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Does the Open Shop Harm Union Collective Action?

Key words: right-to-work, NLRB petitions, union elections, open shop

JEL: K31, L38, J51, J53

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Abstract: In the U.S., right-to-work (RTW) laws impose the open shop on labor unions. I test whether RTW affects the frequency and outcomes of union representation petitions. Quasi-experimental tests reveal no association between RTW law and the volume of certification petitions filed, and that fewer certification petitions are withdrawn under RTW. Thus, unions do not appear to retreat from new member organizing under RTW law. Moreover, RTW correlates with a decline in decertification and unit clarification petitions, implying that under the open shop, RTW law reduces internal union dissension. While fewer decertification cases go to an election under RTW law, unions are more likely to lose cases that proceed to a referendum. These changes benefit smaller more than larger bargaining units. I infer that the law nudges the system away from an industrial union model in favor of smaller, craft or occupation-based units.

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One topic surrounding right-to-work (RTW) legislation in the U.S. is the extent that the law affects new union formation or existing union dissolution. This research examines the association between RTW law and the frequency and outcomes of National Labor Relations Board (NLRB) petitions and supervised elections. The NLRB certification process is the major pathway for new union formation. The NLRB also provides a legal mechanism for unionized workers to terminate their representation, and a process for employers to challenge the majority status of an existing union. Examining relationships between RTW law NLRB representational cases thus offers insight into how RTW affects the usage and outcomes of this area of labor law. Significant effects at these critical organizational moments imply that RTW law influences the strength and character of the union movement.

In the U.S., upon certification by an appropriate regulatory agency, a labor union becomes the exclusive representative of a unit of workers sharing a sufficient "community of interests."[1] Exclusivity protects the union against attempts by employers or other unions to establish competing contractual relations.[2] Paralleling a union's right to be the exclusive agent is an obligation to represent all "without hostile discrimination, fairly, impartially, and in good faith."[3] This "duty of fair representation" (DFR) extends to all individuals in the bargaining unit,

[1] For community of interest criteria, see: NLRB v. Purnell's Pride, Inc., 609 F.2d 1153 (5th Cir. 1980).

[2] J.I. Case Co. v. National Labor Relations Board, 321 U.S. 332, 64 S.Ct. 576, 88 L.Ed. 762 (1944).

[3] Steele v. Louisville & Nashville R. Co., 323 U.S. 192, 65 S.Ct. 226, 89 L.Ed. 173 (1944); Ford Motor Co. v. Huffman, 345 U.S. 330, 73 S.Ct. 681, 97 L. 1048 (1953); Syres v. Oil Workers Local 23, 350 U.S. 892, 76 S.Ct. 152, 100 L.Ed. 785 (1955).

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including persons that abstain from joining the union. Unions in violation of the DFR face civil penalties under Section 301 of the Labor-Management Relations Act.

Union security clauses are provisions in labor-management agreements that regulate the dues and initiation fees (i.e. agency fees) paid by bargaining unit members that abstain from union membership. In non-RTW states, labor and management are permitted to negotiate union security clauses. Where they exist, the threat of dismissal is the enforcement mechanism. Union security clause terms and obligations vary. For instance, while most agency fees paid by non-members remit to the union, some union security arrangements require union objectors to pay agency fees to a mutually agreed charity. Either way, the purpose of a union security clause is to impose a minimum financial sacrifice on all persons that receive union representation.

Section 14(b) of the Taft-Hartley (1947) amendment to U.S. labor law empowered the states to legislate bans on union security clauses in labor-management agreements. So-called RTW laws make the open shop the default condition. In an open shop, persons in the bargaining unit can choose non-member status and pay nothing toward the cost of representation. In contrast, in non-RTW states, persons in the bargaining unit that refrain from joining the union can be contractually required to pay agency fees to support nonpartisan union activities.

Unions oppose RTW law because the open shop augments the incentive for covered persons to "free-ride" on dues payers, which could precipitate member defection. In the extreme, Olson's (1965) collective action theory predicts that when given the opportunity to free ride, economically rational individuals will defect *en masse* to the point of organization collapse.

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In the very least, RTW law may increase internal tension between members and non-members, harming solidarity. A common belief, and our general hypothesis, is that these organizational stressors make unions less effective at certifying new units or defending against unit decertification threats.

To test this presumption, I examine whether RTW law is associated with a probabilistic change in union certification and decertification petition filings and outcomes. The analysis requires two stages. First, I test if RTW law alters the mix of five different representational petitions types flowing into the NLRB docket:

1. Certification (RC) cases are filed by non-union workers seeking union representation.
2. Decertification (RD) cases are filed by unionized workers that are attempting to terminate their union representative.
3. Employer-filed (RM) cases challenge whether a union has majority support from a unionized workforce, and with sufficient evidence triggers a decertification election.
4. Unit clarification (UC) cases are typically invoked when a skilled sub-group of the bargaining unit wishes to break away to establish their own unit for representation purposes.
5. Union security de-authorization (UD) cases are petitions by unionized workers seeking to rescind an existing union security provision of a labor agreement.

A change in the mix of NLRB cases provides clues to how various parties respond to RTW law. A drop in certification petitions after the passage of RTW law would imply that the

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law creates impediments for non-union workers to unionize, or that unions are more selective in new unit organizing in a RTW environment. A rise in decertification petitions would suggest that RTW law induces a loss of support for unionization amongst represented workers, whereas a fall would imply that the law relieves internal union dissention. An increase in employer-filed petitions would suggest that employers are emboldened to challenge existing unions in a RTW environment. Unit clarification cases often symbolize dissatisfaction by a minority group of bargaining unit members, and therefore identify internal member discord. Union security de-authorization cases involve disputes over union security clauses - the precise topic addressed by RTW law. I expect that these cases will decline after RTW law passes because the law removes ambiguity about the legal status of these clauses.

Stage two of the analysis tests for a relationship between RTW law and NLRB petition outcomes. Here, I limit the analysis to the three NLRB case types that involve an election: certification, decertification and employer-filed cases. I analyze certification cases separately, yet combine decertification and employer-filed cases into the same analysis because both petitions challenge existing unions.[4] For each analysis, I test three mutually exclusive outcomes: (1) union wins the election, (2) union loses the election, and (3) the petition is withdrawn. Along with the union win-loss differential, of interest are the results for petition withdrawals. Higher withdrawal rates imply that petitioners have less confidence in a favorable outcome. A final

[4] The small number of employer-filed cases preclude an analysis. Results from only decertification cases are very similar to results when combining decertification and employer-filed cases. Decertification case results are available from the author.

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analysis in stage two tests for an interaction between RTW law, the petition win-loss-withdrawal outcomes, and the size of the participating employee group. I do this to explore whether RTW law affects the size of the union movement by making it harder for larger employee groups to unionize or defend against de-unionization challenges. Results from the two stages provide a comprehensive picture of the effects of RTW law on petitions that enter the NLRB process.

Until recently, RTW law existed in U.S. states that industrialized late (i.e. the South and Mountain West) and largely escaped the surge in union organizing that occurred from the 1930s through 1950s. Union density is therefore lower in states with RTW law in part due to delayed regional development and historical contingency. Yet, starting in 2012, three industrial Mid-West states sequentially signed RTW into law: Indiana, Michigan, and Wisconsin. West Virginia adopted RTW in 2016 and Kentucky in 2017. This recent wave of RTW enactments enables a test for the effects of RTW law in states that had a legacy of union organizing during the great surge, reducing the effects of confounding factors related to developmental history. In this study, I compare NLRB cases in the treatment states: IN, MI, WI, WV and KY with controls in: IL, MN, MO, OH and PA.

Literature, Sample and Model

Research on RTW law must address the potential confounding effect of societal taste. According to the taste hypothesis, RTW law does not cause low union density; instead, both are manifestations of a cultural disapproval of unions. This argument, typically directed at early RTW adopters in the U.S. South, is contested by evidence that elites promoted RTW to protect

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racial privilege by crippling an institution that empowered disadvantaged minorities (Pierce, 2017). To date, quantifiable evidence for the taste hypothesis is indirect; RTW law negatively correlates with union strength when modeled as an exogenous factor, yet often becomes an inert predictor when modeled as an endogenous factor (Moore and Newman, 1985: Table 1). Nonetheless, societal taste is a plausible complication, and isolating the RTW law effect is a challenge for studies in this literature stream.

At best, the body of research using geography as the unit of analysis can only suggest causal relationships. In an analysis of standard metropolitan statistical areas, Hirsch (1980) finds that RTW law negatively affects membership, but not coverage, implying that the law induces member defection but not to the point of organization demise. Hogler, Shulman and Weiler (2004) perform a state-level, cross-section analysis of the relationship between RTW and union density. Including controls for state politics, employer opposition toward unions and a state-level social capital index, Hogler, et al., (2004) estimate that RTW lowers union density by about 8.8 percent.

Exploiting variation in public sector law across and within states, Ichniowski and Zax (1991) narrow the unit of analysis to public sector departments. One advantage to focusing on the public sector is that until recently the passage of RTW predated the wave of public sector unionization that began in the early 1960s. Thus, the sample frame decouples much of the history of RTW law from decisions by government employees to unionize. For added measure, the authors use the strength of public sector bargaining law and private sector union density at

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the state level to capture societal taste for unions. Of the five sectors analyzed: highways, public welfare, police, fire and sanitation, RTW law displays a negative and statistically significant association with unionization in all except sanitation.

The consistency in research findings from these analyses matches our hypothetical expectations and, presuming an absence of publication bias are prima fascia evidence that RTW law reduces union strength. However, one limitation of the population ecology approach is the inability to isolate RTW law as the causal factor given the many unknown potential alternative mechanisms within any defined unit of analysis. The risk of specious results increases with the size and complexity of the geographic unit. By that criteria, credibility is greatest for Ichniowski and Zax (1991), followed by Hirsch (1980) and then Hogler, et al., (2004).

Taking a stock-flow approach, Ellwood and Fine (1987) model a state ratio of workers that won an NLRB election to the total nonagricultural workforce as a dependent variable in order to capture the movement of new unionists into a state. Corroborating population ecology findings, the author's report an average 46 percent decline in new unionists in the five-year period following a RTW law passage. The estimated RTW law effect attenuates with time.

The authors offer insight into the causal mechanism by decomposing the original ratio into three parts. Ellwood and Fine (1987) find no evidence that the union certification election win rate declines in a RTW environment. Instead, the stock flow reduction is attributable to fewer certification elections and smaller certified bargaining units. The first finding implies that unions adjust to RTW law by organizing less or taking fewer certification petitions to the

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election phase; the second that unions place greater emphasis on organizing smaller groups of workers, or that larger units have greater difficulty in achieving a majority vote. Ellwood and Fine (1987) do not examine RTW law's effect on union member outflow from events like decertification elections.

Cook (1983) examines NLRB certification election data for 1979, and reports a 5 percent lower probability of a union election win in a RTW state. NLRB representational elections are the proper unit of analysis to study RTW law's effect on these critical decision-making moments. Yet Cook's cross-section analyses do not address societal taste, and further, he does not adjust for the strategic choice of withdrawing a petition prior to an election, which unions will do when the odds of a win look bleak. As Ferguson (2008) shows, the choice by a union to withdraw a certification petition is conditioned on the expected probability of an election win. Models are required that relax the assumption of independence between alternative outcomes at stages where expectations about case outcomes factor into the strategic choices of petitioners. Ferguson (2008) includes RTW law as an independent factor and concludes it has no effect on the union organizing process.

I use a quasi-experimental design to isolate the effect of RTW law on union strength. Using difference-in-difference (DD) methods, our analysis compares the change in the mix of various NLRB petition types, and the outcomes of certification, decertification and employer-filed elections across states that adopted RTW law with bordering states that did not during the 2008 to 2018 period. Adoption of RTW law is an event that marks a cut-point for the treatment.

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Analyzing the time periods surrounding the treatment enable pre and post comparisons on the probabilities of petition types and election outcomes. Assuming societal attitudes toward unions are temporally stable over the study panel, then change in taste by subjects – the unions, employers and workers involved in NLRB cases - is imperceptible, thus isolating the treatment, RTW law.[5]

Quasi-experimental methods are a relatively new approach to this topic. Eren and Ozbeklik (2016) apply a quasi-experimental method to examine RTW law's effect on labor and economic factors in Oklahoma. Choosing Colorado, Vermont and New Mexico to create a synthetic control, the authors estimate that private sector unionization rates would have been 1.35 percentage points higher in the absence of RTW law, a drop of 30.6 percent from year 2000 levels. Kogan (2017) and Zoorob (2018) use DD methods to test for a RTW effect on state-level inequality and workplace fatalities, respectively. Kogan (2017) finds no evidence that RTW law affects inequality for all states covering a 30-year period yet, like Eren and Ozbeklik (2016), shows that the state of Oklahoma might be an exception. Zoorob (2018) finds that after controlling for union density and other factors, RTW law positively correlates with workplace fatalities, corroborating previous findings (Loomis, et al., 2009; Zullo, 2011).

The present study likewise establishes control and treatment groups. Rather than use the state as the unit of analysis, our units are NLRB representation petitions. Instead of constructing

[5] Gallup polling tracks public attitudes toward unions. During the years covered by this study, nationwide approval of unions increased slightly from 59 to 62 percent. https://news.gallup.com/poll/12751/labor-unions.aspx

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a synthetic control, our control subjects are the NLRB cases in the non-RTW states in the same region that share a border with states that passed RTW law. Limiting the study to a regional subset of contiguous U.S. states with historically high union density reduces the confounding effects of societal taste and historical development.

Like Ferguson (2008), I model the NLRB process in multiple stages. Stage one tests for whether RTW law is associated with a change in the mix of representation-related cases that go before the NLRB. Since there is no reason to suspect strategic discrimination across petition types at the initial filing stage, the test uses a multinomial probit regression that assumes independence across the five different outcomes. Stage two tests for a RTW law effect on the outcomes of cases requiring an election (i.e. certification, decertification and employer-filed). I use multinomial probit equations that allow for dependency between three mutually exclusive outcomes: a union election win, a union election loss and a petition withdraw. Figure 1 illustrates the two-stages and provides statistics for the sample.

Figure 1 about here.

The sample includes all relevantly defined cases filed from January 1, 2008 to December 31, 2019 that closed. I excluded cases dismissed by the NLRB (n=473, 5.0 percent of total). As Figure 1 details, of the 8,833 eligible cases, the majority are union certification (RC) petitions (72.8 percent), followed by union decertification (RD) petitions (19.0 percent) and the remaining case types, employer-filed (RM), unit clarification (UC) and union security de-authorization (UD) cases are at just under 3 percent each. Stage one tests whether RTW law is associated with

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a shift in this mix. Of particular interest is whether RTW law is associated with a relative proportionate number of certification and decertification cases.

Stage two tests for a RTW law effect on certification, decertification and employer-filed petition outcomes. Due to the small number of employer-filed petitions, I combine decertification and employer-filed cases into one test. Thus, stage two tests for a RTW law effect in two situations: (1) the choice for representation by a group of nonunion workers, and (2) the choice to exit representation by a group of unionized workers. The second situation involves petitions brought by both bargaining unit members (i.e. RD petitions) and employers (i.e. RM petitions).

Combining the first two stages enable estimates of a RTW probabilistic effect on union certification and decertification. However, as Ellwood and Fine (1987) suggest, it is possible for RTW law to have no effect on the NLRB election win-loss outcome, but still affect the overall size of the union movement by having varying effects along organization size. A final set of regressions test whether RTW law is associated with the size of employee groups involved in petition outcomes.

Our DD analysis compares two groups: (1) a treatment group in states where RTW took effect, and (2) a control group from contiguous states where RTW did not take effect. Parallel trend tests indicate no significant difference in the dependent measures between the treatment

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and control groups prior to 2012 when the RTW law wave began.[6] For all analyses, the base equation is:

$$Y_{ist} = \beta_0 + \Sigma\beta_1 \text{Year} + \Sigma\beta_2 \text{State} + \delta_0 \text{RTW}_{st} + \mu_{ist}$$

Where Y is the outcome variable of interest for *i* petitions in *s* states over *t* time periods, the β are estimated coefficients for the intercept and the Year and State indicators. This two-way fixed effect regression model controls for differences between the states and for trends common to all states. RTW is a variable with the value 1 if the state had a RTW law at the time of the case open date, zero otherwise. The δ is the estimated coefficient for the treatment and the μ_{ist} are unexplained sample errors.

The Y takes on different forms depending on the stage of the analysis. In the first stage, Y is one of the five NLRB petition types, and the test is to determine if the mix of petition types change with the passage of RTW. I use a multi-variate probit model, with certification cases the excluded category and errors of zero mean and independent. Across four outcomes, the stage one μ_{ist} are:

$$\mu_{ist} \sim N\left[\begin{matrix}0\\0\\0\\0\end{matrix}\right], \left[\begin{matrix}1&0&0&0\\0&1&0&0\\0&0&1&0\\0&0&0&1\end{matrix}\right]$$

In the second stage, Y is one of three outcome types: union win, union loss and withdrawal, and likewise analyzed with a multinomial probit regression for certification cases and decertification

[6] Tests are available from the author.

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case types. Union loss is the excluded category. For stage two, μ_{ist} have a mean of zero yet correlate across outcomes to adjust for contingent strategic decision-making:

$$\mu_{ist} \sim N \begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}$$

Here, ρ is the estimated correlation between the residuals of the model outcomes.[7] Table 1 provides summary statistics for the variables in the models at the two stages and the case counts.

Table 1 about here.

Results

Before examining whether RTW law affects the mix of NLRB petitions, a preliminary step is to assess whether RTW law affects overall NLRB caseloads in relation to the economy. Using a method similar to Ellwood and Fine (1987), I create a ratio of the number of NLRB cases per the number of private establishments extracted from Quarterly Census of Employment and Wage (QCEW) records. I construct three state-level ratios: (1) the sum of all NLRB petitions per 1,000 establishments, (2) the number of certification petitions per 1,000 establishments, and (3) the number of non-certification petitions per 1,000 establishments. The ratios were transformed into natural log values and regressed against an indicator for RTW law, where 1 equals each full calendar year that RTW law was in effect, zero otherwise. The equations include a year trend variable and a random intercepts for each state. Tables 2A and 2B

[7] For a technical discussion on the Stata *mvprobit* command, see Cappellari and Jenkins (2003).

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display the relevant summary statistics and regression results that test whether the NLRB caseload relative to economic activity is associated with RTW law.

Table 2A about here

Table 2B about here

The RTW coefficient is statistically insignificant in equations that predict the ratio of certification petitions to the number of private establishments. Contrary to Ellwood and Fine (1987), there is no indication of a fall-off in certification petitions following RTW law passage. Instead, the evidence indicates a decline in the ratio of non-certification cases. In Table 2B, Model 3, the RTW law coefficient is negative and the null hypothesis rejected at conventional standards ($\alpha < 0.05$).

Table 3 examines the change in non-certification petitions in detail by displaying tests for the effect of RTW law on the mix of NLRB representation-related petitions. The "Treatment" variable signifies the probabilistic shift in non-certification filings relative to certification petitions associated with RTW law passage. Results indicate a statistically significant decline in decertification, unit clarification and union security de-authorization petitions in a RTW context. Tests (not displayed) indicate that this association with RTW law is unrelated to the size of the employee group. The marginal drop in decertification petitions is 5.2 percentage points, which is a 38.3 percentage decline; unit clarification petitions drop 0.8 percentage points, which is 75.4 percent decline; and union security de-authorization petitions drop 1.1 percentage points, for an estimated 86.0 percentage decline relative to non-RTW law contexts.

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The treatment coefficient for employer-filed petitions is positive, indicating a 0.5 percentage point increase, or a gain of 70.5 percent under RTW, but the coefficient does not breach conventional levels of statistical significance.

It is feasible to conceptualize decertification and employer-filed cases as decertification threats from different origins. Combining these case types and examining the changes under RTW at Stage 1, I estimate that the decertification threat under RTW declines by 23.7 percent. Overall, given the drop in non-certification petitions, the share of certification petitions increases 6.6 percentage points, rising 8.0 percent from non-RTW law levels.

Table 3 about here

These results indicate that RTW law reduces certain kinds of NLRB petitions. The drop in union security de-authorization petitions was expected because RTW law annuls union security clauses. The drop in decertification petitions implies fewer attempts by dissenting unit members to terminate representation. The drop in unit clarification petitions similarly indicates less internal friction over the composition of bargaining units. While I conclude no RTW law effect on the proportion of employer filed petitions relative to certification petitions, note the comparably small number of such cases (Figure 1). The size and direction of the treatment coefficient in the employer-filed petition equation hints that a more powerful statistical test could find a positive RTW law effect on the frequency of these petitions.

Tables 4 provides test results for a RTW law effect on the outcomes of certification petitions. Consistent with Ellwood and Fine (1987) and Ferguson (2008), the win-loss outcomes

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for certification petitions are statistically undisturbed by RTW law. However, the treatment coefficient for the certification withdrawal equation is negative, and breach conventional levels of statistical significance (α=0.05) in a one-tailed test. Thus, contrary to Ellwood and Fine (1987), I find no evidence that RTW law is associated with fewer certification petitions moving to an election. Broadly, these results provide no indication that unions retreat from organizing in the immediate years following RTW, or that certification election win rates suffer. Using Model 1 of Table 4, the estimated probabilities for the three outcomes for control versus treatment groups are as follows: union win, 43.5 versus 47.7; union loss, 27.5 versus 28.5; petition withdrawal, 28.9 versus 23.8.

Table 4 about here

Table 4, Model 2 is a multinomial probit equation that tests for whether RTW law is associated with certification petition outcomes conditional along the size of the employee group. As the findings from Ellwood and Fine (1987) suggest, RTW law might have no discernable effect on the probability of a certification petition outcome, yet still negatively affect the size and character of the union movement if larger groups have greater difficulty in successfully taking cases through the certification election process. To test this supposition, an additional variable, Size, is included in the equation, along with an interaction term between size and treatment. Size is the log value of the number of employees involved in the petition. The coefficients for Size in Table 4, Model 2 indicate that larger groups are less likely to withdrawal certification petitions, yet more likely to vote against unionization in an election. Nonetheless, this association is

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unrelated to RTW law as indicated by the statistically insignificant coefficient for the Size-Treatment interaction term.

Table 5 displays results for the outcomes of the combined decertification and employer-filed petitions. In Model 1, the statistically significant treatment coefficient indicates that RTW law affected case outcomes insofar that the withdrawal rate is higher. However, unions are more likely to lose the cases that proceed to an election. Thus, the evidence implies offsetting effects: fewer petitions reach the election phase (i.e. more are withdrawn), but for the cases that do go to an election, unions are less likely to prevail. Estimated probabilities for the three outcomes for control and treatment groups are as follows: union win, 20.3 versus 11.4; union loss, 37.7 versus 36.0; petition withdrawal, 42.1 versus 52.5. Since a union win or a withdrawal both preserve the organization in decertification elections, what matters most for the union movement is the loss percentages. These results offer little evidence that RTW law increases the probability of union dissolution through the NLRB decertification process.

Table 5 about here

The size of the employee group involved in the petition might be a moderating factor. Table 6 summarizes the predicted probabilities from Table 5, Model 2, for a union win, union loss and petition withdrawal in RTW and non-RTW contexts.

Table 6 about here

The estimates show that over the period of analysis the probability of a decertification petition withdrawal declined as the employee group grows. The findings suggest that under

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RTW law fewer decertification cases reach the election phase, but this effect is stronger with smaller bargaining units. For an employee group size that is one standard deviation above the mean (79.7), the decertification case withdrawal rate is 33.7 percent in a RTW context, whereas for an employee group that is one standard deviation below the mean (6.0), the withdrawal probability is 71.0 percent.

Again, because both a petition withdrawal and a union win preserve a union during a decertification challenge, change in union election loss probability matters most in estimating the effect of RTW on union growth or decline. As Table 6 displays, we find a substantive and statistically significant decline in union loss probabilities for smaller bargaining units under RTW law. Considering both the Stage 1 and Stage 2 parts of the whole process depicted in Figure 1, at Stage 1 we estimate an overall decline in decertification cases of 23.7 percent. Unions experience an additional decline in election losses at Stage 2, but this effect is stronger for smaller units, attributed in part to a higher case withdrawal rate. Larger unions, in contrast, are less likely to experience a decertification case withdrawal, and more likely to lose cases that move forward to an election. In sum, the results indicate that RTW law is associated with a decline in the decertification threat. However, this outcome disparately benefits smaller employee groups.

Discussion and Limitations

This study tests the effect of the passage of RTW law on NLRB representation petition submissions and outcomes. Any change in the petition filings and results reveals hints toward

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how principals respond to an open shop context, and in turn how RTW law might affect the size and character of the union movement. Building from the extant literature and collective action theory, our expectation was that RTW law injures the prospects for new union certification and increases the risk of existing union decertification.

Our results fail to support this hypothesis. I detect no drop off in the rate of union certification attempts under RTW. The passage of RTW law is statistically unrelated to the ratio of certification petitions to the number of private firms in the economy. Moreover, win-loss probabilities for certification elections are unaffected by RTW law, and fewer certification petitions are withdrawn in a RTW environment. Taken together, these results offer no evidence that RTW law diminishes the rate by which nonunion workers gain representation through the NLRB process.

What does change is the relative frequency of non-certification petitions, all of which decline as a proportion of the representation caseload. I estimate that worker-initiated decertification petitions, which are the most threatening to unions, experience a 38.3 percent decline in the years following RTW law. Moreover, unit clarification petitions, which typically capture internal membership discord, drop by 75.4 percent.[8] These findings suggest that the open shop acts as a pressure release mechanism that reduces demands by union dissenters to seek

[8] Unit clarification (UC) petitions also include accretion cases whereby a union attempts to grow their unit. The data do not easily distinguish accretion cases from other UC petitions.

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the termination of union representation, and deflates internal union discord over the composition of the bargaining unit.

RTW law does seem to affect decertification election outcomes, but in a positive way for unions. From the perspective of the union movement, fewer decertification petitions (at stage 1) and more withdrawals (at stage 2) is a welcome consequence of RTW law. Offsetting this outcome is that unions win fewer decertification cases that go to an election. For unions operating under RTW law, decertification plus employer-filed election wins drop by 43.6 percent compared with a non-RTW context. Although fewer decertification petitions go to the election stage, the cases that do proceed pose a greater union loss threat.

For these decertification challenges, smaller units appear to benefit more than larger units do. It seems that where bonds of solidarity are strong, such as one might find in a smaller unit, the organization is better able to withstand threats posed by free rider opportunism. In projecting how RTW policy affects the size and character of the union movement, I infer that the law nudges the system away from an industrial union model ushered in by the New Deal in favor of smaller, craft or occupation-based units.

These findings match Olson's (1965) theory on collective action insofar that enabling free rider behavior results in dissenter disengagement from the collective. Fewer decertification and unit clarification petitioners and higher rates of decertification case withdrawals indicates disengagement. Consistent with Olson's framework, the drop in these petitions is evidence that

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in many cases union dissenter objection is economic, rather than ideological. Remove the union dues obligation and objectors become quiescent.

Yet the findings do not support an extension of Olson's theory to its logical deduction that free riders destroy the collective. While students of the union movement should find this statement trite given the presence of labor unions in RTW states, an important purpose of research is to weigh the relevance of competing theories. Our findings underscore the limits of rational economic individualism as a framework for understanding the causes of collective behavior. Clearly, on the topic of the resiliency of worker organizations, forces stronger than self-interest are in play. On one level, this conclusion corroborates the literature that explores the psychological factors for union support insofar that psychometric methods have shown that non-economic reasons exist for choosing and sustaining unions (Bamberger, Kluger, and Suchard, 1999; Fiorito, Padavic, and Russell, 2014).

Hirschman's (1970) exit, voice, loyalty trichotomy, a framework reduced to exit-voice and applied to union contexts by Freeman and Medoff (1984) is an attractive alternative theory for interpreting these results. Dissenters that file decertification or unit clarification petitions are attempting to enact change from within through the NLRB process - conceptually, a form of voice that leverages the rights available through an external legal authority. Workers file these petitions because under a union security clause that requires an agency fee their only way to escape from contributing to the union treasury is to quit employment, which is generally costly and impractical. By imposing the open shop, RTW law establishes a low-cost exit option, thus

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reducing the reasons for dissenters to resort to the NLRB mechanisms. For union dissenters, exit becomes easier than voice under RTW.

The estimates herein are limited to a relatively short time horizon. Union organizing drives can take months, and it is implausible that organizers terminate a protracted drive because RTW law passes during an interim stage. Further, in nearly all of the newly enacted RTW law states, legal challenges by unions placed the status of the law in limbo for a brief period. What this means empirically is that the passage of RTW law might be a fuzzy experimental cut point for this research question. I did test this by substituting a cut point equal to the linear temporal distance from the passage of RTW law, but the essential results were unchanged. A final limitation is that the analysis relied on NLRB data, which does not include certifications through voluntary recognition. In sum, the findings capture a short-term RTW law effect, from 2008 to 2019 and only as it applies to NLRB representational cases.

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Table 1: Sample Summary Statistics, Mean (s.d.)			
	Stage 1	Stage 2	
	Full Sample	RC Sample	RD-RM Sample
Treatment	0.128 (0.335)	0.141 (0.348)	0.103 (0.303)
Size (log employees)		3.125 (1.284)	3.083 (1.296)
Illinois	0.214 (0.410)	0.220 (0.414)	0.197 (0.398)
Indiana	0.062 (0.241)	0.063 (0.244)	0.057 (0.233)
Kentucky	0.033 (0.180)	0.035 (0.183)	0.028 (0.166)
Michigan	0.138 (0.345)	0.141 (0.348)	0.126 (0.332)
Minnesota	0.076 (0.265)	0.069 (0.254)	0.094 (0.292)
Missouri	0.068 (0.251)	0.068 (0.251)	0.066 (0.249)
Ohio	0.132 (0.339)	0.115 (0.318)	0.188 (0.391)
Pennsylvania	0.209 (0.407)	0.228 (0.419)	0.153 (0.360)
West Virginia	0.025 (0.157)	0.027 (0.162)	0.020 (0.139)
Wisconsin	0.043 (0.203)	0.035 (0.183)	0.070 (0.255)
Year 2008	0.115 (0.319)	0.101 (0.301)	0.144 (0.352)
Year 2009	0.089 (0.284)	0.084 (0.277)	0.108 (0.311)
Year 2010	0.095 (0.294)	0.095 (0.294)	0.089 (0.285)
Year 2011	0.090 (0.287)	0.079 (0.269)	0.123 (0.328)
Year 2012	0.090 (0.287)	0.096 (0.294)	0.077 (0.267)
Year 2013	0.083 (0.277)	0.084 (0.278)	0.087 (0.282)
Year 2014	0.095 (0.293)	0.097 (0.296)	0.088 (0.283)
Year 2015	0.082 (0.275)	0.089 (0.285)	0.057 (0.233)
Year 2016	0.071 (0.257)	0.074 (0.262)	0.064 (0.244)
Year 2017	0.068 (0.252)	0.071 (0.256)	0.064 (0.245)

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Year 2018	0.060 (0.238)	0.065 (0.246)	0.049 (0.215)
Year 2019	0.061 (0.240)	0.067 (0.249)	0.049 (0.216)
N	8,833	6,427	1,931

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Table 2A: Summary Statistics for Baseline Test of RTW Effect on NLRB Caseloads		
Variable	Definition	Mean (s.d.)
All Cases	Ratio of all cases per 1000 firms (log)	-1.089 (0.409)
RC Cases	Ratio of certification cases per 1000 firms (log)	-1.460 (0.441)
Non-RC Cases	Ratio of non-certification cases per 1000 firms (log)	-2.359 (0.592)
RTW	Years when RTW was in full effect	0.175 (0.382)

Table 2B. OLS Regressions of NLRB Case Activity in the Economies of Ten U.S. States, 2008 – 2019			
	Model 1 All Cases	Model 2 RC Cases	Model 3 Non-RC Cases
RTW	-0.099 (0.097)	-0.053 (0.115)	-0.264* (0.129)
Year Trend	-0.052*** (0.011)	-0.033* (0.013)	-0.089*** (0.014)
Constant	102.920*** (21.529)	65.453* (25.473)	176.636*** (28.274)
R-Square	0.237	0.0812	0.3734
N	120	120	119
Notes:* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$; Standard errors in parenthesis.			

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Table 3: Probit Regression of RTW Law and NLRB Representation Petitions, 2008-2019.				
	Decertification (RD) Petition	Employer-filed (RM) Petition	Unit Clarification (UC) Petition	Union Security (UD) Petition
Treatment	-0.281**	0.198	-0.491**	-0.678***
	(0.095)	(0.176)	(0.161)	(0.194)
Constant	-1.060***	-1.754***	-2.266***	-1.868***
	(0.076)	(0.101)	(0.127)	(0.118)
LL	-7179.181			
N	8,833			
Notes: RC petitions are the omitted category; robust standard errors in parenthesis; equations include indicator variables for year and state; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$ in a two-tailed test.				

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Table 4: Probit Regressions of RTW Law and Outcomes for Union Certification (RC) Petitions, 2008-2019				
	Model 1		Model 2	
	Union Win	Withdrawal	Union Win	Withdrawal
Treatment	0.105 (0.069)	-0.156* (0.073)	0.036 (0.133)	-0.140 (0.139)
Size (log employees)			-0.062*** (0.014)	-0.059*** (0.014)
Size X Treatment			0.024 (0.037)	-0.003 (0.039)
Constant	-0.081 (0.114)	-0.427*** (0.119)	0.004 (0.123)	-0.284* (0.130)
ρ	-0.972*** (0.003)		-0.099*** (0.005)	
LL	-6856.692		-6774.509	
N	6,427		6,396	
Notes: robust standard errors in parenthesis; equations include indicator variables for year and state; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$ in a two-tailed test.				

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Table 5: Probit Regressions of RTW Law and Outcomes of Union Decertification (RD and RM) Petitions, 2008-2019.

	Model 1		Model 2	
	Union Win	Withdrawal	Union Win	Withdrawal
Treatment	-0.372* (0.144)	0.264* (0.130)	-0.289 (0.405)	1.021*** (0.283)
Size (log employees)			0.379*** (0.036)	-0.136*** (0.024)
Size X Treatment			-0.036 (0.104)	-0.239** (0.084)
Constant	-0.853** (0.287)	-0.279 (0.256)	-2.182*** (0.372)	-0.175 (0.252)
ρ	-0.947*** (0.010)		-0.990*** (0.041)	
LL	-1975.907		-1858.486	
N	1,931		1,909	

Notes: robust standard errors in parenthesis; equations include indicator variables for year and state; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$ in a two-tailed test.

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Table 6: Predicted Probabilities for Union Decertification (RD and RM) Petitions, Smaller and Larger Units						
	Smaller Unit (-1 s.d.)			Larger Unit (+1 s.d.)		
Context	Union Win	Union Loss	Withdrawal	Union Win	Union Loss	Withdrawal
RTW	3.8	25.2	71.0	19.0	47.2	33.7
Non-RTW	7.8	43.8	48.3	33.3	32.0	34.7
Estimates based on Table 5, Model 2 (N=1,909).						

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