The Effect of Right-to-Work Laws on Union Membership and Wages

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This paper represents my own work in accordance with University regulations.

Abstract

I examine the effect of recent right-to-work (RTW) laws on labor unions and wages. Using recent data on Indiana and Michigan, which passed RTW laws in 2012 and 2013, respectively, I investigate whether these RTW laws have had any conclusive effects over the past few years. Using a difference-in-differences model with industry fixed effects, state fixed effects, time fixed effects, and demographic controls, I find that RTW laws decrease the unionization rate by 0.7 percentage points and increase the free rider rate in Michigan by 2.0 percentage points. The estimated effects on wages are not statistically significant. Overall, the results are generally robust to slight variations in model specification, but it is difficult to distinguish significant effects of RTW laws from statistical noise.

1 Introduction

To date, twenty-six states have adopted right-to-work laws as authorized by the Taft-Hartley Act of 1947. Formally titled the Labor Management Relations Act of 1947, this legislation permits states to outlaw union security agreements by passing right-to-work laws, so that private-sector unions cannot require union membership or payment of dues as a condition of employment. While many states enacted right-to-work laws immediately following the passage of the Taft-Hartley Act, three states have only recently done so. These states are Indiana (March 2012), Michigan (March 2013), Wisconsin (March 2015), and West Virginia (July 2016).[1]

One might expect that such legislation would decrease the proportion of private-sector workers in labor unions by limiting the ability of unions to maintain membership and collect dues, even from those covered by the union's contract ("free riders"). One might also expect that by weakening the membership and bargaining power of labor unions, such legislation would result in decreased wages earned by unionized workers. This effect could even extend to non-unionized workers, since employers would face a smaller threat of unionization, reducing the need to provide higher wages closer to the union wage (Farber 2005).

[1] As of April 2016, Wisconsin's right-to-work law has been struck down by a state judge and the case is pending appeal. West Virginia passed its right-to-work law in February 2016, to take effect in July.

However, the negative effect of right-to-work laws on wages may not be so clear. By increasing employment, such laws may actually have the opposite effect. Companies may be attracted to states with right-to-work laws, since they would face weaker labor unions. The increased demand for labor that follows would then drive wages up. Nevertheless, I hypothesize that right-to-work laws do have a negative effect on both union membership and wages.

In this paper, I conduct an empirical investigation of the effect of recent right-to-work laws on both union membership and wages. I take care to consider the very real possibility of omitted-variable bias: both wages and the adoption of right-to-work laws could be correlated with underlying local economic trends. A simple OLS regression could thus overestimate the significance of right-to-work laws. By considering changes in certain variables during the years immediately surrounding enactment of right-to-work laws in Indiana and Michigan, using the difference-in-differences (DID) technique it is possible to estimate the effect of right-to-work laws on these variables and minimize omitted-variable bias. I consider the fraction of private-sector workers in labor unions, the fraction of union workers who are free riders, the wages of union members, and the wages of non-union members, as well as the wages of union members compared to the wages of free riders.

Section 2 summarizes the existing literature on the effects of right-to-work laws. Section 3 describes the data set being used and Section 4 describes the methodology being applied. Section 5 reports results, and Section 6 discusses and concludes.

2 Literature Review

The literature on the effects of right-to-work (RTW) laws is mixed. Moore (1980) finds that RTW laws have no statistically significant effect on union membership after controlling for simultaneous equations bias. Farber (1983) also finds no effect on union membership, attributing the lower extent of unionization in states with RTW laws to lower demand for union representation. Ellwood and Fine (1983) find a 5-10% reduction in union membership immediately after the passage of a RTW law, based on examination of flows of workers organizing into unions rather than the total stock of union membership.

The effects of right-to-work laws on wages are similarly uncertain. Farber (2005) takes RTW laws as an exogenous decrease in the threat of unionization in his analysis of threat effects, controlling for individual demographic measures as well as time-invariant industry and state fixed effects. Using the examples of Idaho and Oklahoma, which enacted RTW laws in 1985 and 2001, respectively, he finds that nonunion wages fell 4.2% after the passage of Idaho's RTW law, but that Idaho union wages, Oklahoma nonunion wages, and Oklahoma union wages all showed no statistically significant effect due to passage of RTW laws. Rinz (2015) focuses on the states that enacted RTW laws immediately after the passage of the Taft-Hartley Act in 1947, arguing that the timing of RTW legislation was exogenous in these cases. Using a difference-in-differences model, and controlling for state, year, and census division by year fixed effects, he finds that the average effect of RTW laws on wages across all sectors is small and slightly negative. Finally, Reed (2003), by controlling for initial economic conditions in these states at the time of passage of the RTW laws, finds that average wages are actually significantly higher in RTW states than in non-RTW states.

While the ambivalent state of the current literature is not particularly encouraging, examining the recent developments in Indiana and Michigan - which are relatively large states - would advance the literature by testing established methodology with new data and adding evidence either in favor of significant RTW effects or none at all. Given recent renewed public interest in right-to-work laws, this analysis would also be interesting to policymakers considering RTW legislation. In investigating whether the recent passage of RTW laws in these three states had significant impacts on union membership (both in terms of the fraction of workers unionized and the fraction of workers who are free riders) and wages (for both union and non-union workers), the results will intrigue both economists and politicians alike.

3 Data

My analysis uses data from the Current Population Survey, a monthly survey of households conducted by the United States Census Bureau for the Bureau of Labor Statistics. The CPS surveys households on employment, earnings, and other demographic characteristics. Households in the survey are interviewed once a month for four months, ignored for eight

months, and then interviewed again for four more months. However, only households in the fourth and eighth months of their rotations are asked questions on usual weekly earnings and hours as well as union membership, which are essential to my analysis of right-to-work laws on wages.[2] Therefore, the files actually employed in the regressions are extracts from the CPS containing only the households being surveyed in their fourth and eighth months. Termed "Monthly Outgoing Rotation Groups", these files were compiled by the National Bureau of Economic Research. Since the CPS is conducted by the Census Bureau, an U.S. government agency specifically tasked with conducting surveys, it is reasonable to assume that the data is fairly reliable and complete.

I use data files from 2009 to 2015, a period of time beginning three years before passage of Indiana's RTW law in 2012 and continuing to the latest data available.[3] I restrict my attention to non-self-employed workers - the "eligible" universe in the data, for which union and earnings data are collected. I also limit my investigation to individuals employed in private, for-profit companies, the ones primarily affected by RTW laws. Out of 2,224,425 total observations from 2009 to 2015, 876,214 satisfy these two criteria.

A complicating factor in the data set is the procedure used when respondents do not answer the survey questions on wages. The Census Bureau and BLS use a process called "hot-decking" to estimate wages and hours of individuals who do not provide this information, based on demographic characteristics of similar individuals such as age, sex, and education.[4] However, the estimation process does not consider union status, so this "allocated" wage data will not produce reliable estimates for union and nonunion workers separately. To this end, when working with wage data, I use allocation flags given in the data to drop 309,281 observations with allocated wage data.

The important variables are described below. All variables whose descriptions begin with "indicates" are binary 0/1 indicator variables.

[2]See the NBER documentation, "CPS Labor Extracts," p. 3

[3]Later, I also expand this time window as a modification to the model in Section 4.5.6.

[4]See the U.S. Census Bureau technical documentation, "Current Population Survey: Design and Methodology (Technical Paper 66)," ch. 9

3.1 RTW Variables

The **right-to-work law** (rtw) variable indicates whether an individual lives in a state with a right-to-work law at the time of the observation. This variable is 1 for residents of Indiana beginning in April 2012 and for residents of Michigan beginning in April 2013.[5] For all other states, it is 1 if the state is a RTW state. Since all other RTW laws were passed before 2002 or after 2014, rtw is not time-dependent for states besides Indiana and Michigan.

The **right-to-work state** (rtw_state) variable indicates whether an individual lives in a state that has ever had a right-to-work law.

3.2 Union Variables

The **covered by a union contract** ($unioncov01$) variable indicates whether an individual is either a member of a union or a non-member covered by a union contract. I assume that all union members are covered by a union contract.[6] This variable is generated from combining two variables in the data set: $unionmme$ and $unioncov$, which indicate union members and non-union members covered by a union contract, respectively.

The **union member** ($unionmme01$) variable indicates whether an individual is a member of a union.

The **free rider** ($unionfr01$) variable indicates whether an individual is a "free rider", defined as workers who are covered by a union contract but not a union member. To be explicit, the universe of this variable is covered workers: $unionfr01$ is 1 when a worker is covered by a union contract but not a union member; 0 when a worker is covered by a union contract and a union member; and [missing] when a worker is not covered by a union contract.

Table 1 illustrates the distinctions between union coverage, union membership, and free-riders. Of the 876,214 observations in the period 2009-2015, all of the 57,393 union members are assumed to be covered by a union contract. Of the 818,821 non-members, 6,203 free

[5] Indiana's RTW law took effect on March 14, 2012; Michigan's RTW law took effect on March 28, 2013.

[6] The CPS data asks the question of union coverage only to respondents who are not members of a union. Thus, it is conceivable that there are union members who are not covered by a union contract, but there is no way to distinguish non-covered union members from covered union members. In light of this issue, I assume all union members are covered.

riders are covered by a union contract.

Table 1: Union Membership vs. Union Coverage, 2009-2015

Union status	Member	Non-Member	**Total**
Covered	57,393	6,203	63,596
Non-Covered	0	812,618	812,618
Total	**57,393**	**818,821**	**876,214**

3.3 Wage variables

The **hourly wage** (*wage*) variable represents hourly wage, in dollars. Consistent with the recommendation of the NBER documentation, this is calculated by dividing weekly earnings (*earnwke*) by weekly hours (*uhourse*). *earnwke* is asked of non-hourly workers and computed for hourly workers as hourly earnings multiplied by weekly hours.[7]

The **log hourly wage** (*logwage*) variable represents the natural logarithm of the hourly wage.

The **allocation flag** (*alloc*01) variable indicates whether either component of *wage* has been allocated: usual hours per week or usual earnings per week.

3.4 Fixed-Effect Variables

The **industry** (*industry*) variable is an NBER-coded industry classification code with 51 categories.

The **state** (*state*) variable represents the individual's state of residence via a unique numeric code for each state.

The **year** (*year*) variable represents the year in which the individual was surveyed.

3.5 Demographic Variables

The **age** (*age*) and **square of the age** (*agesq*) variables represent the age and the square of the age of the individual, respectively.

[7]See the NBER documentation, "CPS Labor Extracts," p. 32

The **education** (*education*) variable represents the education level of the individual. I recode this variable to have only five categories, represented by the numbers 1 through 5. In order from 1 to 5, these are: less than high-school education, HS education, less than 4 years of college, 4 years of college, and greater than 4 years of college.

The **Hispanic** (*hispanic*01) variable indicates whether the individual is Hispanic.

The **male** (*male*01) variable indicates whether the individual is male.

The **marital status** (*marital*01) variable indicates whether the individual is married with his or her spouse present.

The **metropolitan** (*metro*01) variable indicates whether the individual lives in a metropolitan area.

The **race** (*race*) variable represents the race of the survey respondent. I recode this variable to have only five categories: white, black, American Indian, Asian/Pacific Islander, and other. For individuals of mixed race, if any of the races listed is "black", I classify him/her as black; otherwise, if any of the races listed is "white", I classify him/her as white; otherwise, if any of the races listed is "American Indian", I classify him/her as American Indian; otherwise, if any of the races listed is "Asian/Pacific Islander", I classify him/her as Asian/Pacific Islander. The remaining individuals are classified as "other."

3.6 Data Overview

Summary statistics for these variables are given in Table 3 in the Appendix. As usual, the mean of a binary 0/1 indicator variable represents the proportion of observations for which the variable equals 1.

Some comments concerning sample size are in order (recall that the full sample consists of 876,214 observations). First, note that there are only 63,596 observations of $unionfr01$ because the universe of this variable is restricted to workers covered by a union contract, as explained above. Next, there are 58,299 missing values in the wage data, so *wage* has 817,915 observations. Of these, 1,339 observations have a value of 0, so *logwage* is defined for 816,576 of the non-missing wage values. Finally, there are 7,501 missing values in the metropolitan status data, so *metro*01 has 868,713 observations.

Histograms for the wage and age variables are given in Figures 1 and 2 in the Appendix.

There exist 2,403 outliers below the federal minimum tipped wage of $2.13 and 79 outliers above $300 (in the latter case, because weekly hours was less than 10), but the number of outliers is relatively small.[8] The distribution of the hourly wage is roughly what one would expect, with most observations clustered around $10-$20 per hour and gradually tapering off at higher wages. The distribution of ages is also unremarkable: a sharp increase as young adults aged 16-25 enter the workforce, a plateau through the working years, and a gradual decline after age 55 as older adults retire.[9]

As a first step to understanding the data, figures 3 - 6 in the Appendix graph the evolution of the union membership and union free rider rates in Indiana and Michigan over time. The graphs plot monthly and yearly averages in the rates for each state. The time of introduction of the RTW law is given by the red line. I expect that the unionization rate would decrease and the free rider rate would increase after the RTW law is introduced, but it is evident that the data are noisy: only the free-rider rate in Michigan seems to appreciably change after the introduction of the RTW law. Furthermore, the union free rider rate is calculated based on only 210 observed free riders over the 2009-2015 sample period, which is a very small number. This preliminary graphical analysis seems to suggest that my findings will be limited, both because of noise in the data and because of a small sample size in union members and free riders.

For a more formal statistical analysis of the union membership and union free rider variables in the above graphs, Table 4 in the Appendix reports the results of running t-tests to analyze the difference in means before and after the introduction of the RTW law. The p-values imply that Michigan experienced a significant drop in union membership and a significant increase in union free riders after the introduction of its RTW law (at the 5% level), while Indiana did not.

[8] I examine the effect of outliers on the regression results in Section 4.5.7.

[9] Note that according to the documentation, a value of 80 for *age* actually means ages 80-84, and a value of 85 actually means ages 85+.

4 Methodology

The purpose of this paper is to study the effects of right-to-work laws. To this end, I consider the effects on five key variables: the fraction of all workers who are members of labor unions,[10] the fraction of union workers who are free riders,[11] the wages of workers who are covered by union contracts,[12] the wages of workers who are not covered by union contracts,[13] and the wages of union members compared to the wages of free riders.

4.1 OLS

As a first step in examining the effect of RTW laws, I run OLS regressions of each dependent variable of interest on the regressor rtw.

The specifications have the following general form:

$$Y_{ijst} = \beta_0 + \beta_1 \, rtw_{st} + \varepsilon_{ijst} \tag{1}$$

where i indexes individuals, j indexes industry, s indexes state, t indexes time, and rtw_{st} indicates whether a state s has a RTW law at time t.

Equation (1) is estimated with each of the following dependent variables Y: union membership ($unionmme01$); union free riders ($unionfr01$), over a sample of union-covered workers; wages ($logwage$), over a sample of union-covered workers; and wages ($logwage$), over a sample of non-covered workers.

In addition, I estimate wages ($logwage$) with an additional regressor of $unionmme$ over a sample of union-covered workers:

$$log(wage_{ijst}) = \beta_0 + \beta_1 \, unionmme01 + \beta_2 \, rtw_{st} + \varepsilon_{ijst} \tag{2}$$

I use heteroskedasticity-robust standard errors, as I do not wish to make assumptions concerning the residuals.

[10] Later, I also use the fraction of workers who are covered by union contracts as a robustness check.

[11] As previously discussed, this is the fraction of workers who are covered by a union contract, yet are not union members.

[12] Later, I also use the wages of union members as a robustness check.

[13] Later, I also use the wages of workers who are not union members as a robustness check.

To interpret the coefficient β_1 in equation (1), recall that rtw_{st} is a binary 0/1 indicator variable. I see that β_1 represents the difference in the average value of the outcome variable Y in RTW states and the average value of Y in non-RTW states:

$$\begin{aligned}
&\mathbb{E}(Y_{ijst} \mid rtw_{st} = 1) - \mathbb{E}(Y_{ijst} \mid rtw_{st} = 0) \\
&\quad = \mathbb{E}(\beta_0 + \beta_1 \, rtw_{st} + \varepsilon_{ijst} \mid rtw_{st} = 1) - \mathbb{E}(\beta_0 + \beta_1 \, rtw_{st} + \varepsilon_{ijst} \mid rtw_{st} = 0) \\
&\quad = (\beta_0 + \beta_1) - \beta_0 \\
&\quad = \beta_1
\end{aligned}$$

Therefore, β_1 seems to represent the marginal effect of the right-to-work law on the outcome variable Y, as desired. However, the estimate of β_1 is not causal; it does not represent the change in Y that would result from passing a right-to-work law in a given state. The simple OLS specification (1) can reasonably satisfy the OLS assumptions of random sampling and existence of fourth moments, by nature of the CPS survey process and the basic inspection of the data in Section 3, respectively. However, it almost certainly suffers from omitted variable bias, violating the exogeneity OLS assumption and rendering the estimates non-causal.[14] Therefore, I improve upon my basic OLS specifications by using the difference-in-differences technique to mitigate omitted variable bias.

4.2 Difference-in-Differences

The difference-in-differences (DID) technique attempts to isolate the effect of a treatment by controlling for fixed entity and time effects.[15] In this case, I have data on outcome variables (union status and wages) for a control group of certain states (Indiana and Michigan) and a treatment group of other states, both before and after the treatment (the passage of an RTW law) is applied to the treatment states. I then assume the *parallel trends assumption*, which states that in the absence of the RTW law treatment, the treatment and control states follow

[14] For example, there are fixed differences across states that are correlated with both right-to-work laws and union membership, such as political attitudes. Many Southern states have right-to-work laws and low rates of unionization, but one does not necessarily cause the other; rather, regional cultural and political differences are likely responsible.

[15] See (Angrist and Pischke) for more on the difference-in-differences method.

similar trends in the outcome variables. These outcome variables are assumed to change by the same amount over time in both groups, therefore preserving any fixed state differences. Formally, the outcome variable is determined by a state effect and a year effect[16] that are independent of each other:

$$\mathbb{E}(Y^0_{ijst} \mid s, t) = \gamma_s + \lambda_t$$

where i indexes individual, j indexes industry, s indexes state, and t indexes time. I then account for the RTW law treatment by including the dummy variable rtw_{st} that is 1 when state s has a RTW law at time t:

$$\mathbb{E}(Y_{ijst} \mid s, t) = \beta\, rtw_{st} + \gamma_s + \lambda_t$$

The DID model examines the coefficient β on rtw_{st}, which represents the amount by which the trend of the treatment group deviated from the trend of the control group. Under the parallel trends assumption, this coefficient must therefore be the effect of the treatment. The coefficient β represents, in the treatment states after treatment, the difference between the observed average value of Y and the unobserved counterfactual average value of Y in the absence of treatment:

$$\begin{aligned} &\mathbb{E}(Y_{ijst} \mid s = 1, t = 1) - \mathbb{E}(Y^0_{ijst} \mid s = 1, t = 1) \\ &\quad = (\beta\, rtw_{st} + \gamma_1 + \lambda_1) - (\gamma_1 + \lambda_1) \\ &\quad = \beta \end{aligned} \tag{3}$$

The regressions for the basic DID model have the following form:

$$Y_{ijst} = \beta_0 + \beta_1\, rtw_{st} + \beta_2\, treatment_s + \beta_3\, after_t + \varepsilon_{ijst} \tag{4}$$

where i indexes individual, j indexes industry, s indexes state, t indexes time, rtw_{st} indicates whether a state s has a rtw law at time t, $treatment_s$ is a binary 0/1 variable indicating

[16] Here, the state and year effects are binary: the model specifies only whether the state is a control state or a treatment state, and whether the year is before the treatment or after the treatment. The fixed effects will be expanded in Section 4.3.

whether the individual resides in a treatment state, and $after_t$ is a binary 0/1 variable indicating whether the individual was surveyed after the treatment was applied.

Equation (4) is estimated with each of the following dependent variables Y: union membership ($unionmme01$); union free riders ($unionfr01$), over a sample of union-covered workers; wages ($logwage$), over a sample of union-covered workers; and wages ($logwage$), over a sample of non-covered workers.

In addition, I estimate wages ($logwage$) with an additional regressor of $unionmme$ over a sample of union-covered workers:

$$\begin{aligned} log(wage_{ijst}) = \beta_0 + \beta_1\, unionmme01 + \beta_2\, rtw_{st} \\ + \beta_3\, treatment_s + \beta_4\, after_t + \varepsilon_{ijst} \end{aligned} \tag{5}$$

The treatment group here consists of the states Indiana and Michigan, and the control group consists of all other states.[17] The treatment variable rtw_{st} is therefore equal to $treatment_s * after_t$. I use heteroskedasticity-robust standard errors, as I do not wish to make assumptions concerning the residuals.

The coefficients for equation (4) can be interpreted in terms of sample means of Y_{ijst}, as shown in the following table. For example, the average outcome of Y_{ijst} for individuals in control states before the treatment is applied is β_0.

Table 2: Interpretation of DID Coefficients as Sample Means of Y

	Before	**After**	**Diff**
Control states	β_0	$\beta_0 + \beta_3$	β_3
Treatment states	$\beta_0 + \beta_2$	$\beta_0 + \beta_1 + \beta_2 + \beta_3$	$\beta_1 + \beta_3$
Diff	β_2	$\beta_1 + \beta_2$	β_1

I take the difference-in-differences β_1 to be effect of the right-to-work law on Y, controlling for (binary) fixed state and time effects. Now, the estimate of β_1 begins to take on a causal interpretation.

The interpretation of β_1 as the difference-in-differences of sample averages is consistent with the interpretation given in (3) as the difference between the observed average value of

[17] Indiana and Michigan passed their RTW laws at different times. The variable $after$ is coded according to Indiana's date for Indiana only, and according to Michigan's later date for all other states.

Y and the unobserved counterfactual average value of Y in the absence of treatment. First, note that by the parallel trends assumption,

$$\begin{aligned}
&\mathbb{E}(Y^0_{ijst} \mid s = 1, t = 1) \\
&\quad = \mathbb{E}(Y_{ijst} \mid s = 0, t = 1) + [\mathbb{E}(Y_{ijst} \mid s = 1, t = 0) - \mathbb{E}(Y_{ijst} \mid s = 0, t = 0)]
\end{aligned}$$

that is, the unobserved counterfactual average value of Y in the treatment states is given by the observed average value of Y in the control states plus the time-invariant state effect, which is observed at $t = 0$ (before the treatment). Therefore,

$$\begin{aligned}
&\mathbb{E}(Y_{ijst} \mid s = 1, t = 1) - \mathbb{E}(Y^0_{ijst} \mid s = 1, t = 1) \\
&\quad = \mathbb{E}(Y_{ijst} \mid s = 1, t = 1) - [\mathbb{E}(Y_{ijst} \mid s = 0, t = 1) \\
&\qquad\qquad + \mathbb{E}(Y_{ijst} \mid s = 1, t = 0) - \mathbb{E}(Y_{ijst} \mid s = 0, t = 0)] \\
&\quad = (\beta_0 + \beta_1 + \beta_2 + \beta_3) - [(\beta_0 + \beta_3) + (\beta_0 + \beta_2) - (\beta_0)] \\
&\quad = \beta_1
\end{aligned}$$

showing that the interpretation of β_1 as the difference-in-differences of sample averages is the same as the interpretation (3), just using different notation.

With this model, it is evident why the recent changes in RTW laws are so important for analyzing the effect of the laws. Without any changes in the identification of the rtw_{st} variable over time in any state, the presence or absence of a right-to-work law is indistinguishable from a state fixed effect γ_s. Thus, the DID models would suffer from perfect multicollinearity. Furthermore, the simple OLS models in section 4.1 would then clearly suffer from omitted variable bias, as the rtw_{st} variable simply represents fixed differences between two groups of states, not the effect of RTW laws themselves. Once I have a change in RTW status in certain states, I can use the DID models to control for state and time fixed effects to gain a more accurate estimate of the effect of RTW laws. In this case, the DID specification arguably mitigates omitted variable bias, because changes in the RTW status of Indiana and Michigan can be attributed to exogenous political factors, such as the conservative resurgence in the 2010 United States midterm elections, that are not correlated

with the outcome variables.

4.3 Expanding Fixed Effects

A natural extension of the DID model allows for more than two groups and time periods. I expand the difference-in-differences model by allowing for more fixed effects: instead of only two state groups and time periods, I include dummy variables for each state and year, to implement full state and time fixed effects. In addition, I add industry fixed effects as well.

The regressions for the expanded DID model have the following form:

$$Y_{ijst} = \beta_1 \, rtw_{st} + \delta_j + \gamma_s + \lambda_t + \varepsilon_{ijst} \tag{6}$$

where i indexes individual, j indexes industry, s indexes state, t indexes time, rtw_{st} indicates whether a state s has a rtw law at time t, δ_j represents industry fixed effects, γ_s represents state fixed effects, and λ_t represents time fixed effects.

Equation (6) is estimated with each of the following dependent variables Y: union membership ($unionmme01$); union free riders ($unionfr01$), over a sample of union-covered workers; wages ($logwage$), over a sample of union-covered workers; and wages ($logwage$), over a sample of non-covered workers.

In addition, I estimate wages ($logwage$) with an additional regressor of $unionmme$ over a sample of union-covered workers:

$$log(wage_{ijst}) = \beta_0 + \beta_1 \, unionmme01 + \beta_2 \, rtw_{st} + \delta_j + \gamma_s + \lambda_t + \varepsilon_{ijst} \tag{7}$$

The treatment group here consists of the states Indiana and Michigan, and the control group consists of all other states. The interpretation of the coefficient β_1 is the same as the interpretation (3) in the basic DID model. I use heteroskedasticity-robust standard errors as I do not wish to make assumptions concerning the residuals.

4.4 Adding Controls

Finally, I supplement the difference-in-differences model by allowing for control variables. I add several demographic control variables: age, age-squared, education, ethnicity, marital status, metropolitan status, race, and sex.

The regressions for the final DID model have the following form:

$$Y_{ijst} = \beta_1 \, rtw_{st} + \beta x_i + \delta_j + \gamma_s + \lambda_t + \varepsilon_{ijst} \tag{8}$$

where i indexes individual, j indexes industry, s indexes state, t indexes time, rtw_{st} indicates whether a state s has a rtw law at time t, x_i is a vector of individual demographic characteristics, δ_j represents industry fixed effects, γ_s represents state fixed effects, and λ_t represents time fixed effects.

Equation (8) is estimated with each of the following dependent variables Y: union membership ($unionmme01$); union free riders ($unionfr01$), over a sample of union-covered workers; wages ($logwage$), over a sample of union-covered workers; and wages ($logwage$), over a sample of non-covered workers.

In addition, I estimate wages ($logwage$) with an additional regressor of $unionmme$ over a sample of union-covered workers:

$$log(wage_{ijst}) = \beta_0 + \beta_1 \, unionmme01 + \beta_2 \, rtw_{st} + \beta x_i + \delta_j + \gamma_s + \lambda_t + \varepsilon_{ijst} \tag{9}$$

The treatment group here consists of the states Indiana and Michigan, and the control group consists of all other states. The interpretation of the coefficient β_1 is the same as the interpretation (3) in the basic DID model. I use heteroskedasticity-robust standard errors, as I do not wish to make assumptions concerning the residuals.

4.5 Modifications and Robustness Checks

I consider some variations to the final specification (8) to assess the robustness of my results and thus the internal validity of my analysis. In most cases, the interpretation of the coefficient on the rtw variable remains the same; see (3) above.

4.5.1 Heterogeneous Treatment Effects

Originally, I pooled the states of Indiana and Michigan into one treatment group in order to obtain a larger sample size and reduce the standard errors. However, it is possible that the treatment effects of the right-to-work laws in these states are heterogeneous: the effect in Indiana may be different from the effect in Michigan. Therefore, I estimate equations (8) and (9) twice more for each dependent variable: once with a treatment group of Indiana only and once with a treatment group of Michigan only. The control group, all other states excluding Indiana and Michigan, is unchanged.

4.5.2 Control Groups

Originally, I used all states excluding Indiana and Michigan as a control group. As a robustness check, I now estimate equations (8) and (9) twice more for each dependent variable: once with a control group of non-RTW states, and once with a control group of RTW states. The treatment group, Indiana and Michigan, is unchanged.

4.5.3 Probit Models

Because the variables for union membership and free rider status are indicator variables, and an OLS regression estimates the expectation of the dependent variable conditional on the regressors, I am in effect estimating probabilities. That is, if Y is one of the indicator variables, then the OLS regression (1) estimates

$$\begin{aligned} \mathbb{E}(Y_{ijst} \mid rtw_{st}) &= \beta_0 + \beta_1 \, rtw_{st} \\ \implies \mathbb{P}(Y_{ijst} = 1 \mid rtw_{st}) &= \beta_0 + \beta_1 \, rtw_{st} \end{aligned}$$

Therefore, the OLS regressions for these indicator variables are actually linear probability models. As an alternative specification to (8), I run probit regressions as well. The probit model has the advantage of varying the estimate of the treatment effect at different values of the dependent variable. Including the marginal effects of the treatment, the probability

that the dependent variable equals 1 is restricted to lie in the interval [0,1].

$$Y_{ijst} = \Phi(\beta_1 \, rtw_{st} + \beta x_i + \delta_j + \gamma_s + \lambda_t + \varepsilon_{ijst}) \tag{10}$$

I use this probit specification is when the dependent variable Y_{ijst} is a binary indicator variable: union membership ($unionmme01$) and union free riders ($unionfr01$), the latter over a sample of covered workers.

Here, the coefficient β_1 still captures the effect of right-to-work laws on the outcome variable Y, but because I am dealing with a probit model, the marginal effect of RTW laws on Y is not $\hat{\beta}_1$ but rather

$$\frac{\partial \mathbb{P}(Y_{ijst} = 1 \mid x_i, \delta_j, \gamma_s, \lambda_t)}{rtw_{st}} = \beta_1 \, \phi(\beta_1 \, rtw_{st} + \beta x_i + \delta_j + \gamma_s + \lambda_t)$$

where ϕ is the probability distribution function of the standard normal distribution.

The specification (10) is similar to the models outlined in Farber (2005) that estimate the probability of unionization separately for each year.[18] There, the probability of unionization is taken to be a measure of the threat of union organization, but RTW laws are not included as a regressor. In fact, Farber (2005) takes RTW laws as an exogeneous decrease in the likelihood of union organization when analyzing the effect on wages. By including RTW laws in the regression, my probit models are essentially testing his assumption that RTW laws decrease the likelihood of union organization.

4.5.4 Union Membership vs. Union Coverage

Originally, I analyzed the effect of right-to-work laws on the union membership rate and on the wages of union-covered and non-union covered workers. As a robustness check, I estimate equation (8) once more for these dependent variables: union coverage ($unioncov01$); wages ($logwage$), over a sample of union members; and wages ($logwage$), over a sample of non-members.

[18] To be precise, my regressions estimate the fraction of workers in labor unions, which can be thought of as an estimate of the probability that a worker is a member of a labor union. Assuming that workers are independent and identically distributed, by the Law of Large Numbers the fraction of union workers will converge to the probability that an individual worker is a member of a labor union.

These estimates should not differ much from the estimates in Section 4.4, because the only difference between union membership and union coverage is the population of free riders, which is small in number.

4.5.5 Timing of RTW Changes

As mentioned in footnote 5, Indiana's right-to-work law took effect on March 14, 2012 and Michigan's right-to-work law too effect on March 28, 2013. Therefore, as discussed in Section 3 I set rtw equal to 1 for residents of Indiana beginning in April 2012 and for residents of Michigan beginning in April 2013. However, these RTW laws are not retroactive: they only affect future union contracts, not existing ones. It is also conceivable that the possibility of an impending RTW law may affect negotiations of union contracts taking place shortly before the enactment of the law. Therefore, the effect of RTW laws may be diffused over a period of time as existing union contracts expire and are renewed. As a robustness check, I thus experiment with small adjustments to the coding of the rtw variable by changing the time at which Indiana and Michigan are indicated to have a RTW law. I estimate equations (8) and (9) for each dependent variable with alternative codings of the rtw variable, offset -3, +3, and +6 months from the original date.

4.5.6 Sample Size

Originally, I used a sample consisting of the seven-year period 2009-2015. However, since 2009 was deep in the midst of the Great Recession, it makes sense to include earlier data so that the results would then be applicable in both periods of declining labor markets and periods of recovering labor markets. As a robustness check, I therefore estimate equations (8) and (9) for each dependent variable over an expanded 14-year sample period of 2002-2015.

4.5.7 Outliers

Finally, I analyze the robustness of my results to outliers. I estimate equations (8) and (9) twice more for each wage variable: once excluding the highest and lowest 1% of values and once excluding the highest and lowest 5% of values. The variables estimated are wages

($logwage$), over a sample of union-covered workers; wages ($logwage$), over a sample of non-covered workers; and wages ($logwage$), including the regressor ($unionmme$) over a sample of union-covered workers. The thresholds are $1.526056 < logwage < 4.278265$ ($4.60 < wage < 72.12$) for a 1% trim, and $2.002481 < logwage < 3.912023$ ($7.41 < wage < 50.00$) for a 5% trim.

5 Results

The Methodology section above essentially outlines two series of specifications. We now turn to the results of estimating these specifications.

One "baseline" set of regressions, given in Sections 4.1 - 4.4, begins with a simple OLS specification and builds up to a difference-in-differences model with full fixed effects and control variables. This series of regressions aims to investigate the effect of right-to-work laws on the various dependent variables considered: the union membership rate, the union free rider rate, the wages of union-covered workers, the wages of non-covered workers, and the wages of union members compared to the wages of free riders. The results of estimating specifications (1), (2), (4), (5), (6), (7), (8), and (9) are reported in Section 8.2 (Tables 5 - 9) in the Appendix.

The second series of regressions, given in Section 4.5, consists of a battery of robustness checks to analyze the validity of the baseline regressions. I modify various aspects of the full baseline specifications (8) and (9) (with full fixed effects and control variables) one at a time and examine whether the baseline results are sensitive to these slight changes in model specification: heterogeneous treatment effects, control groups, probit models, union membership vs. coverage, timing of RTW laws, sample size, and outliers. Section 8.3 (Tables 10 - 21) in the Appendix reports the results of estimating the modified specifications proposed in Section 4.5.

5.1 Summary

The results indicate that for all variables, omitted variable bias in the OLS and the difference-in-differences models was a significant problem. Adding full industry, state, and year fixed

effects leads to substantial changes in the estimated coefficients, demonstrating that it is important to control for these fixed effects in any estimate that I wish to interpret causally. Adding demographic control variables - age, age-squared, education, ethnicity, marital status, metropolitan status, race, and sex - does not further change the results by much.

I find that RTW laws decrease the unionization rate by 0.7 percentage points, significant at the 5% level. This finding is consistent with my hypothesis that RTW laws weaken unions. I also find that union members earn 6.18% more than free riders, significant at the 1% level. For the other variables, there is no definitive evidence, as the estimates are not statistically significant. The point estimate for the effect of RTW laws on the free rider rate (+1.3 percentage points) has a positive sign as expected, as RTW laws limit the ability of unions to maintain membership. However, the point estimates for the effect on both union wages (+2.17%) and non-union wages (+0.479%) also have positive signs, which is unexpected, because I expect that RTW laws decrease unions' bargaining power and therefore both union and nonunion wages as well.

The results are not robust to the choice of treatment group (except when considering the wages of union members compared to the wages of free riders, which makes sense since the right-to-work law is not the primary regressor of interest). This indicates that the effect of right-to-work laws is heterogeneous: it is different in Indiana and Michigan. The estimates for Michigan are more precise than those of Indiana, though this may just be a result of having more observations in Michigan. Unfortunately, while Michigan shows a statistically significant effect of RTW laws on the free rider rate of +2 percentage points, it also shows a statistically significant effect on union wages of +6%, which contradicts my hypothesis.

The results are generally robust to the other variations in model specification.

5.2 Union Membership

Table 5 reports the results of estimating the baseline specifications. Columns (1), (2), (3), and (4) estimate specifications (1), (4), (6), and (8), respectively, with each column successively adding more control variables.

The results show that controlling for industry, state, and year fixed effects led to a large change in the estimate, indicating substantial omitted variable bias in the simple OLS and

DID specifications. Using the DID model with full fixed effects and demographic controls, RTW laws are estimated to decrease the union membership rate by 0.718 percentage points. This estimate is significant at the 5% level. Such a result is consistent with the hypothesis that right-to-work laws diminish the ability of labor unions to enforce membership among an employer's workers, so that the unionization rate falls when a RTW law is passed. However, the magnitude of the effect is relatively small in percentage terms: about 0.7 percentage points. Nonetheless, this still equates to about 50,000 workers, using a crude estimate of 7 million private-sector workers in Indiana and Michigan combined.

Table 10 reports the results of the robustness checks for varying the treatment group and the control group. The baseline specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 5, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) estimate the same baseline specification but with the treatment groups of Indiana only and Michigan only, respectively, as proposed in Section 4.5.1. This is achieved by excluding Michigan observations from the estimation in column (2) and excluding Indiana observations from the estimation in column (3). Columns (4) and (5) estimate the same baseline specification but with the control groups of non-RTW states and RTW states, respectively, as proposed in Section 4.5.2. This is achieved by including non-RTW states, Indiana, and Michigan observations in the estimation in column (4) and including RTW states, Indiana, and Michigan observations in the estimation in column (5).

The results indicate that the baseline estimate of a 0.7 percentage point decrease in the unionization rate is not robust to the choice of treatment group. The negative effect of RTW on unionization is estimated to be 1.1 percentage points in Michigan, significant at the 5% level, and 0.1 percentage point in Indiana, not significant even at the 10% level. However, the baseline estimate is relatively robust to the choice of control group, with the estimates varying between 0.6 and 0.9 percentage points and retaining significance at the 5% level.

Table 15 reports the results of the probit model robustness check. The OLS specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 5, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) estimates speci-

fication (10), which includes the same regressors but uses a probit model instead of a linear model, as proposed in Section 4.5.3. Column (3) gives the average marginal effect of the *rtw* variable on the dependent variable *unionmme*01, as computed using the `margins` Stata command. This number represents the average marginal effect of the right-to-work law on the union membership rate under the probit model.

The result of the probit model is a 0.5 percentage point decrease in the unionization rate due to the RTW laws. This indicates that the baseline estimate of a 0.7 percentage point decrease in the unionization rate is fairly robust to the choice of a linear or probit model.

Table 17 reports the results of the remaining robustness checks. The baseline specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 5, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) estimates the same baseline specification but with a dependent variable of union coverage (*unioncov*01) instead of union membership (*unionmme*01), as proposed in Section 4.5.4. Columns (3), (4), and (5) estimate the same baseline specification but with slight changes in the coding of the *rtw* variable, as proposed in Section 4.5.5. The *rtw* variable is coded to change from 0 to 1 at three alternative times: 3 months before the effective dates of the RTW laws in Indiana and Michigan, 3 months after, and 6 months after. Column (6) estimates the same baseline specification but with a larger sample size, including observations from 2002-2008 in addition to those from 2009-2015, as proposed in Section 4.5.6.

The results indicate that the baseline estimate of a 0.7 percentage point decrease in the unionization rate is fairly robust to the use of union coverage instead of union membership (0.65 percentage points) and to slight variations in the coding of the *rtw* variable (0.5-0.7 percentage points), though the significance of the result decreases under the *rtw* timing variations. However, the baseline estimate is not particularly robust to expansion of the sample size. The new estimate is a 1.8 percentage point decrease, which is significantly different from the baseline estimate, yet very precise because of the expanded 2002-15 sample period. Fortunately, this new estimate is of larger magnitude than the baseline estimate even with more observations, which strengthens the validity of the original result.

In summary, the estimated effect of RTW laws on the union membership rate is a 0.7

percentage point decrease, consistent with my hypothesis that RTW laws decrease union membership. This number is significant at the 5% level, but it is not robust to the choice of treatment group, meaning that the effect of the RTW law is different for Indiana and Michigan. Indeed, the 0.7 percentage point decrease is a weighted average of a 0.1 percentage point decrease in Indiana and a 1.1 percentage point decrease in Michigan. The estimates are robust to the other modifications, and even increases in magnitude under a larger sample size.

5.3 Union Free Riders

Table 6 reports the results of estimating the baseline specifications. Columns (1), (2), (3), and (4) estimate specifications (1), (4), (6), and (8), respectively, with each column successively adding more control variables.

The results show that controlling for industry, state, and year fixed effects led to a large change in the estimate, indicating substantial omitted variable bias in the simple OLS and DID specifications. Using the DID model with full fixed effects and demographic controls, RTW laws are estimated to increase the union free rider rate by 1.27 percentage points. Such a result is consistent with the hypothesis that right-to-work laws diminish the ability of labor unions to enforce membership among an employer's workers and allow workers to be covered by union contracts even if they are not members of the union, increasing the prevalence of free riders. However, this estimate is not significant even at the 10% level, and the magnitude of the effect is relatively small in percentage terms: about 1 percentage point. It is difficult to separate this estimated effect from statistical noise.

Table 11 reports the results of the robustness checks for varying the treatment group and the control group. The baseline specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 6, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) estimate the same baseline specification but with the treatment groups of Indiana only and Michigan only, respectively, as proposed in Section 4.5.1. This is achieved by excluding Michigan observations from the estimation in column (2) and excluding Indiana observations from the estimation in column (3). Columns (4) and

(5) estimate the same baseline specification but with the control groups of non-RTW states and RTW states, respectively, as proposed in Section 4.5.2. This is achieved by including non-RTW states, Indiana, and Michigan observations in the estimation in column (4) and including RTW states, Indiana, and Michigan observations in the estimation in column (5).

The results indicate that the baseline estimate of a 1.3 percentage point increase in the free rider rate is not robust to the choice of treatment group. The effect is estimated to be a 2 percentage point increase in Michigan, significant at the 5% level, and a 0.1 percentage point decrease in Indiana, not significant even at the 10% level. The baseline estimate is also only somewhat robust to the choice of control group, as the estimate decreases to a 0.8 percentage point increase when using a control group of RTW states. It should be noted that the only statistically significant estimate occurs when using a treatment group of Michigan only.

Table 16 reports the results of the probit model robustness check. The OLS specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 6, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) estimates specification (10), which includes the same regressors but uses a probit model instead of a linear model, as proposed in Section 4.5.3. Column (3) gives the average marginal effect of the rtw variable on the dependent variable $unionfr01$, as computed using the `margins` Stata command. This number represents the average marginal effect of the right-to-work law on the union free rider rate under the probit model.

The result of the probit model is a 1.8 percentage point increase in the free rider rate due to the RTW laws. This indicates that the baseline estimate of a 1.3 percentage point increase is somewhat robust to the choice of a linear or probit model.

Table 18 reports the results of the remaining robustness checks. The baseline specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 6, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2), (3), and (4) estimate the same baseline specification but with slight changes in the coding of the rtw variable, as proposed in Section 4.5.5. The rtw variable is coded to change from 0 to 1 at

three alternative times: 3 months before the effective dates of the RTW laws in Indiana and Michigan, 3 months after, and 6 months after. Column (5) estimates the same baseline specification but with a larger sample size, including observations from 2002-2008 in addition to those from 2009-2015, as proposed in Section 4.5.6.

The results indicate that the baseline estimate of a 1.3 percentage point increase in the free rider rate is fairly robust to slight variations in the coding of the *rtw* variable (1.0-1.7 percentage points) and to expansion of the sample size (1.1 percentage points), even though the estimates are not statistically significant. Interestingly, increasing the sample size decreased the standard error enough to render the estimate of 1.1 percentage points significant at the 10% level, but this is still not a very convincing result.

In summary, the estimated effect of RTW laws on the union free rider rate is a 1.3 percentage point increase. While the sign of this estimate is consistent with my hypothesis that RTW laws increase the free rider rate, the number is not significant even at the 10% level. The estimate is not robust to the choice of treatment group, meaning that the effect of the RTW law is different for Indiana and Michigan. Interestingly, the estimate becomes significant at the 5% level when considering only Michigan: a 2 percentage point increase in the free rider rate. The estimates are fairly robust to the other modifications, but still not significant at the 5% level.

5.4 Union Wages

Table 7 reports the results of estimating the baseline specifications. Columns (1), (2), (3), and (4) estimate specifications (1), (4), (6), and (8), respectively, with each column successively adding more control variables.

The results show that controlling for industry, state, and year fixed effects led to a large change in the estimate, indicating substantial omitted variable bias in the simple OLS and DID specifications. Using the DID model with full fixed effects and demographic controls, RTW laws are estimated to increase union wages by 2.17%, using the DID model with full fixed effects and demographic controls. This result is surprising, because I expect that right-to-work laws, by diminishing the ability of labor unions to enforce membership among an employer's workers, would decrease the bargaining power of unions and lead to decreased

union wages. However, this estimate is not significant even at the 10% level. Therefore, it is difficult to separate this estimated effect from statistical noise, and my hypothesis that RTW laws decrease union wages is not disproved.

Table 12 reports the results of the robustness checks for varying the treatment group and the control group. The baseline specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 7, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) estimate the same baseline specification but with the treatment groups of Indiana only and Michigan only, respectively, as proposed in Section 4.5.1. This is achieved by excluding Michigan observations from the estimation in column (2) and excluding Indiana observations from the estimation in column (3). Columns (4) and (5) estimate the same baseline specification but with the control groups of non-RTW states and RTW states, respectively, as proposed in Section 4.5.2. This is achieved by including non-RTW states, Indiana, and Michigan observations in the estimation in column (4) and including RTW states, Indiana, and Michigan observations in the estimation in column (5).

The results indicate that the baseline estimate of a 2.17% increase in union wages is not robust to the choice of treatment group. The effect of RTW on wages is estimated to be +6.66% in Michigan, significant at the 1% level, and -5.41% in Indiana, not significant even at the 10% level. The estimates in the two states have opposite signs! Surprisingly, the result for Michigan is significant at the 1% level, although with the exact opposite result in Indiana, I am hesitant to fully accept this highly statistically significant result. The results are somewhat robust to the choice of control group (1.9-2.7%), although without statistical significance.

Table 19 reports the results of the remaining robustness checks. The baseline specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 5, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) estimates the same baseline specification but with a sample size of union members instead of union-covered workers, as proposed in Section 4.5.4. Columns (3), (4), and (5) estimate the same baseline specification but with slight changes in the coding of the rtw variable, as proposed in Section

4.5.5. The *rtw* variable is coded to change from 0 to 1 at three alternative times: 3 months before the effective dates of the RTW laws in Indiana and Michigan, 3 months after, and 6 months after. Column (6) estimates the same baseline specification but with a larger sample size, including observations from 2002-2008 in addition to those from 2009-2015, as proposed in Section 4.5.6. Columns (7) and (8) estimate the same baseline specification but exclude extreme values: column (7) excludes the highest and lowest 1% of values of *logwage*, while column (8) excludes the highest and lowest 5% of values of *logwage*.

The results indicate that the baseline estimate of a 2.17% increase in union wages is fairly robust to all of the variations considered, with all but one estimate falling in the range 1.4-2.6%. Changing the sample to union members instead of union-covered workers increased the estimate to 3.4%, significant at the 10% level. None of the estimates are significant at the 5% level.

In summary, the estimated effect of RTW laws on union wages is a 2.17% increase. The sign of this estimate is surprising, because it implies that a law that decreases unions' bargaining power would increase the wages earned by its members. Nonetheless, the number is not significant even at the 10% level, so there is insufficient evidence to reject my hypothesis that RTW laws decrease union wages. The estimate is not robust to the choice of treatment group, meaning that the effect of the RTW law is different for Indiana and Michigan. Interestingly, the estimate becomes significant at the 1% level when considering only Michigan: a 6.66% increase in union wages. The estimates are fairly robust to the other modifications, but still not significant at the 5% level.

5.5 Non-Union Wages

Table 8 reports the results of estimating the baseline specifications. Columns (1), (2), (3), and (4) estimate specifications (1), (4), (6), and (8), respectively, with each column successively adding more control variables.

The results show that controlling for industry, state, and year fixed effects led to a large change in the estimate, indicating substantial omitted variable bias in the simple OLS and DID specifications. Using the DID model with full fixed effects and demographic controls, RTW laws are estimated to increase non-union wages by 0.479%, using the DID model

with full fixed effects and demographic controls. This result is surprising, because I expect that right-to-work laws, by diminishing the ability of labor unions to maintain membership, would decrease the threat of unionization and enable non-union employers to pay lower wages without fear of unionization. However, this estimate is not significant even at the 10% level. Therefore, it is difficult to separate this estimated effect from statistical noise, and my hypothesis that RTW laws decrease nonunion wages is not disproved.

Table 13 reports the results of the robustness checks for varying the treatment group and the control group. The baseline specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 8, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) estimate the same baseline specification but with the treatment groups of Indiana only and Michigan only, respectively, as proposed in Section 4.5.1. This is achieved by excluding Michigan observations from the estimation in column (2) and excluding Indiana observations from the estimation in column (3). Columns (4) and (5) estimate the same baseline specification but with the control groups of non-RTW states and RTW states, respectively, as proposed in Section 4.5.2. This is achieved by including non-RTW states, Indiana, and Michigan observations in the estimation in column (4) and including RTW states, Indiana, and Michigan observations in the estimation in column (5).

The results indicate that the baseline estimate of a 0.479% increase in union wages is not robust to the choice of treatment group. The effect of RTW on wages is estimated to be a 0.79% increase in Michigan and a 0.0075% increase in Indiana, not significant even at the 10% level. The result is somewhat robust to the choice of control group (0.3-0.6%), although without statistical significance.

Table 20 reports the results of the remaining robustness checks. The baseline specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 8, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) estimates the same baseline specification but with a sample size of non-union members instead of non-covered workers, as proposed in Section 4.5.4. Columns (3), (4), and (5) estimate the same baseline specification but with slight changes in the coding of the rtw variable, as proposed

in Section 4.5.5. The *rtw* variable is coded to change from 0 to 1 at three alternative times: 3 months before the effective dates of the RTW laws in Indiana and Michigan, 3 months after, and 6 months after. Column (6) estimates the same baseline specification but with a larger sample size, including observations from 2002-2008 in addition to those from 2009-2015, as proposed in Section 4.5.6. Columns (7) and (8) estimate the same baseline specification but exclude extreme values: column (7) excludes the highest and lowest 1% of values of *logwage*, while column (8) excludes the highest and lowest 5% of values of *logwage*.

The results indicate that the baseline estimate of a 0.479% increase in union wages is somewhat robust to the use of union membership instead of union coverage and to slight variations in the coding of the *rtw* variable, with estimates falling within 0.2-0.6%, and not very robust to trimming outliers, with estimates above 0.9%. Interestingly, expansion of the sample size revises the baseline estimate of the effect of RTW laws on non-union wages to a 2.86% decrease, which is consistent with my hypothesis, and this estimate is significant at the 1% level.

In summary, the estimated effect of RTW laws on non-union wages is a 0.479% decrease. The sign of this estimate is surprising, because it implies that a law that decreases unions' bargaining power would increase the wages earned by its members. Nonetheless, the number is not significant even at the 10% level, so there is insufficient evidence to reject my hypothesis that RTW laws decrease union wages. The estimate is not robust to the choice of treatment group, meaning that the effect of the RTW law is different for Indiana and Michigan. The estimates are somewhat robust to the other modifications, but still not significant at the 5% level. Interestingly, the estimate changes sign when considering a larger sample size: a 2.86% decrease in non-union wages, significant at the 5% level. This may suggest that right-to-work laws do decrease non-union wages, but only over a sufficiently long time horizon.

5.6 Union Member vs. Free Rider Wages

Table 9 reports the results of estimating the baseline specifications. Columns (1), (2), (3), and (4) estimate specifications (2), (5), (7), and (9), respectively, with each column successively adding more control variables.

The results show that controlling for industry, state, and year fixed effects led to a large

change in the estimate, indicating substantial omitted variable bias in the simple OLS and DID specifications. Using the DID model with full fixed effects and demographic controls, I estimate that union members earn 6.18% more than non-members. This estimate is significant at the 1% level. It is an interesting result, but the implications are not obvious. Because labor unions are obligated to represent all workers - not just their members - when negotiating wages with employers, one might expect that free riders would not earn less than union members because they are still covered by union contracts. The difference present in the data may be due to other benefits only available to union members, or some omitted variable bias in my model.

Table 14 reports the results of the robustness checks for varying the treatment group and the control group. The baseline specification in column (1) is specification (9) given in Section 4.4. It is identical to the last specification estimated in Table 9, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) estimate the same baseline specification but with the treatment groups of Indiana only and Michigan only, respectively, as proposed in Section 4.5.1. This is achieved by excluding Michigan observations from the estimation in column (2) and excluding Indiana observations from the estimation in column (3). Columns (4) and (5) estimate the same baseline specification but with the control groups of non-RTW states and RTW states, respectively, as proposed in Section 4.5.2. This is achieved by including non-RTW states, Indiana, and Michigan observations in the estimation in column (4) and including RTW states, Indiana, and Michigan observations in the estimation in column (5).

The results indicate that the baseline estimate of a 6.18% premium in union member wages is very robust to the choice of treatment group and significant at the 1% level. The result is somewhat robust to the choice of control group, with the estimates falling within 4.6-8.0% and retaining significance at the 1% level.

Table 21 reports the results of the remaining robustness checks. The baseline specification in column (1) is specification (8) given in Section 4.4. It is identical to the last specification estimated in Table 9, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2), (3), and (4) estimate the same baseline specification but with slight changes in the coding of the rtw

variable, as proposed in Section 4.5.5. The *rtw* variable is coded to change from 0 to 1 at three alternative times: 3 months before the effective dates of the RTW laws in Indiana and Michigan, 3 months after, and 6 months after. Column (5) estimates the same baseline specification but with a larger sample size, including observations from 2002-2008 in addition to those from 2009-2015, as proposed in Section 4.5.6. Columns (6) and (7) estimate the same baseline specification but exclude extreme values: column (6) excludes the highest and lowest 1% of values of *logwage*, while column (7) excludes the highest and lowest 5% of values of *logwage*.

The results indicate that the baseline estimate of a 6.18% premium in union member wages is robust to all of the variations considered, and all estimates retain significance at the 1% level.

In summary, I estimate that union members earn 6.18% more than free riders. This is a finding that is significant at the 1% level and robust to slight variations in specification. While very convincing in a statistical sense, the real-world significance of this result is less clear: how can union members earn more than free riders if both are covered by the same union contract? There may be non-contract benefits of union membership that free riders do not receive, or there may be omitted variable bias present in my model.

6 Conclusion

This paper examines the effect of recent right-to-work laws on labor unions and wages. The literature on these effects is mixed. Using recent data on Indiana and Michigan, which passed RTW laws in 2012 and 2013, respectively, I investigate whether these RTW laws have had any conclusive effects over the past few years. I focus on several key variables: fraction of workers unionized, fraction of free riders, wages of union workers, wages of non-union workers, and wages of union members compared to wages of free riders. I use a difference-in-differences model with industry fixed effects, state fixed effects, time fixed effects, and demographic controls in order to isolate the effect of RTW laws from other factors.

The results of my analysis are mixed. I find that union members earn about 6.2% more than free riders, significant at the 1% level. In addition, the effect of RTW laws is

estimated to be statistically significant in decreasing the union membership rate. However, the magnitude of the effect - 0.7 percentage points - is small, and the result disappears when the treatment group is restricted to Indiana. RTW laws are also estimated to increase the free rider rate in Michigan only, by 2.0 percentage points. The estimated effects on wages are not statistically significant, and they have positive signs, which is at odds with my hypothesis that right-to-work laws decrease wages. Overall, the results are generally robust to slight variations in model specification.

The lack of statistical significance in many specifications is worrying for the validity of my results. Our statistically significant results may simply be a byproduct of running many regressions under different specifications; after all, on average, 5% of them will be significant at the 5% level, even with random data. As the graphs 3 - 6 suggest, there is seemingly a good deal of statistical noise and a lack of causal effect between right-to-work laws and the dependent variables.

In addition, I may be worried about possible omitted variable bias. Our difference-in-differences model accounts for fixed state and time effects, but does not correct for unobserved variables that may differ both across states and across time, and are correlated with both RTW laws and the outcome variables. Nevertheless, recent changes in RTW laws probably reflect idiosyncratic factors such as the rise of the Tea Party in American politics, so they are likely exogenous to factors driving wage and unionization changes. Omitted variable bias is therefore not a very pressing concern.

My results are consistent with the uncertainty of the existing literature examining the effects of right-to-work laws on unions. Though not very exciting, this fact also reflects a harsh reality for labor unions: with the private-sector unionization rate below 10%, it may be the case that even though RTW laws are damaging to unions, they may have only limited marginal effect on unions that are already weak from years of decline. Even though many of my results lack statistical significance, a silver lining presents itself: when I consider a larger sample size, I estimate a statistically significant 2.9% decrease in non-union wages. This suggest that further analysis focusing on larger sample sizes may be fruitful in uncovering the effects of right-to-work laws on labor unions. As time passes and more data becomes available on Indiana and Michigan after the passage of their RTW laws, it may be easier to

separate the effects of right-to-work laws from statistical noise.

7 References

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8 Appendix

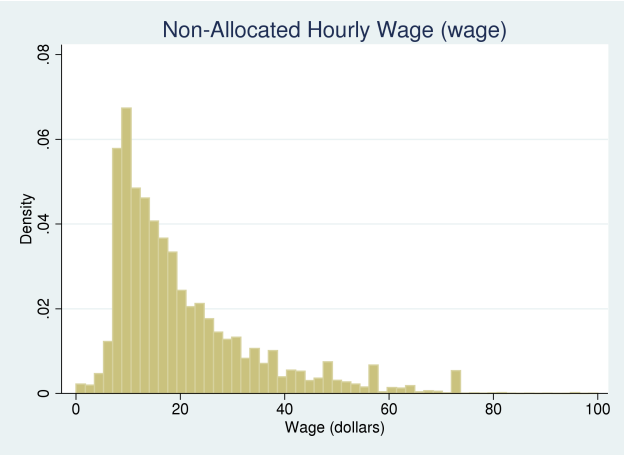
8.1 Tables and Figures

Table 3: Summary Statistics

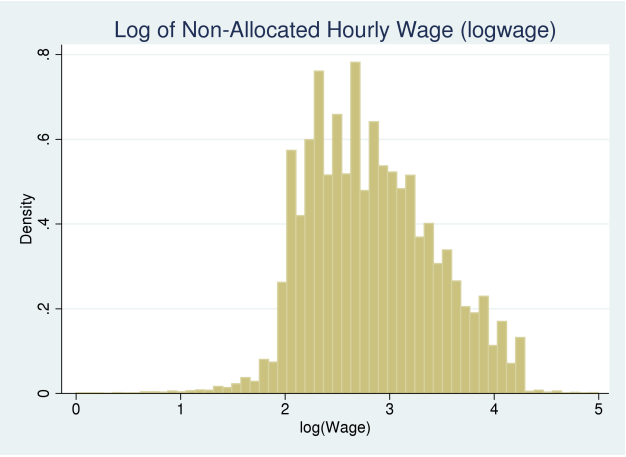
Variable	Mean	Std. Dev.	Min.	Max.	N
RTW variables					
RTW	0.404	0.491	0	1	876214
RTW state	0.425	0.494	0	1	876214
Union variables					
Union-covered	0.073	0.259	0	1	876214
Union member	0.066	0.247	0	1	876214
Union free rider	0.098	0.297	0	1	63596
Wage variables					
Wage	20.718	17.094	0	2884.61	817915
Log(wage)	2.831	0.651	-8.987	7.967	816576
Allocated wage	0.353	0.478	0	1	876214
Fixed-effect variables					
Industry	34.209	151.072	1	3895	876214
State	28.14	15.868	1	56	876214
Year	2012.003	2.003	2009	2015	876214
Demographic variables					
Age	40.88	13.98	16	85	876214
Age squared	1866.644	1205.538	256	7225	876214
Education	2.848	1.104	1	5	876214
Hispanic	0.14	0.347	0	1	876214
Marital	0.523	0.499	0	1	876214
Male	0.541	0.498	0	1	876214
Metropolitan	0.821	0.384	0	1	868713
Race	1.286	0.755	1	5	876214

Figure 1: Hourly Wages

(a) Non-Allocated Hourly Wages



(b) Log of Non-Allocated Hourly Wages



The histogram (a) excludes 608 values above 100 (out of 537,296 total values). The histogram (b) excludes 960 values below 0 and 250 values above 5 (out of 536,566 total values).

Figure 2: Age

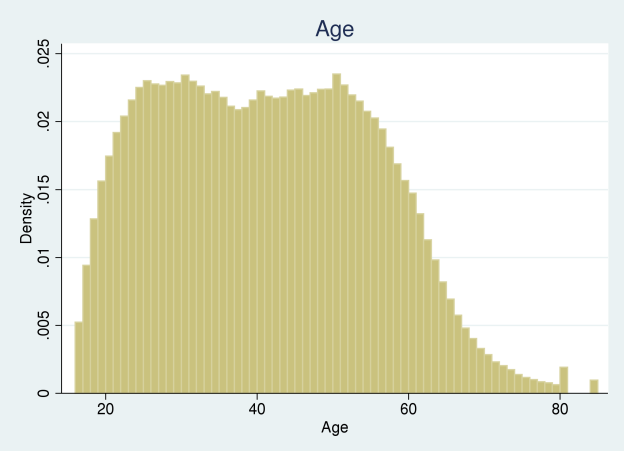
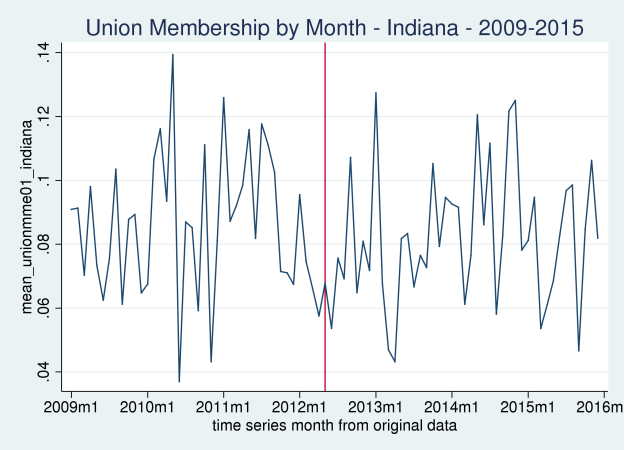
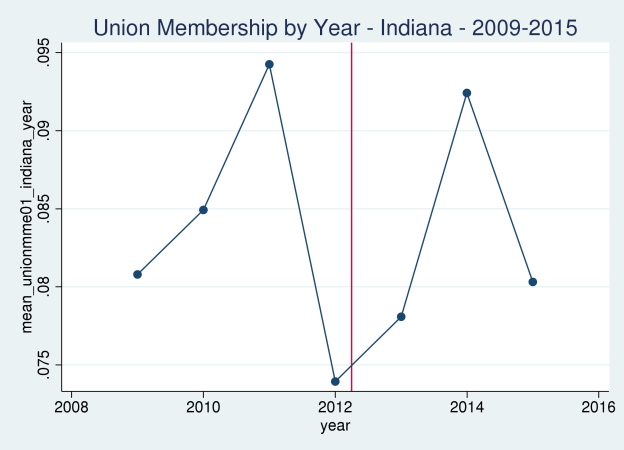


Figure 3: Average Union Membership, Indiana, 2009-2015

(a) By Month



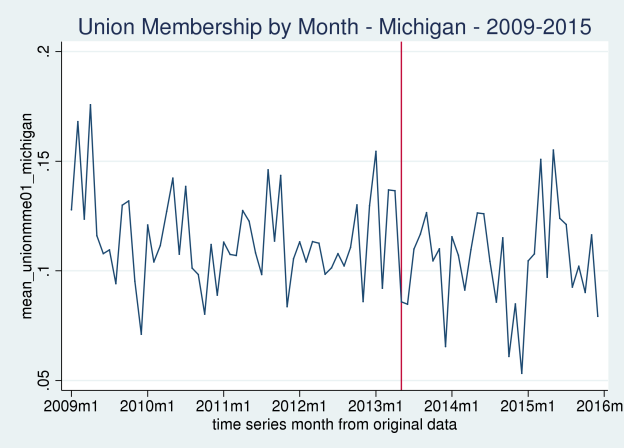
(b) By Year



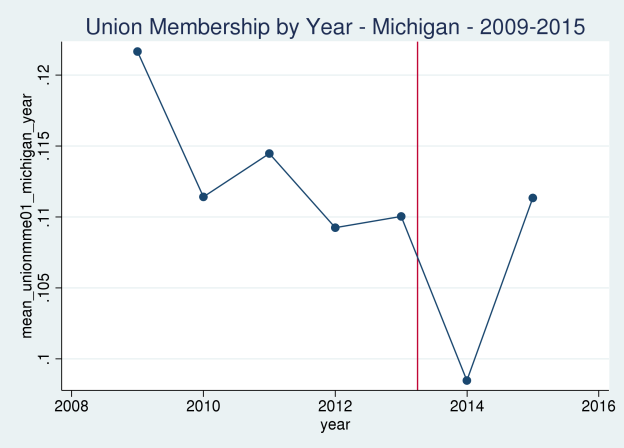
1,178 observations of union members in Indiana.

Figure 4: Average Union Membership, Michigan, 2009-2015

(a) By Month



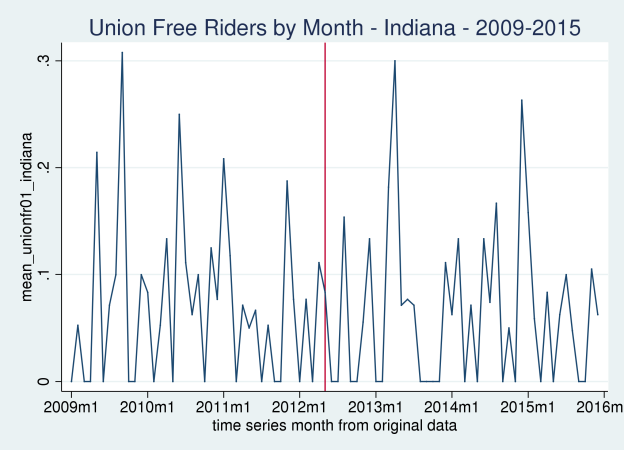
(b) By Year



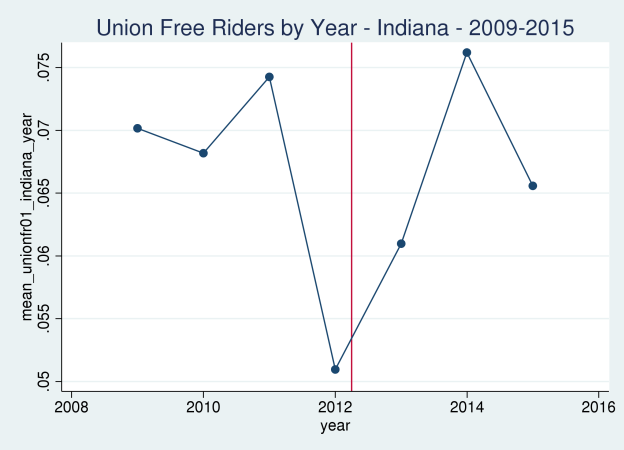
2,236 observations of union members in Michigan.

Figure 5: Average Union Free Riders, Indiana, 2009-2015

(a) By Month



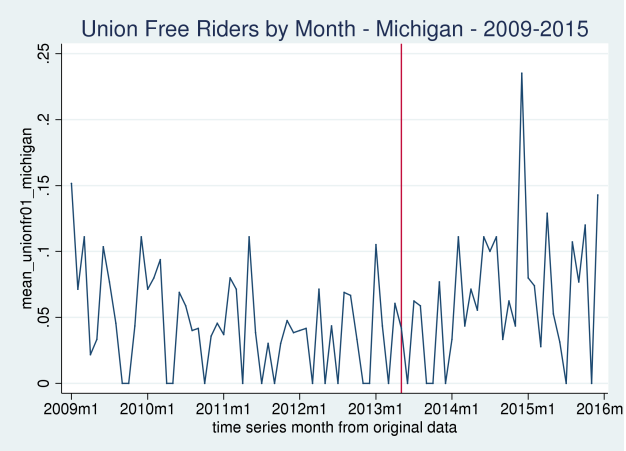
(b) By Year



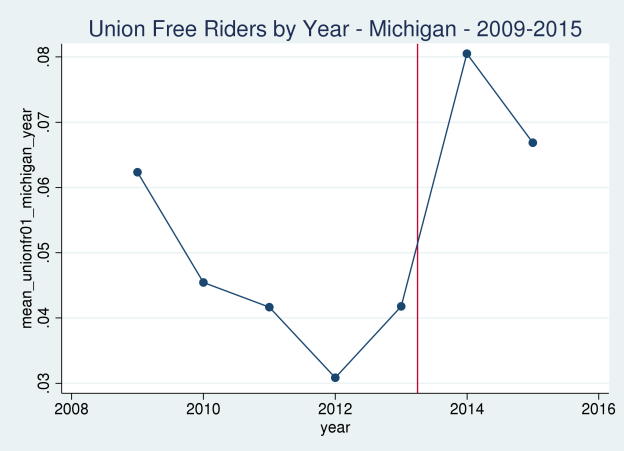
85 observations of free riders in Indiana.

Figure 6: Average Union Free Riders, Michigan, 2009-2015

(a) By Month



(b) By Year



125 observations of free riders in Michigan.

Table 4: Differences in Means (T-Test) Analysis

Variable	**State**	**Means**		**Difference**	**P-value**
		Before RTW	**After RTW**		
unionmme01	IN	0.0860989	0.0814527	0.0046463	0.1605
				(0.004682)	
unionmme01	MI	0.1149444	0.1047314	0.010213	0.0117
				(0.004507)	
unionfr01	IN	0.0677966	0.0668648	0.0009318	0.5263
				(0.0141413)	
unionfr01	MI	0.0461115	0.0638767	-0.0177652	0.0304
				(0.0094695)	

Standard errors given in parentheses.

P-values for $unionmme01$ represent $\mathbb{P}(\text{diff} > 0)$: decreased unionization

P-values for $unionfr01$ represent $\mathbb{P}(\text{diff} < 0)$: increased free riders

8.2 Results: Baseline Specifications

Table 5: Effect of RTW Laws on Union Membership

Dependent variable: *unionmme*01

VARIABLES	(1) OLS	(2) DID	(3) DID+FE	(4) DID+FE+Controls
rtw	-0.0423***	-0.0427***	-0.00745**	-0.00718**
	(0.000504)	(0.000504)	(0.00317)	(0.00315)
treatment		0.0381***		
		(0.00164)		
after		-0.000503		
		(0.000536)		
Constant	0.0826***	0.0814***	0.0159***	-0.0665***
	(0.000381)	(0.000434)	(0.00413)	(0.00457)
Industry FE			✓	✓
State FE			✓	✓
Year FE			✓	✓
Controls				✓
Observations	876,214	876,214	876,214	868,713
R-squared	0.007	0.008	0.072	0.078

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sample: All workers
Treatment group: Indiana, Michigan
Control group: All other states

Table 6: Effect of RTW Laws on Free Riders

Dependent variable: $unionfr01$

VARIABLES	(1) OLS	(2) DID	(3) DID+FE	(4) DID+FE+Controls
rtw	0.0703***	0.0732***	0.0124	0.0127
	(0.00302)	(0.00306)	(0.00821)	(0.00819)
treatment		-0.0554***		
		(0.00416)		
after		-0.00108		
		(0.00242)		
Constant	0.0790***	0.0819***	0.202***	0.297***
	(0.00125)	(0.00154)	(0.0359)	(0.0383)
Industry FE			✓	✓
State FE			✓	✓
Year FE			✓	✓
Controls				✓
Observations	63,596	63,596	63,596	63,163
R-squared	0.011	0.013	0.062	0.068

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sample: Union-covered workers
Treatment group: Indiana, Michigan
Control group: All other states

Table 7: Effect of RTW Laws on Union Wages

Dependent variable: *logwage*

VARIABLES	(1) OLS	(2) DID	(3) DID+FE	(4) DID+FE+Controls
rtw	-0.0293***	-0.0319***	0.0252	0.0217
	(0.00635)	(0.00638)	(0.0220)	(0.0205)
treatment		-0.0483***		
		(0.0125)		
after		0.0569***		
		(0.00588)		
Constant	3.043***	3.025***	2.411***	1.077***
	(0.00334)	(0.00397)	(0.0601)	(0.0584)
Industry FE			✓	✓
State FE			✓	✓
Year FE			✓	✓
Controls				✓
Observations	40,274	40,274	40,274	39,991
R-squared	0.001	0.003	0.227	0.348

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sample: Union-covered workers with non-allocated wage data
Treatment group: Indiana, Michigan
Control group: All other states

Table 8: Effect of RTW Laws on Non-Union Wages

Dependent variable: *logwage*

VARIABLES	(1) OLS	(2) DID	(3) DID+FE	(4) DID+FE+Controls
rtw	-0.120***	-0.124***	0.000937	0.00479
	(0.00190)	(0.00190)	(0.00852)	(0.00734)
treatment		-0.0495***		
		(0.00480)		
after		0.0692***		
		(0.00195)		
Constant	2.852***	2.829***	2.223***	0.851***
	(0.00126)	(0.00144)	(0.0145)	(0.0148)
Industry FE			✓	✓
State FE			✓	✓
Year FE			✓	✓
Controls				✓
Observations	496,292	496,292	496,292	491,605
R-squared	0.008	0.011	0.219	0.401

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sample: Non-covered workers with non-allocated wage data
Treatment group: Indiana, Michigan
Control group: All other states

Table 9: Comparing Union Member and Free Rider Wages

Dependent variable: *logwage*

VARIABLES	(1) OLS	(2) DID	(3) DID+FE	(4) DID+FE+Controls
unionmme01	0.137***	0.138***	0.0763***	0.0618***
	(0.0104)	(0.0104)	(0.00989)	(0.00890)
rtw	-0.0187***	-0.0208***	0.0257	0.0222
	(0.00638)	(0.00641)	(0.0220)	(0.0206)
treatment		-0.0564***		
		(0.0125)		
after		0.0569***		
		(0.00586)		
Constant	2.917***	2.898***	2.348***	1.034***
	(0.0103)	(0.0105)	(0.0610)	(0.0589)
Industry FE			✓	✓
State FE			✓	✓
Year FE			✓	✓
Controls				✓
Observations	40,274	40,274	40,274	39,991
R-squared	0.006	0.008	0.229	0.349

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sample: Union-covered workers with non-allocated wage data
Treatment group: Indiana, Michigan
Control group: All other states

8.3 Results: Modifications and Robustness Checks

8.3.1 Heterogeneous Treatment Effects, Control Groups

Table 10: Effect of RTW Laws on Union Membership

Dependent variable: *unionmme*01

		Treatment Group		Control Group	
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Baseline	IN	MI	Non-RTW	RTW
rtw	-0.00718**	-0.00162	-0.0110**	-0.00665**	-0.00849***
	(0.00315)	(0.00452)	(0.00431)	(0.00318)	(0.00320)
Constant	-0.0665***	-0.0697***	-0.0616***	-0.0632***	-0.0277***
	(0.00457)	(0.00561)	(0.00543)	(0.00468)	(0.00480)
Industry FE	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	868,713	848,539	854,615	536,193	366,792
R-squared	0.078	0.077	0.077	0.082	0.070

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 5, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) change the treatment groups to Indiana only and Michigan only, respectively. Columns (4) and (5) change the control groups to non-RTW states and RTW states, respectively.

Table 11: Effect of RTW Laws on Free Riders

Dependent variable: $unionfr01$

		Treatment Group		Control Group	
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Baseline	IN	MI	Non-RTW	RTW
rtw	0.0127	-0.000963	0.0201**	0.0142*	0.00789
	(0.00819)	(0.0138)	(0.00995)	(0.00822)	(0.00949)
Constant	0.297***	0.318***	0.284***	0.285***	0.331***
	(0.0383)	(0.0403)	(0.0388)	(0.0403)	(0.0792)
Industry FE	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	63,163	60,802	61,900	48,286	18,501
R-squared	0.068	0.068	0.067	0.045	0.100

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 6, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) change the treatment groups to Indiana only and Michigan only, respectively. Columns (4) and (5) change the control groups to non-RTW states and RTW states, respectively.

Table 12: Effect of RTW Laws on Union Wages

Dependent variable: *logwage*

		Treatment Group		Control Group	
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Baseline	IN	MI	Non-RTW	RTW
rtw	0.0217	-0.0541	0.0666***	0.0191	0.0272
	(0.0205)	(0.0363)	(0.0240)	(0.0208)	(0.0220)
Constant	1.077***	1.150***	1.032***	1.262***	1.276***
	(0.0584)	(0.0659)	(0.0604)	(0.0550)	(0.130)
Industry FE	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	39,991	38,633	39,234	30,399	11,707
R-squared	0.348	0.347	0.351	0.350	0.348

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 7, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) change the treatment groups to Indiana only and Michigan only, respectively. Columns (4) and (5) change the control groups to non-RTW states and RTW states, respectively.

Table 13: Effect of RTW Laws on Non-Union Wages

Dependent variable: *logwage*

		Treatment Group		Control Group	
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Baseline	IN	MI	Non-RTW	RTW
rtw	0.00479	7.52e-05	0.00792	0.00585	0.00373
	(0.00734)	(0.0106)	(0.00993)	(0.00743)	(0.00751)
Constant	0.851***	0.857***	0.845***	1.066***	0.958***
	(0.0148)	(0.0168)	(0.0163)	(0.0151)	(0.0200)
Industry FE	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	491,605	481,277	484,293	296,850	212,395
R-squared	0.401	0.400	0.401	0.412	0.378

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 8, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) change the treatment groups to Indiana only and Michigan only, respectively. Columns (4) and (5) change the control groups to non-RTW states and RTW states, respectively.

Table 14: Comparing Union Member and Free Rider Wages

Dependent variable: *logwage*

		Treatment Group		Control Group	
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Baseline	IN	MI	Non-RTW	RTW
unionmme01	0.0618***	0.0622***	0.0631***	0.0469***	0.0792***
	(0.00890)	(0.00901)	(0.00891)	(0.0115)	(0.0134)
rtw	0.0222	-0.0545	0.0675***	0.0195	0.0276
	(0.0206)	(0.0364)	(0.0240)	(0.0208)	(0.0220)
Constant	1.034***	1.109***	0.988***	1.228***	1.230***
	(0.0589)	(0.0665)	(0.0608)	(0.0560)	(0.130)
Industry FE	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	39,991	38,633	39,234	30,399	11,707
R-squared	0.349	0.348	0.352	0.351	0.351

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 9, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2) and (3) change the treatment groups to Indiana only and Michigan only, respectively. Columns (4) and (5) change the control groups to non-RTW states and RTW states, respectively.

8.3.2 Probit Models

Table 15: Effect of RTW Laws on Union Membership

Dependent variable: *unionmme*01

VARIABLES	(1) OLS	(2) Probit	(3) Probit Marginal Effect
rtw	-0.00718**	-0.0462**	-0.00506**
	(0.00315)	(0.0204)	(0.00223)
Constant	-0.0665***	-3.168***	
	(0.00457)	(0.0525)	
Industry FE	✓	✓	
State FE	✓	✓	
Year FE	✓	✓	
Controls	✓	✓	
Observations	868,713	868,713	
R-squared	0.078		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The OLS specification in column (1) is identical to the last specification estimated in Table 5, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) includes the same regressors but uses a probit model instead of a linear model. Column (3) gives the average marginal effect of the *rtw* variable on the dependent variable *unionmme*01, as computed using the `margins` Stata command.

Table 16: Effect of RTW Laws on Free Riders

Dependent variable: $unionfr01$

VARIABLES	(1) OLS	(2) Probit	(3) Probit Marginal Effect
rtw	0.0127	0.116	0.0181
	(0.00819)	(0.0712)	(0.0111)
Constant	0.297***	-0.405**	
	(0.0383)	(0.172)	
Industry FE	✓	✓	
State FE	✓	✓	
Year FE	✓	✓	
Controls	✓	✓	
Observations	63,163	63,163	
R-squared	0.068		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The OLS specification in column (1) is identical to the last specification estimated in Table 6, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) includes the same regressors but uses a probit model instead of a linear model. Column (3) gives the average marginal effect of the *rtw* variable on the dependent variable $unionfr01$, as computed using the `margins` Stata command.

8.3.3 Other Robustness Checks: Union Coverage vs. Union Membership, Sample Size, RTW Timing, Outliers

Table 17: Effect of RTW Laws on Union Membership

Dependent variable: *unionmme*01 (except column (2))

			RTW Timing			Sample Size
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Baseline	Coverage	-3 mo	+3 mo	+6 mo	2002-15
rtw	-0.00718**	-0.00647**	-0.00613*	-0.00535*	-0.00623*	-0.0179***
	(0.00315)	(0.00325)	(0.00316)	(0.00317)	(0.00321)	(0.00258)
Constant	-0.0665***	-0.0691***	-0.0675***	-0.0683***	-0.0674***	-0.0570***
	(0.00457)	(0.00477)	(0.00457)	(0.00458)	(0.00460)	(0.00355)
Industry FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	868,713	868,713	868,713	868,713	868,713	1,807,959
R-squared	0.078	0.076	0.078	0.078	0.078	0.087

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 5, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) changes the dependent variable from *unionmme*01 to *unioncov*01. Columns (3), (4), and (5) make slight changes to the time at which the *rtw* variable changes from 0 to 1, relative to the date on which the RTW laws actually take effect. Column (6) increases the sample size, including observations from 2002-2008 in addition to those from 2009-2015.

Table 18: Effect of RTW Laws on Free Riders

Dependent variable: $unionfr01$

		RTW Timing			Sample Size
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Baseline	-3 mo	+3 mo	+6 mo	2002-15
rtw	0.0127	0.0103	0.0150*	0.0168*	0.0113*
	(0.00819)	(0.00811)	(0.00834)	(0.00862)	(0.00678)
Constant	0.297***	0.300***	0.295***	0.293***	0.267***
	(0.0383)	(0.0383)	(0.0383)	(0.0384)	(0.0241)
Industry FE	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	63,163	63,163	63,163	63,163	141,849
R-squared	0.068	0.068	0.068	0.068	0.065

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 6, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2), (3), and (4) make slight changes to the time at which the rtw variable changes from 0 to 1, relative to the date on which the RTW laws actually take effect. Column (5) increases the sample size, including observations from 2002-2008 in addition to those from 2009-2015.

Table 19: Effect of RTW Laws on Union Wages

Dependent variable: *logwage*

			RTW Timing			Sample Size	Outliers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Baseline	Members	-3 mo	+3 mo	+6 mo	2002-15	1% trim	5% trim
rtw	0.0217	0.0343*	0.0258	0.0149	0.0207	-0.0150	0.0193	0.0142
	(0.0205)	(0.0208)	(0.0204)	(0.0206)	(0.0211)	(0.0169)	(0.0161)	(0.0154)
Constant	1.077***	1.096***	1.073***	1.083***	1.078***	1.013***	1.158***	1.300***
	(0.0584)	(0.0577)	(0.0583)	(0.0584)	(0.0585)	(0.0368)	(0.0503)	(0.0455)
Industry FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	39,991	35,992	39,991	39,991	39,991	90,984	39,421	37,590
R-squared	0.348	0.346	0.348	0.348	0.348	0.369	0.426	0.420

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 7, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) changes the sample from union-covered workers to union members. Columns (3), (4), and (5) make slight changes to the time at which the *rtw* variable changes from 0 to 1, relative to the date on which the RTW laws actually take effect. Column (6) increases the sample size, including observations from 2002-2008 in addition to those from 2009-2015. Column (7) excludes the highest and lowest 1% of values of *logwage*, while column (8) excludes the highest and lowest 5% of values of *logwage*.

Table 20: Effect of RTW Laws on Non-Union Wages

Dependent variable: *logwage*

			RTW Timing			Sample Size	Outliers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Baseline	Non-Members	-3 mo	+3 mo	+6 mo	2002-15	1% trim	5% trim
rtw	0.00479	0.00347	0.00564	0.00262	0.00192	-0.0286***	0.0128**	0.00962*
	(0.00734)	(0.00733)	(0.00729)	(0.00747)	(0.00759)	(0.00611)	(0.00619)	(0.00584)
Constant	0.851***	0.852***	0.850***	0.853***	0.854***	0.848***	0.933***	1.137***
	(0.0148)	(0.0148)	(0.0148)	(0.0149)	(0.0149)	(0.0108)	(0.0113)	(0.0107)
Industry FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	491,605	495,604	491,605	491,605	491,605	1,039,361	480,589	438,835
R-squared	0.401	0.401	0.401	0.401	0.401	0.419	0.497	0.456

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 8, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Column (2) changes the sample from non-covered workers to non-union members. Columns (3), (4), and (5) make slight changes to the time at which the *rtw* variable changes from 0 to 1, relative to the date on which the RTW laws actually take effect. Column (6) increases the sample size, including observations from 2002-2008 in addition to those from 2009-2015. Column (7) excludes the highest and lowest 1% of values of *logwage*, while column (8) excludes the highest and lowest 5% of values of *logwage*.

Table 21: Comparing Union Member and Free Rider Wages

Dependent variable: *logwage*

		RTW Timing			Sample Size	Outliers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Baseline	-3 mo	+3 mo	+6 mo	2002-15	1% trim	5% trim
unionmme01	0.0618***	0.0618***	0.0618***	0.0618***	0.0573***	0.0639***	0.0652***
	(0.00890)	(0.00890)	(0.00890)	(0.00890)	(0.00570)	(0.00717)	(0.00670)
rtw	0.0222	0.0259	0.0155	0.0215	-0.0143	0.0199	0.0147
	(0.0206)	(0.0204)	(0.0206)	(0.0211)	(0.0169)	(0.0161)	(0.0154)
Constant	1.034***	1.031***	1.041***	1.035***	0.974***	1.114***	1.255***
	(0.0589)	(0.0588)	(0.0589)	(0.0590)	(0.0370)	(0.0508)	(0.0459)
Industry FE	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓
Observations	39,991	39,991	39,991	39,991	90,984	39,421	37,590
R-squared	0.349	0.349	0.349	0.349	0.370	0.427	0.422

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The baseline specification in column (1) is identical to the last specification estimated in Table 5, which includes full fixed effects and controls, a treatment group of Indiana and Michigan, and a control group of all other states. Columns (2), (3), and (4) make slight changes to the time at which the *rtw* variable changes from 0 to 1, relative to the date on which the RTW laws actually take effect. Column (5) increases the sample size, including observations from 2002-2008 in addition to those from 2009-2015. Column (6) excludes the highest and lowest 1% of values of *logwage*, while column (7) excludes the highest and lowest 5% of values of *logwage*.