Section 14(b) and the Protective Role of Unions

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Abstract

Focusing on ten states in the Midwest and Appalachia, I analyze the effects of right-to-work (RTW) law on labor unions. There are two main chapters to this report. The first examines whether RTW law affects the frequency and type of National Labor Relations Board (NLRB) representation petitions filed and petition outcomes. The second examines Office of Labor-Management Standards (OLMS) union financial data to estimate the extent that RTW law causes a financial loss to unions, and in turn, whether the passage of RTW law induces a shift in organization strategy, as measured by change in union expenditure patterns.

I find no association between RTW law and the volume of certification petitions filed with the NLRB. Further, RTW law is unrelated with the outcomes of certification petitions. RTW law is, however, associated with a shift in the caseload distribution of NLRB representation petitions. Under RTW law, the frequency of decertification and unit clarification petitions decline, implying that union objectors become less inclined to resort to NLRB legal mechanisms to decertify the union or to seek a separate bargaining unit. By providing a low-cost exit option, RTW law appears to diminish the threats of union decertification and bargaining unit cleavage.

Under RTW law, a larger proportion of decertification petitions are withdrawn before going to an election; however, for the cases that do go to an election the union is more likely to lose. These findings are not universal across union organizations. For the decertification petitions that go to an election, a loss is likelier for larger bargaining units. Thus, larger unions experience less relief from the decertification threat than smaller

unions. The findings imply that sustaining larger bargaining units is more challenging under RTW law than sustaining smaller units.

The second chapter assesses whether RTW law financially harms unions and if unions strategically respond. Financial harm from RTW law is limited to unions not affiliated with the building trades. The non-trades respond by reducing expenses for overhead and politics. The building trades experience no detectable financial harm from RTW law, yet nonetheless respond by increasing representational, political and apolitical contribution expenditures. I surmise that for the building trades RTW law signals a more hostile environment for unions, and their response is a defensive maneuver to protect public construction work, avoid the repeal of prevailing wage law, or both.

From a theoretical lens, the results offer partial support for the hypothesis that RTW law induces unions to be less movement-focused and more member-focused. Consistent with a shift away from movement-focused unionism, the non-trades reallocate resources away from politics. However, I detect no increase in representational resource allocation, which is contrary to arguments that RTW law makes unions more member-focused. Moreover, the law appears to trigger elevated activism by the trades in politics and in providing community support through apolitical resource expenditures.

Broadly, as a policy for labor regulation, RTW law nudges labor relations closer toward a pre New Deal institutional context by imposing a disparate burden on larger organizations. This is because RTW law has fewer negative implications for smaller local unions, especially those bound by craft, occupation, location or market segment. Mass industry organizing, maintaining larger bargaining units comprised of workers with

heterogeneous skills, and representing workers in dynamic labor markets become more challenging in a RTW environment.

Introduction

Few aspects of U.S. labor law are as controversial as Section 14(b) of the Labor Management Relations Act (1947).[1] Section 14(b) allows states to enact "right to work" (RTW) laws that ban union security clauses from labor-management agreements. Union security clauses are the contractual mechanisms that enable labor organizations to collect dues, or "agency fees,"[2] from workers that unions are legally obligated to represent but that choose not to be union members. Given that dues are the primary source for union operational funds, most labor organizations favor union security clauses. By prohibiting union security clauses, RTW laws expose unions to the classic collective action problem whereby free riders can opportunistically withhold support from a non-excludable good provided by others (Olson, 1965). Financially, union treasuries are potentially strained. From an organizational perspective, free rider behavior might undermine an ethos of shared sacrifice that sustains the collective endeavor.

Recent enactments of RTW law in Midwest and Appalachian states provide an opportunity to examine how RTW law affects labor union activity in a comparatively union dense region of the U.S. Since 2012, the industrial Midwestern states of Indiana, Michigan, and Wisconsin invoked 14(b) and signed RTW into law. The Appalachian

[1] Also known as the Taft-Hartley Amendments, was a response to the wave of strikes following WWII, and included new prohibitions on union tactics, such as secondary boycotts, as well as anti-communist pledge requirements for union staff. The Act marked the first legislative incursion into union affairs, outlawing the closed shop and permitting only limited forms of the union shop.

[2] The term "agency fee" refers to dues collected from non-members. Unions often estimate agency fees based on the cost of collective bargaining activities (negotiations, contract administration, grievance handling, and so forth), and exclude expenses relating to politics. Agency fees are thus usually lower than dues paid by members in good standing.

State of West Virginia adopted RTW in 2016 and the State of Kentucky followed in 2017. In every instance, RTW law triggered protests and legal challenges.

Not all recent attempts to pass RTW succeeded. Ohio signed SB 5 into law in March 2011, which would have imposed the open shop on public sector employees, as well as curtailing other bargaining rights. Anti-RTW activists, led by public employee unions mobilized a referendum on the law, and Ohio voters rejected SB 5 that November. Missouri passed a RTW law in February 2017.[3] Soon thereafter, a labor-backed group began gathering signatures, and in August 2017, the group submitted enough signatures to force a referendum, which suspended implementation of RTW. In the August 2018 Primary, Missouri voters rejected RTW.

Prior to this wave of RTW legislative activity, nearly all RTW states were in the U.S. South, Mountain West or Western Plains; regions that industrialized late and largely escaped the surge in union organizing from the 1930s through the 1950s.[4] This legacy partially explains why union density is lower in states with RTW law. Recent enactments of RTW law differ insofar that it strikes at the heart of a U.S. region with a long history of union activism.[5] The upper Mid-West and Appalachia is a hub of union activity in industries such as mining, auto, steel, utilities, construction, trucking, rail transportation

[3] Senate Bill 19.

[4] Early adopters of RTW law did so for a variety of reasons, for instance, historical accounts suggest that RTW law in the South was part of a strategy to protect racial privilege by crippling an institution that brought greater equity to minorities (Pierce, 2017).

[5] Columbus, Ohio, hosted the founding convention of the American Federation of Labor in 1886. Detroit, Michigan is the headquarters of the United Automobile, Aerospace, and Agricultural Implement Workers of America. The American Federation of State, County and Municipal Employees originated in Wisconsin.

and the public sector. The passage of RTW law in Mid-West and Appalachian states enables tests for how an established union movement responds to the law.

Table 1 lists the month and year for RTW law authorization and implementation for the RTW states in this study. Indiana, Michigan, Wisconsin, West Virginia and Kentucky passed RTW law during the time period of this study. Completing the region are the states in the sample without a RTW law, but that are contiguous with the states in Table 1: Illinois, Minnesota Missouri, Ohio and Pennsylvania. Limiting the analyses to states with a history of union organizing reduces bias due to confounding factors related to regional developmental trajectories.

Table 1: State RTW Law Authorization and Implementation Dates

State	Authorization	Implementation
Indiana[1]	February, 2012	February, 2012
Kentucky[2]	January, 2017	January, 2017
Michigan[3]	December, 2012	April, 2013
West Virginia[4]	February, 2016	September, 2017
Wisconsin[5]	March, 2015	March, 2015

Notes: (1) Indiana House Enrolled Act 1001 of 2012; (2) Kentucky House Bill 1 of 2017, upheld in November 2018; (3) Michigan Public Act 348 of 2012; (4) West Virginia Senate Bill 1, the Workplace Freedom Act of 2016; (5) Wisconsin Act 1 of 2015.

The next section provide legal background and places the contemporary RTW movement in historical context. The section afterward outlines the rational for using quasi-experimental methods to address this topic. Next are the two main analytical sections. The first examines whether RTW influences the frequency and outcomes of representational cases handled by the National Labor Relations Board (NLRB). The second examines Office of Labor-Management Standards (OLMS) data to examine how

RTW law affects unions financially, and if the law is associated with change in union strategy. A final section sections highlight major conclusions and limitations.

Law and Context

Once certified by the appropriate regulatory body, a labor union becomes the exclusive representative for a unit of workers that share a "community of interests."[6] Exclusivity protects the union against attempts by employers or other unions to establish competing contractual relations.[7] Paralleling a union's right to be the exclusive agent for a unit of workers is an obligation to represent all "without hostile discrimination, fairly, impartially, and in good faith."[8] This "duty of fair representation" (DFR) extends to all individuals in the bargaining unit, including persons that abstain from joining the union. Unions in violation of the DFR face civil penalties under Section 301 of the LMRA. The threat of penalty intensified in September 2018 with revised instructions from the NLRB General Council that broadened the circumstances in which grievance processing errors rise to DFR violations of the Act.[9]

Union security clauses are provisions in labor-management agreements that regulate the dues and initiation fees paid by bargaining unit members that abstain from union membership. Where they exist, the threat of dismissal is the usual enforcement

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

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

[8] 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).

[9] Memo from Beth Tursell, Associate to the General Counsel, To: All Regional Directors, Officers-in-Charge, and Resident Officers, Subject: General Counsel's Instructions Regarding Section 8(b)(1)(A) Duty of Fair Representation Charges, September 14, 2018.

mechanism. Agency fees do not always remit to the union. Under some arrangements, a mutually agreed charity can receive the agency fee. Either way, the purpose of a union security clause is to impose a minimum financial sacrifice on all persons that receive union representation. RTW laws ban union security clauses from labor-management contracts, making the open shop the default condition. In an open shop context, persons in the bargaining unit can choose non-member status and pay nothing toward the cost of representation. The only option for labor unions that want to avoid the free rider burden in RTW states is to relinquish exclusive bargaining agent status, in which case they lose the right to compel employers to bargain in good faith.

One issue animating RTW policy is the role of labor unions in politics. Union tactics for advancing organizational goals involve activities in both the private (e.g. negotiations with an employer) and public spheres (e.g. lobbying for minimum wage law). While these two orbits are clearly intertwined, (e.g. the rules for private collective bargaining are established and shaped in the political arena) the courts have nonetheless sought to demarcate private dealings with employers from public political activism by shielding represented workers from having to finance the latter.[10] Agency fees are an invention by the courts to limit member abstainers to a per capita dues amount based on costs for "collective bargaining activities" like contract negotiation, union administration and grievance handling. Agency fee calculations exclude expenses related to politics

[10] Janus v. American Federation of State, County, and Municipal Employees, Council 31. No. 16-1466, 585 U.S. (2018).

based on the legal theory that union political activities are a form of protected speech, and any contractual mechanism that compels speech violates the First Amendment.[11]

Investigative reports portray the contemporary RTW movement as one motivated by a desire to drive a deeper wedge between union members and covered non-members based on political divisions.[12] In any given election, a sizable fraction of union members ignore endorsements by union leaders and vote for COPE opposed candidates. Thus, efforts to elect candidates, displays of partisanship or support for a policy platform by a member majority can become a rationale for dissenting members to withhold financial support from a union by becoming non-members. In this way, a labor union in a RTW state incurs greater financial risk for political activities, particularly those that divide factions of the covered workforce. Evidence suggests that even non-partisan, civic activities can be affected; voter turnout is negatively associated with RTW law (Feigenbaum, Hertel-Fernandez, and Williamson, 2018; Zullo, 2008).

From a research perspective, the manner that these laws recently passed invites an application of quasi-experimental methods to examine the effects of RTW law on union organizations. An important condition for quasi-experimental design is that the treatment is exogenous to the subjects. For the present study, this would require that the passage of RTW law happened independently from any affected labor unions and union members,

[11] International Association of Machinists v. Street, 367 U.S. 740, 81 S.Ct. 1784, 6 L.Ed.2d 1141 (1961); Communications Workers v. Beck, 487 U.S. 735 (1988); Janus v. American Federation of State, County, and Municipal Employees, Council 31, No. 16-1466, 585 U.S. (2018).

[12] Pilkington, Ed, "Rightwing Alliance Plots Assault to 'Defund and Defang" America's Unions" The Guardian, August 30, 2017; https://www.theguardian.com/us-news/2017/aug/30/rightwing-alliance-unions-defund-defang. For a similar discussion on Michigan, see: Kroll, Andy. "Meet the New Kochs: The DeVos Clan's Plan to Defund the Left." Mother Jones, January/February, 2014; http://www.motherjones.com/politics/2014/01/devos-michigan-labor-politics-gop/

current and prospective. Several facts and circumstances of the contemporary RTW movement permit a presumption of exogeneity.

First, in every instance, organized labor responded with protests, court petitions, ballot drives, media campaigns, or some combination thereof. Clearly, mainstream unions and most unionists object to RTW law. Second, a partisan alignment of the executive and legislative branches of state government composed of lawmakers that sought to weaken labor unions enabled every passage.[13] Third, wealthy political activists were instrumental in both engineering the political alignment and pressuring for RTW law.[14] Fourth, the acts often circumvented conventional democratic practice, making it improbable that citizen opinion weighed into the decision.[15] Fifth, direct evidence of popular demand for RTW was scarce,[16] and survey evidence indicated high levels of public misinformation about the law (Singer 2006).

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[13] To date, Illinois, unlike its neighboring states blocked RTW law because of an absence of a partisan political alignment. For Indiana, see: Davey, Monica "Indiana Governor Signs a Law Creating a 'Right to Work' State, New York Times, February 1, 2012; for West Virginia, see: DePillis, Lydia. West Virginia House passes Right-to-Work Bill after Harsh Debate. The Washington Post, February 4, 2016; For Ohio, see: McNay (2013); for Wisconsin and Michigan, see: Zullo (2014).

https://www.washingtonpost.com/news/wonk/wp/2016/02/04/amid-rancourous-debate-west-virginia-sets-right-to-work-bill-on-path-to-near-certain-passage/?utm_term=.1ad634dbae67

[14] Exposés describe the RTW movement as driven by wealthy elites. The Guardian posted an April 2016 fundraising letter by Tracie Sharp, President of the State Policy Network (SPN), championing the role of SPN funded organizations in the spread of RTW, and asking for matches to a $1,000,000 gift. See also footnote 8.

[15] For instance, Michigan and Wisconsin followed an expedited timeline that curtailed public debate (Zullo, 2014).

[16] As already mentioned, citizens rejected RTW in two statewide referendums, Ohio in 2011 and Missouri in 2018. Moreover, if RTW were the product of public support for RTW, we would expect to see increased political support for openly pro-RTW politicians. Yet in nearly every instance, the politicians that voted for RTW hid this preference prior to their elections. Further, many leaders casting pro-RTW votes were termed-limited and therefore shielded from risk for their vote, or suffered a decline in popularity afterward.

The totality of evidence suggests that subject attitudes toward unions were not a factor in the recent passage of RTW laws. These legislations were political and solidly partisan maneuvers (Peck, 2016), and not a democratic reflection of societal preference. As with other contexts and eras (Canak and Miller, 1990; Dixon, 2010), this wave of anti-union law was the product of concerted action by labor's elite adversaries taken within a favorable political window.[17]

Quasi-Experimental Design

Our framework for hypotheses testing begins with Olson's (1965) economic-based theory on collective action, which posits that any organization producing a non-excludable good faces the problem of how to finance itself when persons accessing the good can refrain from contributing, thereby shifting the burden of resourcing the good onto others. So-called "free rider" behavior is expected to increase when the marginal gain associated with withholding support to the organization increases, or when the marginal cost of non-membership decreases. By prohibiting agency fees, RTW law widens the gap in the financial sacrifice between members and covered non-members, thereby growing the net marginal benefit for withholding support. Membership and dues revenue should decline after RTW law passes as covered persons reassess the benefits of membership against a larger potential gain from withholding support. In a world of rational economic actors, free rider behavior causes the collapse of the union.

[17] Jacobs and Dixon (2006) perform a repeated cross-section (1960-1990), state-level analysis that predicts the presence of RTW law. Interestingly, measures of partisan politics is an insignificant predictor of the law, a conclusion that appears to conflict with facts surrounding this current RTW movement. In line with the taste argument, Jacobs and Dixon find that conservative citizen ideology correlates with RTW law.

While assessments of the effects of RTW law often approach the topic from this logic, factors outside of rational behavior also creep into the literature as alternative causes or confounding elements. Moore and Newman (1985) canvass the evidence for a relationship between RTW law and union strength. Building from an individual cost-benefit template, the authors list five factors that theoretically determine the demand for union representation: the price of union membership, income or wealth, the size of the union wage differential, the value of nonpecuniary union benefits, and "taste" for union representation.[18]

Of these factors, the most vexing is taste because it is hard to operationalize and, as a psychological construct, seems incongruent with rational decision-making models. Nonetheless, societal taste for unions might explain why states adopt RTW law, and simultaneously why workers in a state refrain from unionization. In relation to taste, Moore and Newman (1985) stress the implications about assumptions on simultaneity, pointing out that modeling RTW law as exogenous generally yields negative associations between RTW law and union strength variables, while research that models RTW law as endogenous tends to conclude that RTW law is inert (Moore and Newman, 1985: Table 1). Isolating RTW law's economically grounded free rider effects requires controls for the psychologically grounded taste effects.[19]

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[18] Missing from the equation is the extent of employer or state opposition. Opposition from employers is a determinant in the decision to unionize (Bronfenbrenner, 1997; Freeman and Kleiner, 1990; Kleiner, 2001). Recent union organizing drives in Southern U.S. auto plants have exposed an additional source of union opposition: high-ranking political leaders. See: Resnikoff, Ned. How Tenn. Politicians Killed Volkswagen Unionization. MSNBC, April 4, 2014. Online at: http://www.msnbc.com/msnbc/tennessee-volkswagen-chattanooga-union#51477.

[19] Moore and Newman (1985) and Moore (1998) describe a third hypothesis; that RTW law reduces the bargaining power of unions, which in turn reduces the instrumental value of joining. Operationally and conceptually, it is difficult to distinguish the bargaining power and the free rider hypotheses; both link the appeal of unionization to the ability of unions to marshal resources

Quasi-experimental designs go far in overcoming this methodological problem. A change in RTW law is an event that marks a cut-point for the treatment. Analyzing the time periods surrounding the cut-point enable pre and post comparisons on the measures. Assuming subject attitudes toward unions are temporally stable, and that change in taste toward unions is imperceptible over the period around the cut point, then the RTW law (i.e. treatment) effect is isolated from any taste effect. This idea holds for unmeasured yet potentially confounding factors that do not vary over the study period, such as historical developmental and political legacies across regions.

One related consideration with this method is the width of the band of time periods around the cut-point. Our interest is in isolating the effects of RTW law from alternative causal factors. The narrower the band of time periods, the more plausible it becomes that RTW law, not some alternative, explains group differentials in the outcome measures. Balanced against this consideration is the robustness of the cut-point as an indicator of treatment exposure. A wider band of is necessary when the cut-point is an imprecise indicator of treatment exposure, or when the treatment is expected to have lagged effects. Circumstances in this study favor a wider band of time periods.

For instance, legal challenges may have created ambiguity over applications of RTW law. The Indiana Legislature passed RTW law in 2012.[20] Unions successfully appealed the law before the Ohio Lake Superior Court,[21] which sent the issue to the Indiana Supreme Court. Contested was whether the Indiana Constitution, which states

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to achieve collective goals. As it seems that the free rider hypothesis subsumes the bargaining power hypothesis by encompassing areas of power that are beyond the bargaining table, the free rider framework is preferred and will be used henceforth.

[20] Senate Bill 0269.

[21] Lake Superior Court, Case No. 45D01-1305-PL-52

"[n]o person's particular services shall be demanded, without just compensation"[22] applies to free riders under RTW law. In November 2014, the State Supreme Court reversed the lower Court ruling and held that the language in the State Constitution applies to *State* mandates, but that RTW law was a *Federal* matter.[23] Similar motions were filed and overturned in Wisconsin[24] and West Virginia.[25] It is plausible that employers and unions waited for legal resolutions before responding to the law.

Moreover, most new RTW law did not require an immediate amendment to labor agreements, but rather, allowed employers and unions to wait until the existing agreement expired before eliminating union security clauses. Thus, the actual effect on many unions occurred sometime after the official state implementation date. In some cases, where the bargaining relationship allowed, unions secured extensions to union security clauses, typically through side letters. On a more fundamental level, not every labor agreement in a non-RTW state has a union security clause (Davis and Huston, 1993), and where they do exist, it is reasonable to speculate that labor unions in states that adopt RTW need time to assess the new environment and make adjustments.

These legal challenges and administrative delays likely muddy the exactness of the RTW law passage date as a treatment cut-point. A RTW effect might therefore be

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[22] Article 1, Section 21.

[23] Indiana Supreme Court, Case No. 45S00-1309-PL-596; See also: United States Court of Appeals, 7th Circuit, case no. 13-1264.

[24] The law was initially ruled unconstitutional by Dane County Circuit Court, case No. 2015CV000628, and subsequently overturned by the United States Court of Appeals, 7th Circuit, case no. 16-3736.

[25] West Virginia passed a RTW law in February 2016. The Circuit Court of Kanawha County issued an injunction in February 2017, case no. 16-C-959-969, which suspended implementation. In September 2017 the injunction was overturned by the State of West Virginia Supreme Court of Appeals, case no. 17-0187.

delayed or perhaps strengthen with time. To capture RTW effects, the analysis will favor a wide band around the cut point by maximizing the post treatment periods following implementation, given data limitations. To capture the possibility of a graduated effect, I perform tests that treat the cut point as both a discrete event and as a linear time distance from the date of enactment. Overall, these sensitivity tests did not produce significant differences in the findings, and so I present only the discrete cut-point results.

Quasi-experimental approaches are relatively new to this topic. An example is Eren and Ozbeklik's (2016) analysis of the effect of Oklahoma's adoption of RTW law in 2001 on unionization, employment and wages. The authors build a synthetic control by matching pre-treatment measurement trends in Oklahoma against other states. Choosing Colorado, Vermont and New Mexico as counterfactuals the authors conclude that RTW law in Oklahoma reduced unionization, but had no discernable effect on employment and wages in the 6 years after passage. Comparisons of trends in aggregate state data are however vulnerable to Type I hypothesis testing error due to serial correlation (Bertrand, Duflo and Mullainathan, 2004), casting doubt on the Eren and Ozbeklik (2016) findings.

The present study deals with this potential validity threat by using micro data. While it is plausible for the outcome variable and RTW variable to be jointly endogenous when analyzing state aggregated data (Moore 1998), this threat diminishes when micro data are modeled as a function of RTW law. Findings in the chapter titled "RTW Law and NLRB Petitions and Outcomes" relies on individual NLRB case data. The NLRB data are not longitudinal, and therefore serial correlation is not an issue. The chapter titled "Labor Union Response to RTW Law" analyzes longitudinal local union data. In

these analyses, to adjust for potential error clustering, the mulit-level models include random intercepts at the state and union levels.

RTW Law and NLRB Petitions and Outcomes

The NLRB certification process is the major pathway for new union formation. The NLRB also provides legal mechanisms for unionized workers to terminate their representation, or for employers to challenge the majority status of an existing union. Understanding the relationship between RTW law and NLRB representation-related cases can therefore offer insight into how RTW affects union organization birth or existing union demise.

I test 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 for whether RTW law alters the mix of five different representational petitions types flowing into the NLRB docket:

1. RC cases filed by non-union workers seeking union representation.
2. RD cases filed by unionized workers that are attempting to decertify their representative.
3. RM cases filed by employers challenging whether a union has majority support from a unionized workforce.
4. UC cases brought forward by union members seeking clarity on their unit composition (hence, UC). A typical UC case is invoked when a skilled sub-group of the bargaining unit wishes to break away to establish their own unit for representation purposes.

5. UD cases brought by unionized workers challenging the union security provisions of a labor agreement.

A change in the mix of NLRB cases can provide clues to how various parties respond to RTW law. A drop in RC petitions after the passage of RTW law would imply that the 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 RD 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. Employers initiate RM petitions, thus any increase associated with RTW law implies that employers are emboldened to challenge existing unions under RTW law. UC cases often symbolize dissatisfaction by a minority group of bargaining unit members, and therefore likely track internal member discord. UD cases involve disputes over union security clauses - the precise topic addressed by RTW law. I anticipate that UD petitions will decline after RTW law passes because the law removes ambiguity about the legal status of such provisions.

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: RC, RD and RM cases. I analyze RC cases separately, yet combine RD and RM cases into the same analysis because both petitions challenge existing unions. 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 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 the transitions of petitions through the NLRB process.

Literature

A common approach to understand the effect of RTW law on unions is to perform a correlational analysis with geography as the unit of analysis. Hirsch (1980) regresses union membership and union coverage on RTW law by SMSA, and concludes that RTW law harms membership, but not coverage, suggesting that RTW law induces member defection. Hogler, Shulman and Weiler (2004) perform a state-level, cross-section regression 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 concept and passage of RTW predates the wave of public sector unionization that began in the early 1960s, which in part decouples the history of RTW law from decisions by government employees to unionize. The authors use the strength of public sector bargaining law and private sector union density at 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 population studies matches our hypothetical expectations and, assuming an absence of publication bias, are prima fascia evidence that RTW law negatively effects 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 (Holmes, 1998). The risk of rendering specious results increases with the size and complexity of the geographic unit.

Adopting 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.

Insight into the causal mechanism is revealed when the authors decompose the original ratio into three parts. Ellwood and Fine (1987) find no evidence that the union RC 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 (i.e. fewer petitions filed) or taking fewer RC petitions to the election phase (i.e. higher rates of petition withdrawals). The second finding implies 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 RD and RM petitions.

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 elections are the proper unit of analysis to study RTW law's effect on these 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 discrete choice by a RC petitioner to withdraw a petition correlates with the probability of an election win. Models are thus required that relax the assumption of independence between alternative outcomes at stages where strategic choices by petitioners factor into expectations about case outcomes. Ferguson (2008) concludes that RTW law has no effect on the union organizing process.

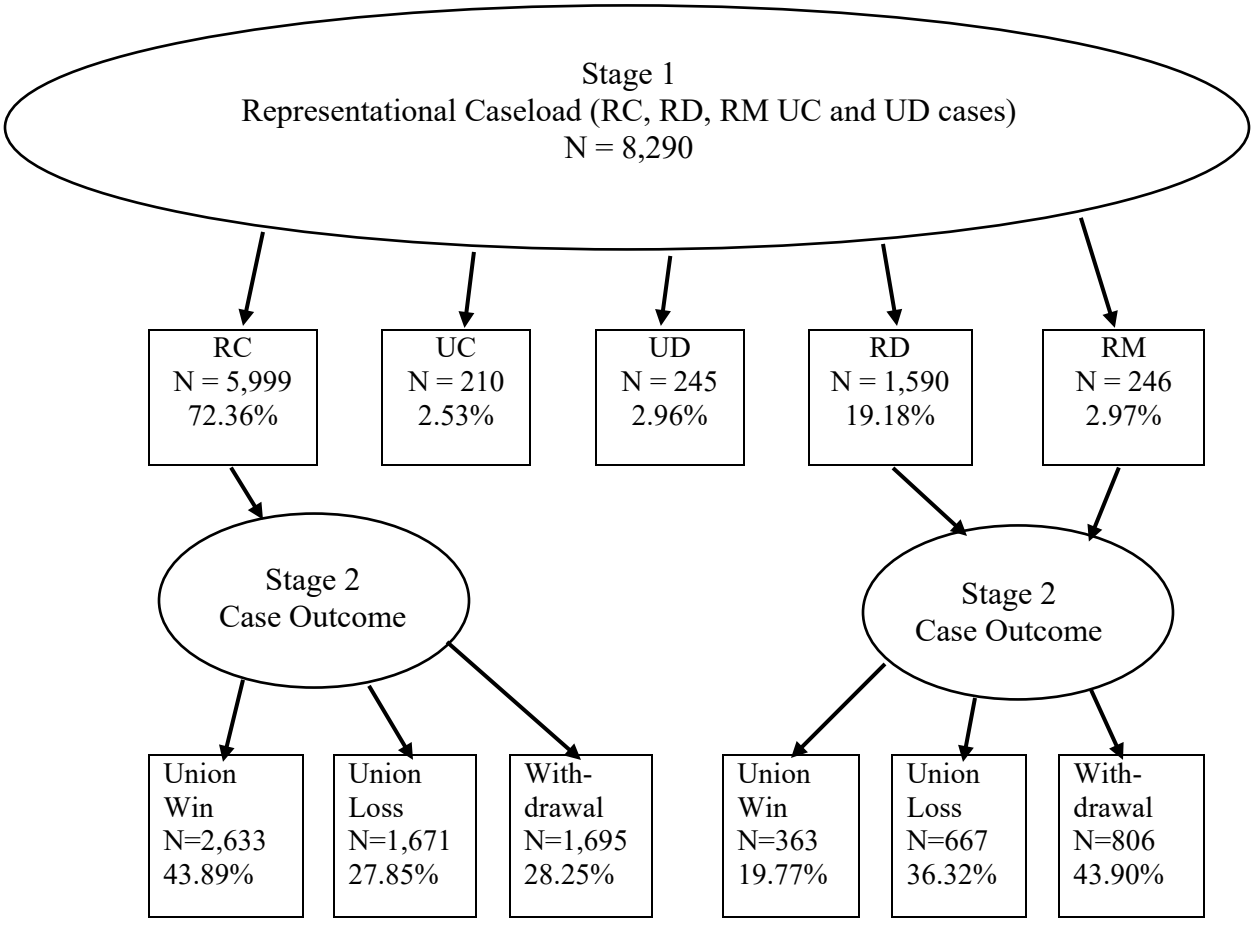
Empirical Model

I employ a difference-in-difference (DD) design whereby the treatment group are NLRB petitions filed in states that passed RTW law during the period of study, and the control group are petitions filed in the non-RTW states in the region that share a border with the treatment states. For this chapter, the treatment states include IN, KY, MI, WI and WV, and the control states are IL, MN, MO, OH and PA. Parallel trend tests indicate no interaction between time and treatment state status, suggesting a suitable comparison (see results from the tests in Appendix C).

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 (i.e. RC, RD, RM, UC and UD) that go before the NLRB. Since there is no reason to suspect strategic discrimination across petition types, I perform the test with a multinomial probit regression that assumes independence across the five exclusive outcomes. Stage two tests for a RTW law effect on the outcomes of cases requiring an election (i.e. RC, RD and RM). Following Ferguson (2008), in this stage I employ multinomial probit equations that allow for dependency between three outcomes: a petition withdraw, a union election win and a union election loss. Figure 1 illustrates the two-stages and provides statistics for the sample.

Figure 1. Analyses Stages and Sample Statistics



As per Figure 1, of the 8,290 eligible cases, the majority are union representation petitions (72.36 percent), followed by union decertification petitions (19.18 percent) and the remaining case types, RM, RD and UC at just under 3 percent each. Stage one tests whether RTW law is associated with a shift in this mix. Stage two tests for RC, RD and RM petition outcomes. Due to the small number of RM petitions, I combine RD and RM into one test. Thus, stage two tests for a RTW law effect on two situations: (1) the choice by a group of nonunion workers to be represented, and (2) the choice by a group of represented workers to end collective representation. 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. Yet, 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 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. For all analyses, the base equation is:

$$Y_{ist} = \beta_0 + \beta_1\lambda_t + \beta_2 R_s + \delta_0 (\lambda * R) + \mu_{ist}$$

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 main factors, and δ the estimated coefficient for the treatment. R is a dummy variable with the value 1 if the state has a RTW law at any time during the study period, zero otherwise. Symbol λ is a continuous year time variable. Symbol δ is the coefficient for the RTW law and time

interaction, equaling 1 for petitions filed after the passage of RTW law, zero otherwise. 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: RC, RD, RM, UC and UD. I use a multi-variate probit model, with RC the excluded category and independent errors assumed. 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. Union loss is the excluded category. For stage two, μ_{ist} are allowed to correlate to adjust for contingent strategic decisions across outcomes.

Table 2 provides summary statistics for the variables in the models at the two stages. Approximately 30 percent of the NLRB cases are in states that passed RTW law for both stages. At stage 1, 11.7 percent of the cases are under treatment conditions and at stage 2, 12.8 percent are in the treatment for RC cases while 9.2 percent of the RD-RM cases are subject to RTW law.

Table 2: Sample Summary Statistics for NLRB Analyses, Mean (s.d.)

	Stage 1	Stage 2	
	Full Sample	RC Sample	RD-RM Sample
Treatment	0.117 (0.321)	0.128 (0.334)	0.092 (0.289)
Size (log employees)		3.134 (1.281)	3.084 (1.295)
RTW State	0.301 (0.457)	0.300 (0.458)	0.301 (0.459)
Year	2012.532 .(3.104)	2012.709 (3.090)	2012.105 (3.092)
N	8,290	5,999	1,836

Regression Results

Table 3 examines the decline in non-RC petitions in detail by displaying tests for the effect of RTW law on the mix of NLRB representation-related petitions. Variable *Treatment* signifies the probabilistic shift in RD, RM, UC and UD filing relative to RC petitions associated with RTW law passage. Results indicate a statistically significant decline in RD, UC and UD petitions in a RTW context. Further tests (not displayed) indicate that this association with RTW law is unrelated to the size of the employee group. The marginal drop in RD petitions is 4.4 percentage points, which is a 22.6 percentage decline relative to pre-RTW law levels; UC petitions drop 1.4 percentage points, which is 48.3 percent below pre-RTW law levels; UD petitions drop 1.7 percentage points, for an estimated 56.7 percentage decline.

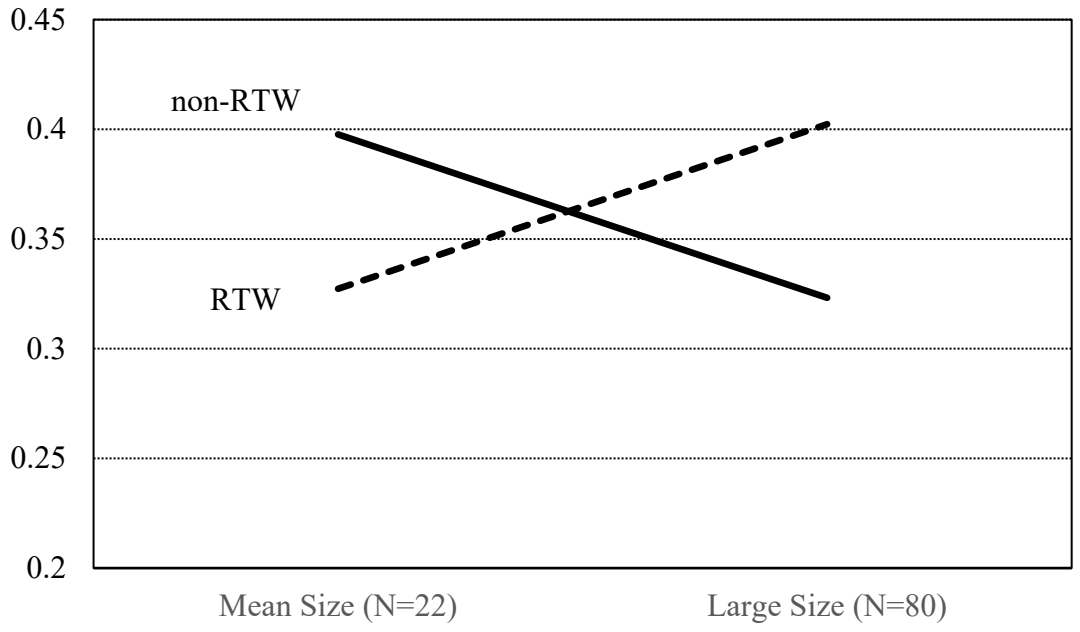
Table 3: Multinomial Probit Regression of RTW Law and NLRB Representation Petitions, 2008-2018.

	RD Petition	RM Petition	UC Petition	UD Petition
Treatment	-0.253**	0.238	-0.443**	-0.509**
	(0.095)	(0.169)	(0.160)	(0.194)
RTW State	0.009	-0.448***	0.108	0.012
	(0.067)	(0.121)	(0.115)	(0.104)
Year	-0.035***	-0.075***	-0.014	-0.062***
	(0.008)	(0.013)	(0.014)	(0.013)
Constant	69.454***	149.406***	26.484	121.694***
	(16.047)	(26.860)	(28.655)	(25.980)
LL	-6912.627			
N	8,290			

Notes: Equations include controls for four NLRB regions; robust standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

The *Treatment* coefficient for RM petitions is positive, indicating a 2.1 percentage point increase, or a rise of 84.8 percent, but the gain does not breach conventional levels of statistical significance. Given the drop in RD, UC and UD petitions, the share of RC petitions increases 5.2 percentage points, rising 7.2 percent from pre-RTW law levels. Figure 2 illustrates this proportionate shift.

Figure 2: Union RD-RM Election Loss Ratios by RTW Context and Size



These results imply that RTW law reduces certain kinds of NLRB petitions. The drop in UD petitions is unsurprising given that RTW law annuls union security clauses. Unexpected was the drop in RD petitions, which indicate fewer attempts by dissenting unit members to terminate representation. The drop in UC petitions similarly suggests less internal friction over the composition of bargaining units. While I conclude no RTW law effect on the proportion of RM petitions, take note of the comparably small number of RM cases (Figure 1). The size and direction of the *Treatment* coefficient in the RM

petition equation hints that a more powerful statistical test could find a positive RTW law effect on the frequency of RM petitions.

Tables 4 provides results for a RTW law effect on the outcomes of RC petitions.

Table 4: Multinomial Probit Regressions of RTW Law and Outcomes for RC Petitions, 2008-2018

	Model 1		Model 2	
	Union Win	Withdrawal	Union Win	Withdrawal
Treatment	0.097 (0.067)	-0.125 (0.071)	0.004 (0.134)	-0.083 (0.143)
Size (log employees)			-0.057*** (0.014)	-0.059*** (0.015)
Size X Treatment			0.034 (0.037)	-0.015 (0.041)
RTW State	-0.132** (0.045)	0.127** (0.047)	-0.139** (0.045)	0.124** (0.047)
Year	0.010 (0.006)	-0.015* (0.006)	0.008 (0.006)	-0.014* (0.006)
Constant	-20.267 (11.809)	29.883* (12.556)	-15.742 (11.885)	28.680* (12.565)
ρ	-0.971*** (0.003)		-0.986*** (0.015)	
LL	-6437.298		-6376.355	
N	5999		5978	

Notes: Robust standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Consistent with Ferguson (2008), the win-loss-withdrawal outcomes for RC petitions are statistically undisturbed by RTW law. Note, however, that the *Treatment* coefficient for the RC withdrawal equation is negative, and would breach conventional

levels of statistical significance (α=0.05) in a one-tailed test. Thus, I find no evidence that RTW law is associated with fewer RC petitions moving toward an election, or any indication that unions retreat from organizing in the immediate years following RTW, or that the union certification win rate suffers. Using Model 1 of Table 4, the estimated probabilities for the three outcomes for control versus treatment groups are as follows: union win, 43.9 versus 44.7; union loss, 27.5 versus 29.3; petition withdrawal, 28.6 versus 26.0.

Table 4, Model 2 is a set of multinomial probit equations that test for whether RTW law is associated RC 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 RC 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 RC election process. To test this supposition, 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 indicate that while larger groups are less likely to withdrawal RC petitions, they are also more likely to vote against unionization in a RC election. Nonetheless, this association is 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 RD and RM petitions. In Model 1, the statistically significant *Treatment* coefficient indicates that RTW law affected case outcomes. In a RTW context, petitioners are more likely to withdrawal RD-RM petitions, and of the cases that do go to an election, unions are more likely to lose.

Estimated probabilities for the three outcomes for control and treatment groups are as follows: union win, 20.0 versus 18.0; union loss, 36.3 versus 35.9; petition withdrawal, 43.7 versus 46.1. Thus, fewer petitions reach the election phase (more withdrawals), but for the cases that do go to an election, unions lose a larger share.

Table 5: Multinomial Probit Regressions of RTW Law and Outcomes of RD-RM Petitions, 2008-2018.

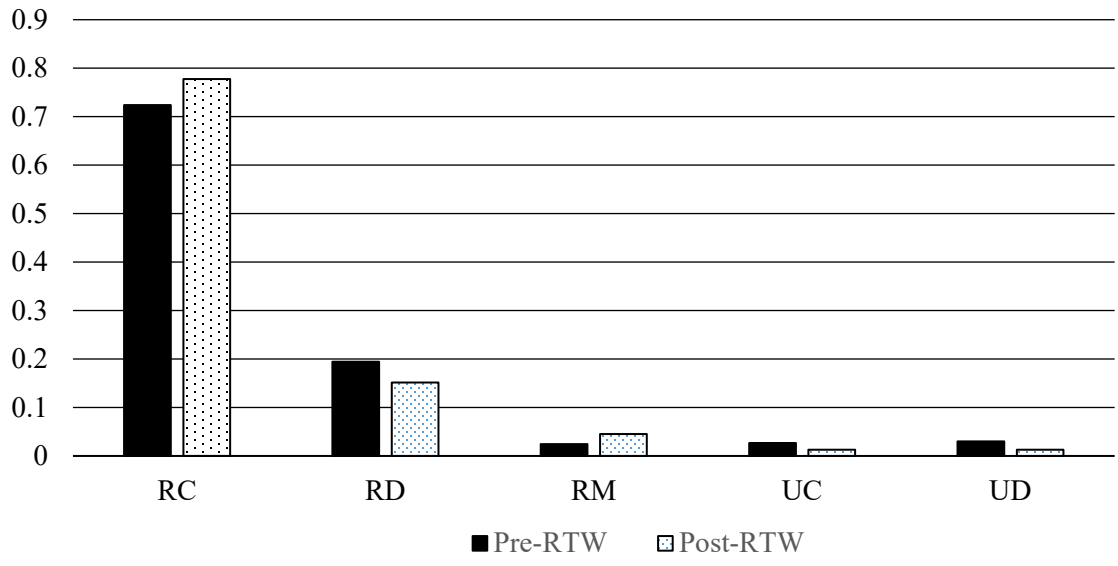
	Model 1		Model 2	
	Union Win	Withdrawal	Union Win	Withdrawal
Treatment	-0.402**	0.423**	-0.492	1.060***
	(0.139)	(0.126)	(0.373)	(0.281)
Size (log employees)			0.341***	-0.132***
			(0.029)	(0.024)
Size X Treatment			0.014	-0.208*
			(0.106)	(0.085)
RTW State	0.359***	-0.230**	0.383***	-0.244**
	(0.082)	(0.075)	(0.083)	(0.076)
Year	0.015	-0.049***	0.019	-0.049***
	(0.012)	(0.010)	(0.012)	(0.011)
Constant	-30.175	99.028***	-40.631	99.625***
	(24.238)	(21.047)	(24.721)	(21.387)
ρ	-0.994***		-0.960***	
	(0.012)		(0.012)	
LL	-1905.703		-1817.663	
N	1836		1819	

Notes: Robust standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

The size of the employee group involved in the petition is a moderating factor. The probability of a RD-RM petition withdrawal declines as the employee group grows.

Using Table 5, Model 2, the estimated probabilities of union win, union loss and petition withdrawal in a RTW context are 9.0 percent, 32.7 percent and 58.3 percent, respectively, for an average sized employee group (N=22). Raise the size of the group by 1 standard deviation (N=80), and the union win, union loss, and withdrawal estimates are 18.9 percent, 40.2 percent and 40.8 percent, respectively. Since a union win and a withdrawal both preserve the organization in RD-RM cases, what likely matters most for the union movement is the loss percentages. I conclude that RTW law threatens larger employee groups more than smaller ones through the NLRB decertification process. Figure 3 plots the union loss rates in RTW and non-RTW environments by size of employee group.

Figure 3: NLRB Case Distribution Pre and Post RTW Law



Chapter Conclusions

This chapter tested the effect of the passage of RTW law on NLRB representation petition submissions and outcomes. Building from the extant literature and Olson's (1965) economic-based theory on collective action, our expectation was that RTW law

negatively affects the prospects for new union certification and increases the risk of existing union decertification.

Contrary to these predictions, I detect no drop off in the rate of RC petition filings under RTW. What does change is the relative frequency of RD, UD and UC petitions, all of which decline as a proportion of the NLRB representation caseload. I estimate that RD petitions, which are the most threatening to unions, experience a 22.6 percent decline in the years following RTW law. Moreover, UC petitions, which typically captures internal membership discord, drop by 48.3 percent. These findings suggest that the open shop acts as a pressure release mechanism that reduces demands by union dissenters to petition for the termination of union representation or to challenge the composition of the existing bargaining unit.

RTW law does seem to affect RD-RM case outcomes. RD-RM petition withdrawals increase by 5.5 percent after RTW law passage. From the perspective of the union movement, fewer RD petitions (stage 1, Figure 1) and higher rates of withdrawals (stage 2, Figure 1) is a positive consequence of RTW law. Offsetting this salutary result is that unions win fewer RD-RM cases that go to an election. For unions operating under RTW law, RD-RM election wins drop by 10.0 percent, and larger bargaining units lose more frequently.

Labor Union Response to RTW Law

Under an open shop environment, the threat of free-rider behavior intensifies because membership loss imposes a higher cost on the organization in terms of both finances and internal solidarity. Does this affect union behavior? For instance, to reinforce the value of membership to persons in the bargaining unit, do unions spend

more resources on representational activities? Alternatively, do unions retreat from politics to placate politically conservative members of the unit that object to the partisan use of union resources? This section examines how unions respond to RTW law, and by extension, how the open shop shapes the role of unions in society.

Literature

Empirical tests for an association between RTW law and union strategic behavior are scarce. Moore and Newman (1985) do not mention the topic in their review. In a subsequent review, Moore (1988) provides one relevant citation. Bennet and Johnson (1980) perform a single year, cross-section study using aggregate state data and find no evidence that RTW law is associated with lower union dues, lower officer compensation and lower union operational costs.

Research from individual level data hint at a union response to RTW law. Budd and Na (2000) use Current Population Survey (CPS) data to estimate wage differentials between union members and covered non-members in states with RTW laws. Finding a member premium, the researchers speculate that in RTW contexts members and covered non-members receive unequal treatment. These findings imply that labor unions adjust their representational role in an open shop environment, perhaps by relaxing their duty of fair representation obligations. Sobel's (1995) analysis of CPS data implies that whatever adjustments unions make, they do appear to prevent widespread member defection.

Our main contribution is to directly test for a union response to RTW law. Like Ichniowski and Zax (1991), our units are organizations, yet here the organizations are unions instead of employers. Further, this study examines finances, rather than members.

Although labor unions prefer a growing membership, Olson's (1965) theory of collective action centers on economics, which is best captured by analyzing union finances.

Data, Measures and Hypotheses

The data are from the annual labor-management (LM) forms filed with the Office of Labor Management Standards (OLMS), U.S. Department of Labor. I compiled OLMS data for years 2007 to 2016, inclusive, for LM-2 filers. Organizations that are not subject to RTW law were excluded, such as those regulated by the Railway Labor Act, unions in government and organizations with a foreign address. The OLMS data includes three levels of organization: peak, intermediary, and local. Our analysis is limited to local organizations. Observations that provided data on terminal reports were omitted to reduce extreme measurement outliers.

The first test is whether unions experience financial harm under the open shop. Two measures derived from the OLMS financial data are employed to track the financial performance of unions:

Equity per Member = (Assets-Liabilities)/Members

Income Margin = (Receipts-Disbursements)/Receipts

The first, *Equity per Member* proportionately adjusts to changes in the size of the membership base. Variation in the measure might be due to change in the financial statistics (i.e. numerator) or because of change in membership size (i.e. denominator). Notably, the measure can read as financial improvement because of a decline in membership - an outcome that is usually undesirable for unions. In contrast, *Income Margin* is composed from the two main components of an income statement, and therefore tracks union financial health independent of membership size. The alternative

hypotheses testing whether unions experience greater financial hardship in the open shop context are:

H1 (a): *Equity per Member* will decline following RTW.

H1 (b): *Income Margin* will decline following RTW.

The second inquiry is unions' response to RTW law. Central to theorizing the union response is in appreciating an ongoing dilemma for union leaders. In any union, a sizable fraction of members want dues spent in situ for improving wages, hours and conditions of employment. Yet, achieving and sustaining gains requires political involvement to protect bargaining rights and to advance broader workplace protections. Further, new member organizing requires resources. Hence, union leaders must balance demands by dues payers for parochialism against the use of resources for societal betterment and organizational growth. Our expectation is that the open shop will lead to a rebalance of union resources in order to minimize membership defection.

Conceptually, I anticipate that unions will become more "member-focused" and less "movement-focused." The movement versus member contrast has an enduring lineage in the literature. Furniss (1925: 391) wrote that the "most significant distinction, in our opinion, is that between unions which are unconscious that their efforts tend toward a new social order and so adapt their strategy solely to the immediate situation, and unions that are conscious of their desire for a new order, and so base their strategy on more fundamental considerations." Generations later, scholars embraced the term "social movement unionism" to classify unions that direct resources toward activities that uplift the working class (Eimer, 1999; Fairbrother, 2008). The conceptual alternative to social movement unionism, "Business unionism," refers to labor organizations that prioritize

member needs, are insular in character, and that demur committing to campaigns that seek widespread progressive reform (Hattam, 1993; Hoxie, 1917). For our purposes, the terms "movement-focused" and "social movement unionism" are proximate analogs, as are "member-focused" and "business unionism." Our general hypothesis is that under a RTW environment unions become more member-focused and less movement-focused.

Operationally, the extent that unions spend resources toward representing existing dues payers, they are "member-focused;" and the extent that unions spend resources on politics or apolitical social causes, they are "movement-focused." Our expectation is that increased pressure to sustain a critical mass of dues payers under the open shop will pressure unions away from a movement-focused strategy and toward member-focused representation, and this shift should be observable by tracking union expenditures.

I test for an adjustment in union strategy using OLMS data. The OLMS financial data categorizes union discretionary expenditures into five major categories for labor unions that file LM-2 forms. The categories are:

1. *Representation Activities*: Disbursements associated with the negotiation of collective bargaining agreements and their enforcement. This category includes disbursements for efforts to organize new units or to prevent unit decertification.
2. *Political Activities*: Disbursements associated with political contributions, lobbying for the passage or defeat of laws, influencing the selection, nomination, election, or appointment of anyone for political office, political communications with unit members and their families, registration, get-out-the-vote and voter education campaigns.

3. *Contributions, Gifts and Grants*: Disbursements to all entities and individuals associated with charities or scholarships, such as for medical research, community development, job retraining, education, disaster relief, and athletic and youth sponsorships.
4. *General Overhead*: Disbursements for expense items that cannot be allocated to the other categories, which includes facility charges, utilities and personnel for such activities as building maintenance and security.
5. *Union Administration*: Disbursements relating to the nomination and election of officers, membership meetings, union disciplinary proceedings, the administration of trusteeships and apprenticeships, and non-political member education.

I standardized these financial measures in two ways: first, as a proportion of the total discretionary expenses, and second, as a per capita member allocation. Testing for a resource reallocation, the alternative hypotheses are:

H2 (a): Unions will shift resources toward *Representational Activities* following RTW.

H2 (b): Unions will shift resources away from *Political Activities* following RTW.

H2 (c): Unions will shift resources away from *Contributions, Gifts and Grants* following RTW.

H2 (d): Unions will shift resources away from *General Overhead* following RTW.

H2 (e): Unions will shift resources away from *Union Administration* following RTW.

Our final area of inquiry is to explore for factors that counter rational, cost-benefit opportunism. It is well known that workers do not behave as purely rational individuals in accord with Olson's (1965) framework. If that were true, unions would not exist in states with RTW law. This does not happen, implying the existence of sociological or psychological bonds that override economic rationalism. Of the possible factors, one is institutional identification.

The OLMS data has two measures that likely correlate with institutional identification strength: building trade affiliation and organization size. The building trades nurture member identification through jointly-sponsored apprenticeship programs, hiring hall traditions and union-administered benefits. Regarding size, smaller organizations typically allow for greater levels of interpersonal interaction, which should positively associate with institutional identification. In terms of the response to the open shop, the alternative hypotheses are:

H3 (a): RTW law will have less effect on affiliates of the building trades.

H3 (b): RTW law will have less effect on smaller union locals.

Logarithmic transformations of the variables reduce measurement skew and allows for the expression of results as marginal percentages. Table 6 lists the variables, definitions and descriptive statistics.

Table 6: Measures, Definitions and Summary Statistics for OLMS Analyses		
Measure	Definition	Mean (s.d.)
Treatment	RTW law in effect = 1, 0 otherwise.	0.078 (0.268)
RTW State	State that passed RTW law = 1, 0 otherwise	0.304 (0.460)
Equity Per Member	Log (((Assets-Liabilities) /Members) + *k*)	8.402 (0.452) [3141.939]
Income Margin	Log (((Receipts-Disbursements)/Receipts) + *k*)	0.094 (0.173) [0.007]
Representational Exp. Ratio	Log ((Representational Exp./Total Exp.) + *k*)	1.147 (0.067) [0.410]
Political Exp. Ratio	Log ((Political Exp./ Total Exp.) + *k*)	-4.916 (1.650) [0.021]
Contributions Exp. Ratio	Log ((Contributions Exp./ Total Exp.) + *k*)	-3.689 (1.067) [0.041]
Administration Exp. Ratio	Log ((Administration Exp./ Total Exp.) + *k*)	-1.069 (0.496) [0.277]
Overhead Exp. Ratio	Log ((Overhead Exp./ Total Exp.) + *k*)	-0.919 (0.433) [0.251]
Representational Exp. Per Member	Log ((Representational Exp./Members) + *k*)	5.373 (1.099) [1268.126]
Political Exp. Per Member	Log ((Political Exp./ Members) + *k*)	1.151 (2.190) [26.562]
Contributions Exp. Per Member	Log ((Contributions Exp./ Members) + *k*)	2.493 (1.506) [71.534]
Administration Exp. Per Member	Log ((Administration Exp./ Members) + *k*)	4.888 (1.086) [276.132]
Overhead Exp. Per Member	Log ((Overhead Exp./ Members) + *k*)	4.695 (1.364) [405.967]
Trades	Affiliate with the building trades=1, 0 otherwise	0.402 (0.490)
Members/1000	Members/1000	1.991 (4.524)
Notes: "Total Exp." is the sum of the five discretionary expenses; *k* is a constant for each variable that eliminates negative and zero values; untransformed means are in [brackets].		

As per Table 6, slightly over 30 percent of the observations are in the states that passed RTW law and nearly 70 percent are in control states. Of the total observations, 7.8 percent are subject to the treatment. Proportionately and nominally, unions dedicate the largest amounts of discretionary resources to representation (41.0 percent), and the smallest to political activities (2.1 percent). Expenditures for apolitical (e.g. charitable) causes are about twice as large as for politics. Administration and overhead expenses are similar in magnitude, each about a quarter of the discretionary budget. Unions in the building trades are 40 percent of the sample. Average membership size is nearly 2,000 persons, with a standard deviation greater than 4,500.

Analytical Method

Our analytical approach is a quasi-experimental, difference-in-difference (DD) design. Our DD analysis compares two groups: a treatment group in states where RTW took effect, and a control group from states where RTW had not passed during the time period of the study. Using these criteria, the treatment states are IN, MI, WI and WV, and the control states are MO, KY, OH, IL, MN and PA.[26] The base equation for the DD tests is:

$$Y_{ist} = \beta_0 + \sum \beta_j \lambda_t + \beta_2 R_s + \delta_0(\lambda * R)_{st} + \alpha_i + \alpha_s + \mu_{ist}$$

Where Y is the variable of interest for *i* unions in *s* states over *t* time periods and the β are estimated coefficients for the intercept and main factors. R is a dummy variable with the value 1 for states with a RTW law at any time during the study period, zero otherwise. Symbol λ is a categorical indicator for each *j* year. Symbol δ is the estimated

[26] Unlike the analysis of NLRB data, Kentucky is a control state in this analysis because it did not enact RTW law until 2017.

coefficient for the RTW law and year interaction (i.e. the treatment), equaling 1 for every full year that RTW law was in effect.[27] In these multi-level regressions, the α_i and α_s are random intercepts for union and state, respectively, and are included to capture effects from unmeasured factors at those levels (Rabe-Hesketh and Skrondal, 2008). Including random intercepts in panel data designs reduces the standard error deflation effect caused by serial correlation (Bertrand, Duflo and Mullainathan, 2004), thus reducing the chance of committing Type I hypothesis-testing error. The μ are unexplained sample errors.

Regression Results

Table 7 presents the tests for H (1), whether the open shop financially harms unions. Table 7 presents two variables: *Equity per Member* and *Income Margin*.[28] Each variable has three models: first, a base equation that features the treatment factor; second, a model that parses out the building trades from other unions; and third, a model that tests for a differential RTW effect along organization size.

[27] δ is structured as a dichotomous cut-point for RTW policy, which assumes discrete RTW effects beginning during the first full year of enactment. As discussed, there are reasons to suspect that the enactment date is an imprecise cut-point that has lagged effects. To explore this possibility, the λ*R interaction was modified to be a linear monotonic variable with 0 as the year of enactment, 1 the year after, 2 the year after that, and so forth. This did not substantively change δ, and so the results for the dichotomous model is used.

[28] A third measure, *Income per Member* ((Receipts-Disbursements)/Members) was tested but was statistically insignificant across all models and thus not displayed.

Table 7: RTW Law and Union Organization Finances, 2007-2016.						
	Equity Per Member			Income Margin		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treatment	0.009 (0.009)	-0.023* (0.010)	0.016 (0.010)	-0.015* (0.007)	-0.011 (0.008)	-0.016* (0.008)
RTW State	-0.016 (0.043)	-0.010 (0.036)	-0.023 (0.040)	-0.004 (0.010)	-0.005 (0.010)	-0.004 (0.011)
Trades		0.175*** (0.022)			-0.013** (0.005)	
Trades X RTW Law		0.099*** (0.017)			-0.015 (0.013)	
Members (1,000)			-0.012*** (0.001)			-0.000 (0.000)
Members X RTW Law			-0.005* (0.002)			0.000 (0.002)
Constant	8.328*** (0.027)	8.258*** (0.024)	8.352*** (0.025)	0.059*** (0.008)	0.065*** (0.008)	0.059*** (0.008)
LL	-29.6	22.6	37.2	4438.9	4444.5	4439.0
N, Organizations	1,673	1,673	1,673	1,674	1,674	1,674
N, Observations	12,619	12,619	12,619	12,628	12,628	12,628
Notes: all models include fixed year identifiers and state and union organization random components; standard errors in parenthesis; * = $\alpha < .05$ level; ** = $\alpha < .01$; *** = $\alpha < .001$						

In terms of *Equity per Member*, there is a significant decline after the passage of RTW law, but the effect is concentrated on the non-trades. The Model 1 coefficient for *Treatment* is statistically indistinguishable from zero, suggesting that across all union types there is no change in *Equity per Member*. Parsing the trades from the non-trades in Model 2, however, produces an estimated 2.3 percent decline in the measure for the non-trades, along with an offsetting 9.9 percent increase for the trades. This result is consistent with H1 (a) and H3 (a) by providing evidence that unions are financially

harmed, but that only the non-trades are affected. Similarly, results from Model 3 infer that RTW law more acutely affects larger unions, which supports H3 (b). Unions with larger memberships generally have less *Equity per Member* than unions with fewer members, and under RTW law, *Equity per Member* is 1.7 percent lower as union locals increase by 1000 member increments.

Income Margin tracks the proportionate level of surplus (or deficit) from revenues and expenses data. Supporting H1 (b), Model 4 indicates a 1.5 percent decline in the measure under RTW law for all unions in the sample - trades and non-trades, larger and smaller organizations. What this finding implies is that union's broadly experience an increase in expenses, decrease in revenue, or both after RTW law passage.

Tables 8 to 12 examine labors' strategic response by testing whether the open shop context affects union allocation of discretionary resources. I display results for five expense categories: representation, political, contribution, overhead and administration. I examine each expense in two ways: (1) as a proportion of the total discretionary expenditures and (2) as a per capita member expenditure. The models in each table replicate the structure in Table 7, featuring a base model, a model that parses out the building trades and a model that tests for a differential effect along organization size. Table 8 displays the results for representation expenses.

Table 8: RTW Law and Representation Expenditures, 2007-2016						
	Representation Exp. Ratio.			Representation Per Member		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treatment	-0.002 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.001 (0.018)	-0.022 (0.021)	0.009 (0.020)
RTW State	0.004 (0.009)	0.005 (0.008)	0.004 (0.009)	-0.047 (0.065)	-0.048 (0.063)	-0.060 (0.067)
Trades		0.033*** (0.003)			-0.053 (0.058)	
Trades X RTW Law		-0.003 (0.002)			0.071* (0.034)	
Members (1,000)			-0.001*** (0.000)			-0.035*** (0.002)
Members X RTW Law			-0.000 (0.000)			-0.008 (0.005)
Constant	1.148*** (0.005)	1.134*** (0.005)	1.149*** (0.006)	5.170*** (0.038)	5.191*** (0.044)	5.241*** (0.039)
LL	24225.0	24277.5	24234.5	-9406.0	-9403.5	-9276.3
N, Organizations	1,674	1,674	1,674	1,674	1,674	1,674
N, Observations	12,628	12,628	12,628	12,628	12,628	12,628
Notes: all models include fixed year identifiers and state and union organization random components; standard errors in parenthesis; * = $\alpha < .05$ level; ** = $\alpha < .01$; *** = $\alpha < .001$						

Unlike other expense categories, theory predicts that allocations for representation activities will increase after RTW law passes. Table 8 results do not fully support this hypothesis. Coefficients for *Treatment* are statistically insignificant for all models. Where there is evidence of a shift toward a member-focused model, it is for the building trades and only in the equation predicting per capita expenditures. Contrary to H3 (a), the Model 5 results indicate that the trades allocate 7.1 percent more per capita resources toward representation in the open shop context. On both a proportionate and per capita

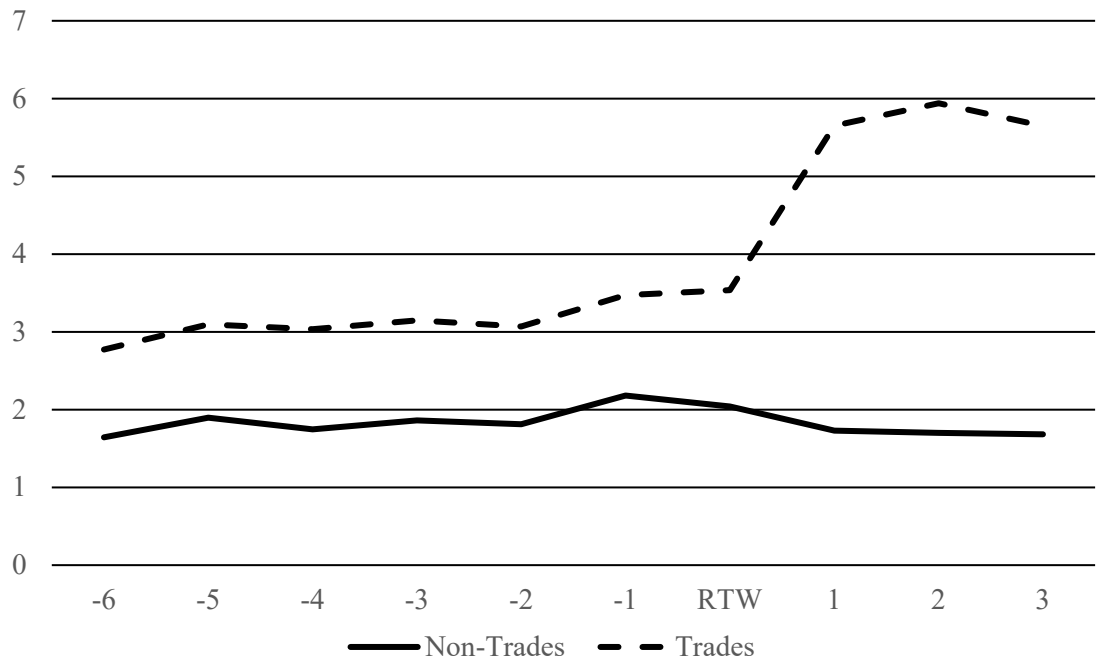
basis, larger unions spend less on representation (Models 3 and 6), which may reflect economies of scale, yet this is unrelated to RTW law.

Table 9: RTW Law and Political Expenditures, 2007-2016						
	Political Exp. Ratio			Political Per Member		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treatment	-0.015 (0.036)	-0.161*** (0.042)	-0.002 (0.041)	0.003 (0.045)	-0.209*** (0.052)	0.030 (0.050)
RTW State	0.075 (0.186)	0.084 (0.184)	0.084 (0.183)	0.039 (0.233)	0.065 (0.2274)	0.028 (0.235)
Trades		0.177* (0.077)			0.470*** (0.102)	
Trades X RTW Law		0.463*** (0.068)			0.671*** (0.085)	
Members (1,000)			0.026*** (0.004)			-0.025*** (0.005)
Members X RTW Law			-0.004 (0.010)			-0.017 (0.013)
Constant	-5.169*** (0.116)	-5.241*** (0.119)	-5.219*** (0.1015)	0.739*** (0.145)	0.549*** (0.146)	0.788*** (0.147)
LL	-17596.6	-17569.8	-17576.9	-20463.1	-20419.1	-20450.7
N, Organizations	1,674	1,674	1,674	1,674	1,674	1,674
N, Observations	12,628	12,628	12,628	12,628	12,628	12,628
Notes: all models include fixed year identifiers and state and union organization random components; standard errors in parenthesis; * = $\alpha < .05$ level; ** = $\alpha < .01$; *** = $\alpha < .001$						

Table 9 displays results for political expenditures. As theory predicts, unions retreat from political expenditures after RTW law passes, supporting H2 (b), but this happens only for the non-trades. The *Political Exp. Ratio* is 16.1 percent lower after RTW passes (Model 2), and the *Political Exp. per Member* is 20.9 percent lower for non-trade unions (Model 5). Unanticipated was the increase in political spending by building

trades. For the trades, the *Political Exp. Ratio* and *Political Exp. per Member* estimates grow by 46.3 percent and 67.1 percent, respectively, under open shop conditions (Models 2 and 5). Figure 4 illustrates the predicted per capita political expenses by the non-trades and trades in the band of years around RTW law. Note a parallel trend between the two groups in the years prior to RTW, and the divergence afterward.

Figure 4: Estimated Political Expense Per Member by Union Type



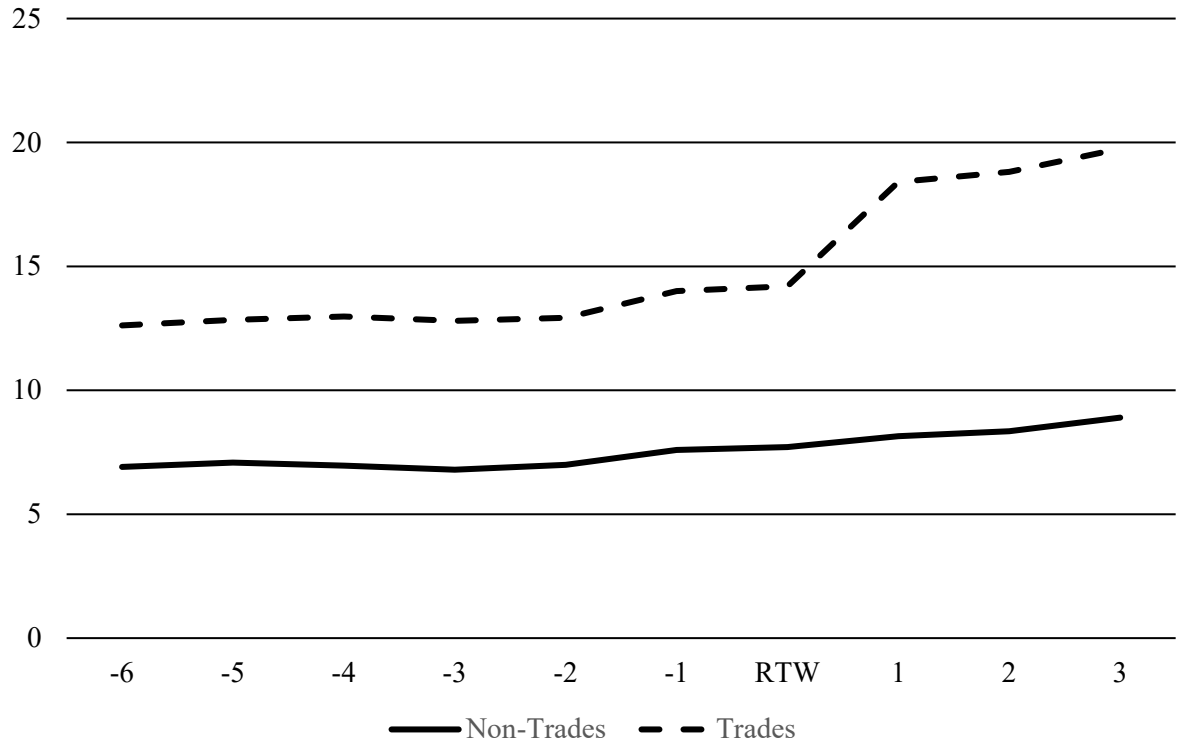
There is no evidence that the change in political expenditures is conditioned on union size. However, larger unions do spend a greater proportion of discretionary funds on politics (Table 9, Model 3), and less on politics when measured as a per capita expenditure (Model 6).

Table 10: RTW Law and Contribution Expenditures, 2007-2016						
	Contribution Exp. Ratio			Contribution Per Member		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treatment	0.055* (0.026)	0.041 (0.030)	0.070* (0.029)	0.059 (0.034)	-0.007 (0.039)	0.095* (0.038)
RTW State	-0.038 (0.055)	-0.017 (0.053)	-0.041 (0.055)	-0.121 (0.093)	-0.085 (0.064)	-0.131 (0.093)
Trades		0.235*** (0.050)			0.569*** (0.069)	
Trades X RTW Law		0.046 (0.049)			0.211** (0.064)	
Members (1,000)			-0.008** (0.003)			-0.030*** (0.004)
Members X RTW Law			-0.009 (0.007)			-0.023* (0.010)
Constant	-3.686*** (0.034)	-3.782*** (0.038)	-3.670*** (0.034)	2.331*** (0.057)	2.109*** (0.053)	2.391*** (0.058)
LL	-13262.5	-13250.6	-13257.9	-16829.7	-16789.8	-16795.4
N, Organizations	1,674	1,674	1,674	1,674	1,674	1,674
N, Observations	12,628	12,628	12,628	12,628	12,628	12,628
Notes: all models include fixed year identifiers and state and union organization random components; standard errors in parenthesis; * = $\alpha < .05$ level; ** = $\alpha < .01$; *** = $\alpha < .001$						

Table 10 displays the results for apolitical contributions. Contrary to H2 (c), the proportion of expenses dedicated to apolitical causes increases by 5.5 percent after RTW law (Model 1). Models 2 and 3 suggest the effect is shared across affiliate type and between large and small unions. On a per member basis, expenditures for apolitical causes are 21.1 percent higher for the trades following RTW (Model 5). This disparity between the trades and non-trades is illustrated in Figure 5. Note the disproportionate

shift in per capita expenditures toward apolitical causes by the trades against the relatively stable rise in the same expenses by the non-trades.

Figure 5: Estimated Contribution Expense Per Member by Union Type



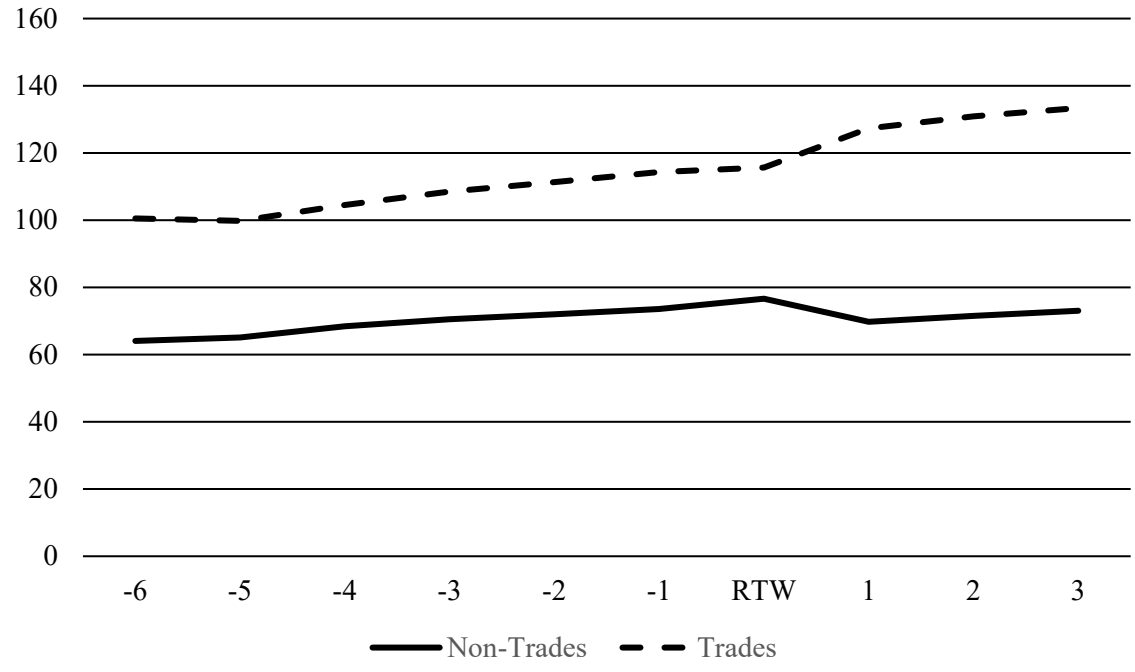
Smaller unions give more, both proportionately and as a per capita expenditure. The results offer some evidence that RTW law is a factor; estimated per member contributions decline by 2.3 percent for every 1000 members under the open shop (Model 6). Thus, H2 (c) is supported insofar that it is conditional on organization size; larger unions reduce contributions to apolitical causes, a result that also supports H3 (b). Aside from this exception, however, the results contradict expectations.

Table 11: RTW Law and Overhead Expenditures, 2007-2016						
	Overhead Exp. Ratio			Overhead Per Member		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treatment	-0.031*** (0.008)	-0.041*** (0.010)	-0.030** (0.010)	-0.091*** (0.023)	-0.148*** (0.027)	-0.076** (0.026)
RTW State	-0.032 (0.057)	-0.028 (0.057)	-0.032 (0.057)	-0.181 (0.178)	-0.162 (0.176)	-0.202** (0.168)
Trades		0.080*** (0.021)			0.392*** (0.065)	
Trades X RTW Law		0.030 (0.016)			0.178*** (0.044)	
Members (1,000)			0.000 (0.001)			-0.040*** (0.003)
Members X RTW Law			-0.000 (0.002)			-0.012 (0.007)
Constant	-0.913*** (0.036)	-0.945*** (0.037)	-0.913*** (0.036)	4.546*** (0.111)	4.388*** (0.113)	4.625*** (0.105)
LL	421.9	431.5	422.0	-12560.1	-12533.1	-12456.6
N, Organizations	1,674	1,674	1,674	1,674	1,674	1,674
N, Observations	12,628	12,628	12,628	12,628	12,628	12,628
Notes: all models include fixed year identifiers and state and union organization random components; standard errors in parenthesis; * = $\alpha < .05$ level; ** = $\alpha < .01$; *** = $\alpha < .001$						

Table 11 presents the findings on overhead expenses. The coefficient for *Treatment* is negative and statistically significant across all models, providing strong evidence that unions respond to RTW law by reducing overhead. The response, however, is limited to the non-trades (Model 2), providing support for H3 (a) and corroborating the findings on union finances in Table 7. Evidently, the open shop most negatively affects the non-trades financially, and the non-trades pragmatically respond by reducing

overhead. The trade versus non-trade difference is most visible in the per capita member equations, and illustrated in Figure 6.

Figure 6: Estimated Overhead Expense Per Member by Union Type



As per Model 2, RTW law is associated with 4.1 percent lower overhead as a proportion of discretionary expenses for the non-trades. Model 4 predicts that overhead is an average 9.1 percent lower for all organizations after RTW law. The Model 5 coefficient for *Treatment* indicates an estimated 14.8 percent reduction in the per capita member expenditure for overhead after RTW law passes for the non-trades, which is offset by the 17.8 percent higher per capita expense for the trades. The overhead reduction response does not vary along union membership size (Models 3 and 6), although larger unions generally spend less on overhead per member (Model 6).

Table 12 displays results for the association between RTW law and administrative expenses. What is remarkable about this set of tests is the absence of evidence that RTW law affects cost items classified under administration. The coefficient for *Treatment* is

statistically insignificant throughout. The building trades spend significantly more on administration, which is likely due to the cost of apprenticeships that fall within the administration expense category. Moreover, reflecting economies of scale, the per member administrative expenses decline as organization size increases.

Table 12: RTW Law and Administrative Expenditures, 2007-2016

	Administration Exp. Ratio			Administration Per Member		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treatment	-0.002 (0.010)	0.010 (0.012)	0.003 (0.012)	-0.020 (0.021)	-0.031 (0.024)	-0.008 (0.023)
RTW State	0.048 (0.056)	0.052 (0.053)	0.048 (0.056)	-0.008 (0.107)	0.006 (0.089)	-0.206 (0.105)
Trades		0.076** (0.023)			0.408*** (0.053)	
Trades X RTW Law		-0.027 (0.020)			0.035 (0.039)	
Members (1,000)			-0.002 (0.001)			-0.047*** (0.002)
Members X RTW Law			-0.001 (0.003)			-0.011 (0.006)
Constant	-1.074*** (0.035)	-1.105*** (0.035)	-1.071*** (0.035)	4.710*** (0.067)	4.549*** (0.059)	4.802*** (0.065)
LL	-2061.8	-2055.9	-2061.0	-10925.8	-10896.4	-10739.2
N, Organizations	1,674	1,674	1,674	1,674	1,674	1,674
N, Observations	12,628	12,628	12,628	12,628	12,628	12,628

Notes: all models include fixed year identifiers and state and union organization random components; standard errors in parenthesis; * = $\alpha < .05$ level; ** = $\alpha < .01$; *** = $\alpha < .001$

Supplemental Results: DDD Models

One possible extension to the DD approach is to add an additional differencing layer by including a comparable class of union organizations that exist in both the control and treatment environments, but that are unaffected by RTW policy. This sub-section reports on difference-in-difference-in-difference (DDD) models that treat the building trades as the control organizations. While the trades are under the purview of the LMRA, and therefore subject to RTW law, the trades enjoy a considerable degree of structural shelter from the effects of RTW law due to apprenticeship training, hiring provisions in multi-employer agreements and the hiring hall deployment system. These institutions make it highly unlikely that non-members of the trades can free ride.

The estimated treatment effect in a DDD model is the coefficient of a three-way interaction between RTW state, time, and non-trade union organizations (see Appendix B). Table 13 provides the results for all five categories of expenses as expressed as a ratio of total discretionary spending. Table 14 displays results for the five categories of expenses expressed on a per member basis.

Table 13: RTW Law and Administrative Expenditures Per Total Discretionary Exp. (log), 2007-2016					
	Model 1 Representation	Model 2 Political	Model 3 Contributions	Model 4 Overhead	Model 5 Administration
Treatment	-0.000 (0.003)	-0.408*** (0.074)	-0.152** (0.053)	-0.036* (0.018)	0.022 (0.022)
RTW State	-0.007 (0.009)	0.196 (0.216)	-0.472*** (0.088)	-0.058 (0.065)	0.028 (0.063)
Non-Trades	-1.068** (0.408)	19.825 (11.406)	-30.712*** (8.193)	-2.053 (2.702)	-1.817 (3.320)
Non-Trades X RTW State	0.021** (0.007)	-0.181 (0.172)	0.704*** (0.109)	0.049 (0.046)	0.039 (0.052)
Non-Trades X Time	0.001* (0.000)	-0.010 (0.006)	0.015*** (0.004)	0.001 (0.001)	0.001 (0.002)
RTW State X Time	-0.002 (0.002)	0.263*** (0.061)	0.151** (0.044)	-0.007 (0.014)	-0.013 (0.018)
Constant	1.172*** (0.006)	-5.116*** (0.130)	-3.397*** (0.048)	-0.856*** (0.039)	-1.020*** (0.037)
LL	24284.787	-17567.826	-13224.235	432.294	-2055.514
N, Organizations	1,674	1,674	1,674	1,674	1,674
N, Observations	12,628	12,628	12,628	12,628	12,628
Notes: all models include fixed year identifiers and state and union organization random components; standard errors in parenthesis; * = $\alpha < .05$ level; ** = $\alpha < .01$; *** = $\alpha < .001$					

Table 14: RTW Law and Administrative Expenditures Per Member (log), 2007-2016					
	Model 1 Representation	Model 2 Political	Model 3 Contributions	Model 4 Overhead	Model 5 Administration
Treatment	-0.060	-0.574***	-0.276***	-0.162**	-0.002
	(0.037)	(0.092)	(0.070)	(0.048)	(0.043)
RTW State	0.137	0.240	-0.565***	-0.194	0.050
	(0.110)	(0.269)	(0.122)	(0.199)	(0.116)
Non-Trades	2.726	35.493*	-14.416	6.484	12.235
	(5.665)	(14.178)	(10.784)	(7.407)	(6.578)
Non-Trades X RTW State	-0.302*	-0.282	0.740***	0.052	-0.070
	(0.129)	(0.228)	(0.152)	(0.145)	(0.118)
Non-Trades X Time	-0.001	-0.018*	0.007	-0.003	-0.006
	(0.003)	(0.007)	(0.005)	(0.004)	(0.003)
RTW State X Time	0.042	0.406***	0.244***	0.022	-0.015
	(0.030)	(0.076)	(0.058)	(0.040)	(0.035)
Constant	5.088***	0.932***	2.816***	4.780***	4.932***
	(0.059)	(0.160)	(0.066)	(0.120)	(0.066)
LL	-9400.752	-20415.223	-16777.260	-12532.547	-10894.371
N, Organizations	1,674	1,674	1,674	1,674	1,674
N, Observations	12,628	12,628	12,628	12,628	12,628
Notes: all models include fixed year identifiers and state and union organization random components; standard errors in parenthesis; * = $\alpha < .05$ level; ** = $\alpha < .01$; *** = $\alpha < .001$					

In these models, the coefficient for *Treatment* is the estimated RTW law effect on non-trade unions. Reflecting the findings from the DD analyses, the most compelling effect of RTW law is the decline in resources directed at political activities. Our models estimate an expense decline of 40.8 percent as a proportion of all discretionary expenses, and a 57.4 percent decline when political expenditures are standardized on a per member basis. The estimated decline in apolitical contributions is 15.2 percent as a proportion of the total and 27.6 percent on a per member basis. Finally, overhead expenses decline as a

proportion of total discretionary expenses by 3.6 percent. When overhead is standardized on a per member basis, the reduction is estimated at 16.2 percent under RTW law. There is no evidence that representation and administration expenses change with RTWE law.

The DDD estimates for reductions for politics and apolitical contributions are larger in magnitude than the DD analyses. The reason is that the building trades are a comparison group and part of the counterfactual in the DDD analyses, whereas in the DD analyses the building trades are a category of union organization that RTW law might influence. Accepting the more compelling DDD estimates carries the assumption that the building trades were behaviorally neutral about RTW law. This is unlikely. In every context, the building trades actively opposed the passage of RTW law. For the trades, the passage of RTW law represented a potential start to a larger anti-union agenda that could eventually touch policies that have a significant bearing on the building trades, such as prevailing wage laws. It is plausible that the building trades did respond strategically to RTW law, and the non-neutral role of the building trades favors the DD results.

Chapter Conclusions

This second chapter tests for a negative financial effect of RTW law on unions, and examines how unions adjust to open shop conditions. For labor unions under the LMRA, the passage of RTW mandates the open shop, which intensifies the problem of securing contributions from all persons covered by the labor contract. Economic theory holds that the open shop harms unions financially because the ability to require agency fees from non-members ends, while the economic incentive for marginal members to defect increases. Our theoretical formulation posits that unions become more member-

focused and less movement-focused as a way to minimize member defection. I also test for whether the response is conditional along affiliate type and union organization size.

Results for the non-trades partly conform to theoretical expectations. Financially, the non-trades experience a decline in *Equity per Member* and in *Income Margin* in the open shop context. The reduction in *Equity per Member* grows with union size, implying that larger non-trade organizations liquidate assets to achieve financial sustainability. Consistent with expectations, the non-trades respond to RTW law by reducing overhead expenses. Economizing on organizational overhead is a practical step that does not signal a change in strategy.

Another response by the non-trades is to reduce efforts toward politics, perhaps as a way to limit objections over the partisan use of resources. Retreating from politics in consistent with less movement-focused representation, and might explain the negative association between RTW law and voter turnout (Feigenbaum, et al., 2018; Zullo, 2008) and other election outcomes that are typically sought by labor (Feigenbaum, et al., 2018).

Contrary to theory, there was no evidence that the non-trades shift additional resources toward representation, or that administration is cutback. In the short-run, these expenses appear fixed. Representation expenditures include allocations related to advancing member interests as well as new member organizing, so it is possible that some undetected resource shifting happens within this category. Significant reallocation within the representation category is possible, yet doubtful, given that the study included only local union bodies and new member organizing is usually the responsibility of peak or intermediary organizations. The absence of discernable change in representation

expenditures after RTW passes contradicts one popular argument for banning union security clauses, i.e. that the open shop forces unions to be attentive to member needs.

Apolitical contributions is a this category of expenses that are arguably the most symbolic of unions' involvement in community causes (Zullo, 2011; 2013), and thus theoretically the most susceptible to decline should unions shift away from a movement-focused societal role. Larger unions do reduce apolitical giving after RTW law passes, consistent with member-focused representation. For the trades, evidence indicates that after RTW law apolitical contribution expenditures grow. One possibility is that trade unions artfully repackage political spending in an apolitical form to defend against a change in law that more directly affects members.

For the non-trades, the totality of the results lend only partial support for a strategic shift from a movement-focused to member-focused role. Retreating from politics is consistent with a defensive maneuver to retain politically conservative members, and the decline in apolitical effort by larger union organizations fits the member-focused model. However, the results offer no evidence of an affirmative redirection toward representation.

One clear finding is that RTW law affects the building trades differently than the non-trades. The trades experience no change in the *Income Margin*, and possibly a gain in *Equity per Member* after RTW law. Further, there are no observable cuts in overhead for the trades. These findings imply that financially the trades are largely unaffected by RTW law. A possible explanation is that the hiring hall system and union-administered benefits structurally proscribe free rider behavior.

The trades do respond to RTW law. Expenditures on politics and apolitical contributions jump following enactment. While these findings conform to our definition of a shift toward movement-focused representation, it is also possible that these results reflect defensive posturing. Publicly funded projects represents a large share of work for the building trades. Maybe the passage of RTW law signals a more hostile environment towards unions, which the trades respond to by upgrading political activities and making more apolitical contributions that curry favor with decision-makers. In that vein, these results might be capturing preemptive efforts to protect other threatened legal protections that have stronger relevancy to the trades, such as prevailing wage laws.

Report Conclusion: What does RTW law do?

Overall, I conclude that RTW law is not the poison to collective action that unions feared and anti-union interest groups sought. The law will not end the union movement because forces much stronger than rational individualism drive collective behavior by workers. Nonetheless, the results do suggest that RTW law imposes a disparate burden on certain types of unions, and this may affect the character of the union movement.

Prior to the New Deal, the labor movement was largely comprised of smaller bargaining units, with members bonded by craft, occupation, location or market segment. Employers negotiated because these unions wielded enough power to force employers to the table. By obligating employers to bargain in good faith with unions, The National Labor Relations Act (NLRA) of 1935 gave workers with less power a path for securing a labor agreement, of which union security clauses were a valued component. With the backing of a popular U.S. President and an activist NLRB, the NLRA led to mass union organizing along major industries in the 1940s and 1950s.

Conceptually, RTW law nudges U.S. labor relations back in the direction of a pre New Deal institutional context in the sense that unions that struggled before the New Deal fare worse under RTW law. Larger groups of workers, with a heterogeneous skill set, organized in industries where occupational turnover is comparatively high rely on union security clauses to generate steady operational resources. Union security rules that mandate universal dues collection enable larger, diverse organizations in dynamic labor markets to sustain an effective organization. The same contractual rules are less critical for organizations of smaller units, especially those bonded by trade or occupation.

Several findings from this study suggest that RTW law challenges the New Deal industrial model of union organization. First, the reduction in risk from RD and UD petitions, while a robust finding, benefited smaller units more than larger units. Fewer RD petitions went to an election, but the ones that did were more likely to result in a union decertification, especially for larger organizations. Second, the results indicate that the non-trades experience a short-term decline in equity per member following RTW law passage, whereas the building trades do not. These findings are consistent with the premise that that where bonds of solidarity are strong, as one might find in a smaller unit of skilled workers, the organization is better able to withstand threats posed by free rider opportunism.

Third is the response to RTW law by the non-trades. In particular, the retreat from politics by the non-trades harkens back to the voluntarist traditions of the original American Federation of Labor. Voluntarism shelters labor against the vicissitudes of party politics by asserting no party allegiance. Sustaining friends in both camps of the two-party system protects labor against retribution from a hostile, rising party, while

guarding against stale and unresponsive allegiances to a friendly party. The evidence herein suggests a shift toward voluntarism accompanies RTW law, perhaps as a way to limit membership defection over the partisan use of resources.

Fourth, and related, apolitical contributions decline after RTW law for larger, non-trade organizations. Apolitical contributions can be donations to charities; however, they can also be strategic investments in communities meant to curry long-term social and political support. Building a network of sympathetic interest groups, through both politics and apolitical giving, can be essential when workers strike, organize, or engage in other forms of concerted activity. Similarly, broad based coalitions can be instrumental for passing protective legislation that improves conditions for the bottom of the labor market (e.g. minimum wage). Workers in lower skilled and lower paid occupations disproportionately benefit from sympathetic coalitions and improved labor market protections. Predictably, the retreat from politics and apolitical contributions will have the greatest negative consequences for these unions.

Finally, the findings highlight how social policy can yield competing effects and unanticipated consequences. Although RTW law might lure former union members to become covered non-members, and the loss of dues harms unions financially, there does not appear to be a short-term effect on the rate of RC petition filings or the results of RC elections. Perhaps the reason for this null result is that the open shop eliminates a key talking point used in union-avoidance campaigns; specifically, that unionization will lead to forced dues deduction. Contrary to theory and expectations, the results on union decertification and unit clarification petitions indicate that RTW law relieves existing unions of these types of internal pressures.

Study Limitations

Concerning the NLRB data analysis, our study only includes petitions filed with the NLRB, and thus excludes similar petitions filed by public sector unions at the state and municipal government levels. Moreover, while RC petitions are the main vehicle for new union formation, some unions prefer to use voluntary recognition. The NLRB lacks data on voluntary recognition cases. In sum, for the NLRB analysis, the findings apply to private sector cases processed through the NLRB.

For the OLMS data analysis, the most comprehensive set of union financial measures are from LM-2 forms, which are required from larger unions with over $250,000 in receipts. The results thus exclude smaller organizations that, as the results suggest, might respond differently to the open shop context. The other major exclusions are public sector unions, which represent roughly half of union members in the U.S., and unions covered by the Railway Labor Act. Thus, the analysis assess the short-term response to the open shop by larger labor organizations in private industry governed by the LMRA.

Finally, the estimates produced in this study are limited to a relatively short time horizon. The first state in the sample that passed RTW law was Indiana in 2012. The NLRB analysis therefore includes cases in a six-year treatment window, and the OLMS analysis in a four-year treatment window. It could be that behavioral change to RTW law has a longer lag period than allowed by this study. Replicating this study with a longer-term analysis might yield different results.

Appendix A: Data Sources

The NLRB representation petition filings includes all RC, RD, RM UC and UD cases filed from January 1, 2008 to December 31, 2018 that closed.[29] The pertinent data include petition type, state, size of the workforce, date of filing and outcome. I excluded cases dismissed by the NLRB (N=442; 5.1% of total petitions).

The NLRB data are only for supervised elections, and do not include cases where labor unions achieve recognition through card check. Card check is one of the strategic options for unions in an open shop context (Oswalt, 2017). This alternative method for organizing new members cannot be modeled with the data.

I sorted NLRB data based on the date the file date of the petition. Case filed date was then matched against the RTW law implementation date in Table 2. Missing data existed for the fields dealing with the size of the employee group. Where available, I imputed from the field "number of voters" to fill missing data points for employee the group size. Any final missing data resulted in list wise deletion.

The second data are the Labor-Management (LM) forms filed with the Office of Labor Management Standards (OLMS), U.S. Department of Labor. The level of detail in a report varies depending on the size of a union's annual receipts. Smaller union locals, those with under $10,000 in receipts fill out the shorter LM-4 form; midsized locals with receipts between $10,000 and $250,000 fill out the longer LM-3 version; organizations with over $250,000 in receipts complete the LM-2. Our study is limited to LM-2 forms, which offer the most comprehensive data on factors such as officer pay, membership dues rates, and whether the union makes political contributions. It varies by year, but

[29] I thank Jodilyn Breirather at the National Labor Relations Board for assistance with the NLRB data. https://www.nlrb.gov/news-outreach/graphs-data/recent-filings?f%5B0%5D=ct%3AR

LM-2 filers are about 20 percent of total filers, LM-3 filers about 50 percent, and LM-4 filers about 30 percent. Annual OLMS data are updated periodically.[30] Due to lags in reporting, I compiled data from 2007 to 2016 in order to prepare a relatively complete panel data set.

Part of the original proposal included hypotheses that distinguished between the building trades and the non-trades. Organizations classified as trades have the following acronyms: BAC, BCTD, BSOIW, CJA, HFIA, IATC, IUEC, IUOE, LIUNA, MTD, NAOSTU, NIA, OPCM, PAT, PPF, PTE, RWAW and SMART.

[30] https://olms.dol-esa.gov/query/getYearlyData.do

Appendix B: Analytical Methods

Every DD analysis compares two groups: (1) a treatment group in states where RTW took effect, and (2) a control group from states where RTW did not take effect. For all analyses, the base equation is:

$$Y_{ist} = \beta_0 + \beta_1\lambda_t + \beta_2 R_s + \delta_0 (\lambda * R) + \mu_{ist} \qquad (1)$$

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 main factors. Symbol λ is a time variable common to control and treatment groups, and R is a dummy variable with the value 1 if the state had a RTW law in effect at any time during the study period, zero otherwise. The μ_{ist} are unexplained errors.

Symbol δ is the coefficient for the RTW law and time interaction, or the estimated effect of the treatment. For the analysis of NLRB data, symbol δ equals 1 for petitions filed after the passage of RTW law, zero otherwise. For the analysis of OLMS data, symbol δ is equals 1 for every full year that RTW law was in effect. Expected values are

$E[Y_{R,(2)}] = \beta_0 + \beta_{1(2)} + \beta_2 + \delta_0$

$E[Y_{R,(1)}] = \beta_0 + \beta_{1(1)} + \beta_2$

$E[Y_{B,(2)}] = \beta_0 + \beta_{1(2)}$

$E[Y_{B,(1)}] = \beta_0 + \beta_{1(1)}$

Here, the Y subscripts are R = RTW state and B = bargaining state in the period before RTW policy (1) and when RTW policy is in place (2). Our main interest is in δ_0, the coefficient for the interaction between time period and RTW law. Under correct assumptions, the estimated effect of the treatment is:

$$\hat{\delta}_0 = [(\bar{Y}_{R,(2)} - \bar{Y}_{R,(1)}) - (\bar{Y}_{B,(2)} - \bar{Y}_{B,(1)})]$$

Models for NLRB Data

For the analyses of NLRB data, 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: RC, RD, RM, UC and UD. I test for change in the proportions of these petitions using a multi-variate probit model, with RC the excluded category. This first stage model assumes independent errors of zero mean. The stage one μ_{ist} are:

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

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. 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:

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

Where ρ is the estimated correlation between the residuals of the model outcomes.[31] Table 1 provides summary statistics for the variables in the models at the two stages.

Model for OLMS Data

The analyses of OLMS panel data requires modifications to equation (1) to address the potential for clustering at the state or union organization level due to serial correlation (Bertrand, Duflo, and Mullainathan, 2004). The full equations feature random intercepts for union and state, α_i and α_s, respectively, to capture effects from unmeasured

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

factors at those levels (Rabe-Hesketh and Skrondal, 2008). Symbol λ is structured as dichotomous year indicators in order to factor out year-specific sources of variation in the measures. The full DD equation for the OLMS analyses is:

$$Y_{ist} = \beta_0 + \sum \beta_j \lambda_t + \beta_2 R_s + \delta_0 (\lambda * R)_{st} + \alpha_i + \alpha_s + \mu_{ist}$$

Models for Supplemental DDD Analyses

One key finding in the OLMS regressions was a differential response to RTW law by the building trades compared with the non-trades. While the building trades are subject to the LMRA, and therefore not exempt from RTW policy, as a practical matter the threat posed by RTW law is much less significant compared to other types of union organizations due to journeyperson certifications, the terms set in signatory agreements, and the hiring hall system for labor deployment.

The distinction between the trades and non-trades allows for a supplementary analysis using difference-in-difference-in-difference (DDD) models. In this study, a DDD analysis compares: (1) a treatment group residing in states where RTW took effect, (2) a control group from states where RTW policy did not change, and (3) a control group residing in all states *but* were structurally immune to RTW policy (i.e. unions affiliated with the building trades). The expanded DDD equation is:

$$Y_{igst} = \beta_0 + \sum \beta_j \lambda_t + \beta_2 R_s + \beta_3 G_i + \delta_0 (\lambda_t * R_s) + \delta_1 (R_s * G_i) + \delta_2 (\lambda_t * G_i) + \delta_3 (\lambda_t * R_s * G_i) + \alpha_i + \alpha_s + \mu_{igst}$$

Y is the outcome variable of interest for *i* unions within *g* groups in *s* states over *t* time periods, the β are estimated coefficients for the intercept and main factors, and the δ are the estimated coefficients for interactions. Variable symbols are equivalent to DD, with the exception of G, a dummy variable with the value of 1 if a union is not affiliated

with the building trades (and thus not structurally immune to RTW law), zero otherwise. Differencing at three levels produces the estimated coefficient for the treatment, δ_3:

$$\hat{\delta}_3 = [(\bar{Y}_{R,G,(2)} - \bar{Y}_{R,G,(1)}) - (\bar{Y}_{B,G,(2)} - \bar{Y}_{B,G,(1)})] - [(\bar{Y}_{R,E,(2)} - \bar{Y}_{R,E,(1)}) - (\bar{Y}_{B,E,(2)} - \bar{Y}_{B,E,(1)})]$$

The additional subscripts include G, for union organizations unaffiliated with the building trades and E, for the building trades that are structurally exempt from RTW law. Through differencing, this DDD model controls for changes in unions across states (e.g. RTW and Bargaining), and changes in unions within the RTW states (i.e. Groups subject to RTW and Exempt groups).

Appendix C: Parallel Trend Tests

One assumption behind DD designs is that subjects in the treatment and control groups were similar prior to the treatment. I test this assumption with parallel trend tests that estimate whether pre-treatment measurement patterns differ between subjects in the treatment and control groups.

The supplemental tables in this section display the results of the parallel trend tests. Each measure was regressed on the RTW state indicator, a linear time trend and their interaction for the years prior to 2012 when the first RTW laws passed. Statistically significant slope differentials between the groups imply a divergence in the measurement trends before the treatment, and thus potentially an invalid quasi-experimental design.

Supplemental Table 1: Parallel Trend Test for Multinomial Probit Regression of RTW Law and NLRB Representation Petitions, 2008-2011.

	RD Petition	RM Petition	UC Petition	UD Petition
RTW State	-62.933 (126.099)	-56.401 (242.423)	-103.656 (200.627)	-257.340 (195.562)
Year	-0.005 (0.035)	-0.031 (0.056)	-0.053 (0.064)	-0.083 (0.059)
RTW State X Year	0.031 (0.063)	0.028 (0.121)	0.052 (0.100)	0.128 (0.097)
Constant	8.362 (70.536)	61.639 (113.299)	105.261 (128.674)	164.862 (117.911)
LL	-3231.179			
N	3,433			

Notes: Equations include controls for four NLRB regions; robust standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Supplemental Tables 1 and 2 provide tests for the NLRB analyses. Supplemental Table 1 is the test for the Figure 1, Stage 1 analysis, and corresponds with Table 3 in the report body. Supplemental Table 2 is the test for Figure 1, Stage 2, and correspond with Tables 4 and 5 in the report. For all models, the coefficient for the interaction term between RTW State and Time are statistically insignificant, suggesting that the NLRB cases in the treatment and control groups shared similar trend patterns prior to the RTW law passage.

Supplemental Table 2: Parallel Trend Test for Multinomial Probit Regressions of RTW Law and Outcomes for RC and RD-RM Petitions, 2008-2011

	RC Petitions		RD-RM Petitions	
	Union Win	Withdrawal	Union Win	Withdrawal
RTW State	41.078 (104.627)	-112.371 (105.673)	-6.105 (171.162)	-119.122 (157.040)
Year	-0.027 (0.028)	-0.017 (0.029)	0.009 (0.053)	-0.182*** (0.043)
RTW State X Year	-0.021 (0.052)	0.056 (0.053)	0.003 (0.085)	0.059 (0.078)
Constant	54.152 (56.5889)	34.531 (59.093)	-18.512 (106.142)	365.235*** (86.827)
ρ	-0.974 (0.005)		-0.959 (0.013)	
LL	-2481.726		-913.104	
N	2,301		897	

Notes: Robust standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Supplemental Tables 3 through 8 are the tests for the OLMS union financial data. For each measure, I perform tests for the full sample, and for subsamples of the building trades and the non-trades.

Of the 12 measures, *Equity per Member*, *Overhead Exp. Ratio* and *Overhead Exp. Per Member* yield significant trend differentials at $\alpha > 0.05$. All three, however, pass the parallel trend test for the subsamples. Encouragingly, trend differentials for the non-trade subsample fail to breach conventional levels of statistical significance for all measures.

Overall, these results suggest that the non-trade unions in the six contiguous regional states are valid controls for the non-trades in four RTW states. These results also provide justification for factoring out the trades in the equations. Nonetheless, tests for *Equity per Member*, *Overhead Exp. Ratio* and *Overhead Exp. Per Member* do indicate pre-treatment trend differences between the treatment and control groups, and should be regarded as preliminary and open to revision.

Supplemental Table 3: Parallel Trend Test for Regressions of RTW Law and Union Finance Measures, 2008-2011						
	Equity Per Member			Income Margin		
	Total	Non-Trades	Trades	Total	Non-Trades	Trades
RTW State	-12.794* (5.244)	-12.365 (6.660)	-14.882 (8.564)	0.239 (6.046)	-6.689 (7.725)	4.678 (9.807)
Year	0.010*** (0.001)	0.009*** (0.002)	0.011*** (0.002)	0.016*** (0.002)	0.009*** (0.002)	0.025*** (0.002)
RTW State X Year	0.006* (0.003)	0.006 (0.003)	0.007 (0.004)	-0.000 (0.003)	0.003 (0.004)	-0.002 (0.005)
Constant	-11.673*** (2.904)	-9.794* (3.863)	-13.879** (4.401)	-31.632*** (3.352)	-17.552*** (4.492)	-49.845*** (5.019)
LL	1164.748	747.041	460.685	2256.453	1325.119	948.291
N, Organizations	1,531	940	591	1,532	941	591
N, Observations	6,456	3,842	2,614	6,465	3,848	2,614
Notes: Standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.						

Supplemental Table 4: Parallel Trend Test for Regressions of RTW Law and Union Finance Measures, 2008-2011						
	Representation Exp. Ratio			Representation Exp. Per Member		
	Total	Non-Trades	Trades	Total	Non-Trades	Trades
RTW State	0.681 (0.925)	-0.298 (1.221)	2.867* (1.417)	15.632 (13.986)	32.454 (17.612)	-17.974 (23.106)
Year	0.001*** (0.000)	0.001** (0.000)	0.000 (0.000)	0.017*** (0.004)	0.016** (0.005)	0.017** (0.006)
RTW State X Year	-0.000 (0.000)	0.000 (0.001)	-0.001* (0.001)	-0.008 (0.007)	-0.016 (0.009)	0.009 (0.012)
Constant	-0.658 (0.512)	-1.296 (0.708)	0.207 (0.728)	-28.418*** (7.746)	-27.618** (10.217)	-28.795* (11.872)
LL	12545.093	7375.642	5233.349	-5156.638	-2909.762	-2192.365
N, Organizations	1,532	941	591	1,532	941	591
N, Observations	6,465	3,848	2,617	6,465	3,848	2,617
Notes: Standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.						

Supplemental Table 5: Parallel Trend Test for Regressions of RTW Law and Union Finance Measures, 2008-2011						
	Political Exp. Ratio			Political Exp. Per Member		
	Total	Non-Trades	Trades	Total	Non-Trades	Trades
RTW State	3.893 (26.775)	42.159 (35.901)	-58.620 (39.924)	-5.580 (33.399)	41.928 (44.360)	-87.989 (50.675)
Year	-0.007 (0.007)	0.002 (0.010)	-0.019 (0.010)	0.006 (0.009)	0.012 (0.013)	-0.004 (0.013)
RTW State X Year	-0.002 (0.013)	-0.021 (0.018)	0.029 (0.020)	0.003 (0.017)	-0.021 (0.022)	0.044 (0.025)
Constant	9.467 (14.831)	-8.225 (20.831)	33.068 (20.506)	-10.267 (18.499)	-24.296 (25.738)	9.112 (26.030)
LL	-8911.519	-5465.518	-3406.005	-10419.285	-6301.249	-4077.848
N, Organizations	1,532	941	591	1,532	941	591
N, Observations	6,465	3,848	2,617	6,465	3,848	2,617
Notes: Standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.						

Supplemental Table 6: Parallel Trend Test for Regressions of RTW Law and Union Finance Measures, 2008-2011						
	Contribution Exp. Ratio			Contribution Exp. Per Member		
	Total	Non-Trades	Trades	Total	Non-Trades	Trades
RTW State	0.402 (20.104)	-9.940 (24.537)	25.627 (34.612)	7.223 (26.197)	-6.446 (31.710)	35.306 (45.569)
Year	-0.024*** (0.006)	-0.018* (0.007)	-0.032*** (0.009)	-0.011 (0.007)	-0.008 (0.009)	-0.015 (0.012)
RTW State X Year	-0.000 (0.010)	0.005 (0.012)	-0.013 (0.017)	-0.004 (0.013)	0.003 (0.016)	-0.018 (0.023)
Constant	44.951*** (11.136)	31.687* (14.236)	61.479** (17.771)	24.779 (14.510)	18.782 (18.400)	32.381 (23.400)
LL	-6862.228	-3941.895	-2876.467	-8679.796	-4972.184	-3645.888
N, Organizations	1,532	941	591	1,532	941	591
N, Observations	6,465	3,848	2,617	6,465	3,848	2,617
Notes: Standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.						

Supplemental Table 7: Parallel Trend Test for Regressions of RTW Law and Union Finance Measures, 2008-2011						
	Overhead Exp. Ratio			Overhead Exp. Per Member		
	Total	Non-Trades	Trades	Total	Non-Trades	Trades
RTW State	13.534* (6.394)	10.519 (7.894)	18.017 (10.856)	43.254* (18.544)	34.871 (23.858)	53.342 (29.731)
Year	0.006*** (0.002)	0.006* (0.002)	0.007* (0.003)	0.037*** (0.005)	0.033*** (0.007)	0.041*** (0.008)
RTW State X Year	-0.007* (0.003)	-0.005 (0.004)	-0.009 (0.005)	-0.022* (0.009)	-0.017 (0.012)	-0.027 (0.015)
Constant	-13.860*** (3.541)	-12.750** (4.580)	-15.124** (5.576)	-68.749*** (10.271)	-61.613*** (13.841)	-77.327*** (15.273)
LL	144.783	199.104	-31.708	-6825.917	-4058.598	-2736.477
N, Organizations	1,532	941	591	1,532	941	591
N, Observations	6,465	3,848	2,617	6,465	3,848	2,617
Notes: Standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.						

Supplemental Table 8: Parallel Trend Test for Regressions of RTW Law and Union Finance Measures, 2008-2011						
	Administration Exp. Ratio			Administration Exp. Per Member		
	Total	Non-Trades	Trades	Total	Non-Trades	Trades
RTW State	5.293 (8.034)	-0.636 (9.954)	19.087 (13.571)	22.835 (16.254)	3.166 (20.116)	59.120* (27.506)
Year	0.002 (0.002)	0.004 (0.003)	-0.001 (0.003)	0.026*** (0.004)	0.024*** (0.006)	0.027*** (0.007)
RTW State X Year	-0.003 (0.004)	0.000 (0.005)	-0.009 (0.007)	-0,011 (0.008)	-0.002 (0.010)	-0.029* (0.014)
Constant	-4.256 (4.450)	-8.469 (5.775)	1.141 (6.970)	-46.832*** (9.002)	-44.206*** (11.700)	-50.001*** (14.127)
LL	-1158.871	-599.583	-535653	-5873.490	-3439.222	-2397.407
N, Organizations	1,532	941	591	1,532	941	591
N, Observations	6,465	3,848	2,617	6,465	3,848	2,617
Notes: Standard errors in parenthesis; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.						

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