into equation (19) yields the reduced form equation for wages:

$$Y_{ist} = \alpha_s + \delta_t + \sum_{j=1}^{j=10} \mathbf{1}\{I_{it} = j\} \cdot (\theta_j + \pi_j R_s) + X_{it}\psi + \varepsilon_{ist}, \tag{20}$$

where the main industry effect is now given by $\theta_j = \tilde{\theta}_j + \gamma\theta_j^u$, and the wage effect of RTW in industry j is:

$$\pi_j = \gamma\phi_j. \tag{21}$$

Under the assumption that RTW has no direct effects on wages, the union wage gap γ can be estimated using the interaction between industry dummies and RTW status as instruments for unionization, with the main effects included as controls in the wage equation. The exclusion restriction is that the interaction between industry dummies and RTW status appears in the union status equation (eq. 18) but not in the wage equation (eq. 19). Unlike the models considered in Section 4, we are now in a situation 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 γ.

If the model is correctly specified, the effect of the instruments in the reduced form (the π_j's) and first-stage equation (the ϕ_j's) should be proportional to each other, and their ratio should be equal to γ, as in equation (21). This stringent restriction means that RTW only affects industry wages through its impact on the unionization rate at the industry level and is testable using a conventional overidentification test. While the restriction is unlikely to hold when the effect of unions on wages is heterogeneous, examining the relationship between π_j and ϕ_j provides a useful way of visualizing the impact of RTW on wages and unionization rates.

As in Section 3, we could also introduce a direct effect of RTW on wages. A parsimonious way of doing so assumes that, as in equation (5), union threat effects are proxied by the average unionization rate by state and industry, $\bar{U}_{sjt}$. Using the approach introduced in Section 3, the industry-specific direct effect of RTW is then given by $\lambda_j = \gamma_T \phi_j$, which is proportional to the effect of RTW on the industry unionization rate, ϕ_j. As in equation (6), it follows that IV estimation yields an estimate of the "full" effect of unionization that includes threat effects, $\gamma + \gamma_T$, instead of a "pure" estimate of the union wage gap, γ.

5.1 Main Results

Figure 7 plots the effect of RTW on industry wages (π_j) against the effect on unionization rates (ϕ_j) 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, that is, 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. It is equal to 0.347 with a standard error of 0.088 in this simple specification. Consistent with the previous discussion, RTW appears to have a huge impact on the three "high-unionization" industries –construction, education, and public administration—in the upper right part of Figure 7.

The main results are reported in Table 4. Since the differential 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).[19] In each period, we report results for men and women separately, 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 π_j and ϕ_j for the high- and mid-

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

unionization industries relative to the low-unionization ones. Standard errors are clustered at the level of the treatment, that is, at the state by industry level in this differential exposure design.

Consistent with Figure 7, the IV results reported in Panel A of Table 4 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 wage 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 estimate (0.22 to 0.40) is plausible because it lies slightly above the OLS estimates of the effect of unions on wages.

5.2 Robustness Checks

We next conduct several robustness checks to probe the validity of the identification strategy. Since the results reported in Table 4 are relatively similar for the different time periods, we limit the robustness checks to the 2003-19 period and focus on the pooled results for men and women combined.

Relative to the ESD discussed earlier, a first shortcoming of the differential exposure design is that the analysis cannot be conducted within a given industry as it relies on differences across industries. However, it is still possible to compute separate estimates for public and private sector workers. The distinction is essential since the ES/DD estimates are pretty sensitive to the inclusion of public sector workers from Wisconsin.

The first column of Table 5 reproduces the benchmark estimates for all workers reported in Table 4. Next, Table 5 shows separate estimates for public and private sector workers in columns 2 and 3, respectively. Although the results by sector are less precisely estimated than for all workers pooled together, they remain statistically significant and are larger than in the pooled specification reported in column 1. These findings alleviate the earlier concerns that the ES/DD estimates were unduly affected by the Wisconsin public sector and that evidence of RTW impacts in the private sector was tentative at best.

The second set of robustness checks aims to probe the key identification assumption that inter-industry wage differences would be the same in RTW and non-RTW states in absence of RTW. Since most RTW states are located in the South, the assumption would be violated if inter-industry wage differentials were different in different U.S. regions for reasons having little to do with RTW.

We first allow for regional differences by interacting the (four) region indicators with the industry dummies. Column 4 shows that doing so has little impact on the results. However, the standard errors increase since these estimated models solely rely on the variation in RTW within each of the four regions of the United States.

We next remove "always-takers" states from the sample since they may be less comparable to RTW adopters than "never-takers" states. For instance, unionization rates in the RTW adopter states are much closer to those of the never taker than the always taker states. Instead of constraining inter-industry wage differentials to be the same in all states, the restriction is now imposed on a narrower set of states (RTW adopters and never-takers). The results reported in column 5 are similar to those for all states in column 1. Interestingly, the model now passes the overidentification test reported in the last row of the table. Recall from the discussion of Figure 7 that while the effect of RTW on wages and unionization line up reasonably well, the fit is far from perfect, with an R-square of 0.66. The overidentification test provides a formal way of testing whether the dispersion around the regression line in Figure 7 is due to estimation noise. The small p-value in column 1 suggests that it is not. By contrast, the hypothesis can no longer be rejected when focusing on the smaller set of states in column 5. This result supports the validity of the estimates obtained using the differential exposure design.

In column 6, we push the model further by allowing for different industry wage effects (in absence of RTW) in RTW adopter and never-taker states. This model is very similar to the triple-difference model reported in Table 3. There the main effect of being in a high-unionization industry is allowed to be different in RTW adopters and never-takers, and the interaction is used as IV. The only substantive difference between the two models is that we use interactions between all ten industries and RTW as IV in Table 5, instead of a simple interaction between high-union industries and RTW in Table 3. Using all the interactions is more efficient and provides testable overidentification restrictions. Column 6 shows that while the estimated union wage effect remains positive, it is no longer statistically significant.

We further narrow the estimation sample in columns 7 and 8 by only considering control (never taker) states from the Rust Belt states. The estimated union wage effect when industry wage effects are constrained to be the same in treated and untreated states (column 7) remains similar to those when using all states in the analysis (column 1). This model passes the overidentification test. Allowing for different industry effects in treated and untreated states (column 8) substantially increases the standard errors once again, though the estimated union wage effect remains borderline significant. Overall, the results reported in Table 5 strongly support the validity of the differential exposure design.

In a final robustness check, we allow industry wage premia to differ while remaining proportional in RTW and non-RTW states. Recall from equation (20) that θ_j represents the industry wage effect in non-RTW states, while π_j is the impact of RTW on industry wages. Thus, industry wage effects in RTW states are given by $\theta_j + \pi_j$. We generalize the model by introducing a factor loading parameter ρ to allow for systematic differences in industry wage premia in RTW and non–RTW states. With this additional parameter, the industry wage premia in RTW states become $\rho\theta_j + \pi_j$. This additional parameter allows for industry premia to be systematically wider (or narrower) in RTW states while restricting the premia without RTW to remain proportional in the two sets of states. It is impossible to leave industry wage premia completely unrestricted in the two sets of states, as the model would no longer be identified. Thus relaxing the restriction in a parametric fashion provides a feasible way of probing the robustness of the results to the assumption that industry wage premia would be similar in RTW and non-RTW states in absence of RTW.

The model is estimated using a two-step procedure. In the first step, we estimate the reduced form equations for wages (equation 20) and union status (equation 18) separately for RTW and non-RTW states.[20] Define the estimated industry effects in the reduced form equations

[20] In practice, we run a pooled regression with interactions between industries and RTW to compute regression adjusted wage and union industry effects.

for wages as π_j^R and π_j^N, and the industry effect for unionization rates as ϕ_j^R and ϕ_j^N. Since $\pi_j^N = \theta_j$, $\pi_j^R = \rho\theta_j + \pi_j$, and $\phi_j^R - \phi_j^N = \phi_j$, combining these expressions with equation (21) yields:

$$\pi_j^R = \rho\pi_j^N + \gamma \cdot \left(\phi_j^R - \phi_j^N\right). \tag{22}$$

Equation (22) shows that the effect of unions, γ, can be estimated by running an industry-level regression of the industry wage premia in RTW states on the industry wage premia in non-RTW states and the difference in unionization rates in the two set of states. Note that in the special case where $\rho = 1$, the model becomes a simple relationship between wage differences and unionization rate differences like the one plotted in Figure 7. The first column of Table 6 shows the estimates of this difference-on-difference model where the regression is weighed by the fraction of observations in each industry. As expected, the point estimate of 0.355 is similar to the slope of the fitted line in Figure 7 and to the IV estimate reported in the first column of Table 4.[21]

Column 2 of Table 6 shows the estimates of equation (21). The estimated factor loading parameter ρ is smaller than one. This suggests that industry wage premia are less dispersed in RTW than non-RTW states, although the difference is not statistically significant. More importantly, the estimated union wage effect only slightly declines from 0.355 to 0.330 when ρ is not constrained to equal one.

On balance, the evidence reported in Tables 5 and 6 suggests that the estimated union wage effect is robust to departures from the assumption that industry wage premia in RTW and non-RTW states would be similar in absence of RTW laws. Based on this evidence, our preferred estimate of the union wage effect is the one obtained using the differential exposure design for all workers in 2003-19: 0.355 in column 1 of Table 4.

6. Discussion and concluding comments

[21] The slope of the regression line depicted in Figure 7 is slightly different from the one reported here (0.346 vs. 0.355) because of minor differences in the control variables included in the regressions. The IV estimate in column 1 of Table 4 is identical to the one reported here. Using the Frisch-Waugh theorem, it can be shown that the two-step regression is simply a residualized version of the two-stage least squares estimates of equation (19).

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 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-examining 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. It can be used to estimate the long-term impact of RTW laws on wages and unionization rates. On the one hand, an important advantage of this cross-sectional approach is that it yields much more precise estimates than those obtained using the ESD/DD approach. On the other hand, a potential limitation of the approach is that it relies on the arguably stronger assumption that inter-industry wage differentials would be similar in different states if not for RTW, which disproportionally affects the high-unionization rate industries. However, several robustness checks suggest that the results remain similar under alternative assumptions about differences in industry wage premia in RTW and non-RTW states.

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 most considerable potential impact suggests that RTW is a parsimonious and credible explanation for the observed differences in the inter-industry wage structure. 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.

While the evidence from the ES/DD design is not as conclusive, the similarity of the results obtained using the two approaches is reassuring.

Having established that RTW affects unionization rates and wages, an important question is whether RTW can be used as an IV for union status to obtain a causal estimate of the union wage gap. As discussed in Section 3, IV estimates can be interpreted as the LATE of unions on wages, provided that RTW laws have no 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 0.355 compared to an OLS estimate of 0.159, using the estimates for all workers in 2003-2019 based on the exposure design (column 1 of Table 4).

One possible explanation –discussed in Section 3-- for the large effect size is that the union wage effect is particularly large for "compliers" who are induced out of unionization by the enactment of RTW laws. If compliers were disproportionately coming from the lower part of the wage distribution, the large union wage effects would be consistent with existing research showing that the union wage effect tends to be larger for low-skill workers than high-skill workers (e.g. Card 1996). This is unlikely to be the case, however, since RTW has a particularly negative impact on unionization in public administration and education, two sectors where union workers tend to have relatively high skill levels.[22]

The large effect size more likely 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,

[22] Card, Lemieux, and Riddell (2020) show that union workers in the public sector are disproportionally concentrated at the upper end of the distribution. Although they do not analyze these patterns by industrial sector, most public sector workers are concentrated in education and public administration.

unionization is an important channel through which RTW leads to lower wages. More generally, our findings are consistent with the view that public policies that make it harder for unions to organize and represent workers lead to lower bargaining power and wages.

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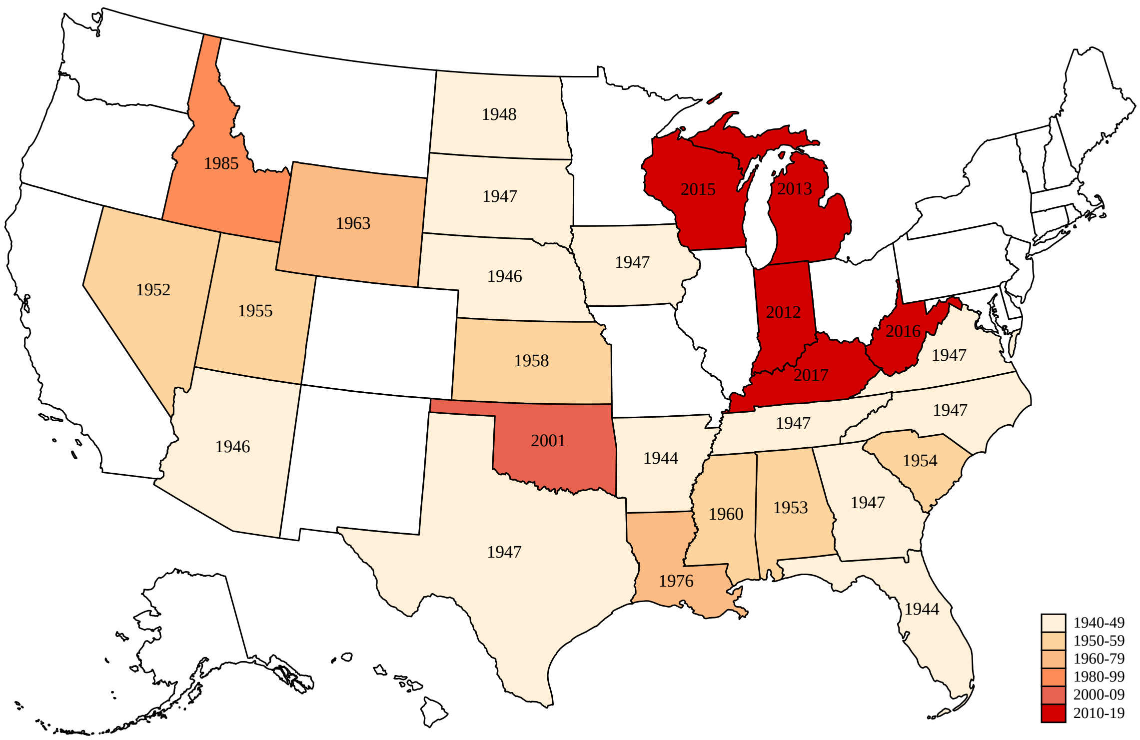


Figure 1: The Expansion of Right-to-Work Coverage

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.

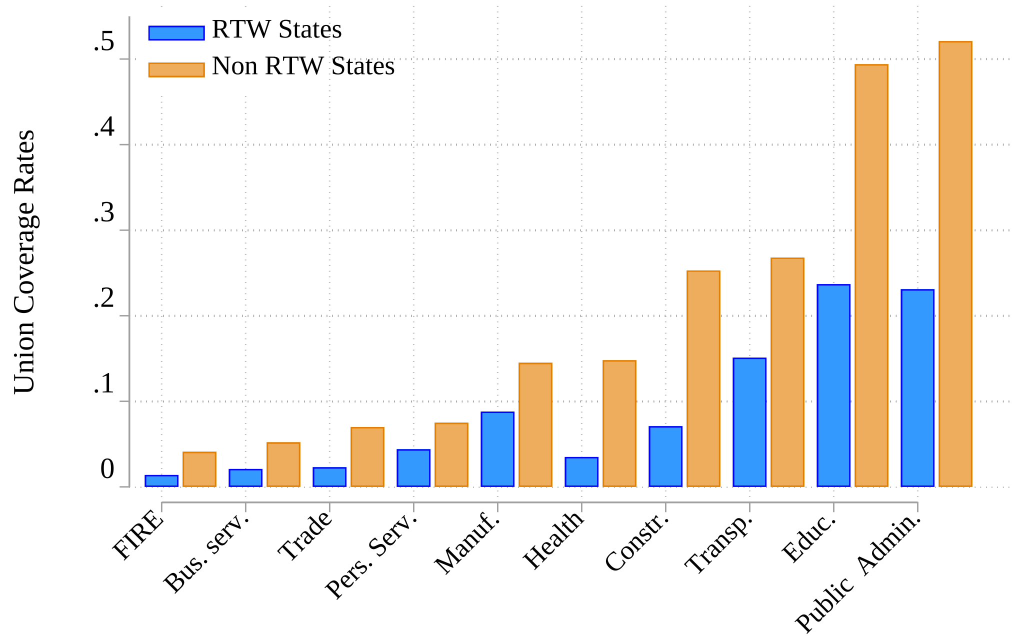


Figure 2: Differences in Union Coverage Rates across RTW and non-RTW States

Note: Compares Union Coverage Rates for RTW and non-RTW states over the 2000-2019 period.

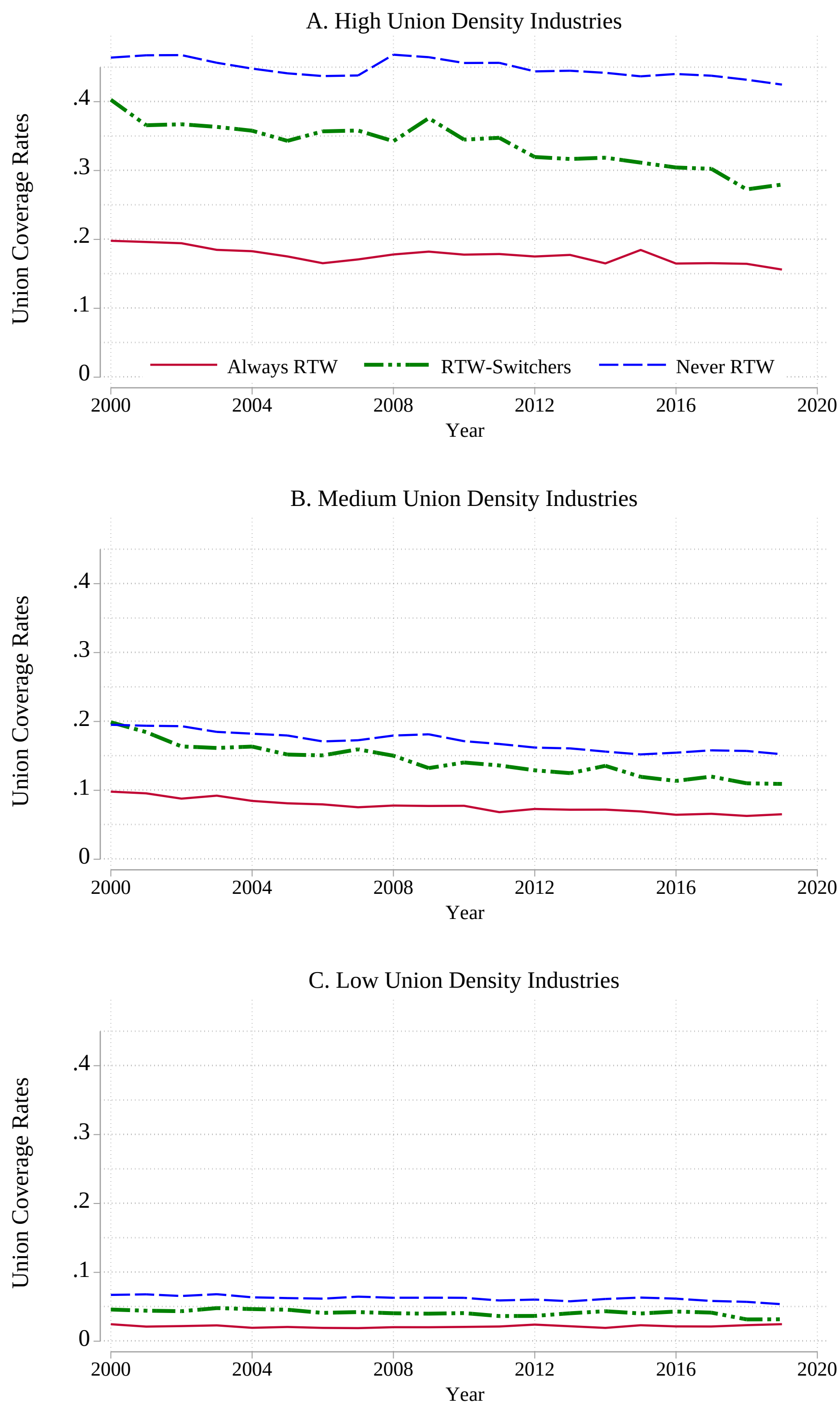
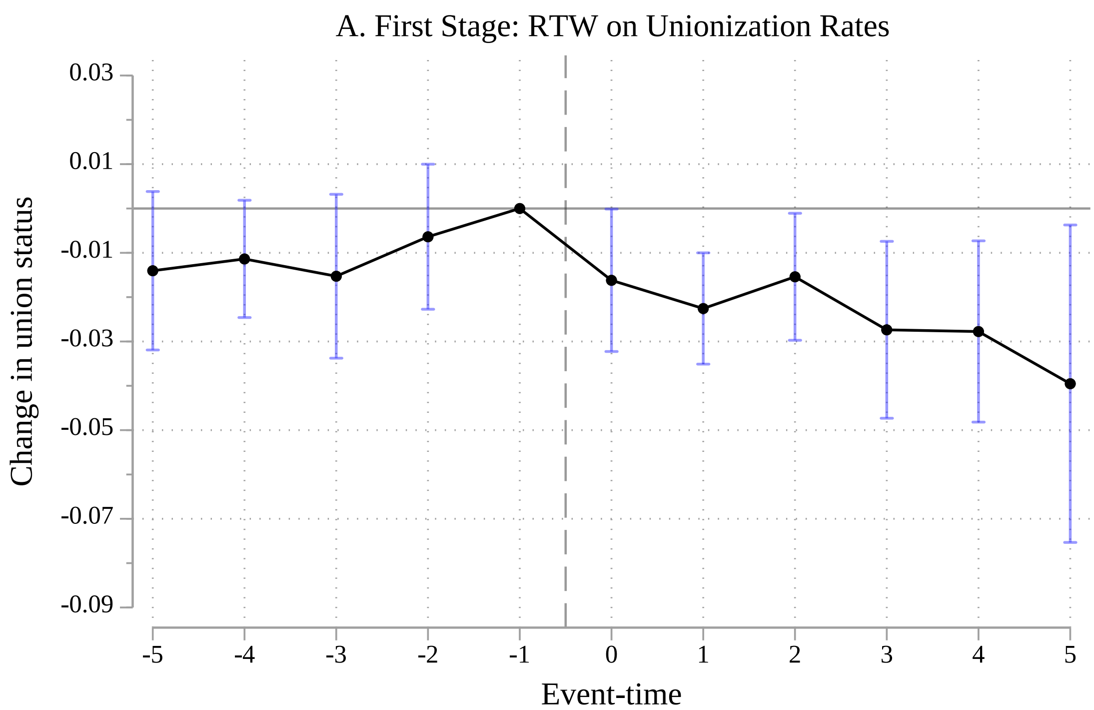


Figure 3: Trends in Union Coverage Rates across Types of Industries

Note: High union density industries include construction, education, and public administration; medium union density industries are manufacturing, health, transportation, and utilities, and low union density industries, personal and business services, trade, and FIRE..



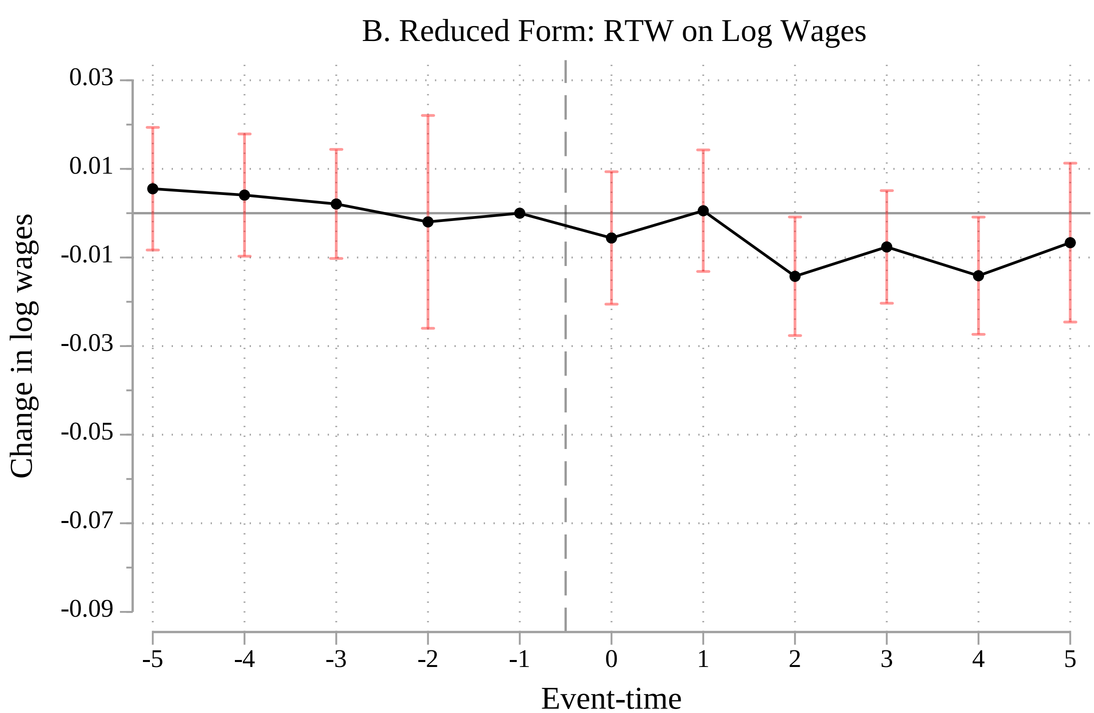


Figure 4: Event-study Estimates of RTW Laws

Note: Studies the implementation of RTW laws from 2011 to 2017. See text for details.

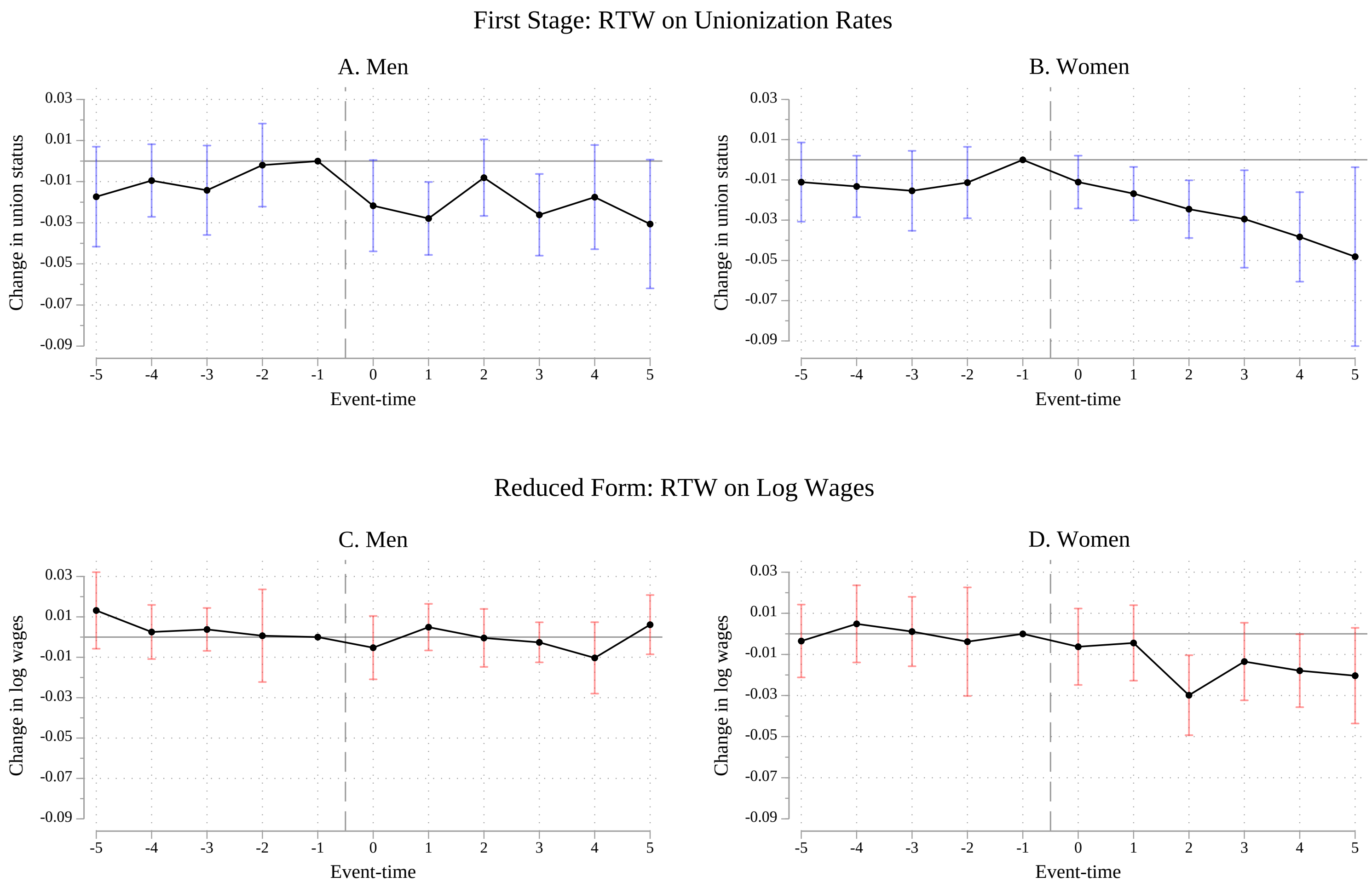


Figure 5: Event-study Estimates of RTW Laws by Gender

Note: Studies the implementation of RTW laws from 2011 to 2017. See text for details

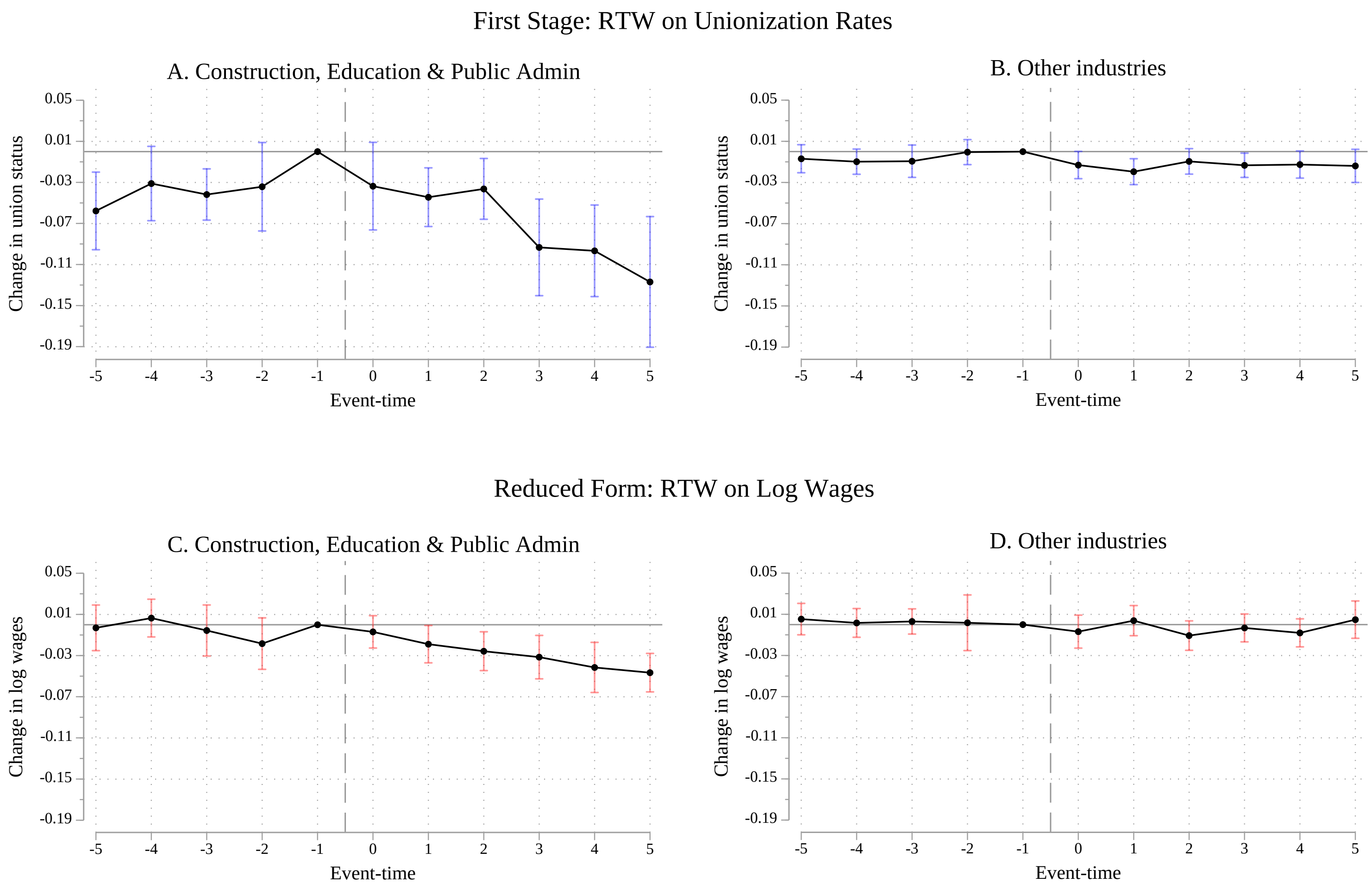


Figure 6: Event-study Estimates of RTW Laws by Types of Industries

Note: Studies the implementation of RTW laws from 2011 to 2017. See text for details

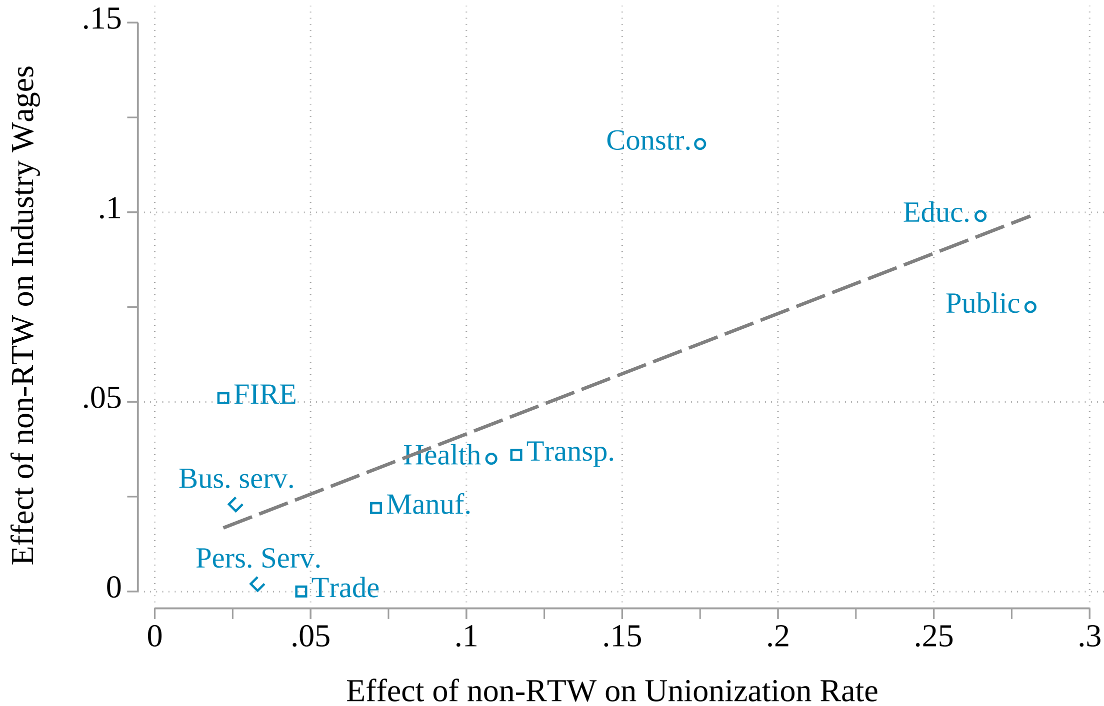


Figure 7: Relative Effect of RTW on Industry Wages and Unionization Rates

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)	Private	Public
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. First-stage: Effect of RTW on unionization rates								
	-0.0185	-0.0147	-0.0226	-0.0467	-0.0099	-0.0340	-0.0084	-0.0616
	(0.0048)	(0.0047)	(0.0061)	(0.0127)	(0.0033)	(0.0111)	(0.0029)	(0.0165)
B. Reduced Form: Effect of RTW on log wages								
	-0.0123	-0.0070	-0.0184	-0.0253	-0.0084	-0.0172	-0.0098	-0.0202
	(0.0035)	(0.0040)	(0.0043)	(0.0058)	(0.0039)	(0.0069)	(0.0038)	(0.0067)
C. IV estimates of the effect of unions on wages (RTW as IV for union status)								
	0.6638	0.4733	0.8124	0.5410	0.8500	0.5053	1.1693	0.3278
	(0.2268)	(0.2810)	(0.2532)	(0.1801)	(0.4412)	(0.2545)	(0.5433)	(0.1333)
D. OLS estimates of the effect of unions on wages								
	0.1622	0.1849	0.1279	0.1706	0.1437	0.1606	0.1754	0.1056
	(0.0031)	(0.0036)	(0.0035)	(0.0041)	(0.0041)	(0.0032)	(0.0041)	(0.0036)
Obs.	772283	384392	387891	175095	597188	772283	639189	133094

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

Outcome variable:	Unionization	Wages		Unionization	Wages
	(1)	(2)		(3)	(4)
1. Benchmark estimator	-0.0185	-0.0123	2. Imputation estimator	-0.0167	-0.0110
	(0.0048)	(0.0035)		(0.0047)	(0.0020)
	[0.0131]	[0.0037]		[0.0131]	[0.0027]
3. State-by-state estimates					
a) Indiana	-0.0051	-0.0066	b) Michigan	-0.0071	-0.0117
	(0.0047)	(0.0074)		(0.0047)	(0.0064)
c) Wisconsin private	-0.0208	-0.0143	d) Wisconsin public	-0.1923	-0.0379
	(0.0037)	(0.0036)		(0.0234)	(0.0091)
e) West Virginia	0.0073	0.0154	f) Kentucky	0.0124	-0.0184
	(0.0056)	(0.0115)		(0.0094)	(0.0058)
4. State average	-0.0170	-0.0118			
	(0.0029)	(0.0037)			
	[0.0011]	[0.0026]			

using the same variables and control states (never adopters) as in Table 2. Row 1 reproduces the benchmark estimates for all workers reported in Table 2. Row 2 presents the imputation estimator of Borusyak, Jaravel, and Spiess (2021). Row 3 reports state-specific treatment effects for each state in a pooled model where the treatment variable is interacted with treated states. The average treatment effect (weighted by the fraction of treated observations in each state) is reported in row 4. Standard errors clustered at the state-year level are in parentheses, while those clustered at the state level are in square brackets.

Table 4: 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)									
	0.355	0.420	0.293	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									
	0.159	0.174	0.132	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 sector affiliation, 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.

Table 5: Robustness Checks of the Differential Exposure Design Estimates for 2003-2019

Sample:	All	Private	Public	All	Never RTW and RTW adopters		Rust Belt	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. IV estimates of the effect of unions on wages (RTW x industry as IV)								
	0.355	0.604	0.558	0.442	0.343	0.613	0.375	0.657
	(0.046)	(0.093)	(0.140)	(0.075)	(0.091)	(0.413)	(0.128)	(0.331)
B. OLS estimates of the effect of unions on wages								
	0.159	0.169	0.112	0.155	0.156	0.155	0.157	0.157
	(0.007)	(0.010)	(0.005)	(0.007)	(0.009)	(0.009)	(0.011)	(0.011)
Separate industry effects by:								
Region (4)				Yes				
RTW adopters & never RTW						Yes		Yes
Overid test (p-value)	0.001	0.000	0.000	0.012	0.058	0.384	0.091	0.510
Observations:	1737180	1435188	301963	1737180	1059177	1059177	419937	419937

Notes: Estimated using the 2003-2019 MORG CPS data. All models control for education, experience, gender, marital status, race, MSA status, part-time status, occupation, public sector affiliation, 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. Column 1 reproduces the results for all workers from column 1 of Table 4. Columns 2 and 3 show separate results for the private and public sectors. Column 4 allows for separate industry effects in each of the four Census regions. Columns 5 and 6 limit the analysis to states adopting RTW and those never adopting RTW. Column 6 allows for separate industry effects for these two sets of states. Columns 7 and 8 further limit the analysis to Rust Belt states. Standard errors are clustered at the state-industry level.

Table 6: Industry-level Estimates of Union Wage Effects with
Differences in Industry Wage Premia in RTW and non-RTW states

Dependent variable:	Industry wage premia in RTW states	
	(1)	(2)
Difference in unionization rate	0.355 (0.083)	0.330 (0.087)
Industry wage premia in non-RTW states	1.000	0.935 (0.068)

Notes: The table reports industry-level regressions (10 observations) where regression-adjusted industry wage and unionization rates are computed separately for RTW and non-RTW states. The regression models used to adjust wages and unionization rate are similar to the specifications described in Table 4, but also include a full set of interactions between industry and RTW states.

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)	Private	Public
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. First-stage: Effect of RTW on unionization rates								
	-0.0185	-0.0147	-0.0226	-0.0467	-0.0099	-0.0340	-0.0084	-0.0616
	(0.0131)	(0.0092)	(0.0177)	(0.0352)	(0.0062)	(0.0268)	(0.0039)	(0.0401)
B. Reduced Form: Effect of RTW on log wages								
	-0.0123	-0.0070	-0.0184	-0.0253	-0.0084	-0.0172	-0.0098	-0.0202
	(0.0037)	(0.0033)	(0.0054)	(0.0079)	(0.0050)	(0.0119)	(0.0041)	(0.0105)
C. IV estimates of the effect of unions on wages (RTW as IV for union status)								
	0.6638	0.4733	0.8124	0.5410	0.8500	0.5053	1.1693	0.3278
	(0.4018)	(0.3291)	(0.5458)	(0.4524)	(0.5167)	(0.6058)	(0.5321)	(0.2863)
D. OLS estimates of the effect of unions on wages								
	0.1622	0.1849	0.1279	0.1706	0.1437	0.1606	0.1754	0.1056
	(0.0082)	(0.0094)	(0.0078)	(0.0116)	(0.0114)	(0.0084)	(0.0109)	(0.0086)
Obs.	772283	384392	387891	175095	597188	772283	639189	133094

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.

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						
	-0.0068	-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.

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	-0.019	-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.