

APPROVAL SHEET

Title of Dissertation: The Impact of Right-to-Work Laws on Wages and Employment

Name of Candidate: Samuel Rowe
Doctor of Philosophy, 2016

Dissertation and Abstract Approved: (_____)
(T.H. Gindling)
(Professor)
(School of Public Policy)

Date Approved: _____

NOTE: *The Approval Sheet with the original signature must accompany the thesis or dissertation. No terminal punctuation is to be used.

Samuel Rowe
(801) 949-5011
srowe3435@gmail.com

Education

Ph.D. University of Maryland, Baltimore County, School of Public Policy expected 2016
M.S. Florida State University, August 2005
B.S. University of Utah, December 2003

Dissertation

“The Impact of Right-to-Work Laws on Wages and Employment”

This study assesses the impact of Right-to-Work laws on labor outcomes for individuals in the short-run in Indiana, Michigan, and Wisconsin. The study also assesses the impact of Right-to-Work laws on labor outcomes at the county level along Oklahoma’s state borders.

Dissertation Readers: Professor T.H. Gindling (chair), Professor Lisa Dickson, Professor Lauren Edwards

Professional Experience

Department of Labor
Statistician

Jul 2016 to Present

Bureau of Labor Statistics
Economist

Sept 2005- Jul 2016

Publications

The Correlates of Income Mobility in Costa Rica, 2001-2007, Economic Journal of the University of Costa Rica

Productivity Growth in Construction, Journal of Construction Engineering and Management

ABSTRACT

Title of Document:

THE IMPACT OF RIGHT-TO-WORK LAWS ON WAGES AND EMPLOYMENT.

Samuel Rowe, Doctor of Philosophy, 2016

Directed By:

Professor T.H. Gindling, Department of
Economics

This research focuses on whether state-level “Right-to-Work” (RTW) laws improve or worsen labor outcomes for workers. The impact of RTW laws is controversial, with proponents arguing that these laws benefit workers and opponents arguing that they harm workers. Proponents for RTW laws argue that RTW laws help workers, since these laws improve employment growth and labor market flexibility. Opponents of RTW laws have argued that RTW laws have adverse effects on worker wages and employment, since it reduces collective bargaining and increases the ease of firing.

This dissertation utilizes policy changes in Indiana, Michigan, and Wisconsin for short-run analyses. Utilizing panel data on individuals derived from the Current Population Survey, the impact of RTW laws on unionization, employment, unemployment, and weekly earnings are assessed using a difference-in-difference methodology. This methodology controls for unobserved heterogeneity that may bias the impact of RTW laws.

It also utilizes a policy change in Oklahoma for a long-run analysis of RTW laws.

For the long-run analysis, county-level data from the Quarterly Census of Employment

and Wages are used with a regression discontinuity design before and after Oklahoma's RTW law. This methodology assesses the impact of RTW laws along Oklahoma's state borders before and after its law. As a comparison, the same methodology is applied to all RTW/union-shop state borders.

The results find that there is support for opponents of RTW laws, while it finds little to no support for proponents of RTW laws. The results find that RTW laws do have an impact on individual outcomes in the Midwest. RTW laws are associated with a 1.4 to 2.2 percentage-point reduction in the likelihood of being a union member. Also, RTW laws are associated with a 2% to 4% reduction in earnings. The impact of RTW laws on employment and unemployment is consistent, but not always statistically significant. RTW laws were associated with an increased likelihood of being unemployed and with a decreased likelihood of being employed.

For a long-run analysis of RTW laws, the research finds very limited support for proponents of RTW laws. RTW laws were associated with higher employment shares in manufacturing along Oklahoma's state borders before its RTW law, but these discontinuities remained after Oklahoma adopted a RTW law. However, other states that did not have a policy change in RTW laws experienced slight declines employment shares between RTW states and union shop states. These results likely point to other state policies and factors causing these discontinuities along state borders. Also, these results may have been due to low unionization in Oklahoma before the adoption of its RTW law.

THE IMPACT OF RIGHT-TO-WORK LAWS ON WAGES AND EMPLOYMENT.

By

Samuel Rowe

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, Baltimore County, in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2016

ProQuest Number:10246658

All rights reserved

INFORMATION TO ALL USERS

The quality of this reproduction is dependent upon the quality of the copy submitted.

In the unlikely event that the author did not send a complete manuscript and there are missing pages, these will be noted. Also, if material had to be removed, a note will indicate the deletion.



ProQuest 10246658

Published by ProQuest LLC (2017). Copyright of the Dissertation is held by the Author.

All rights reserved.

This work is protected against unauthorized copying under Title 17, United States Code
Microform Edition © ProQuest LLC.

ProQuest LLC.
789 East Eisenhower Parkway
P.O. Box 1346
Ann Arbor, MI 48106 – 1346

© Copyright by
Samuel Rowe
2016

Dedication

This dissertation is dedicated to my wife, Milena Prodanova, for her patience and perseverance during this journey. This dissertation is also dedicated to my daughter, Mikayla Rowe Prodanova, as she begins her journey in life.

Acknowledgements

I would like to acknowledge my chair and mentor Dr. T.H. Gindling for his thoughts, insights, and feedback. Dr. Gindling has provided great opportunities to look into the intersections of labor economics and public policy. I would also like to acknowledge Dr. Leo Sveikauskas for his invaluable feedback throughout this process and the great opportunities he provided to work on productivity research. I would also like to acknowledge the other members of the committee, Dr. Lisa Dickson, Dr. Lauren Edwards, and Dr. David Mitch, who provided invaluable feedback during this process.

Table of Contents

Dedication	ii
Acknowledgements	iii
Table of Contents	iv
List of Tables	vii
List of Figures	viii
List of Abbreviations	ix
Chapter 1: Introduction	1
Section 1: The Problem	1
Section 2: Academic Motivation	1
Section 3: Why Study Right-to-Work Laws?	2
Subsection 1: Proponents.....	2
Subsection 2: Opponents	3
Section 4: Trends	4
Subsection 1: Legislative Trends	4
Subsection 2: Unionization Trends	5
Subsection 3: Covered Nonmember and Free-riding Trends.....	7
Section 5: Organization	9
Chapter 2: Review of Right to Work Laws and Related Laws	10
Section 1: What Are Right-to-Work Laws?	10
Section 2: Labor-Management Relations Laws	11
Subsection 1: National Labor Relations Act	12
Subsection 2: Taft-Hartley Act and Amendments to NLRA.....	13
Subsection 3: Railway Labor Act.....	16
Subsection 4: Federal Service Labor-Management Relations Statute.....	17
Subsection 5: State Laws on Collective Bargaining and Union Security Agreements	18
Section 3: Recent RTW Legislation	22
Chapter 3: Empirical Literature Review	26
Section 1: Overview of the Hypotheses of RTW Laws	27
Subsection 1: Free-Rider Hypothesis	28
Subsection 2: Bargaining Power Hypothesis	29
Subsection 3: Taste Hypothesis.....	30
Subsection 4: Nonunion Workers and RTW Laws	31
Section 2: The Impact of RTW on Unionization and Collective Bargaining	32
Subsection 1: RTW Laws and Endogeneity	33
Subsection 2: RTW Laws and Free Riding.....	35
Subsection 3: Concluding Thoughts on Unionization and RTW Laws	46
Section 3: A Systematic Review of The Impact of RTW on Wages and Employment	47
Subsection 1: Brief Description of the Systematic Review Process	47
Subsection 2: Selected Abstracted Information.....	50
Subsection 3: Findings	69
Subsection 4: Concluding Thoughts from the Systematic Review	73
Section 4: The Contribution of Dissertation Research	74

Chapter 4: Research Questions, Conceptual Model, and Hypotheses	77
Section 1: Research Questions	77
Subsection 1: Short-Run Impacts.....	78
Subsection 2: Long-Run Impacts.....	79
Section 2: Conceptual Model of RTW Laws and Labor Markets	79
Overview of Conceptual Model.....	80
Subsection 1: Unionized Labor Market without a RTW Law.....	81
Union Goals and Wage-Employment Trade Off.....	82
Subsection 2: Nonunionized Labor Market without a RTW Law.....	96
Subsection 3: Union Services, RTW Laws, and Free-Riding.....	99
Subsection 4: Unionized Labor Market with a RTW Law.....	107
Subsection 5: Nonunionized Labor Market with a RTW Law.....	112
Section 3: Hypotheses	115
Subsection 1: Short-Run Impacts.....	115
Subsection 2: Long-Run Impact.....	117
Chapter 5: Methodology and Data	119
Section 1: Short-Run Impact Analysis	120
Subsection 1: Difference-in-Difference.....	121
Subsection 2: Data.....	125
Subsection 3: Limitations.....	130
Section 2: Long-Run Impact Analysis	133
Subsection 1: State Border/Regression Discontinuity Methodology.....	133
Subsection 2: Data.....	140
Subsection 3: Limitations.....	143
Chapter 6: Short-Run Analysis	146
Section 1: Short-run Research Questions	146
Section 2: Descriptive Statistics and Pre-Post Analysis	147
Subsection 1: Descriptive Statistics.....	147
Subsection 2: Pre-Post Analysis.....	157
Section 3: Main Findings	164
Subsection 1: Indiana Main Findings.....	165
Subsection 2: Michigan Main Findings.....	166
Subsection 3: Wisconsin Main Findings.....	168
Subsection 4: All Worker Specification Test.....	169
Subsection 5: Time-Varying Robustness Check.....	172
Section 4: Other Findings and Tests	173
Subsection 1: Falsification Tests.....	174
Subsection 2: Pooled CPS MORG.....	176
Subsection 3: Panel Basic CPS.....	179
Section 5: Conclusions	182
Chapter 7: Long Run Analysis	184
Section 1: Long-run research questions	186
Section 2: Descriptive and Graphical Analysis	187
Subsection 1: Descriptive Statistics.....	187
Subsection 2: Graphical Analysis.....	189
Section 3: Main Oklahoma Borders Findings	203
Subsection 1: Employment.....	205
Subsection 2: Wages.....	207

Subsection 3: Establishments	209
Section 4: State Border Impacts for All States	211
Section 5: Conclusions	219
Subsection 1: Limitations	221
Chapter 8: Discussion, Policy Significance, and Future Research Questions	223
Section 1: Discussion of Empirical Findings	224
Subsection 1: Short-Run	224
Subsection 2: Long-Run	231
Subsection 3: Limitations	238
Section 2: Policy Recommendation and Future Research Questions	241
Subsection 1: Policy Recommendation	241
Subsection 2: Future Research Questions	243
Appendix: Systematic Review Process	248
Section 1: Search Strategy	248
Subsection 1: Databases Electronically Searched	248
Subsection 2: Keywords	249
Subsection 3: Filters	250
Subsection 4: Results	251
Section 2: Inclusion and Exclusion Criteria	251
Subsection 1: Inclusion and Exclusion Criteria	252
Subsection 2: Results of Abstract and Title Review	255
Subsection 3: Results of In-Depth Review	256
Section 3: Study Quality	259
Subsection 1: Research Design	259
Subsection 2: Unobserved Heterogeneity	260
Subsection 3: Endogeneity and Simultaneous Equations	260
Subsection 4: Assessment of Research Designs	261
Subsection 5: Data	262
Subsection 6: Study Quality Assessment	263
Subsection 7: Abstracted Information	265
References	271

List of Tables

Section	Page #
Table 1-1: Unionization Rates	7
Table 2-1: States with Right-to-Work Laws and Year of Enactment	15
Table 2-2: Public Sector Right-to-Work Laws	21
Table 3-1: Ranking of Empirical Study Quality	50
Table 3-2: Types of Outcomes by Study	70
Table 3-3: Wage Findings	72
Table 3-4: Employment Findings	73
Table 6-1: Descriptive Statistics Indiana	150
Table 6-2: Descriptive Statistics Michigan	153
Table 6-3: Descriptive Statistics Wisconsin	156
Table 6-4: Pre-Post Analysis Indiana	159
Table 6-5: Pre-Post Analysis Michigan	161
Table 6-6: Pre-Post Analysis Wisconsin	163
Table 6-7: Main Empirical Findings from Panel Data	165
Table 6-8: Additional Specification Test from Panel Diff-in-Diff	170
Table 6-9: Main Specification with State Unemployment Rates	173
Table 6-10: Union Falsification Tests	175
Table 6-11: Pooled Difference-in-Difference Findings	179
Table 6-12: Panel Basic CPS Findings	181
Table 7-1: Descriptive Statistics Oklahoma and RTW States in 2000	188
Table 7-2: Descriptive Statistics Oklahoma and RTW States in 2010	189
Table 7-3: Employment Outcomes around Oklahoma	206
Table 7-4: Wage Outcomes around Oklahoma	209
Table 7-5: Establishment Outcomes around Oklahoma	211
Table 7-6: Employment Outcomes around All States	215
Table 7-7: Wage Outcomes around All States	217
Table 7-8: Establishment Outcomes around All States	218
Table A1: Inclusion-Exclusion Process of AOK One Click Search	258
Table A2: Inclusion-Exclusion Process of JSTOR Search	258

List of Figures

Section	Page #
Figure 1-1: Unionization Rates 1983-2014	6
Figure 4-1: Employment-Wage Trade-off	84
Figure 4-2: Unionized Labor Market	85
Figure 4-3: Efficient Contracts Model	90
Figure 4-4: Spillover Effect in Nonunion Labor Market	97
Figure 4-5: Threat Effect in the Nonunion Labor Market	99
Figure 4-6: Median Voter and Demand for Union Services	104
Figure 4-7: Elimination of Excludability on Demand for Union Services	107
Figure 4-8: Loss of Bargaining Power in Union-Monopolist Model	109
Figure 5-1: Oklahoma-Texas Border in 2000 Before RTW Law	136
Figure 5-2: Oklahoma-Texas Border in 2011 10 Years After RTW Law	137
Figure 7-1: LN Employment in Manufacturing	193
Figure 7-2: Employment Shares in Manufacturing	194
Figure 7-3: Total Wage Shares in Manufacturing	197
Figure 7-4: LN Average Weekly Wages in Manufacturing	198
Figure 7-5: LN Establishments in Manufacturing	201
Figure 7-6: Establishment Shares in Manufacturing	202
Figure 7-7: Unionization Rate in Manufacturing	221

List of Abbreviations

CPS – Current Population Survey

FSLMRS – Federal Service Labor-Management Relations Statute

NLRA – National Labor Relations Act

QCEW – Quarterly Census of Employment and Wages

RLA – Railway Labor Act

RTW – Right-to-Work

Chapter 1: Introduction

Section 1: The Problem

This dissertation focuses on the impacts of state-level Right-to-Work (RTW) laws on labor outcomes. This research evaluates whether state-level RTW laws improve or worsen employment, job opportunities, and wages for individuals in the short and long-run.

Section 2: Academic Motivation

There are several academic motivations for this dissertation. First, this dissertation seeks to contribute to the literature on RTW laws. The literature on RTW laws has been mixed (Newman, 1998) and this dissertation attempts to provide a clearer understanding of the impact of RTW on labor outcomes. Most empirical work has focused on cross-sections of wages and employment. This dissertation adds to the literature by exploring the impact of RTW laws on different populations and time periods. In addition, many empirical studies have focused on aggregate measures of employment and wages, while this dissertation uses micro data to explore the impact of RTW laws on individuals and their labor outcomes.

This dissertation uses recent adoptions of RTW laws to test the empirical effects of the laws. Most of the empirical work in the literature has focused on states that had adopted RTW laws during the 1950's and 1960's with limited methodological structure and econometric techniques. This dissertation concentrates on states, which have recently adopted RTW legislation, to conduct a short-run analysis of wage and

employment. In addition, this work also conducts a long-run analysis of the effects of RTW laws.

Section 3: Why Study Right-to-Work Laws?

RTW laws are controversial with proponents saying they improve employment and wages, while opponents saying these laws hinder labor outcomes. RTW laws are state-level labor laws that focus on union and collective bargaining. Under the Taft-Hartley Act of 1947 states are permitted to adopt legislation that eliminate union security agreements under the National Labor Relations Act (NLRA). Union security agreements require all workers covered by a bargaining unit to pay an agency fee or union dues to a union for collective bargaining services provided to workers covered by a bargaining unit (Collins, 2014). Furthermore, RTW laws are designed to give workers the option to join a union or decline membership after obtaining employment.

Subsection 1: Proponents

Proponents of RTW laws argue that RTW laws help workers, since these laws improve employment growth and labor market flexibility. Proponents say that unions may enact rules that restrict employment and wages and RTW may reduce inefficiencies in the labor market, since RTW gives workers the right to vote with their feet (Brown & Medoff, 1978; Wessel, 1981). It is argued that forcing workers to join a union gives unions control over employment, which may prevent workers from moving to more efficient jobs and tasks (Carroll, 1983; Freeman & Medoff, 1984; Vedder, 2010). Unions may also increase inefficiencies by restricting hiring practices and work practices through

work rules (Doucouliagos & LaRoche, 2003). Furthermore, Vedder (2010) argues that union shops can prevent workers from bargaining their own wages. This suggests that the wages of high-productivity workers are worsened as unions compress wages in the workplace. Proponents can argue that a RTW law can increase wage outcomes for high-productivity workers. Also, proponents say that union monopolies may overcharge their members more than the cost of union services and RTW laws give workers the option to exit (Wessel, 1981). Finally, a RTW law may make a state's labor force more attractive for new industries, which promotes new job opportunities for workers (Palomba & Palomba, 1971).

Subsection 2: Opponents

Opponents of RTW laws argue that RTW laws have adverse effects on worker wages and employment. Opponents argue that the collective voice side of unions protects jobs, increases worker productivity, and increase the wages of members and that union shop contracts are an important source of union strength in collective bargaining against powerful employers (Carroll, 1983; Freeman & Medoff, 1984). Furthermore, unions can provide a collective voice for workers to prevent high turnover rates and increase investments in firm specific training (Freeman, 1976). RTW laws undermine collective bargaining by preventing unions from requiring universal membership under the bargaining unit, which leads to free-riding (Moore & Newman, 1985). Free-riding occurs since covered nonmembers can benefit from union services without having to pay for them and this leads to higher union dues for the same union services (Moore & Newman, 1985). Opponents also argue that RTW will reduce the benefits of lower turnover rates and reduce productivity (Brown & Medoff, 1978). They argue that by

hampering unions, RTW laws make it easier for employers to fire workers on whim, especially those workers who might agitate for worker rights (Freeman & Medoff, 1984). If an employee gets sick, stays home for a child's illness, or has a violation other than production quotas, then opponents say that RTW laws make it easier for employers to fire these workers (Lafer, 2013). If this were true, then RTW laws should increase the probability that workers with jobs lose their jobs and drop out of the labor force, switched into part-time work, or are forced to change employers. If they change employers they are likely to earn less than before they were fired, especially if they end up in a non-union job.

Section 4: Trends

There have been many important trends over the years in regard to RTW laws. One is the rate of adoption of RTW laws by states. Another is the general decline in unionization over the years. A third important trend is the difference in the number of free-riders and covered nonmembers in different states.

Subsection 1: Legislative Trends

After years of legislative dormancy, there has been a recent burst in legislative activity considering RTW laws. Recent adoption has occurred primarily in the Midwest. Wisconsin adopted a RTW law in 2015 and Michigan and Indiana adopted laws in 2012 (Collins, 2014). Prior to these legislative actions, most states adopted RTW laws during the 1950's or 1960's with a few states adopting RTW laws after the 1960's (Collins, 2014). After gaining full control of legislative chambers and governor offices in the 2010

elections in Indiana, Michigan, and Wisconsin, Republicans advocated and adopted RTW laws in these Midwestern states in the coming years (Davey, 2015). This appears to fit into the punctuated equilibrium model, where legislative activity on RTW laws is kept suppressed until a conspicuous event occurs that pushes legislation through (Baumgartner, Berry, Hojnaski, Kimball, and Leech, 2009). Since the early adoptions of RTW laws, there have been distinct trends in unionization and free-riding in collective bargaining coverage.

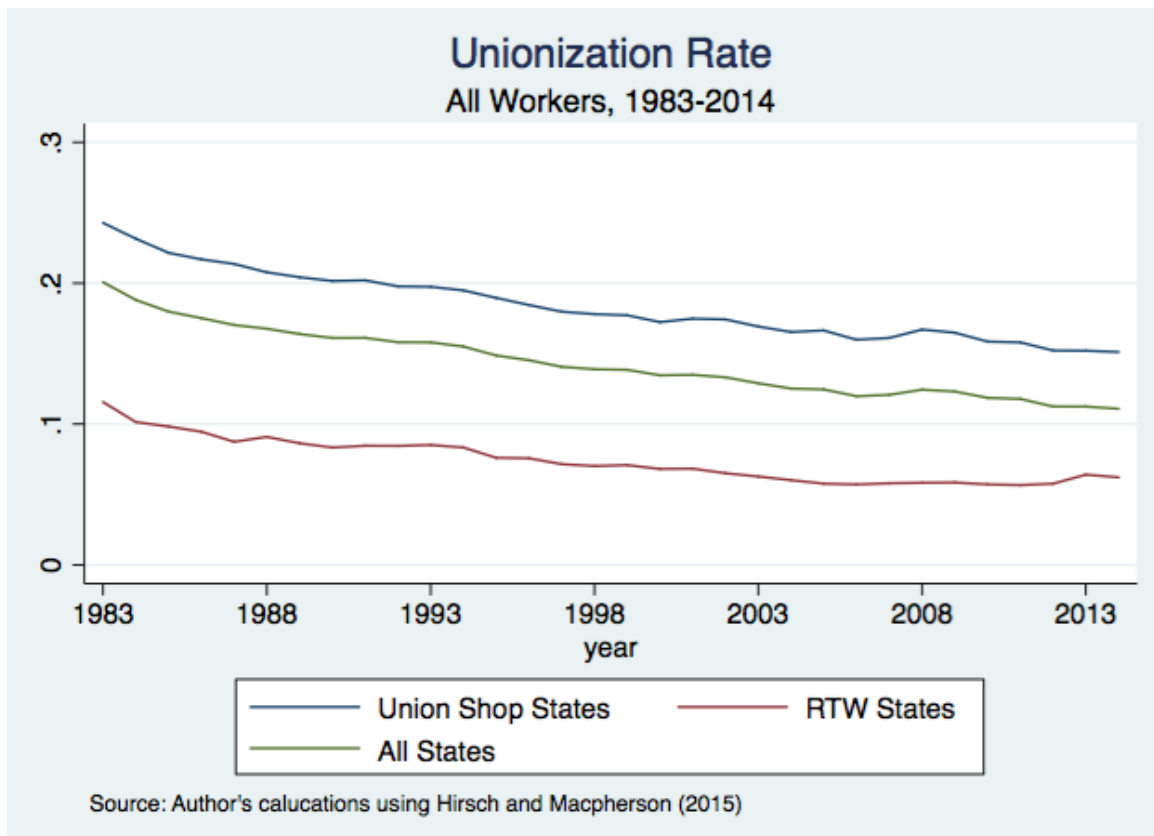
Subsection 2: Unionization Trends

Rates of overall unionization have been steadily declining over time. Utilizing data from the Current Population Survey (CPS), Hirsch and Macpherson (2015) show that rates of unionization for all wage and salary workers fell from 24.0 percent in 1973 to 11.1 percent in 2014. However, unionizations rates in the public and private sectors show different trends. Hirsch and Macpherson (2015) provide the unionization rate of private wage and salary workers was 24.2 percent in 1973, but 6.6 percent in 2014. Conversely, unionization of public sector wage and salary workers increased from 23.0 percent in 1973 to 35.7 percent in 2014.

States with and without RTW laws show similar trends in unionization, but have different levels. Utilizing CPS data from Hirsch and Macpherson (2015), union membership rates for all employees decreased from 11.6 percent in 1983 to 6.2 percent in 2014 in states with RTW laws. For states without RTW laws, or union shop states, union membership rate for all employees fell from 24.3 percent in 1983 to 15.1 percent in 2014. Overall, the declining trend in unionization seems to be similar in all states, but RTW

states consistently have lower rates of unionization than union shop states. However, different trends emerge when unionization rates are disaggregated to the public and private sectors.

Figure 1-1: Unionization Rates 1983-2014



Conspicuous trends in unionization rates between states with RTW laws and union shop states appear in the private and public sectors. Between RTW states and union shop states, unionization rates appear to converge in the private sector between 1983 and 2014, while unionization rates in the public sector appear to diverge in the same time period. In 1983, private sector unionization rates were 20.0 percent in union shop states compared to 9.4 percent in RTW states. In 2014, private sector unionization rates in union shop states were 8.8 percent compared to 4.0 percent in RTW states. The public sector experienced a divergent trend in the rate of unionization between union shop states

and RTW states. In 1983, public sector unionization in union shop states was 44.8 percent compared to 21.1 percent in RTW states. By 2014, the unionization rate for public sector employees in union shop states increased to 49.5 percent, while unionization for public sector employees in RTW states fell to 18.9 percent.

Table 1-1: Unionization Rates

National Unionization Rates		
Overall Unionization Rate	1973	2014
Private and Public Sectors	24.0%	11.1%
Private Sector	24.2%	6.6%
Public Sector	23.0%	35.7%

Unionization Rate by States		
Private Sector Unionization Rates	1983	2014
RTW States	9.4%	4.0%
Union Shop States	20.0%	8.8%
Public Sector Unionization Rates	1983	2014
RTW States	21.1%	18.9%
Union Shop States	44.8%	49.5%

Source: Author's calculations using www.unionstats.com (Hirsch and Macpherson, 2015)

Subsection 3: Covered Nonmember and Free-riding Trends

While rates of unionization are declining in all states, except for the public sector in union shop states, another important trend emerges from data provided by Hirsch and Macpherson (2015). RTW states have more nonunion members benefitting from union services than union shop states.

The percentage of covered nonmembers is always higher in RTW states than union shop states. A covered nonmember is a nonunion member that is covered by a union collective bargaining agreement Davis and Huston (1993). In 1983, the percent of covered nonmembers compared to all covered workers in RTW states was 21.0 percent,

while the percent of covered nonmembers to all covered workers in union shop states was 11.8 percent. In 2014, the covered nonmembers were 16.5 percent in RTW states compared to 7.2 percent in union shop states. While both states show declines in covered nonmembers, the RTW states have higher rates of covered nonmembers. Similar trends occur in both public and private sectors. RTW states have higher rates of covered nonmembers than union shop states in the public and private sectors.

Sobel (1995) notes that not all covered nonmembers are free-riders. Some covered nonmembers are free-riders, whose marginal benefit of union services is greater than the marginal cost of union services. However, there also may be induced or forced-riders, where the marginal benefit of union services are lower than the cost of union services. Looking into the Current Population Survey in 1989 and 1991 for private sector workers, Sobel (1995) found that free-riders were 17.3 percent of covered private sector workers in RTW states, but were only 7.4 percent of all covered workers in union shop states. Sobel (1995) reports that various RTW states have different rates of free-riding, with South Dakota being the highest at 39 percent and Georgia being the lowest at 9 percent. Among union shop states, Delaware had the highest rate of free-riding at 19 percent, while Wisconsin had the lowest at 3 percent.

These trends imply a positive correlation between RTW adoption and free-riding workers covered by collective bargaining and a potential negative correlation between RTW adoption and unionization rates. These trends may spillover into labor outcomes of employment and wages. This dissertation will explore such impacts.

Section 5: Organization

The dissertation is organized into the following chapters. The second chapter covers the background of RTW laws and who is impacted by different type of RTW laws. The third chapter investigates the impact of RTW laws on labor outcomes. This chapter focuses on the mechanisms of RTW laws, such as free-riding and reduced unionization, and empirical findings of RTW law impacts on employment and wage outcomes. The fourth chapter discusses the research questions, conceptual model, and the hypotheses. This chapter goes in-depth about how the elimination of union security agreements leads to reduced unionization and changes in labor outcomes. The fifth chapter focuses on the methodology, data, and limitations to test the hypotheses. The sixth chapter focuses on the analysis of the short-run research questions. It analyzes the short-run impacts of RTW laws in Indiana, Michigan, and Wisconsin. The seventh chapter delves into the long-run research questions, which focuses on the impact of RTW laws around Oklahoma. The final chapter consists of a discussion of the results, which is corroborated by the literature review and conceptual models. It also provides a policy recommendation from the results, along with potential future research questions to better understand the impact of RTW laws.

Chapter 2: Review of Right to Work Laws and Related Laws

This chapter of the dissertation discusses Right-to-Work (RTW) laws and investigates the legislation on collective bargaining and laws related to RTW laws. Before a proper analysis of RTW laws can be conducted, it is important to understand what RTW laws are, when and where they were enacted, and whom do they affect. The first section discusses what RTW laws are. The next section delves into labor-management relation laws. This section is divided into different federal and state labor-management relations laws. The final section expands upon recent RTW legislation. Knowledge of these laws is necessary to properly evaluate the empirical literature and to establish appropriate research questions, methodologies, and data.

Section 1: What Are Right-to-Work Laws?

It is often unclear what are RTW laws and what are their purposes. RTW laws are provisions that eliminate, prohibit, or restrict union security agreements in labor contracts (Collins, 2014; Hegji, 2012). States have the option to enact laws that eliminate union security agreements for collective bargaining for specified workers in a given state from federal and state legislation. The most notable piece of legislation is the Taft-Hartley Act of 1947, which was an amendment to the National Labor Relations Act. This amendment permitted states to enact legislation that eliminates union security agreements, or a RTW law, for private-sector employees (Hegji, 2012). It is important to note that different laws affect union security agreements for public and private workers.

Union security agreements are the main provision in employer-union labor contracts that prevent workers from benefiting from collective bargaining without paying

for collective bargaining services. When a union security agreement provision is in effect, it requires that all workers covered by collective bargaining to pay a fee or dues at least equal to the cost of representation as a condition of employment (Hegji, 2012). Without union security provisions, preventing free-riding is more difficult in collective bargaining.

There are three major types of union security agreements: union shop agreements, agency shop agreements, and closed shop agreements. Union shop clauses require new employees to become union members as a condition of employment after a 30-day probationary period (Ichniowski & Zax, 1991; Collins, 2014). Agency shop agreements are clauses that require employees to pay a fee to cover collective bargaining, but the employee can join or decline union membership (Collins, 2014). Closed shop union security agreements make union membership a prerequisite for employment. However, closed shop union security agreements are prohibited from the Taft-Hartley Act of 1947 (Hegji, 2012). States without union security agreements or RTW laws are usually considered “open-shop” states (Department of Labor, 2015). The regulation of union security agreements for types of workers in the private and public sectors falls into different pieces of legislation of labor-management relation laws. The next section will review labor-management relations laws.

Section 2: Labor-Management Relations Laws

To understand the impact of RTW laws on union security agreements, it is important to understand legislation affecting labor-management relations and how union security agreements work in these pieces of legislation. Federal legislation is the primary

legislation impacting labor-management relations in the private sector and most federal employees (Hegji, 2012). For state and local workers in the public sector, state legislation is the primary legislation affecting labor-management relations (Sanes & Schmitt, 2014).

Subsection 1: National Labor Relations Act

Federal laws are the main source of legislation affecting union security agreements, collective bargaining, and labor-management relations for private sector workers. The primary piece of legislation affecting most private sector workers is the National Labor Relations Act (NLRA), or the Wagner Act, which was enacted in 1935 (Collins, 2014). As a successor to the National Industrial Recovery Act of 1933, which was found unconstitutional, this law was passed to try to give employees more protection and rights to exert more pressure on employers for higher wages and more purchasing power (Taylor & Whitney, 1992). Furthermore, this law was enacted to pacify labor unrest from labor strikes and other forms of industrial unrest and to promote commerce (NLRA of 1935, 2015a).

The Wagner Act establishes the framework for private sector employee rights and union security agreements. The law guarantees employees the right to organize and collectively bargain over employee concerns, such as hours, wages, and working conditions (NLRA of 1935, 2015c). Furthermore, it established procedures for union elections, certifications of unions, and prevention of unfair labor practices, such as discouraging workers from joining unions (Collins, 2014). The National Labor Relations Board (NLRB) was also established as an independent federal agency to handle labor-

management relations issues (NLRA of 1935, 2015b). This agency administers and enforces the NLRA by investigating representation disputes, complaints of unfair labor practice, and contract disputes, along with certifying and decertifying unions as employee representation (Hegji, 2012).

The NLRA also established union security agreements in labor contract when private sector workers collectively bargain. The NLRA establishes that employees can elect a union to represent their bargaining unit's¹ interest and, by a majority vote by the members of a bargaining unit, the union can collectively bargain after the election is certified by the NLRB or voluntarily recognized by the employer (Hegji, 2012). However, under the NLRA, newly hired employees covered by a bargaining unit must join the union or pay a fee for collective bargaining, whether or not the employee joins the union, after a period of 30 days (Collins, 2014). The NLRA does not cover all private sector workers. There are notable exemptions. First, railway and airline workers are covered under the Railway Labor Act of 1926. Second, there are several types of private sector workers that are not classified as an employee covered by the NLRA, which are agricultural workers, domestic workers, self-employed and unpaid family workers, independent contractors, and supervisors (Hegji, 2012).

Subsection 2: Taft-Hartley Act and Amendments to NLRA

The ability for states to adopt RTW laws that prohibits or restricts union security agreements comes from the Taft-Hartley Act of 1947. This piece of federal legislation

¹ A bargaining unit is a group of two or more employees who share common interest and may be grouped together for collectively bargaining (National Labor Relations Board, 1997)

amended the NLRA in 1947 and gave more freedoms to employers and more restrictions to labor unions, since many felt that the NLRA gave too much power to labor unions (Hegji, 2012). This amendment prohibited unfair labor practices, such as striking without notification, closed shop union security agreements, and secondary boycotts and allowed employees to decertify a labor union (Taft-Hartley Act of 1947, 2015a; Hegji, 2012) Furthermore, this act greatly impacted the union's ability to have union security agreements, since states could adopt clauses that preempt the NLRA to prohibit or restrict union security agreements from Section 14(b) of the Taft-Hartley Act (Taft-Hartley Act of 1947, 2015b; Devinatz, 2011). Before the enactment of the Taft-Hartley Act, the NLRA preempted any state law to restrict or prohibit union security agreements (Hegji, 2012). Any new state RTW law has potential to impact the union security agreement for most private sector workers. This clause that prohibits union security agreements will be the basis of the research questions that will be addressed.

While twenty-six states have RTW laws, most state adoptions of RTW laws occurred soon after the implementation of the Taft-Hartley Act. From Table 2-1, Twelve states enacted RTW laws in 1947, Arizona, Arkansas, Florida, Georgia, Iowa, Nebraska, North Carolina, North Dakota, South Dakota, Tennessee, Texas, and Virginia (Collins, 2014). Alabama, Kansas, Mississippi, South Carolina, Utah, and Wyoming adopted RTW laws soon after during the 1950's and 1960's. More recent adoptions occurred in 1976 in Louisiana, 1985 in Idaho, 2001 in Oklahoma, 2012 for Indiana and Michigan, 2015 in Wisconsin, and 2016 for West Virginia (Collins, 2014; NCSL, 2015; National Right to Work Legal Defense Foundation, 2016c).

Table 2-1: States with Right-to-Work Laws and Year of Enactment

State	Year of Enactment
Alabama	1953
Arizona	1947
Arkansas	1947
Florida	1947
Georgia	1947
Idaho	1985
Indiana	2012
Iowa	1947
Kansas	1958
Louisiana	1976
Michigan	2012
Mississippi	1954
Nebraska	1947
Nevada	1951
North Carolina	1947
North Dakota	1947
Oklahoma	2001
South Carolina	1954
South Dakota	1947
Tennessee	1947
Texas	1947
Utah	1955
Virginia	1947
West Virginia	2016
Wisconsin	2015
Wyoming	1963

Source: Collins (2014); NCSL (2015); and NTRW (2016)

The NLRA has been amended slightly since the Taft-Hartley Act. The NLRA was amended by the Labor Management Reporting Act of 1959, or the Landrum-Griffin Act. This act provided five basic rights for union members, such as the following: equality of rights; safeguards against improper discipline; freedom of speech; freedom from interference; and freedom from increased fees and dues except when authorized by a majority vote (Hegji, 2012). The NLRA was also amended in 1974 to expand the NLRA to cover employees of private, non-private hospitals (Collins, 2014). It was

amended once again in 1980 to allow employees covered by collectively bargaining to object to financially supporting a labor union based on religious reasons. However, the equivalent dues must be donated to a charitable organization (Collins, 2014).

Subsection 3: Railway Labor Act

The NLRA covers most private sector workers, but the federal Railway Labor Act (RLA) covers railway and airline workers. The RLA was enacted before the NLRA in 1926 to maintain industry peace between railway workers and the railroad industry. During this time, the nation was dependent on railways to promote commerce and any disruptions from labor-management disputes hurt the nation's economic welfare (Hegji, 2012). The RLA ensured employees' rights to join a labor union and the right to elect union representatives for collective bargaining (Railway Labor Act of 1926, 2015a; Hegji, 2012).

Subsequent amendments to the RLA affected collective bargaining and union security agreements. The 1934 amendment strengthened the ability of employees to elect representation for collective bargaining freely without interference and allowed union representation by class or craft for collective bargaining by a majority before recognition (Railway Labor Act of 1926, 2015b; Hegji, 2012). This amendment also established the National Railroad Adjustment Board to interpret contracts and settle deadlocked negotiations (Hegji, 2012). The 1936 amendment expanded the RLA to cover commercial airline carriers and employees of this industry. This expanded coverage did not include aviation manufacturing and general aviation employees, who fall under the NLRA (Hegji, 2012).

The RLA also establishes union security agreements for railway and airline workers. The 1951 amendment to the RLA allowed unions of railway and airline carriers to establish union security agreements. Furthermore, it prevents a state's RTW law from overriding the RLA union security agreement (Railway Labor Act of 1926, 2015b; Hegji, 2012). Therefore, any new state RTW law will not impact the union security agreement for railway and airline workers in that state.

Subsection 4: Federal Service Labor-Management Relations Statute

The final class of workers affected by federal laws for collective bargaining and union security agreements are federal employees. The basis of collective bargaining and union security agreements for federal employees comes from recommendations from the Task Force on Employee-Management Relations in the Federal Service and Executive Order 10988. This executive order signed by President Kennedy in January 1962 and it was based upon the recommendations of the task force (Bullocks, 2007). The executive order allowed federal employees to join labor unions and collectively bargain, but they were not allowed to negotiate over wages, not allowed to strike, and not allowed to bargain during official time (Hegji, 2012). Furthermore, the recommendations from the task force and subsequent executive order did not include any union security agreement, but they did allow for voluntary employee dues check off (Bullocks, 2007). In August 1971, President Nixon signed Executive Order 11491 that extended bargaining to official time, but without any union security agreements (Bullocks, 2007). In 1978, Congress codified the executive orders into the Civil Service Reform Act of 1978, or the Federal Service Labor-Management Relations Statute (FSLMRS) (Shimabukuro, 2011). While

the FSLMRS provides collective bargaining rights for federal employees, the statute also includes a prohibition of union security agreements (Hegji, 2012).

Not all federal employees are eligible to join labor unions and collective bargain. The definition of federal employees eligible to join labor unions exempts noncitizens, members of uniformed services, supervisors, management officials, employees of the Foreign Service, and employees who engage in strikes (Hegji, 2012). Furthermore, some agencies are exempt from the FSLMRS, such as the Government Accountability Office, The Federal Bureau of Investigation, the Central Intelligence Agency, the National Security Agency, the Tennessee Valley Authority, the Federal Labor Relations Authority, the Federal Impasses Panel, and the U.S. Secret Service (Hegji, 2012). While most federal employees are able to freely join labor unions and collectively bargain, the prohibition of union security agreements makes them more similar to private sector workers in states with RTW laws. However, unlike private sector workers, federal service workers cannot bargain over wages set by law.

Subsection 5: State Laws on Collective Bargaining and Union Security Agreements

There is a lot of variation in state laws governing public sector workers' ability to join labor unions, collectively bargain, and have union security agreements. At the state and local level, the right to collectively bargain, the scope of collective bargaining, and the union security agreement are usually a combination of state and local laws (Sanes & Schmitt, 2014). However, state-level laws on collective bargaining and union security agreements affect most state and local public sector workers more strongly than local-level laws do (Freeman & Valletta, 1988). Before 1960, few laws addressed collective bargaining rights for state and local public sector workers. However, during the 1960's,

many states began adopting laws that allowed for public sector collective bargaining (Freeman & Valletta, 1988). After these collective bargaining laws were enacted, there was tremendous growth in public sector unionization during the 1970's (Valletta & Freeman, 1988). Some states, however, enacted anti-union legislation by prohibiting collective bargaining for all, most, or some state and local public sector workers (Freeman & Valletta, 1988; Sanes & Schmitt, 2014). While there are a lot of complexities with laws governing collective bargaining and union security agreements for state and local public sector workers, it is important to investigate these laws, since 35.7 percent of public sector workers are unionized (Hirsch & Machperson, 2015).

Most states allow some collective bargaining and wage negotiations, but do not allow public sector workers to strike. In a majority of states, collective bargaining is allowed or it is permissible for firefighters, police, and teachers (Sanes & Schmitt, 2014). In six states, collective bargaining and wage negotiations for at least one type of public sector workers is illegal: Texas bans teachers from collective bargaining and wage negotiations; Tennessee bans firefighters and police from collective bargaining and wage negotiations; Georgia bans police and teachers from collective bargaining and wage negotiations; North Carolina, South Carolina, and Virginia ban all three from collective bargaining and wage negotiations (Sanes & Schmitt, 2014).

Union security agreements for state and local public sector workers are usually related to the amount of collective bargaining rights for these public sector workers. Usually states with stronger union security agreements tend to have stronger collective bargaining laws for public sector employees, while states with RTW laws tend to have weaker collective bargaining laws for public sector employees (Valletta & Freeman,

1988). There are exceptions to this correlation and this also varies by class of employee. Most states that adopted RTW laws for private sector workers also adopted these laws to cover public sector workers (Ichniowski & Zax, 1991).

While most states with private sector RTW laws have public sector RTW laws, there are exceptions. From Table 2-2, there are sixteen states with RTW laws specifically aimed at public sector workers: Alabama, Arizona, Arkansas, Florida, Idaho, Indiana, Iowa, Kansas, Michigan, Nebraska, Nevada, North Dakota, Oklahoma, South Dakota, Tennessee, Utah, and Wisconsin (Indiana HB 1001, 2011; Sanes, 2014; Wisconsin Act 10, 2011). In 2011, Indiana codified a 2005 executive order from Indiana Governor Mitch Daniels that eliminated collective bargaining rights for state employees (Oddi, 2011). The elimination of union security agreements for state workers, but not local workers, was included in the budget bill (Indiana HB 1001, 2011). Wisconsin enacted a public sector RTW law for most public sector workers before enacting a private sector worker RTW law (NCSL, 2015; Wisconsin Act 10, 2011). Some states have private sector RTW laws, but not public sector RTW laws, such as Georgia, North Carolina, South Carolina, Texas, Tennessee, and Virginia (Zax & Ichniowski, 1990; Sanes, 2014). These states might not need RTW laws since collective bargaining is restricted for their public sector workers. Some states have private sector RTW laws, but the restriction or permission to have public sector union security agreement is not specified. These states include Louisiana, Mississippi, and Wyoming.

Table 2-2: Public Sector RTW

State	Private RTW	Public RTW	Public RTW Applies to
Alabama	Yes	Yes	Firefighters
Alaska	No	No	N/A
Arizona	Yes	Yes	Public Employees
Arkansas	Yes	Yes	State & Municipal
California	No	No	N/A
Colorado	No	No	N/A
Connecticut	No	No	N/A
Delaware	No	No	N/A
D.C.	No	No	N/A
Florida	Yes	Yes	Public Employees
Georgia	Yes	-	Illegal to Public Sector Bargain
Hawaii	No	No	N/A
Idaho	Yes	Yes	General RTW (Public and Private)
Illinois	No	No	N/A
Indiana	Yes	Yes	State Workers, teachers, and public safety
Iowa	Yes	Yes	Public Employees
Kansas	Yes	Yes	Public Employees
Kentucky	No	No	N/A
Louisiana	Yes		None Specified
Maine	No	No	N/A
Maryland	No	No	N/A
Massachusetts	No	No	N/A
Michigan	Yes	Yes	Public employees, except public safety
Minnesota	No	No	N/A
Mississippi	Yes		None Specified
Missouri	No	No	N/A
Montana	No	No	N/A
Nebraska	Yes	Yes	State Employees
Nevada	Yes	Yes	General RTW (Public and Private)
New Hampshire	No	No	N/A
New Jersey	No	No	N/A
New Mexico	No	No	N/A
New York	No	No	N/A
North Carolina	Yes	-	Illegal to Public Sector Bargain
North Dakota	Yes	Yes	Teachers, Public Employees
Ohio	No	No	N/A
Oklahoma	Yes	Yes	Firefighters, Police, and Municipal Employees
Oregon	No	No	N/A
Pennsylvania	No	No	N/A
Rhode Island	No	No	N/A
South Carolina	Yes	-	Illegal to Public Sector Bargain
South Dakota	Yes	Yes	Public Employees
Tennessee	Yes	Yes	Teacher, Illegal to Public Sector Bargain
Texas	Yes	-	Illegal to Public Sector Bargain
Utah	Yes	Yes	General RTW (Public and Private)
Vermont	No	No	N/A
Virginia	Yes	-	Illegal to Public Sector Bargain
Washington	No	No	N/A
West Virginia	Yes	Yes	Public Employees
Wisconsin	Yes	Yes	Public Employees, except public safety
Wyoming	Yes		None Specified

Source: Indiana HB 1001 (2011); Sanes (2014); Wisconsin Act 10 (2011), West Virginia Senate Bill 1 (2016)

Section 3: Recent RTW Legislation

Historically, most RTW laws were located in western and southeastern states. All of the former confederate states adopted RTW laws, while most Great Plains states did as well, except for Oklahoma until 2001 (Holmes, 1998). This dissertation will concentrate on recent adoptions of RTW laws in the Midwestern states of Indiana, Michigan, and Wisconsin, along with the Great Plains state of Oklahoma. While West Virginia adopted a RTW law in 2016, there are no data currently available to assess the impact of this law.

After years in legislative dormancy, there has been a recent burst in legislative activity concerning RTW laws. In 2012, nineteen state legislatures debated and considered RTW bills, while twenty-one state legislatures and twenty state legislatures considered these bills in 2013 and 2014, respectively (NCSL, 2015). Even though many state legislatures debated RTW laws, only three states enacted RTW laws over the past several years. Wisconsin adopted a private sector RTW law in 2015, while Michigan and Indiana adopted private sector RTW laws in 2012 (NCSL, 2015). Michigan also adopted a public sector RTW law in 2012, along with Wisconsin in 2011 (Sanes, 2014; Wisconsin Budget Repair Bill, 2011).

A decade before this burst in legislature debates, one of the few remaining non-RTW states in the Great Plains adopted a RTW bill. Oklahoma adopted a RTW law in 2001 affecting both private and public sector workers (Collins, 2014). While Oklahoma is not a midwestern state, it is a potential state of interest, since it was a non-RTW state surrounded by states with RTW laws. Each one of these states and their laws will be reviewed.

In early 2012, Indiana was the first state, since Oklahoma, to adopt a RTW for private sector workers. Indiana House Bill 1001 was introduced on January 4, 2012 and was signed and enacted by the governor on February 1, 2012 (NCSL, 2015). This bill makes it a misdemeanor to do the following: require an individual private worker to join or remain in a labor union as a condition of employment; require dues or fees to a labor organization as a condition of employment; or require dues or payment to a charity that represents dues to a labor organization as a condition of employment (Indiana HB 1001, 2012). This bill does not affect federal employees, employees affected by other laws, such as the RLA, or state and local public employees (Indiana HB 1001, 2012). In 2011, Indiana adopted a new section in the 2011 budget bill that banned the requirement of state workers to financial support or join a union (Indiana HB 1001, 2011). Furthermore, the budget bill codified the 2005 executive order by Indiana Governor Mitch Daniels that eliminated collective bargaining for state workers and reorganization of unions as representative for state workers (Oddi, 2011).

Michigan enacted two bills in 2012 that applied to workers in the private and public sectors. While Michigan enacted RTW laws for private sector workers in 2012, it was much later in the year than Indiana. Michigan Senate Bill 116 was introduced in February 9, 2011, but it did not get out of the economic development committee until December 6, 2012 and was enacted by the signature of the governor on December 12, 2012 (NCSL, 2015). This bill amended the Michigan Public Act 176 of 1939, which deal with labor-management relations for the private sector (Michigan Senate Bill 116, 2012). It makes private employees, defined under the NLRA, not obligated to join a union, pay

union dues, pay agency fees, or pay agency fees to charity as a condition of employment (Michigan Senate Bill 116, 2012).

The second RTW law that Michigan passed was applicable to public sector workers. This bill was introduced in January 13, 2011, but was not reported to the whole Committee of Commerce until December 1, 2012 and the governor also signed this bill on December 12, 2012 (NCSL, 2015). This bill amended Michigan Public Act 336 of 1947, which deals with labor-management relations for Michigan public sector workers (Michigan House Bill 4003, 2012). The bill says that public employees, including both state and local workers, shall not be forced, intimidated, or unlawfully threatened to the following: join, remain, or financially support a labor organization; refrain from joining a labor union or financially supporting a labor organization; or pay a fee to charity in lieu of financially supporting a labor organization (Michigan House Bill 4003, 2012).

Wisconsin also passed two bills that affected public and private sector workers. The Wisconsin Senate Bill 44 of 2015 was introduced on February 23, 2015 and was enacted as 2015 Wisconsin Act 1 with the governor's signature on March 9, 2015 (NCSL, 2015). This bill prohibits the requirement of labor organization membership or payments to a labor organization as a condition of employment for private sector workers (Wisconsin Act 1, 2015).

Wisconsin passed a bill in 2011 that severely limited the scope of collective bargaining for public sector workers. The Committee on Assembly Organization introduced the Budget Repair Bill, which Governor Scott Walker requested from the Wisconsin State Legislature on February 15, 2011 and the governor signed the bill as Wisconsin Act 10 of 2011 on March 11, 2011 (NCSL, 2015). After a series of legal

challenges, the law came into effect on June 29, 2011 (Wisconsin State Legislature, 2011). This law modified the Municipal Employment Relations Act and State Employment Labor Relations Act to alter collective bargaining and union security agreements. All state and local public employees, except public safety workers, would be limited to collective bargaining for base wages only. Furthermore, the act prohibits the deduction of dues from a public sector worker's earnings for labor organizations and public sector workers, except public safety, can remain in the bargaining unit without having to pay agency/fair-share fees or dues to labor organizations (Wisconsin State Legislature, 2011).

Before the adoption of a RTW bill, Oklahoma was surrounded by the states with RTW laws. Texas, Kansas, and Arkansas adopted RTW laws for private sector workers soon after the enactment of the Taft-Hartley Act of 1947 (Collins, 2014). Furthermore, these states had public sector RTW laws in effect, as well (Sanes, 2014). On September 25, 2001 the Oklahoma State Legislature amended the Oklahoma Constitution with State Question 695, which bans labor contracts that require joining or paying dues to a labor organization as a condition of employment (Oklahoma State Legislature, 2001). This constitutional amendment is applied to private and public sector employees, since it applies to all persons (Oklahoma Constitution Article 23 §1A, 2001). However, before the adoption of this law, Oklahoma had union security agreement restrictions for local public safety workers (Sanes, 2014).

Chapter 3: Empirical Literature Review

This chapter investigates the current literature and empirical findings on the impact of Right-to-Work (RTW) laws on labor outcomes. In summary, the evidence is mixed on the impact of RTW laws on wages and employment, but it is found that free-riding reduces unionization, which is the central mechanism for understanding RTW laws (Moore, 1998). In addition, a systematic review and analysis of RTW laws on wages and employment shows that RTW laws are more likely to increase employment while decreasing wages. While the evidence is mixed, the systematic literature review includes a discussion of the quality of the studies included. This dissertation attempts to add to the literature in three ways: 1) investigate direct evidence to the impact of RTW laws on individual employment and wages; 2) analyzes the impact of RTW laws over the short-run and long-run; and 3) focuses on the adoption of RTW laws to control for endogeneity issues.

When considering the impact of RTW laws on labor outcomes, panel data and research designs help provide stronger evidence for analyzing these laws. Studies that utilize panel data and micro data help control for the endogeneity problems that Newman and Moore (1985) and Moore (1998) discuss. In addition, many earlier studies of RTW laws analyze the impacts on aggregate or state-level outcomes that include workers not subject to the NLRA, which weaken the results (Davis & Huston, 1995). Studies utilizing panel data or microdata have shown that RTW laws reduce union organizing and collective bargaining. When investigating impacts of laws at the micro-level, state level laws can be considered given since individual do not directly affect laws, but omitted variable bias still remains (Hundley, 1988; Moore, 1998). However, studies using micro

data generally show that the impact of RTW laws on labor outcomes is stronger and statistically significant than studies using state-level aggregated data (Moore, 1998).

The quality of the research design of analyzing RTW laws is important for validity. Many earlier studies used cross-sectional data, along with weak identification modeling and functional form assumptions (Ellwood & Fine, 1987). Strong research designs that help with identification issues and attempt to control for time-invariant and time-varying unobserved heterogeneity provide better estimates of the impact of RTW laws (Kennedy, 2008). Higher quality research designs, such as difference-in-difference, instrumental variable, fixed effects, and regression discontinuity methods attempt to control for unobserved heterogeneity and are more valid than the structural equation models popular in the 1980's (Angrist & Pischke, 2010).

The remainder of this chapter discusses findings from the empirical literature, along with the methods, data, and overall quality. The first section provides an overview of how RTW laws can affect labor outcomes. The second section investigates the impact of RTW laws on unionization and collective bargaining with a specific focus on free-riding. The third section discusses the systematic review conducted to examine the impact of RTW laws on wages and employment. The chapter concludes with a discussion of how analyzing the impact of RTW laws on employment and earnings will help fill gaps in the current literature.

Section 1: Overview of the Hypotheses of RTW Laws

Before investigating the impact of RTW laws on employment and wages, an investigation of RTW laws on unionization and the channels on how RTW laws can

affect wages and employment will be discussed. There are several hypotheses that are discussed in the literature. Moore (1998) provides an overview of the different ways RTW laws can affect economic outcomes and the author notes that these hypotheses are not always mutually exclusive.

Subsection 1: Free-Rider Hypothesis

The first hypothesis to be discussed is the free-rider hypothesis. This hypothesis suggests that free-riding will be greater in RTW states than union shop states, since the bargaining agents can no longer exclude workers from benefitting without paying (Moore, 1998; Zax & Ichniowski, 1990). Under the NLRA, a union operating as a bargaining agent must negotiate for all members under the collective bargaining agreement whether or not a worker under the collective bargaining unit is a member or not a member (Eren, 2009). Therefore, in RTW states, workers can reject union membership and benefit from union services (Ichniowski & Zax, 1991).

The free-rider problem is relevant to unions for several reasons. First, collective bargaining, workplace characteristics, and union organizing can be analyzed as public goods, since benefits of these goods are nonrival, but only excludable through enforcement mechanisms, such as union security agreements (Zax & Ichniowski, 1990). RTW laws forbid unions to levy a tax on the provision of workplace public goods so that they must rely on voluntary payments (Farber, 1984). Given the lack of excludability, free-riders will increase the marginal cost and total cost of union services and lead to a less than optimal supply of union services (Freeman & Medoff, 1984; Moore, 1998).

Second, given the democratic nature of unionization, union organizing, and collective bargaining agreements, the median voter theorem is relevant to the supply of union services (Hirsch & Addison, 1986; Zax & Ichniowski, 1990). If the median voter in a collective bargaining unit faces higher marginal cost for union services from free-riders, then the median voter may vote for lower union services (Moore, 1998). The effect of free-riding on wages and employment will be discussed and further analyzed throughout this dissertation.

Subsection 2: Bargaining Power Hypothesis

Another potential way that RTW laws can potentially affect wages and employment is the bargaining power hypothesis. This hypothesis is related to the free-rider hypothesis. However, the free-rider hypothesis is focused on increased marginal costs, while the bargaining power hypothesis focuses on marginal benefits. Since RTW laws prevent universal membership and unions can no longer exclude workers who do not pay in the bargaining unit, RTW laws directly reduce a union's bargaining power and position (Newman & Moore, 1985). A union's bargaining power is reduced, since RTW laws reduce union membership and resources for bargaining power, such as monetary resources for strikes (Moore, 1998). As a result of weaker bargaining power, the marginal benefits of unionization are further reduced, which leads to decreased demand for union services in the long-run (Farber, 1984).

Subsection 3: Taste Hypothesis

A third hypothesis in which RTW laws may be associated with changes in employment and wages is the taste hypothesis. The taste hypothesis says that RTW laws are related to tastes and preferences for anti-union sentiment and RTW laws are just a reflection of these tastes and have no independent effects on labor outcomes (Moore, 1998). In RTW states, workers and employers have preferences for non-unionization and the RTW law reflects for these preferences (Devinatz, 2011). The taste hypothesis suggests that, after controlling for tastes and preferences, RTW laws have no further effect on the demand or supply for union services (Wessels, 1981; Newman & Moore, 1985). According to this hypothesis, if a state were to repeal its RTW law, the impact on labor outcomes will not be affected, since tastes and preferences remain the same (Newman & Moore, 1985).

One of the more important problems that Moore (1998) and Newman and Moore (1985) discuss is endogeneity. Given this problem, the taste hypothesis is relevant, since RTW laws are not adopted randomly (Moore, 1998). The taste hypothesis suggests that lower unionization rates induce states to adopt RTW laws, while states with high unionization rates are less likely to adopt RTW laws (Newman & Moore, 1985). Before the recent adoption of RTW laws, many states had RTW laws on the books for several decades, which made endogeneity more problematic and made a valid analysis of RTW laws more difficult without an appropriate exogenous instrument. Furthermore, much prior research have treated RTW laws as exogenous instead of endogenous, which can be problematic. However, more recent research has implemented stronger research methods focusing on utilizing pre-post designs instead of cross-sectional structural equations.

These improved methods show that there is a drop in unionization after, not before, a RTW law (Ellwood & Fine, 1987).

This dissertation focuses primarily on the free-rider and bargaining power hypotheses. It is important to mention the taste hypothesis, since the literature focuses on this hypothesis. Moore (1998) concludes that more recent research shows that RTW laws have more than a symbolic effect on labor outcomes than prior research as summarized in Newman and Moore (1985). This dissertation will account for the taste hypothesis by using panel data and controlling for personal tastes and preferences. Panel data and individual-level data are important, since most studies that found RTW laws ineffective were state-level analyses (Davis & Huston, 1995). In this dissertation, the main unit of analysis is at the micro level, which helps account for tastes in the econometric analysis of the impact of RTW laws (Moore, 1998).

Subsection 4: Nonunion Workers and RTW Laws

There are two indirect effects that RTW laws can have on nonunionized labor outcomes. Lewis (1963) discusses two effects that unionization can have on nonunion outcomes, which are the spillover effect and the threat effect. While these are direct effects of unionization on nonunion labor outcomes, RTW laws can potentially reverse these effects by decreases in the extent of unionization from the free-rider and bargaining power hypotheses. However, these two effects can be offsetting, so it is unclear which effect will dominate.

The spillover effect is associated with the union-monopolist model, which will be discussed in the next chapter. In essence, the spillover effect is the result of increased

labor supply of nonunion workers from unemployed union workers who move into the nonunion labor market because rent-seeking unions bargain for higher wages and lower union employment (Farber, 2005). The increased labor supply in the nonunion worker section reduces equilibrium wages and increases nonunion labor employment (Nicholson, 2005). If RTW laws reduce the bargaining power of the union monopoly model through the bargaining power hypothesis, then union premiums will be reduced for union workers, which reduces the spillover effect in the nonunion labor market.

The threat effect refers to the threat of unionization to employers. If employers worry that nonunion workers may organize, then they may increase their compensation above the wage equilibrium to avoid unionization (Farber, 2005). The threat effect is related to the probability of unionization (Farber, 2005). If a RTW law is adopted, the probability or threat of organizing will likely be decreased through the free-rider hypothesis. Given that free-rider increase the marginal cost of organizing, the probability of organizing is reduced and the cost of union avoidance will fall (Moore, 1998). Therefore, it is expected that RTW laws will reduce the threat effect.

Section 2: The Impact of RTW on Unionization and Collective Bargaining

To see if RTW laws work through lower unionization to affect wages and employment, a brief discussion of the literature of RTW laws on unionization will be investigated. One of the most cited hypotheses for the impact of RTW laws is the free-rider hypothesis. This dissertation considers free-riding and bargaining power as major mechanisms, which reduce union organizing and collective bargaining and spills over into labor market outcomes. Since collective bargaining and organizing have public good

aspects, free-riding makes these goods more expensive and RTW laws can reduce unionization and reduce bargaining power (Farber, 1984; Sobel, 1995; Ellwood & Fine, 1987). This section investigates the evidence of the impacts of RTW laws on unionization and addresses the issues of endogeneity and free-riding.

Subsection 1: RTW Laws and Endogeneity

One of the most important studies that tackles the issue of endogeneity is a study written by Ellwood and Fine (1987). The main issue with endogeneity is that it is unclear if lower unionization causes an adoption of RTW laws or RTW laws cause lower unionization (Ellwood & Fine, 1987). Utilizing a flow model on unionization and union organizing, the authors show that RTW laws cause lower private sector union organizing, but reduced private sector unionization does not cause RTW adoption.

While the authors do not utilize micro data, they are better able to identify the effect of RTW laws with a pre-post research design and show that union organizing and union elections decline after RTW enactment. Utilizing pooled annual data from 1951 to 1977 on union organizing certified by the NLRB in seven states before and after the adoption of a RTW law, the authors look at a model of union organizing flows and utilize state fixed effects. To investigate endogeneity, Ellwood and Fine (1987) conduct additional Granger/Sims simultaneity tests to see if union organizing falls before or after an enactment of a RTW law. The authors argue that if RTW laws had a real impact on union organizing, then an effect should be seen only during the period after adoption of a RTW law and not before. In the seven states that adopted RTW laws, union organizing

was not statistically depressed, but union organizing declined 32 percent within the first 10 years after the adoption of a RTW law, which was statistically significant.

In addition to the simultaneity tests, Ellwood and Fine (1987) investigate all 50 states to look for bias from endogeneity. The authors compare the results from the 50-state analysis to the results from the 7-state analysis. Utilizing a organizing flow model, along with socioeconomic characteristics, tastes and preference for unionization characteristics, regional dummy variables, and 5-year intervals dummy variables since RTW law adoption, Ellwood and Fine (1987) find similar results to the simultaneity results. The dummy variables show RTW laws significantly reduce organizing flows by 46 percent in the first five years after adoption and by 30.1 percent 5 to 10 year after adoption of the law. As in the simultaneity tests, time periods after that do not affect organizing flows, but union membership is presumably lowered permanently from the initial adoption (Ellwood & Fine, 1987). Given the similarity of the results from the simultaneity tests and from the 50-state analysis, concerns about endogeneity are lessened.

To provide further evidence of the impact, Ellwood and Fine (1987) look at the adoption and repeal of the Indiana RTW law in 1957 and 1965, respectively. The two-year moving average in organizing experiences a sharp, discontinuous decline after the 1957 adoption of a RTW law and a sharp, discontinuous jump after the 1965 repeal. The authors state that RTW laws appear to have a real impact on union organizing, which makes organizing more difficult and costly. This corroborates the idea of the free-rider hypothesis, which will be examined further below.

Subsection 2: RTW Laws and Free Riding

While Ellwood and Fine (1987) investigated the endogeneity issue, they only speculated on the mechanisms about how RTW laws reduce union organizing. While the next chapter examines the theoretical effect of RTW laws on free-riding and bargaining power, this subsection looks at empirical evidence of RTW laws and free-riding. Later subsections discuss private sector and public sector RTW laws on free-riding.

Private Sector Free-Riding

Davis and Huston (1993) directly analyze the impact of RTW laws on free-riding. Given that RTW laws eliminate the union security agreement, the authors directly test the relationship on RTW laws on the propensity to be a free-rider. The authors define a free-rider as someone who is covered by a collective bargaining contract, but is not a member. Utilizing data from the April, May, and June 1985 Current Population Survey (CPS), Davis and Huston (1993) analyze workers who are covered by a collective bargaining agreement, but who are not union members. To be consistent with workers covered by the NLRA, the authors exclude self-employed workers, supervisors, government workers, agricultural workers, and airline and railway workers.

While Davis and Huston (1993) do utilize micro data, the empirical model could have been stronger, since it looks at a cross-section of unionization utilizing a probit and ordinary least squares (OLS) model. The authors attempt to control for factors related to the demand for union services and free-riding, which include race, gender, marital status, age, education, region, occupation, industry, and firm size. Firm size is one factor that the authors try to control for, since free-riding is easier in larger firms than smaller firms

(Davis & Huston, 1993). It is expected that free-riding is more difficult in smaller firms where social pressure to join the union is expected to be greater (Moore, 1998). To control for anti-union tastes, the authors utilize a variable called COPE, which is the proportion of votes cast by the legislative delegation of the individual's state in 1984 that agrees with the AFL-CIO positions. They try to control for anti-union preferences further by including a dummy variable for the southeastern region.

Davis and Huston (1993) show that RTW laws are positively associated with free-riders. They find that an individual is 8.0 percent more likely to free-ride in a state with a RTW law. The authors also find that the COPE and the RTW measures are highly correlated and potential multicollinearity seems to make the COPE coefficient statistically insignificant. Furthermore, as Sobel (1995) points out, not all covered nonmembers are true free-riders. While the authors find that RTW laws induce free-riding, the lack of a pre-post research design makes the results weaker. However, Davis and Huston (1995) provided a follow-up study on RTW and unionization for private sector workers using CPS micro data and an instrumental variable research design. They find that COPE and RTW are statistically significant in the expected direction. This study will be discussed further below.

Sobel (1995) also investigates the impact of RTW laws on free-riding in private sector unionization, but the author concerns a more nuanced definition of free-riders. The author distinguishes nonunion members covered by a collective bargaining agreement from true free riders and induced riders. True free riders are covered nonmembers, whose marginal benefit of unionization is greater than the marginal cost of union membership. However, they do not pay for membership, since they receive the

benefits of unionization and collective bargaining anyway. Sobel (1995) defines an induced rider as a covered nonmember, whose marginal benefit of unionization is less than the marginal cost of union members. However, they are induced to consume union services and collective bargaining, since they do not have to pay for it without a union security agreement.

Sobel (1995) uses CPS micro data from the 1989 and 1991 Merged Outgoing Rotation Group (MORG) to analyze the impact of RTW laws on private sector unionization. The author uses a two-step process to determine the number of true free riders and induced free riders. The first step is to estimate coefficients, which affect supply and demand for union jobs. The empirical method the author utilizes is a reduced-form equation probit model on the decision to take a union job or nonunion job. The author's reduced form equation is the probability of being a union member based upon the demand and supply for a union job. To test the equation, Sobel (1995) excludes covered nonmembers for all private sector workers from the CPS. Unfortunately, the author does not exclude self-employed, agricultural, and railway and airline workers who are not subject to the NLRA and RTW laws. Some of the variables utilized are demographic characteristics, years of schooling groups, experience and experience-squared, part-time status, veteran status, industry and occupation dummy variables, and state and regional dummy variables. There is no attempt to capture tastes for anti-union status, such as COPE, which Davis and Huston (1993) utilize. As a side note, the specification that includes all states and a RTW dummy variable finds that RTW laws are statistically significant and negatively associated with union membership.

After obtaining estimated coefficients for the predicted probability of union membership, the author proceeds to the second step. Sobel (1995) applies the estimated coefficients to the sample of covered nonmembers to predict the individual predicted probability of each individual being a union member. Averaging across specifications, the author finds that the predicted true free riders do not differ greatly between RTW and non-RTW states.

The research design is interesting and the use of microdata is helpful, but the results are weak. First, there is no pre-post test of the impact of RTW laws on covered nonmembers, which reduces any implication of causality. Also, the structural equation does not take into account tastes and preferences for anti-union sentiment. Also, individuals who self-select into covered nonmembers status may differ from individuals who self-select into covered members and noncovered nonmembers in nonrandom ways (Angrist & Pischke, 2010). Therefore, applying the predicted coefficients of covered members and noncovered nonmembers to covered nonmembers may be inappropriate. While the propensity to free-ride is challenged by Sobel (1995), the research design is not strong enough to reject the notion that free-riding happens from RTW laws.

Public Sector Free-Riding

Free-riding in public sector unionization is also investigated after assessing mixed empirical findings of RTW laws on free-riding in private sector unionization. Free-riding in the private sector unionization and public sector unionization may vary, so it is important to analyze both and discuss important studies in the area of public sector unionization.

To further investigate the impact of free-riding and exclusionary clauses in union security agreements on the provision of collective bargaining, Zax and Ichniowski (1990) analyze the impact of public sector RTW laws on local unionization. They look into the impact of free-riding on the provision of collective bargaining stating that benefits of collective bargaining agreements are nonrival goods for all employees covered by a bargaining agreement. Zax and Ichniowski (1990) find that free-riders from statutory prohibitions on excludability mechanisms, or union security agreements, have substantial effects on the provision of collective bargaining from public sector unions. Furthermore, they find that private informal incentives to cooperate are nowhere near as potent as formal excludability mechanisms. If a nonrival good, such as collective bargaining, relies only on informal incentives, then a substantial reduction in the level of the public good provision will occur.

Zax and Ichniowski (1990) model excludability on the decision to unionize and how RTW laws impact that decision. The authors model the expected utility of unionization and nonunionization by contrasting the expected increase in wage against the expected decrease in employment and potential higher bargaining cost from lack of excludability. The authors show that a formal excludability mechanism maximizes the difference between expected value of unionization and expected value of nonunionization. They hypothesize that the expected value of nonunionization will be greater than unionization in RTW states, since individuals may be less likely to support a union if they can free-ride.

The authors use data, which differs from typical analyses of RTW laws on unionization. Utilizing a sample of 16,861 local government departments from the 1977

and 1982 Census of Governments along with the 1980 Census of Population and Housing, Zax and Ichniowski (1990) examine whether a local government department without a local bargaining unit in 1977 formed a local bargaining unit between 1977 and 1982. The authors hypothesize that RTW laws will lower bargaining unit formations and RTW laws will negatively affect the formation of bargaining units for larger departments due to anonymity making free-riding in larger departments easier (Moore, 1998; Zax & Ichniowski, 1990). The authors note that most public sector RTW laws were adopted decades before local unionism grew in popularity.

Along with RTW law presence and size of the department, additional variables are included to test the hypotheses. Unlike the private sector, there are other types of laws that can affect collective bargaining, such as prohibitions on strikes and prohibitions on collective bargaining (Zax and Ichniowski, 1990). The authors create six mutually exclusive groups for collective bargaining rights, which are the following: duty-to-bargain with striking allowed, duty-to-bargain with interest arbitration, duty-to-bargain without striking or interest arbitration, bargaining permitted, no law set, and bargaining prohibited. It should be noted that many states with public sector RTW laws allow collective bargaining and these state differences in laws have to be taken into account in analyzing the effect of RTW laws on unionization in the public sector. Future work, which empirically analyzes the effect of public sector RTW laws, needs to take these different laws into consideration.

Zax and Ichniowski (1990) include other factors that may affect the formation of bargaining units in local government departments. The authors use conditions in local economic climates, which may affect local union abilities to form bargaining units, such

as local unemployment rate and local union density. Demographic characteristics of local jurisdictions are also included, such as population, median income, percentage of college graduates, and percentage of racial groups. In addition, dummy variables for metropolitan statistical areas and counties, along with department dummies to control for time-invariant characteristics.

Utilizing a probit model with fixed effects over time, Zax and Ichniowski (1990) find that RTW laws are statistically significant in reducing the propensity for forming bargaining units. When a RTW law exists in a state with mandatory bargaining laws, the magnitude of the RTW variable reducing propensity to form a bargaining unit doubles, which helps to indicate the potential impact of free-riding. Furthermore, the authors find that the interaction of RTW and local department size is statistically significant in reducing the probability of a local bargaining unit forming for states with mandatory bargaining laws but for all states. The severity of free-riding appears to increase with the size of a local government department when mandatory bargaining and RTW laws are in effect simultaneously (Zax and Ichniowski, 1990).

This analysis of a panel of local departments provides a better analysis of RTW laws on collective bargaining since it can control for time-invariant heterogeneity, but it is not without bias concerns. Departments may self-select into collective bargaining agreements in nonrandom ways between states with public sector RTW laws and states without them even after controlling for time-invariant characteristics of departments (Angrist & Pischke, 2010). However, utilizing better methodology than cross-sections that Davis and Huston (1993) and Sobel (1995), Zax and Ichniowski (1990) help point to

the impact of free-riding on forming a local bargaining unit, especially since the size of the department was related to the reduced probability of forming a local bargaining unit.

Ichniowski and Zax (1991) follow up their study on the effect of RTW laws on local bargaining unit formation in the public sector by investigating the difference between the taste hypothesis and the free-rider hypothesis. The authors try to isolate the free-rider hypothesis from the taste hypothesis, but focusing on the unique characteristics of local sector unionization. They find that the free-rider hypothesis provides a more compelling explanation of the reduction in union membership in local government departments, since the prohibition of a union security agreement reduces union membership regardless of the strength of collective bargaining laws or the extent of private-sector unionization. If the taste hypothesis was more compelling, RTW would reduce unionization and associations similarly, but they find that RTW laws reduce unionization more than association.

The authors argue that they are able to isolate the free-rider hypothesis from the taste hypothesis for several reasons. Unlike private sector RTW laws, public sector RTW laws were adopted before public sector unionism grew in popularity. Also, Ichniowski and Zax (1991) argue that there are other variables that are more appropriate for measures of anti-union tastes and preferences than a RTW law. Finally, the authors say that while both free-riding and tastes should lower unionization, these two hypotheses have different effects on their relative frequencies of bargaining and nonbargaining local unions. Such that, unlike unions, associations are unable to obtain a security clause, so associations in local sector are vulnerable to free-riding in states with and without public sector RTW laws. Furthermore, associations are not able to collectively bargain, so free-riders should

not have an effect on association. Therefore, anti-union sentiment may reduce preferences for any collective activity, including associations and unions (Ichniowski & Zax, 1991). If anti-union sentiment is for collective bargaining rather than collective activity, then the impact of bargaining laws will be more important for tastes than the presence of a public sector RTW law. The authors do note that RTW laws may reduce the formation of associations if associations are precursors to bargaining unions.

To test the isolation of these two hypotheses, the authors utilize the 1982 Census of Governments with a sample of 18,471 local government departments for police, fire, sanitation, public welfare, and highways in 5,811 local jurisdictions, along with the 1980 Census of Population and Housing. Utilizing these data, with reduced form equations, the authors construct three dependent variables to test the isolation of the free-rider and taste hypotheses, which are the following: percentage of employees unionized in a local government department, binary variable for the presence of a nonbargaining association, and a binary variable for the presence of a bargaining association. They also include the six mutually exclusive bargaining law variables from Zax and Ichniowski (1990) to control for the strength of collective bargaining. Other factors such as local demographics, private industry employment, and private sector unionization are used.

The authors utilize a reduced form equation in a tobit model to analyze the impact of RTW laws on the percent of unionization in all local departments. After controlling for a range of taste controls and bargaining law controls, Ichniowski and Zax (1991) find that RTW laws reduce the percent of unionization by -5.3 percentage points for highway departments, -12.4 percentage points for public welfare departments, -11.9 percentage

points for police departments, and -11.5 percentage points for fire departments. RTW laws did not have a statistically significant impact on sanitation departments.

To isolate the difference between the free-rider and tastes hypothesis, Ichniowski and Zax (1990) utilize an ordered probit model, where a hierarchy of bargaining choice is established. The categories include bargaining unit, association, and nonunion employees. Controlling for the same tastes and bargaining variables, the authors find that RTW laws reduce the probability of a bargaining unit formation in public welfare departments, police departments, and fire departments more than the probability of an association formation. The authors conclude that this reduces the explanatory power of the tastes hypothesis, since tastes should reduce all form of collective activity including associations with little bargaining power.

While the authors provide an interesting and compelling test to distinguish between the free-rider hypothesis and taste hypothesis, the cross-sectional research design does reduce the validity of its findings. There are many factors that may vary in nonrandom way among departments with unions, associations, or no unions. However, the use of microdata, along with the innovative approach to distinguishing between the two hypotheses helps point in the direction of the impact of free-riding on unionization.

Other Notable Studies of RTW and Unionization

While the free-rider hypothesis seems to be compelling, especially from Ellwood and Fine (1987) and Zax and Ichniowski (1990), many of the earlier works had issues with research designs. Many earlier studies looked into all types of outcomes, including wages and employment that will be discussed in the next two sections, found that the

taste hypothesis seemed to be more important than the free-rider or bargaining power hypotheses. However, these studies mainly focused on stock models and structural equations with aggregate data and many used structural equations instead of focusing on research designs (Moore, 1998). Many studies investigated the impact of RTW laws on the extent of unionization, but only a few more will be discussed here.

Farber (1984) uses a cross-section of CPS micro data and QES micro data utilizing a structural identification method of union supply and demand equilibriums for private sector workers. The author finds RTW laws are associated with lower rates of union representation, but higher union wage premiums. Utilizing a cross-sectional analysis of micro data, there is no pre-post analysis of RTW laws. Another issue is that the author states that managerial, sales, construction, and self-employed are removed from the samples, but does not mention if public sector employees are removed. Given that RTW laws in the public sector and private sector differ, there is likely measurement error for the RTW law. Also, there is likely omitted variable bias, since RTW laws are not randomly adopted. However, even after controlling for southern states, RTW laws still are associated with lower rates of unionization. However, the author says that the taste for anti-unionism is the reason for the decreased extent for unionization in RTW states.

For an additional study on the public sector, Hundley (1988) utilizes a sample of 12,557 state and local government workers from the 1985 CPS to examine the impact of public sector RTW laws on the extent of public sector unionization. As Zax and Ichniowski (1990; 1991) discuss, Hundley (1988) mentions that heterogeneous collective bargaining laws need to be controlled when investigating public sector RTW laws in the

public sector. Hundley (1988) uses a reduced form equation to examine the impact of RTW laws on municipal, state, teachers, police, and fire public workers. The author tries to control for a set of nonwage advantages, cost of membership for unionization, taste variables, such as extent of private-sector unionization, and bargaining laws. Even though the research design is limited, the author finds that public sector RTW laws reduce union membership for local and state public sector workers by 16 percentage points.

Subsection 3: Concluding Thoughts on Unionization and RTW Laws

Stronger research designs by Ellwood and Fine (1987) and Zax and Ichniowski (1990) help provide insight into the mechanisms of RTW laws. Many of the earlier studies concluded RTW laws are only symbolic and RTW laws have no impact on unionization (Newman & Moore, 1985). However, these studies were plagued by poor research designs and aggregate data that reduced the validity of the results (Angrist & Pischke, 2010). First, Ellwood and Fine (1987) show in a pre-post analysis of RTW laws that unionization formation drops for private sector workers and that endogeneity is possibly not as problematic for studying RTW laws as Newman and Moore (1985) discuss. Second, after controlling for time-invariant characteristics, Zax and Ichniowski (1990) show that public sector local departments are much less likely to form bargaining units in RTW states than non-RTW states. As a follow up to the review by Newman and Moore (1985), Moore (1998) concludes that RTW laws do appear to reduce the extent of unionization. There are many signs that point to free-riding as the reason for this decline in unionization and this will be analyzed in the next chapter. However, the spillovers on

employment and wages from RTW laws on unionization are investigated in a systematic review of the literature.

Section 3: A Systematic Review of The Impact of RTW on Wages and Employment

From the analysis of literature of RTW laws on unionization and free-riding, it appears that RTW laws negatively impact union organizing and levels of unionization. This section will analyze the literature to see if these negative impacts of RTW laws on unionization can also have an effect on wage and employment. A systematic review of the literature was conducted to find relevant studies on wages and employment. The first subsection provides a brief discussion of the systematic review process with a more detailed approach in the appendix. The next subsection investigates the abstracted information from the relevant studies, which includes data, research designs, variables, and findings. The last two subsections discuss the findings of RTW laws from the systematic review process, along with concluding thoughts on the impact of RTW laws on wages and employment.

Subsection 1: Brief Description of the Systematic Review Process

This systematic review employed a search strategy to find relevant articles and established inclusion and exclusion criteria to keep relevant articles for the systematic review during the spring semester of 2014. The strategy for the systematic review included several vital steps. First, a search strategy was deployed to find relevant articles. This included the databases electronically searched and keywords to find

relevant articles. The second step was to establish inclusion and exclusion criteria. This step included filtering studies that had incorrect policy interventions, incorrect outcomes, and poor research designs, along with qualitative or background articles. The third step included an assessment of the study quality of the papers that passed the inclusion and exclusion criteria. This step focused on the research design and data employed to investigate the impact of RTW laws. A more nuanced discussion of the search strategy, inclusion and exclusion criteria, and study quality and how quality is assessed can be found in the appendix.

From the systematic review, there were 16 articles that passed the inclusion and exclusion criteria to examine the impact of RTW laws on wages and employment. Out of 1840 citations found on the meta-database, AOK One Click, provided by Albin O Kuhn Library from the University of Maryland, Baltimore County, only 94 studies passed the initial title and abstract review, while 10 studies passed the inclusion and exclusion criteria from a detailed review of each study. Out of the 425 citations found on JSTOR provided by the Department of Labor, 12 citations passed the initial title and abstract review, while 6 citations were included in the final review.

Each of the 16 studies was assessed for research design quality and data quality. Studies were ranked in terms of quality from 1 to 5, where a 1 was lowest acceptable quality and 5 was the highest quality. Higher rankings were given to studies that did a better job of handling unobserved heterogeneity and endogeneity. Table 3-1 shows the quality assessment of each of the studies. The systematic review was conducted in 2014.

The research design is an essential component of an empirical study.

Randomized control trials are the gold standard for evaluating interventions (Trochim &

Donnelly, 2008). Unfortunately, RTW laws are adopted nonrandomly and need to utilize quasi-experimental designs for assessing outcomes. Given the issues of unobserved heterogeneity and endogeneity, higher rankings were given to research designs that employed difference-in-difference, instrumental variables, and regression discontinuity designs (Angrist & Pischke, 2010).

Furthermore, data quality was assessed as well. Studies that utilized panel data were considered the highest quality, since panel data provides a way to control for unobserved heterogeneity in micro units of analysis and provides more variation that reduces multicollinearity (Kennedy, 2008). Cross-sectional data were utilized by many earlier studies, but these data do not provide controls for time-invariant heterogeneity. Pooled data were ranked lower than panel data but higher than cross-sectional data, since they can help control for time-invariant heterogeneity, but cannot follow the same micro units of analysis over time (Kennedy, 2008).

Table 3-1: Ranking of Study Quality

Authors	Year	Quality	Design	Data
Li	2012	4	Diff-in-Diff	Panel
Farber	2005	3	Diff-in-Diff	Pooled Data
Holmes	1998	3	RDD/County Border	Cross-sectional
Kalenkoski & Lacombe	2006	3	RDD/County Border	Cross-sectional
Kunce	2006	3	Fixed Effects	Pooled Data
Newman	1983	3	Fixed Effects	Pooled Data
Schumacher	1999	3	Fixed Effects	Pooled Data
Stevans	2009	3	3SLS IV	Pooled Data
Carroll	1983	2	2SLS IV	Pooled Data
Hanley	2010	2	Random Effects	Pooled Data
Moore, Dunlevy, & Newman	1986	2	2SLS IV	Pooled Data
Wessels	1981	2	2SLS IV	Cross-sectional
Farber	1984	1	OLS	Cross-sectional
Garofalo & Malhotra	1992	1	Reduced Form	Cross-sectional
Moore	1980	1	OLS	Cross-sectional
Reed	2003	1	OLS	Cross-sectional

Subsection 2: Selected Abstracted Information

This subsection will discuss the information abstracted from the some of the 16 studies used in this systematic review. Each study will have an abstract of the outcome measures, explanatory variables, research design, data, and results of each study. One additional study, from Eren and Ozbeklik (2016), was added after it was published in early 2016. The additional information from studies not included can be found in the appendix.

Carroll (1983)

Carroll (1983) investigated the impact of RTW laws on unionization, real average hourly wages, and state-level unemployment utilizing pooled state-level data from 1964 to 1978. Using a two-stage least squares (2SLS) methodology with pooled data for manufacturing workers, the author assesses that the impact of RTW laws on manufacturing workers at the state level. The author finds that RTW laws were associated with a -0.322 decline in real average hourly wages, while RTW laws were not associated with unemployment rates.

Carroll (1983) estimates first-stage endogenous variables utilizing different characteristics. The main first-stage equation consists of a direct test of RTW laws on the proportion of nonagricultural workers in a state. The author utilizes the job mix of a state, a dummy variable for southern states for anti-union tastes, real average hourly wages in a state, unemployment rate in a state, and a series of time dummies. The other first-stage equation consists of a direct test of RTW laws on state unemployment, state average wages, and state real output per worker while using job mix, real average hours wages, unionization proportions, and region and time dummies as additional explanatory variables. It is important to note that the author treats RTW laws as exogenous variables in both first-stage regressions, which is debated by Moore, Dunlevy, and Newman (1986). The author also creates predicted values for real hourly wages and real output per worker in manufacturing based upon unionization, unemployment rates, and region and time dummies.

The second stage equations estimate the impact of RTW laws on state's real average hourly wages and unemployment rates. The second-stage regression utilizes the

first-stage predictions of unionized proportions, unemployment rates, real hourly wages, real output per workers, along with other factors to control for tastes. These tastes variables include demographic and education variables, along with industry dummies and proportions of production workers to paid workers.

Carroll (1983) finds that RTW laws are associated with a statistically significant 5.1 percentage point drop in unionization, a 0.32 point drop in real wages, and a statistically insignificant relationship with unemployment rates. In addition, utilizing a specification of real average hourly wages without RTW dummy but with estimate percentage unionized, Carroll (1983) finds that RTW laws through lower estimated union proportions indirectly impact real average hourly wages. A 1-percentage point drop in estimated proportion of unionized workers leads to a 0.038 drop in real average hourly wages. Carroll concludes that RTW laws do matter if RTW reduces wages indirectly through lower unionization. It is important that this model is shown to be sensitive when it is debated with Moore, et al., (1986). Furthermore, the first-stage instrument variables do not completely provide a source of exogeneity and the instruments are considered weak.

Eren and Ozbeklik (2016)

Eren and Ozbeklik (2016) analyze the impact of Oklahoma's RTW law on state-level outcomes for unionization, employment, and wages. The authors utilize a synthetic controls method to deal with identification issues and to create control non-RTW states to compare to Oklahoma. The authors find that Oklahoma's RTW law reduced state-level unionization and unionization in manufacturing, but did not find any impacts of RTW

laws on employment-population ratios, manufacturing employment, natural log of average private sector wages, and natural log of average manufacturing wages.

The authors focus on state-level outcomes to assess the impact of Oklahoma's RTW law. Current Population Survey data from 1983 to 2007 are combined with synthetic controls method to create a pre-post analysis of Oklahoma's law. This method is based upon a data-driven process that combines weighted unexposed comparison states to provide a counterfactual for Oklahoma. To estimate the significance of the impact of the RTW laws on different outcomes, the authors use placebo tests. The authors find that Oklahoma's RTW law reduced private sector unionization by 30.6 percent and manufacturing unionization rate by 21.8 percent. However, the authors find no impact on employment-population ratios, manufacturing employment, logged average hourly wages, and logged average manufacturing wages. The authors conclude that Oklahoma's population and unionization rate may have been too low for its RTW law to impact wages and employment.

Farber (2005)

Farber (2005) investigated the impact of RTW laws on nonunion wage rates and looks at the threat of unionization. A difference-in-difference method, along with pooled data from the CPS MORG, is utilized to see the impact of the implementation of RTW law in Idaho and Oklahoma. The author finds that the RTW law in Idaho was associated with a 4.2 percentage point drop in nonunion wages, but the RTW law in Oklahoma on nonunion wages was statistically insignificant.

Farber (2005) argues that looking at policy changes in Idaho and Oklahoma has an exogenous change on the likelihood of union organizing, threats of organizing, and nonunion wages, since individuals do not directly impact adoption of a policy. Using union status and wage data from the pooled cross-sections from the CPS MORG from 1977-2002, the author looks at the natural log of wages for a six-year period for Idaho and a five-year period for Oklahoma. The author then establishes an earnings function within a difference-in-difference methodology utilizing within-state variation over time to assess the impact on wage rates. The author includes additional explanatory variables to explain variation in the natural log of wages, which includes education, age, age-squared, race, Hispanic ethnicity, sex, marital status, marital status interacted with sex, industry dummy variables, state dummy variables, year fixed effects, RTW variable and Post-RTW dummy variable. The result was the Post-RTW variable had statistically significant impact on nonunion workers by reducing wages by 4.2 percent. However, RTW laws were statistically insignificant on nonunion wages in Oklahoma and for union wages in Idaho and Oklahoma.

While Farber (2005) concludes by stating that there is some support RTW laws reduce the threat effect from nonunion workers. However, there might be a few reasons why the impacts were statistically insignificant for Oklahoma. First, it is possible that not enough time had passed, since the adoption of the law to impact wages. Another issue is that the author utilizes all states in the analysis instead of comparable states and, given the relatively small labor force sizes of Oklahoma, there may not have been enough variation to find an effect of RTW. It is also possible that unionization is low in these states that RTW laws might had a marginal impact at best (Farber, 2005). Even with

these caveats, Farber (2005) provides a higher quality analysis of RTW laws than many other studies reviewed in this literature review. Given the exploitation of the policy changes and difference-in-difference methodology, this study did a better job at controlling for time-invariant heterogeneity and is one of the stronger research designs studied.

Garofalo and Malhotra (1992)

Garofalo and Malhotra (1992) assess the impact of RTW laws on unionization and wages. To see the effect of RTW laws on these outcomes, the authors establish a reduced-form equation to test two types of channels, which are the wage effect and productivity effect of RTW laws. The authors calculate reduced-form estimates from OLS regressions with cross-sectional, state-level data from the 1982 Census of Manufacturers. They find that RTW laws work through the wage effect more than the productivity effect, since the productivity effect is small in most states.

To assess the impact of RTW laws on manufacturing workers through wage and productivity effects, Garofalo and Malhotra (1992) establish a system of equations to model input-demand equilibrium. To model this system of equations, the authors establish an equation for price of labor, rate of unionization, and cost share for inputs. For the price of labor regressions, the natural log of average hourly wages is regressed upon values of capital and energy for manufacturing by state, which are obtained from the 1982 Census of Manufacturers. They also regress the natural log of the rate of unionization by each state upon proportions of employment in manufacturing workers, females, blacks, white-collar jobs, and price of labor. The cost share of labor is the third

equation that is derived from the other two regressions. Additional data are included in the three equations, such as RTW law dummies and RTW law dummies interacted with unionization.

After obtaining the OLS coefficients from the three regressions, Garofalo and Malhotra (1992) calculate reduced-form coefficients. The authors find that RTW laws have a statistically significant direct impact on unionization by lowering unionization by 4.7 percent, but not a statistically significant direct impact on wages. However, the authors find that RTW laws were associated with an indirect effect of 9.4 to 18.3 percent reduction in wages through lowered unionization. It is important to note that the study was notably weaker, given the cross-sectional nature and lack of pre-post research design. In addition, RTW laws are assumed to be exogenous and no policy changes are assessed. Furthermore, the assumption of 2000 hours per year across states seems to be a strong assumption that might be weak. However, it does provide an intriguing model to assess the indirect effects of RTW laws on wage through reduced unionization.

Hanley (2010)

Hanley (2010) explores the impact of RTW laws on income inequality from pooled metropolitan and state data from 1970 to 2000. If RTW laws induce industrial relocation, then there is expected to be decreases in income inequality in RTW states from additional employment opportunities, but higher income inequality from states losing industrial establishments. Income inequality is estimated as the ratio between the 90 percentile and 10 percentile for a metropolitan area. The author pools individual data and metropolitan data with a hierarchical linear model with random effects to assess the

impact of RTW on income inequality. The author finds that RTW laws were associated with reductions in income inequality.

To test the impact of RTW laws on income inequality, Hanley (2010) utilizes a hierarchical model with random effects. The author utilizes individual and metropolitan (MSA) data from the Census Bureau's IPUMS 1% sample. The dependent variable is the 90/10 ratio in 1970, 1980, 1990, and 2000. Explanatory variables at the metropolitan level included by the author are demographic and population characteristics of a MSA area, including race, unemployment rates, education, median rents, minimum wage, share of employment in manufacturing, share of employment in the public sector, and the natural log of population. State-level hierarchical explanatory variables include state-level variables, such as a RTW law dummy, union density, unemployment rate, natural log of state GDP, natural log of 10th percentile in 1970, and racial inequality.

The author finds that RTW laws were associated with 0.17 to 0.21 point reduction in income inequality growth. RTW states had higher levels of income inequality in 1970 but these states had stronger growth in the 10th percentile when compared to union shop states. Thus, income inequality measured by the 90/10 ratio decrease in RTW states. While this study does not directly look at wages, it does show that RTW laws are associated with stronger wage convergence. However, it did not discuss any pre-post analysis of RTW laws.

Holmes (1998)

Holmes (1998) provides an insightful identification strategy to deal with unobserved heterogeneity associated with assessing the impact of "pro-business" policies.

The author assesses the impact of RTW laws on manufacturing employment shares and manufacturing employment growth within a county. The author uses RTW laws as a proxy for “pro-business” policies and compares counties along the borders of RTW states and non-RTW states. This identification strategy is inspired by a regression discontinuity design, but the author does not disentangle RTW laws from other “pro-business” policies.

Holmes (1998) models the impact of RTW by theorizing a discontinuous jump in employment shares at state borders when a RTW law is in effect. As establishments are located farther away from the border, the opportunity cost to move from a union-shop state to a RTW state grows and the impact of the policy diminishes between the two states. Utilizing two outcome variables, Holmes investigates the impact of RTW law or “pro-business” laws on county employment shares and employment growth in manufacturing. Data on county employment shares in manufacturing and employment growth in manufacturing are obtained from the Census of Manufacturers and Census County Business Patterns. Employment growth is the growth in manufacturing employment between 1947 and 1992, while county employment shares are assessed in 1992. The running variables for the regression discontinuity design are distance from the border and distance along the border. The author does not include any other additional explanatory variables.

The author finds that RTW laws were associated with a 6.6 percent increase in manufacturing employment shares and 23.1 percent growth in manufacturing employment. This identification strategy provides stronger internal validity to assess the impact of RTW laws on employment, but the validity is threatened from time-varying unobserved heterogeneity given that only cross-sectional data were utilized.

Furthermore, it is difficult to disentangle the effects of RTW laws from other “pro-business” laws.

Kalenkoski and Lacombe (2006)

Kalenkoski and Lacombe (2006) provide a study that investigates the impact of RTW on employment share of manufacturing in counties from the 2000 Decennial Census. The research design was inspired by the identification strategy of Holmes (1998), but the authors employ a spatial autocorrelation model, along with the border county strategy, to see the impact of RTW laws. In regard to the data, the authors used a cross-section from 2000 for employment shares. They find that RTW laws are positively associated with increases in manufacturing employment shares.

Kalenkoski and Lacombe (2006) discuss the importance of spatial autocorrelation when assessing the impact of RTW laws on county employment. The authors utilize a mixed regressive-spatial autoregressive (SAR) model to control for omitted variable bias from spatial correlation from agglomeration economies, employment centers, and measurement errors related to employment shares. The authors use cross-sectional census data of selected economic characteristics to test the impact of RTW in a SAR model. The main dependent variable is manufacturing employment as a share of total private employment in a county in 2000. The authors do look at shares of employment for other industries, as well. Explanatory variables include a RTW law dummy variable, along with county-level variables for labor supply and anti-union tastes, such as education, population, sex, race, and mean travel time to work. To disentangle RTW

laws from pro-business laws, the authors include the Small Business Survival Index from the Small Business & Entrepreneurship Council to control for pro-business climates.

The authors find that RTW laws were associated with a 2.12 percent increase in manufacturing employment share. The authors conclude that the results were 30% less than Holmes (1998), since spatial correlations were controlled in this study. The authors provided a strong research design that helps improve the identification strategy of Holmes (1998). The authors did discuss panel data, but choose to utilize cross-sections, which limited the overall strong study.

Kunce (2006)

Kunce (2006) studies the impact of RTW and other “pro-business” laws on manufacturing employment. The author focuses on four SIC 2-digit industries to examine factors that affect employment in these industries. A fixed effect research design, along with state-level pooled data from the Annual Survey of Manufacturers from 1974 to 1994, is used to assess the impact on RTW laws on industry-level employment. The author finds that RTW laws are associated with higher employment in states with RTW laws.

Kunce (2006) utilizes the Annual Survey of Manufacturers to assess the impact of state policies on manufacturing employment. The dependent variable to be assessed is change in state-level manufacturing employment over five 5-year periods from 39 out of 48 contiguous states for Chemical and Allied products, Food and Kindred products, Paper and Allied products, and Primary Metals industries. To assess the impact on manufacturing employment change, the author establishes a two-step fixed effects

method for the chemical, food, and metal industries and a two-step generalized least squares for the paper industry to allow for state heteroskedasticity, cross-state correlation, and within state autocorrelation. Explanatory variables include urban highways miles, percentage of manufacturing employment unionized in the state, average price of natural gas, pollution abatement expenditures, a tax effort index, the number of heating days, percent of state population with at least 12 years of education, percentage of state manufacturing employment to state total employment, and a presence of a RTW law. Paper and Allied products is the only industry affected by RTW laws, since all of the other industries experienced no variation in RTW laws and were swept out by state fixed effects.

The author finds that RTW laws were associated with a 0.394 percent point increase in paper manufacturing employment growth. This study provides higher quality study of the impact of RTW laws on employment, since it controls for time-invariant unobserved heterogeneity and utilizes a change in the RTW variable. However, it only examines the impact of RTW laws on the paper industry and the sample size is quite small. Furthermore, the ASM is a sampling of establishments within states and not a true panel where establishments are followed over time. Therefore, the data are classified as pooled data instead of panel data.

Li (2012)

Li (2012) investigates the impact of RTW laws on wages and employment before and after mergers. The author utilizes panel data of establishment-level microdata of wages and employment from the Longitudinal Business Database (LBD) from 1976 to

2005. A difference-in-difference methodology is used to assess the impact of RTW laws before and after mergers. The author finds that RTW laws exacerbate negative wage and employment outcomes for unionized workers.

Li (2012) assesses the impact of company takeovers in the short-run wage and employment outcomes. The author establishes crossover, establishment-level panel data on employment and wages from Census LBD and company mergers from the Securities Data Company's domestic mergers and acquisitions database from 1981 to 2002.

Additional data on industry-level unionization are obtained from the Union Membership and Coverage Database. The four dependent variables that are analyzed are annual wage per worker, employment, wage growth rates, and employment growth rates. The methodology utilized is a difference-in-difference that utilizes the impact of mergers on labor outcomes with and without a RTW law. To further assess the impact of union, a triple difference-in-difference is utilized with a post variable, RTW variable, and percentage unionized variable interacted. No additional explanatory variables are utilized other than establishment fixed effects and year fixed effects.

Li (2012) finds that wage losses and employment losses for unionized workers are worse in RTW states compared to non-RTW states after a merger. Establishments with a RTW law and unionized workers were associated with a reduction of 10.2 percent in level of wages for unionized workers and a 15.3 percent reduction in wage growth compared to establishments in a union-shop state with a unionized workforce. Establishments with a RTW law and a unionized workforce were associated with a reduction of 0.5 percent in the level employment and 1.2 percent reduction in employment growth compared to establishments without a RTW law and a unionized

workforce. This study provided the strongest level of internal validity given that the author controls for unobserved heterogeneity through panel data and pre-post tests. It is also consistent with the bargaining power hypothesis and free-rider hypothesis given the RTW laws ability to weaken unions.

Moore (1980)

Moore (1980) provides an earlier examination of the impact of RTW laws on unionization, union wages, and nonunion wages. A cross-section from 1970 from the Panel Study of Income Dynamics (PSID) is used to assess the impact. The research design employs an OLS regression along with industry fixed effects. The author finds that RTW laws were not associated with worker earnings.

Moore (1980) assesses the impact of RTW laws on union and nonunion earnings utilizing a human-capital model. To analyze the impact of RTW laws, the author uses a sample of 5,000 heads-of-households from a cross-section of the 1970 PSID, which contains valuable labor force characteristics and location. The dependent variable is the natural log of the individual's 1970 average hourly wage rate. Explanatory variables included in the human-capital model are education dummies, experience dummies, location dummies, occupation dummies, industry dummies, race, sex, marital status, union status, and a RTW dummy variable. The author provides estimates for five groups: the total sample, union sample, nonunion sample, RTW sample, and union-shop sample.

The author finds no statistically significant effect of RTW laws on hourly earnings. However, the author does find a small (1.41 percent) relative wage advantage for union members in RTW states. The research design employed had several

limitations. It utilized an OLS regression along with industry, location, occupation, and other fixed effects. While the use of microdata helps with multicollinearity, the author did not exploit the panel data that could have reduced time-invariant unobserved heterogeneity. Furthermore, the author includes public sector employees and possibly other private sector employees not under the NLRA, which will likely downward bias the impact of RTW laws.

Reed (2003)

Reed (2003) assesses the impact of RTW laws on wages of all workers. To test the impact, the author uses a cross-section of data from the Bureau of Economic Analysis (BEA) from 2000, along with an OLS regression model. While this would have been excluded in the inclusion-exclusion criteria, the author attempts to control for factors that affected a state's likelihood to adopt RTW laws in 1945 and puts the model through a sensitivity analysis. The author finds that RTW laws are positively associated with wages for all workers, which was unlike most other studies assessed in this literature review.

Reed (2003) attempts to directly assess the impact of RTW laws on state-level average wages. The dependent variable utilized by the author is the natural log of average wages in 2000 from BEA. The author attempts to control for initial conditions of RTW states in 1945 before most states adopted RTW laws. These explanatory variables include state's average annual temperature, share of employment in agriculture in 1945, share of employment in manufacturing in 1945, population density in 1945, southern regional dummy variable, state-level educational attainment in 1945, a RTW dummy

variable, and an RTW law dummy interacted with a natural log of BEA Per Capita Personal Income in 1945. The author uses both the Akaike Information Criterion (AIC) and Schwarz Information Criterion (SIC) for determining model specification in the OLS regression.

The author finds that RTW laws are positively associated with wages for all workers. When controlling for per-capita income in 1945, average wages in RTW states are 6.68 percent higher than union-shop states. The author does provide an important notion on the inclusion of initial economic conditions before adopting a RTW law when assessing the impact of RTW laws on labor outcomes. It might indicate that poorer states that adopted RTW laws had faster wage growth. However, this study likely provides a limited assessment of the impact of RTW laws due to the data and research designs utilized. First, the author drops policy changing states of Idaho, Louisiana, Oklahoma, and Wyoming, which could have been utilized for variation in RTW laws. Second, the author uses average state wages and does not explicitly mention whether or not public sector workers and non-NLRA private sector workers are included. Third, over a long period of time, there may be many time-varying unobserved heterogeneity that may upward bias the results.

Stevans (2009)

Stevans (2009) investigates the impact of RTW laws on economic and business conditions within a state. The author discuss the endogenous nature of states with pre-existing RTW laws and uses an instrumental variable design along with a three-stage least squares (3SLS) method. Pooled data are utilized from U.S. Small Business

Administration's Small Business Economic Indicators to assess the impact. The author finds that RTW laws are associated with a decline in state-level wages for workers, but RTW laws are associated with a rise in state-level proprietor income.

Stevans (2009) investigates the impact of RTW laws on state-level worker wages, proprietor income, and unemployment rates. Other dependent variables investigated are beyond the scope of this systematic review. To assess the impact the author utilizes pooled data from U.S. Small Business Administration's Small Business Economic Indicators from 1990, 1995, 2000, and 2005. Additional data on proprietor income, employment rates, and state GDP are obtained from BEA. The author utilizes a three-stage least squares (3SLS) method to assess the impact of RTW laws on these outcomes. First, the author utilizes a probit model to generate the probability of a state having a RTW law. The instruments utilized are the ratio of farm to non-farm employment, ratio of service employment to non-farm employment, growth in state population, the ratio of college graduates to high school graduates, year fixed effects, and regional fixed effects. After obtaining estimated probabilities of RTW, Stevans (2009) estimates the state's real state GDP growth rate in the second stage using the RTW variable, predicted probability of RTW, year fixed effects, and regional fixed effects as explanatory variables. The third stage includes the dependent variables of interest and regresses them upon the RTW variable, predicted real state GDP growth, year fixed effects, and regional fixed effects.

The author finds that RTW laws are associated with a 2.3 percent decline in state-level wages and salaries and with a 1.9 percent rise in state-level proprietor income. The author finds that RTW laws are not associated with the state-level unemployment rates. The author investigates the impact of RTW laws on state-level data instead of micro-level

data, which may be more vulnerable to unobserved heterogeneity and higher multicollinearity. The author also utilizes an intriguing research design that appears to be more valid than the earlier 2SLS research methods. However, the instrument variables do not completely provide a source of exogeneity and the instruments are considered limited.

Wessels (1981)

Wessels (1981) provides another early study of the impact of RTW laws on economic outcomes, such as unionization and wages. The author uses a simultaneous equation framework, along with a 2SLS research design. The author was an early adopter in utilizing instrumental variables to account for potential endogeneity with RTW laws. To test the impact the author uses cross-sectional data from the 1972-1973 Quality of Employment Survey (QES) and finds that the impact of RTW laws was statistically insignificant.

Wessels (1981) wants to model the impact of unionization, strikes, and wages in a framework that accounts for the interrelationship of the outcomes. The author explores the impact of RTW laws on state-level data using 1970 Census data. State-level dependent variable is the natural log of production worker hourly wages in 1970. The author sets up four simultaneous state-level equations, including a first-stage equation of RTW sentiment. RTW sentiment is estimated with a probit model to see the estimated probability of having a RTW law. The RTW sentiment variable is regressed upon the natural log of hourly wages for production workers, the percent of nonagricultural union workers in a state, the 4-year average of work stoppages in a state, the presence of a

RTW law, and other explanatory variables, such as median years of schooling, percent of nonunion occupations, firm sizes, average strike cost, urban density, population density, female population, nonwhite population, and a North Central region dummy. The estimated RTW sentiment is then used in the second stage for wages, percent unionized, and strikes. The second stage for natural log of production worker hourly wages at the state level is regressed upon estimated RTW sentiment, the presence of a RTW law, percent of nonagricultural union workers in a state, the 4-year average work stoppages in a state, as well as other explanatory variable explained above. The author finds that RTW laws are not statistically associated with wage outcomes at the state level.

Wessels (1981) also investigates the impact of RTW laws on individual labor outcomes. The author utilizes the 1972-1973 QES, which includes a sample of employed workers holding a single job, 16 years or older, employed for pay, and working 20 hours or more per week. The author's first-stage for the impact of RTW on individuals is to model the individual's decision to join a union. The second stage is to model individual wages. The first-stage union member status is regressed upon individual-level variables, such as education dummies, occupation dummies, industry dummies, regional dummies, tenure dummies, hours worked, and number of kids in a household and a state-level RTW variable and other state-level variables discussed in the prior paragraph. The estimated union status is then included in the second stage. The second-stage regression regresses the natural log of income upon the estimated union status, individual explanatory variables used in the first stage, and state-level variables. The author finds that RTW laws are not associated with worker incomes.

The results from this study show that RTW laws were not statistically significant on wages for workers at the individual and state levels. This was another study that utilized a weak instrument to predict the adoption of a RTW law. The first stage instrument does not provide a strong source of exogeneity and reduces the internal validity of the study. Also, the first-stage for union members does not include marginal cost or marginal benefit variables. In addition, the QES sample included workers not subject to the NRLA, such as public sector workers, that downward bias the results.

Subsection 3: Findings

A total of sixteen studies were included in the systematic review. Out of the sixteen studies, there were twenty outcomes of interest. Carroll (1983), Holmes (1998), Garofalo & Malhotra (1992), and Stevans (2009) have more than one outcome in their studies. The different outcomes and authors can be seen in Table 3-2. The findings are separated into two parts, which are wage findings and employment findings. The systematic review kept studies focused on the criteria discussed in the appendix. The descriptive overviews of the studies, including research designs, data, and results, can be found in subsection 2.

Table 3-2: Types of Outcomes by Study

Authors	Year	Primary Outcome	RT W	Secondary Outcome	RT W
Kunce	2006	Employment	+	Employment Growth	+
Holmes	1998	Employment Share	+		
Newman	1983	Employment Share	+		
Kalenkoski & Lacombe	2006	Employment Share	+		
Schumacher	1999	Wage Differentials	-	Unemployment Rate	NS
Hanley	2010	Wage Inequality	-		
Carroll	1983	Wages	-		
Farber	1984	Wages	-		
Farber	2005	Wages	-		
Garofalo & Malhotra	1992	Wages	-		
Li	2012	Wages	-	Union Employment	-
Moore	1980	Wages	NS		
Moore, Dunlevy, & Newman	1986	Wages	NS		
Reed	2003	Wages	+	Unemployment Rate	NS
Stevans	2009	Wages	-		
Wessels	1981	Wages	NS		

Wage Findings

Many studies focused on the impact of RTW laws on wages. From the review of the literature, there were mixed results from the wage findings. For the impact of RTW laws on level of wages, there were 10 outcomes and the findings were the following: 6 studies found that RTW laws were associated with decreases in wages; 1 study found that RTW laws increase wages; and 3 studies found that RTW laws were not associated with wages. However, studies with better research designs were more likely to find that RTW laws are associated with decreases in wages. Table 3-3 shows that the only study to find that RTW laws were positively associated with the level of wages was from Reed (2003).

Levels of wages by types of workers followed a similar pattern. For different types of workers, 3 out of 6 outcomes found that RTW laws were associated with lower levels of wages for all workers, while 2 were statistically insignificant and Reed (2003) found the only positive association. Focusing on only manufacturing workers, 2 out of 3 studies found that RTW laws were associated with lower wages, while Moore, Dunlevy, and Newman (1986) found RTW laws to not be associated with wages. Farber (2005) found that RTW laws were associated with lower levels of wages for nonunion workers, which the author concludes that the threat effect kept wages higher in non-RTW states.

There were other wage-related outcomes that were assessed along with level of wages. Hanley (2012) finds that RTW laws were associated with milder income inequality growth. This was driven by growth in wages at the 10th percentile for individuals in RTW states compared to non-RTW states. This may go along with Reed (2003) findings when initial 1947 characteristics of RTW state are considered. Given that RTW states were poorer when RTW laws were adopted, it is hard to distinguish how much of the RTW policy has had on the convergence of income and wages between poorer southern states and more affluent northern states. Schumacher (1999) finds lower union wage differentials when unions are weak and free-riders are highly concentrated in RTW states, while Farber (1984) and Moore (1980) found that RTW laws are associated with larger union wage premiums. However, Schumacher (1999) also finds that RTW laws were associated with higher free-rider premiums. This wage differential grew for union members in RTW states compared to non-RTW states, which the author concluded that unions have to sell benefits to get more members.

Table 3-3: Wage Findings

Wages	All Wage Concepts	Wage Levels	Wage Differential	Income Inequality
RTW increases	2	1	1	0
RTW decreases	8	6	1	1
RTW was insignificant	3	3	0	0
Total	13	10	2	1

Wages Levels	Wages Levels	All Workers	Manufacturing Workers	Nonunion Workers
RTW increases	1	1	0	0
RTW decreases	6	3	2	1
RTW was insignificant	3	2	1	0
Total	10	6	3	1

Employment Findings

Many studies also focused on the impact of RTW laws on employment. From the systematic review, there was some evidence that RTW laws are positively associated with manufacturing employment. However, they were not associated with lower unemployment rates at the state level. From Table 3-4, 5 out of 8 employment outcomes found that RTW laws were positively associated with manufacturing employment. Newman (1983), Holmes (1998), Kunce (2006), and Kalenkoski and Lacombe (2006) find that manufacturing employment shares or employment growth in manufacturing. 2 out of 8 employment outcomes found that RTW laws were not associated with employment. Carroll (1983) and Stevans (2009) found that RTW laws were not associated with state-level unemployment rates. Li (2012) found that RTW laws were associated with declines in levels of union employment after mergers. It is possible that

RTW laws affect industry employment at the establishment level, but are ineffective at the state-level.

Table 3-4: Employment Findings

Employment	All Employment Concepts	Employment Share of MFG	Employment Growth in MFG	Unemployment Rate	After Mergers
RTW increases	5	3	2	0	0
RTW decreases	1	0	0	0	1
RTW was insignificant	2	0	0	2	0
Total	8	3	2	2	1

Subsection 4: Concluding Thoughts from the Systematic Review

The findings of this systematic review show opposing effects on wages and employment from RTW laws. When higher quality research designs are taken into account, RTW laws are associated with lower wages, but higher employment.

Furthermore, the findings provide evidence of the free-riding hypothesis, bargaining hypothesis, and the threat effect. In addition, there may be a connection between RTW laws and employment growth in the long-run for competitive industries, such as manufacturing.

The findings from studies with strong research designs give some notion that the bargaining hypothesis weakens a union's ability to bargain for higher wages and that the free-rider hypothesis reduces quantity supplied for unionized employment. For wages, Li (2012) utilization of panel data and strong research design helps show that RTW reduces wages for union workers and employment after a merger. Schumacher (1999) finds that free-riders are more likely to concentrate in industries and occupations were union

bargaining power is weak. In addition, Farber (2005) finds that wages for nonunion workers fell after Idaho adopted a RTW law. This provides some evidence that RTW laws reduce the threat effect. This might indicate that RTW laws are associated with short-run wage losses.

For employment, Holmes (1998) and Kalenkoski and Lacombe (2006) show a strong research design that RTW laws and proximity to the state border matter to the manufacturing employment. These findings show that nonunion employment and manufacturing employment shares in more unionized industries may have gains when a RTW law is present. Combining these results with Hanley (2010) and Reed (2003) that find RTW laws reduce income inequality growth and have positive impacts on wage when controlling for initial characteristics, employment gains in early years might imply gains in establishment and capital relocation, which can spillover to wage gains in the long-run. This goes along with Newman (1983) finding of industrial relocation to southern states. For this dissertation, a similar research design to Holmes (1998) will be utilized and discussed for the long-run analysis of RTW laws.

Section 4: The Contribution of Dissertation Research

This dissertation hopes to contribute to literature in several ways. First, while several authors have utilized panel microdata sets, no author has analyzed the impact of RTW utilizing individual transitions or individual fixed effects. If RTW laws reduce unionization through the free-rider hypothesis and bargaining hypothesis, it can be investigated if individuals are more likely to experience wage declines after the implementation of a RTW law. Individuals with union wage premiums may feel the

pressure to renegotiate wages or face unemployment (Li, 2012). Furthermore, if employment and RTW laws are positively correlated, then there may be more employment opportunities for individuals (Kalenkoski & Lacombe, 2006). Such that, employed individuals may find more opportunities to change jobs and unemployed individuals may have more opportunities to find work. Overall, unionized workers with union wage premiums or nonunionized workers with threat effect wage premiums may feel pressure to cut wages or lose a job, while unemployed individuals may get the opportunity to gain employment and thus have a higher wage than before (Farber, 2005).

Another factor is the analysis of RTW laws on wages and employment in the long-run. Ellwood and Fine (1987) did look at unionization by states in the long-run, but at higher aggregate levels. This dissertation hopes to combine a long-run analysis utilizing the research design employed by Holmes (1998) and Kalenkoski and Lacombe (2006) to see the long-run impact of RTW laws on wages and employment. These short-run and long-run wage and employment concepts will be discussed in further detail in the conceptual model chapter.

Two of the biggest issues in the empirical literature is unobserved heterogeneity and endogeneity. Many times, authors tried to get around this issue with reduced form equations or 2-stage least square regressions with weak instrumental variables. As Ellwood and Fine (1987) show, it is important to look at the impact of the adoption of a RTW law on labor outcomes. They found that endogeneity did not bias their findings. Furthermore, as Moore (1998) discusses, utilizing panel microdata can get around the issue of endogeneity, since individuals are not able to directly impact the adoption of a RTW law. The upcoming data and methodology chapters will discuss how this

dissertation will utilize both research designs and panel microdata to control for endogeneity.

Chapter 4: Research Questions, Conceptual Model, and Hypotheses

This chapter discusses the research questions, the conceptual model, and hypotheses for the dissertation. After an investigation of the central issues, the legal framework of Right-to-Work (RTW) laws, and the empirical literature on RTW laws, the research questions are ready to be established. The research questions are divided into two major portions, which are short-run and long-run impacts. In order to evaluate the research questions properly, this chapter develops a conceptual model. This model is corroborated by the results from the empirical literature review. The final section establishes the relevant hypotheses for the short-run and long-run impacts of RTW laws.

Section 1: Research Questions

This dissertation tests empirical support for the views of proponents and opponents of RTW laws discussed in Chapter 1. Evidence that favors RTW laws would occur if RTW laws lead to a significant increase in the probability that workers with no jobs obtain jobs. Evidence against RTW laws exists if RTW laws increased the probability that workers lose their jobs for workers in RTW states. Proponents claim that RTW laws give more flexibility and better earnings for individuals, while opponents claim that loss of bargaining power will reduce wages. Evidence in favor of proponents will show increases in earnings, while evidence for opponents of RTW laws will show decreases in earnings. Evidence that supports opponents of RTW laws will show that individuals will be less likely to be union members after a RTW law, while evidence for

proponents will show no change in union status for individuals. The empirical literature review provided information that RTW laws were associated with lower unionization and lower wages, but higher employment. It is possible that both opponents and proponents are partially correct in their arguments. Overall, moving down a typical labor demand curve leads to decreases in wages and increases in employment.

Furthermore, this dissertation tests the proponents' argument that RTW laws make a state's labor force more attractive for industries, which promotes new job opportunities. In the long-run, establishments and capital have time to readjust to labor wages (Ehrenberg & Smith, 2015). Therefore, a steady state can develop after RTW has been in place for a prolonged period of time. Holmes (1998) and Kalenkosk and Lacombe (2006) find that larger employment shares occur at state borders between RTW and union shop states. However, the authors did not examine policy changes. Evidence in favor of proponents will show a larger increase in establishments, wages, and employment after a state adopts a RTW law. Evidence that wages and employment are lower after a state adopts a RTW law would support opponents.

Subsection 1: Short-Run Impacts

Since Ellwood and Fine (1987) find unionization is most affected by RTW laws within the first ten years of adoption, this dissertation will examine four questions concerning the impact of RTW on short-run impacts. Li (2012) and Farber (2005) find short-run declines in union and nonunion wages for workers in RTW states, so this dissertation will examine what happens to individuals in the short-run after adoption of a RTW law. Examining panel data, do RTW laws increase or decrease the probability that

a worker with no job will find a job? Second, do RTW laws increase or decrease the probability that a worker with a job will lose their job? Third, do RTW laws increase or decrease wages or earnings for an individual? Forth, do RTW laws increase or decrease the probability that a worker will remain a union member?

Subsection 2: Long-Run Impacts

For a long run analysis, this dissertation will investigate three questions related to county-level outcomes. Holmes (1998) and Kalenkoski and Lacombe (2006) find that employment shares are higher when analyzing border discontinuities. This dissertation will examine the impact of RTW laws on potential discontinuities at state borders for states that have had RTW implemented for many years. A state border discontinuity is a discontinuous jump in outcomes at a state border that are similar in many aspects except the treatment. Focusing on manufacturing, which is a historically union-intensive sector, the research questions will look at RTW impacts along state borders before and after a RTW law. First, do RTW laws increase or decrease the number of manufacturing establishments or employment shares in manufacturing in a county? Do RTW laws increase or decrease average county-level manufacturing employment or manufacturing share of employment? Finally, do RTW laws increase or decrease average weekly wages in manufacturing or shares of total wages for manufacturing?

Section 2: Conceptual Model of RTW Laws and Labor Markets

The economic analysis of RTW laws is an analysis of unions and labor markets.

The conceptual model is established by discussing union and nonunion labor markets

when formal excludability mechanisms, such as a union security agreement, are legal. The conceptual model considers what happens when RTW laws eliminate union security agreements and reduces the amount of union services provided. The reduction of union services provided then spills over into labor outcomes in both the union and nonunion labor markets.

Overview of Conceptual Model

When a legal excludability mechanism is in place, unions are able to collectively bargain in a monopolized labor market and bargain for greater than equilibrium wages, which can result in employment losses. With the threat of unionization, nonunionized labor market may threaten to organize, which induces firms to provide wages beyond the perfectly competitive level to offset the threat while reducing employment. When a RTW law is adopted, the conceptual model will show that RTW laws eliminate the excludability mechanisms for providing union services and free-riders increase the cost for providing union services to the median voter of the union. This spills over into the unionized and nonunionized labor markets.

For the unionized labor market, RTW laws reduce the amount of union services provided. This decrease leads to a reduction in bargaining power and hinders unions from bargaining for wage differentials, or wage differences, for union members. In view of the lower wage differentials, more workers opt out of the union labor market for the nonunion labor market. Even with a decline in wage differential, union wage premiums, or union wages relative to nonunion wages, can still exist, since former union workers opting into the nonunion labor market puts downward pressure on nonunion wages and

entice employers to utilize nonunion workers instead of union workers. In addition, RTW laws reduce the threat of organizing through higher organizing costs. This reduces the threat effect and nonunion wages move closer to perfectly competitive wages and employment. The conceptual model will be utilized along with the findings from the empirical literature review to establish hypotheses concerning the research questions.

The conceptual model will be divided into five subsections to analyze the theoretical framework of RTW laws in unionized and nonunionized labor markets. The first subsection provides a brief discussion of union goals and models of union analysis when a formal excludability mechanism, such as union security agreements, remains legal. The second subsection investigates the theoretical impacts of unionization on the non-unionized labor market when excludability mechanisms are enforced. The third subsection discusses the public good characteristics of collective bargaining and the median voter model. This section also discusses what happens to union services when RTW laws eliminate union security agreements, or the excludability mechanism. The final two subsections examine labor market outcomes when unions are no longer able to exclude free-riders.

Subsection 1: Unionized Labor Market without a RTW Law

This subsection investigates unions as an institution in the labor market and their goals in the labor market when excludability is possible. As an institution, trade unions have been a subject of economic inquiry since Adam Smith (Freeman & Medoff, 1984). Typically, labor unions are viewed as associations of employees with common interest to improve the well-being of their members (Hirsch & Addison, 1986). When unions are

certified as a sole representative agent, they try to improve their members' well-being through collective bargaining and the political-lobbying process (Hirsch & Addison, 1986). While unions work on behalf of the members as the sole bargaining agent with the employer, they must bargain on behalf of all employees in the collective bargaining unit whether they are a member or not (Eren, 2009). As long as an excludability mechanism, such as a union security agreement, is in place, all workers who benefit from union services have to pay a cost for representation. This subsection considers several models of analysis and there are some effects of unionization that are generally agreed upon for wages and employment in a unionized labor market. It is expected that wages will be higher than wage outcomes in a perfectly competitive market and it is expected that employment will be lower than employment in a perfectly competitive market.

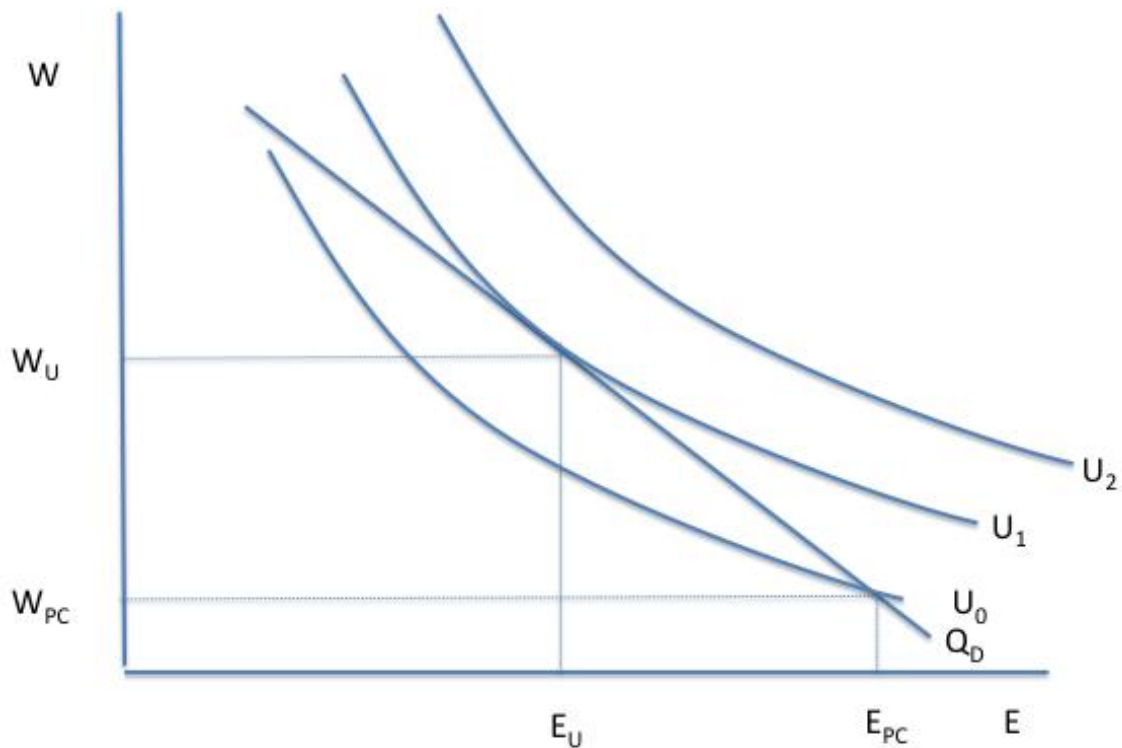
This subsection discusses the economic analysis of unions in the labor market in several sections. First, when unions bargain collectively, they must face a trade-off between their wages and employment goals. These choices depend upon elasticities in the labor and product markets, along with market structure. Second, models for analyzing the impact of unions are based upon the framework of Freeman and Medoff (1984). The efficient-contracts model is briefly discussed as well. Third, differences in unionization between the public and private sector are discussed briefly. Finally, there is a brief discussion of how the impact of unionization varies in the short and long-run.

Union Goals and Wage-Employment Trade Off

Unions face a trade-off between wages and employment when collectively bargaining. While unions have many goals, such as increasing compensation, improving

working conditions, benefit composition, job security, and influencing workplace jurisprudence, the main focus of the union goals is wages and employment (Hirsch, 2008). When a firm faces a unionized labor market, the representative union is the exclusive bargaining agent to provide a collective voice for the worker preferences in setting goals (Hirsch, 2008). To simplify the union's goal, the utility of a union is a function of wages and employment. The union faces a trade-off when bargaining for higher wages along the labor demand curve for unionized labor, since wages above competitive wages, or wage differentials, results typically in employment losses (Nicholson, 2005). The union can maximize utility subject to the constraint of the labor demand curve at the point of tangency (Ehrenberg & Smith, 2015). In Figure 4-1, the utility of the union is maximized at WU and EU on the indifference curve U1. The competitive market outcome of higher employment and lower wages lies on the union indifference curve U0, but a union maximizes its utility function by bargaining for higher wages and lower employment.

Figure 4-1: Employment Wage Tradeoff



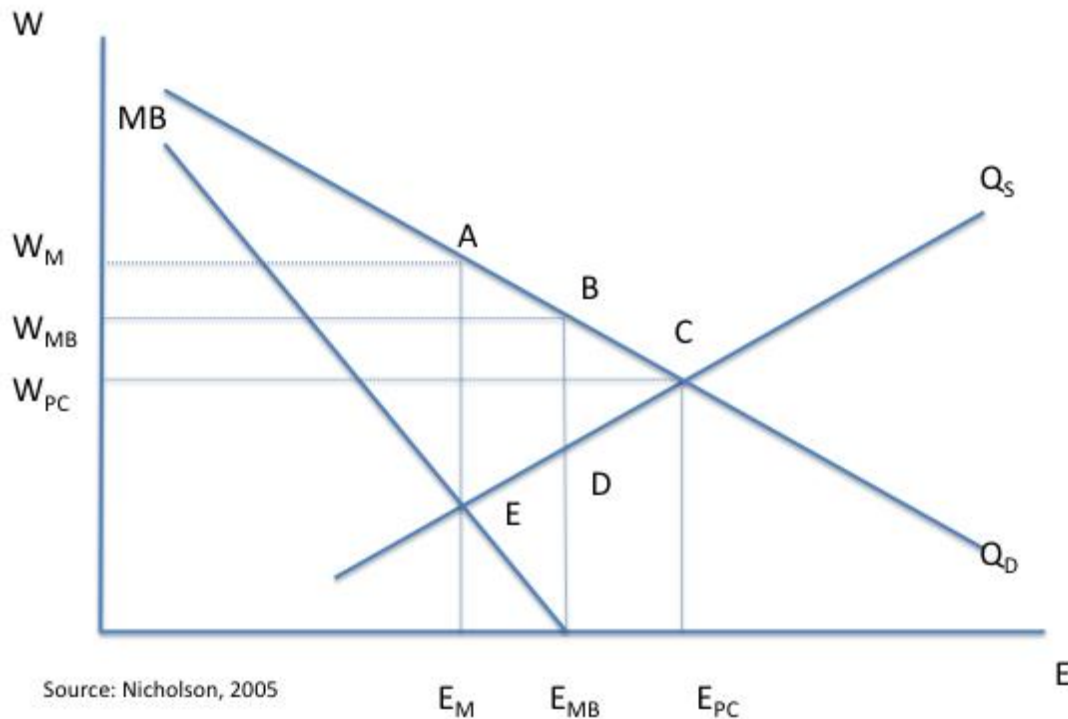
Source: Ehrenberg & Smith, 2015

Monopoly Model

The standard neoclassical economic analysis of unions considers a monopolized labor market. This framework will be the basis of the conceptual model for the analysis of the impact of RTW laws. Under the NLRA, the union agent becomes the exclusive bargaining agent for the workers (Hirsch, 2008). With exclusive bargaining power, unions obtain a form of market power to obtain union goals (Addison & Belfield, 2004). Furthermore, a union does not own the unionized labor, but they do set a common wage for the covered workers and the firm adjusts employment according to the wages

bargained (Kaufman, 2004). This is not the only possible model of analysis for unionized labor, since unions also have other potential impacts on the collective characteristics of the workplace (Freeman & Medoff, 1984).

Figure 4-2: Unionized Labor Market



In general, the union-monopolist model presumes that unions act like monopolists in the labor market and raise wages above equilibrium, which leads to unemployment, societal welfare losses, and a misallocation of resources (Hirsh & Addison, 1986). The assumption of a unionized labor market is that unions are rent-seeking institutions that desire to bargain for higher wages and higher rents to transfer to their members. The major sources of market power for unions come from the ability to strike, to establish apprenticeship programs, or to obtain excludability agreements (Kaufman, 2004). It is important to note that without strike cost or other disagreement costs, the union and the firm have little motivation to bargain (Kaufman, 2004).

Market power allows the union the ability to bargain for greater than perfectly competitive wages. However, maximizing rent is one of several possible objectives for a union. In Figure 4-2, if unions try to maximize rent, wages are raised above equilibrium at Point WM and the marginal cost of union labor exceeds the marginal benefit, which results in an excessive quantity supplied of union labor, which leads to lower employment at Point EM, along with welfare loss of ACE and unemployment. Another potential goal is to maximize the total wage bill, where the marginal benefit to the union from labor demand is set to zero (Nicholson, 2005). In Figure 4-2, the union sets the marginal benefit to the union from labor demand, Line MB, is set to zero at Point B and sets wages at Point WMB. The firm responds by setting employment at Point EMB. However, societal deadweight loss of BCD exists and unemployment remains. A union may also choose to maximize employment through setting wages to a perfectly competitive wages. In Figure 4-2, a union sets the wage to Point WPC and the firm sets employment to Point EPC, which results maximum societal surplus. However, it will be assumed that unions try to raise wages above perfectly competitive levels, since wage differentials are a factor in the demand for union services (Hirsch & Schumacher, 2001). Other factors that affect a union's ability to raise wages include wage elasticity of demand, price elasticity of demand, and the market structure of a product.

Wage elasticity of demand for unionized labor affect the wage-employment trade-off. For union bargaining for above competitive wages, employment losses will be greater if the wage elasticity of demand is more elastic and the surplus will be small. However, when faced with inelastic demand for union labor, unions are able to bargain for higher wages and extract more surplus (Kaufman, 2004). Unions may try to make the

wage elasticity of demand for union employment more inelastic. This can be accomplished by restricting the substitution of nonunion labor or capital for union labor. Contract restrictions, work rules, and technology restrictions can reduce the substitutability of union labor for capital or nonunion labor (Kaufman, 2004).

The price elasticity of demand for the product being produced also influences employment-wage tradeoffs. This is because the firm's demand for labor is derived from customer demand for the product (Kaufman, 2004). When unions bargain in a competitive industry that faces an elastic demand curve, gains in wages will be small and losses in employment will be conspicuous (Hirsch, 2008). However, if the price elasticity of demand for the product being produced is inelastic, then unions are able to gain higher wages and surplus without reducing employment by much (Kaufman, 2004).

The market structure of the product markets also affects the wage-employment trade off. When imperfect competition for a product exists, unions will be able to bargain for higher wage differentials with lower employment losses and greater surplus extract. This is due to the inelastic demand curve for the product that comes from imperfect competition (Kaufman, 2004). When a firm faces a highly competitive market, the price elasticity of demand for a product is elastic and unions are unable to bargain for higher wage differentials (Hirsch & Schumacher, 2001). DiNardo and Lee (2004) find that unions are unable to raise wages when a product competes in a highly competitive market, which includes foreign competition and firms utilizing nonunion labor.

A final point on the impact of the union-monopolist model of unions on wage and employment is the market structure of the labor market. The neoclassical model assumes that unions raise wages, extract rent, and reduce employment compared to a

perfectly competitive market. However, unions may reduce negative welfare effects of a monopsonistic labor market (Kaufman, 2005). A monopsonistic labor market makes labor more immobile and makes job transfers more costly. In addition, the firm has the ability to set wages below competitive market, but collective bargaining by a union can offset the monopsonistic effects (Kaufman, 2004). Therefore, the structure of the labor market matters for the wage and employment outcomes in a unionized labor market.

Voice Model

The neoclassical monopoly model is typically the basis for analysis of the unionized labor market. However, the standard neoclassical model ignores the collective voice impact of unions (Freeman and Medoff, 1984). Without a collective voice, the only voice workers can provide in the labor market is an exit voice, where unsatisfactory workplace conditions can be resolved by exiting a firm. However, unions can provide an alternative voice in the labor market by providing a collective voice for working conditions without having to quit (Addison & Belfield, 2004). This alternative voice provides a way to align preferences with the collective goods of the workplace and reduce turnover.

There are several notable aspects of the voice side of unions, including providing a collective voice and aligning preferences. A collective voice is multidimensional and can provide information exchange for workers, governance for worker contracts, and influence and pressure that is similar to the monopoly model (Addison & Belfield, 2004). Aligning preferences to the median worker provides a correction to the collective good problems of the workplace. This includes aligning workplace conditions and

employment terms closer to the democratic preference of the median worker (Freeman & Medoff, 1984; Kaufman, 2005). It also may be better for a collective voice to allocate workplace characteristics than an individual voice. Individuals may be unwilling to provide voice in public good characteristics of the workplace given potential retaliation, but a collective voice can provide a more socially optimal level of public goods in the workplace (Addison & Belfield, 2004).

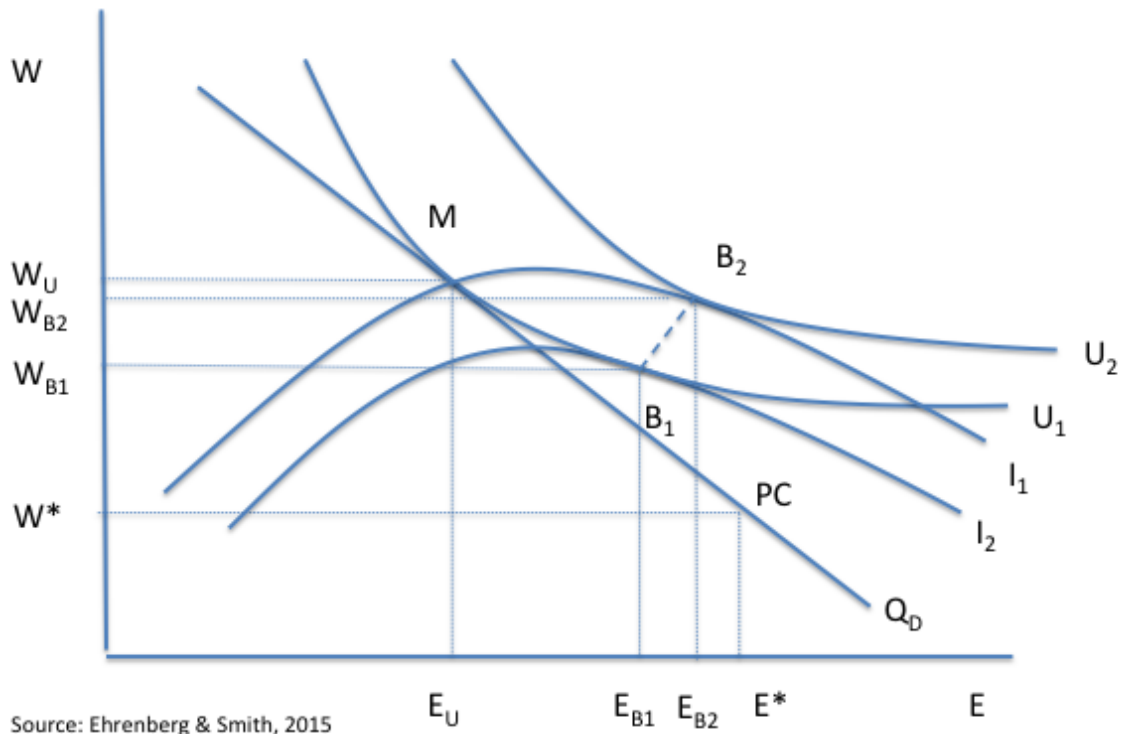
This collective voice can reduce quits, absenteeism, malingering, and quiet sabotage (Freeman & Medoff, 1984). Artz (2011) finds some evidence that unions better align preferences for the median worker. The author finds that firms with a majority of female workers have increased access to family-friendly benefits. A better package of benefits for the median worker may reduce the propensity to quit to obtain a better package of benefits. Such a reduction in turnover and quits can lead to impacts on wages and employment.

One of the central concepts from the voice model of unionization is the notion that labor unions can improve productivity. In this framework, productivity increases from reduced turnover rates from a collective voice. Reductions in turnover can reduce the cost of hiring and training and increase investments in firm-specific training (Freeman & Medoff, 1984). If labor unions are able to increase productivity, then higher wage differentials that firms face are offset by higher productivity and negating any negative effects (Blanchflower & Bryson, 2004). However, the empirical literature on improved productivity is mixed and unions are likely only able to influence productivity gains in the short-run (Hirsch, 2008).

Efficient Contracts Model

Another potential model of economic analysis of unions is the efficient contracts model. Bargained wages by a monopolist union are not efficient given the deadweight loss from wages above competitive equilibrium. However, if the union and the firm agree on wages and employment jointly, then both the union and the firm could be better off. Therefore, a set of wage and employment combinations off the labor demand curve that could be preferable to wages set by a union-monopolist, since one party can be better off without making the other party worse off. Even though unions and firms might be better off, these efficient contracts are not Pareto-efficient, since societal outcomes may be worse off (Ehrenberg & Smith, 2015).

Figure 4-3: Efficient Contract Model



Source: Ehrenberg & Smith, 2015

The efficient contracts model leads to different wages-employment outcomes than the union-monopolist outcome. Given a set of isoprofit curves and a set of union utility functions, the efficient-contract model can be established. An isoprofit curve represents combinations of wages and employment for profit unchanged and combinations are maximized for a given level of profit at the labor demand curve. From Figure 4-3, I1 and I2 represent isoprofit curves, but I2 represents higher profits, since wages associated with each level of employment are lower along the lower isoprofit curve (Ehrenberg & Smith, 2015). Isoprofit curves increase profit moving down the labor demand curve until profit maximization point of PC. The isoprofit curves increase profitability moving down the locus of the labor demand curve (Hirsch & Addison, 1986).

Unions have a set of indifference curve of wage and employment combinations from the wage-employment trade-off from the union objective function. From Figure 4-3, the union indifference curves are represented by U1 and U2 and U2 has a higher utility than U1 given the higher wages and employment discussed in Figure 4-1. At Point M, the union-monopolist model occurs when the union indifference curve is tangent to the labor demand curve and utility is maximized subject to the constraint of the labor demand curve (Ehrenberg & Smith, 2015). Wages are higher and employment is lower compared to Point PC.

From Figure 4-3, there are conflicting desired outcomes between the union and the firm along the labor demand curve. The firm prefers Point PC, where marginal cost of union labor is equal to the marginal benefit or the perfectly competitive point to maximize profit. The union prefers Point M, where the union utility is maximized subject to the constraint from the labor demand curve. However, there are points of

tangency between the isoprofit curves and utility indifference curves off of the labor demand curve. In Figure 4-3, Point B1 is just as desirable for the union as Point M, since wages are lower but employment is higher along the same indifference curve U1. For the firm, Point B1 is more desirable than Point M, since it is on a lower isoprofit curve of I2. At Point B1, the firm can be made better off without making the union worse off. Similarly, at Point B2 the union can be made better off without making the firm worse off compared to Point M, since Point M and Point B2 lie on the same isoprofit curve I1. If the union has bargaining power, then the union can bargain for Point B2, which leads to higher wages and employment than Point B1 (Ehrenberg & Smith, 2015).

In Figure 4-3, the line between B1 and B2 represents the contract curve. This curve is the locus of tangent points between the firm's isoprofit curve and the union's utility curve, where employers and unions are willing to trade wages for employment at the margin (Ehrenberg & Smith, 2015). The contract curve is the locus of efficient contracts between the union and the firm. Along the efficient contract locus, the outcome is as good as the union-monopolist outcome for at least one party. The point along the contract locus depends upon the bargaining power of the firm and the union. It is important to note that Point B2 may be vertical over Point PC (Ehrenberg & Smith, 2015). It is also important to note that efficient-contracts are not Pareto-efficient, since there is still societal loss compared to Point PC.

Given that an efficient contract outcome may lie off of the labor demand curve, employers may want to renege on the contracts to move wages downward and employment inwards towards the labor demand curve for higher profits. Unions can try to prevent firm opportunism by work rules and capital restriction (Kaufman, 2004).

There is some support for this from Li (2012). The author finds that union workers face lower wages and higher unemployment after a merger occurs. Given the rule changes after a merger, the firm had an incentive to renege or renegotiate the contracts for union workers. While wage and employment outcomes may differ from the union-monopolist model, wages are still above the marginal benefit to marginal cost equilibrium and employment is still excessive compared to the perfectly competitive market. It is important to note that empirical evidence of efficient contracts is limited (Kaufman, 2004).

Private Sector and Public Sector Unions

There are several notable differences between unions in the private sector and the public sector. The main difference is that public sector unions have limited employment losses when bargaining for higher wages for several reasons. First, public unions lobby more successfully to increase demand for union labor than private sector unions. Second, public sector unions face more inelastic demand, since government services typically are imperfectly competitive. However, there are several notable limits to the ability of public sector unions to bargain for higher wages.

Public and private sector unions face differences in wage-employment trade-offs. One of the main differences between private and public sector unionism is the ability to influence labor demand through lobbying. Unlike private sector unions, public sector unions are more successful in lobbying to increase the demand for union services for the provision of goods and services (Freeman, 2005). Another major factor is the difference in management opposition. Management opposition to union lobbying in the public

sector will likely be smaller, since public management and unionized labor may both demand higher wages and employment for the increased provision of public goods (Freeman, 2005). Typically, in a more competitive market structure, private sector unions face more hostility and resistance from management in bargaining and organizing to limit costs and to remain flexible (Hirsch & Schumacher, 2001).

The ability to raise union wages is a key difference between public and private sector unions. The ability to strike is highly constrained for public sector unions. The NLRA allows private sector unions to strike, while most states have laws against public sector unions' ability to strike (Freeman, 1986). The loss of the ability to strike does reduce some of the market power and bargaining power of public sector unions, which leads to lower wage differentials in the public sector than the private sector (Budd, 2005). The market structure for public services is another factor that affects the ability to raise wage differentials. Typically, public sector employers can be seen as local monopolies, since they provide essential services (Freeman, 1986). While it might be easier for public sector unions to bargain in this market structure than private sector unions in more competitive markets, consumers of public goods, or taxpayers, directly affect public sector unions. This limits their ability to raise wage differentials, since taxpayers can elect officials, push for referenda, restrict settlements, and use the Tiebout adjustment by moving jurisdictions to limit public sector union power (Freeman, 1986).

Unionization in Short-Run and Long-Run

There are notable differences in the unionized labor market in the short-run and in the long-run. In the short-run, unions may be able to bargain for union premiums and

possibly increase productivity. However, in the long-run, unions face more elastic demand curves and pressure from capital mobility.

From the voice model, unions can potentially increase productivity that can offset higher wage differentials, at least in the short-run. However, in the short-run, given the preferences of the median worker, the lowest member of the union will face unemployment for higher wage differentials. As the labor demand curve becomes more elastic in the long-run, median voter preference of higher wages premiums keeps chiseling away at the bottom member employment in order to maintain wage differentials, which reduces the employment of unionized labor (Freeman, 2005). This leads to one potential source of long-run lower employment in unionized labor markets.

In the long-run, union wage differentials can act as a tax on returns to long-term capital, such as research and development, in a competitive market. This reduces capital investments by the firm and reduces productivity and competitiveness in the long-run (Hirsch, 2008). Furthermore, union rent on capital returns could increase the propensity for capital mobility, which would increase the elasticity of labor demand and reduce bargaining power (Hirsch, 2008). Holmes (1998) and Kalenkoski and Lacombe (2006) find some evidence that manufacturing employment shares are higher in states with RTW laws compared to union shop states. Even in a monopsonistic labor market, collective bargaining can raise labor unit costs and hasten the exit of capital if the firm is unable to generate economic profits (Kaufman, 2004). When unions rent seek on competitive returns to capital, lower rates of employment and lower bargaining power is expected in the long-run.

Subsection 2: Nonunionized Labor Market without a RTW Law

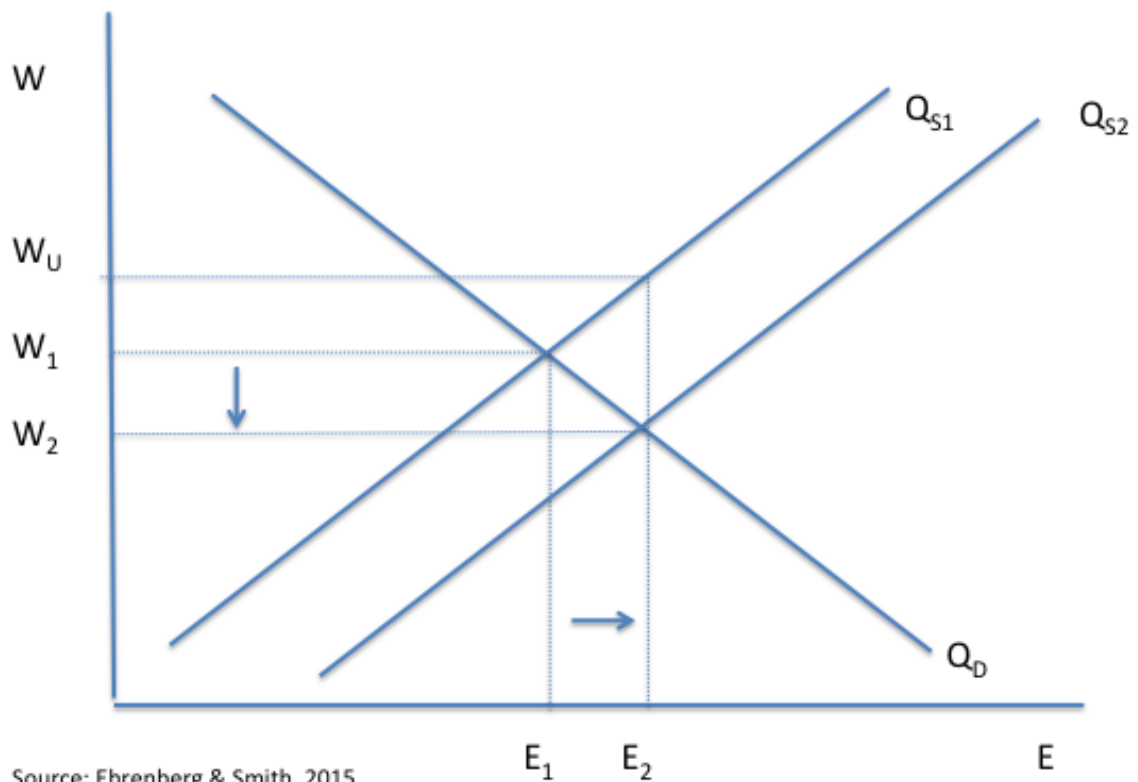
When unions bargain for higher wages there are external effects that impact the nonunion labor market. Assuming a perfectly competitive labor market with no transaction costs and perfectly mobile labor, the nonunion labor market will face a competitive wage where the marginal cost of supplying labor is equal to the marginal benefit of buying labor. However, when there is a unionized labor market along with a nonunion labor market, the bargaining power of the union can have potential impacts on the nonunion labor market. The two major theoretical considerations are the threat effect and the spillover effect. Overall, the theoretical considerations of these two concepts are offsetting and the overall impact depends on the response of the workers not employed in the union labor market (Ehrenberg & Smith, 2015). Therefore, a straightforward prediction of wages and employment outcome cannot be made for the nonunion labor market under a union security agreement.

Spillover Effect

The first impact to consider on nonunion labor markets is the spillover effect. This effect is the result of union bargaining successfully for higher wages relative to the perfectly competitive wage. When unions bargain for higher wages, there are unemployed union members that may choose to wait or queue for union jobs or spill over into the nonunion labor market (Ehrenberg & Smith, 2015). If all unemployed union members move or spill over into the nonunion labor market, unemployment in the union market is eliminated. However, given the increase in the supply of nonunion workers from unemployed union workers moving into the nonunion market, there is downward

pressure on nonunion wages (Ehrenberg & Smith, 2005). From Figure 4-4, a complete spillover effect implies that nonunion labor supply shifts from Q_{S1} to Q_{S2} . This results in wages shifting down from W_1 to W_2 and employment shifting out from E_1 to E_2 in the nonunion labor market. The expected result from this effect in the nonunion labor market is higher employment in the nonunion market and lower wages.

Figure 4-4: Spillover Effect in the Nonunion Labor Market.



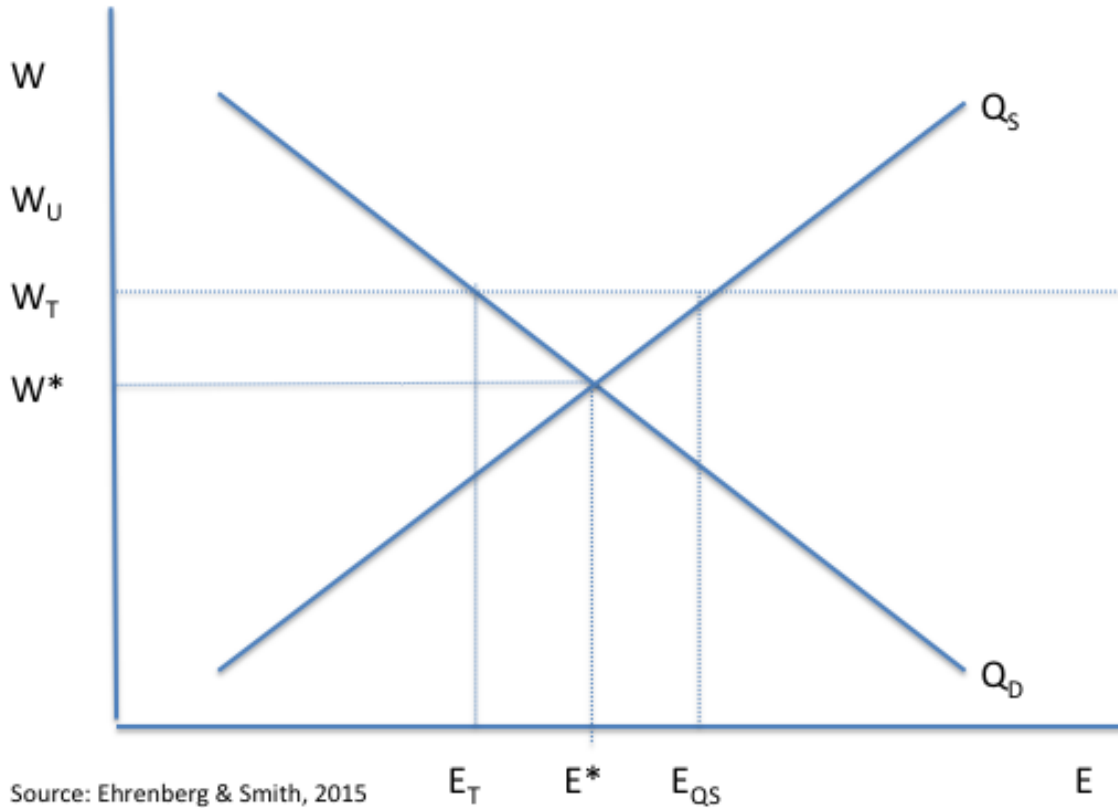
A potential reduction in the spillover effect may come from wait unemployment. The spillover effect assumes that all union workers that become unemployed move into the nonunion labor market at lower wages. Wait unemployment is the voluntary unemployment that occurs when unemployed union workers reject lower paying nonunion jobs to seek out higher paying union jobs (Ehrenberg & Smith, 2015). If this occurs, then there will be less downward pressure on wages in the nonunion labor market

from the spillover effect, but there might be lower employment and higher unemployment. If the demand for union jobs is strong enough, nonunion workers may leave the nonunion labor market to search for a union job in the unionized labor market and put upward pressure on nonunion wages (Ehrenberg & Smith, 2015).

Threat Effect

Another potential outcome from union members bargaining for wage differentials is the threat effect. After successfully bargaining for higher wages, nonunion workers may be interested in organizing for better wage outcome and threaten employer with the possibility of organizing (Ehrenberg & Smith, 2005). To ensure that the threat of organizing is reduced, employers may provide wages above competitive equilibrium but below union wages to prevent nonunion workers from organizing. Since organizing is costly, a wage above competitive equilibrium and below union wages may be sufficient to offset the propensity for organizing (Ehrenberg & Smith, 2015). The result of the threat effect is higher than competitive equilibrium wages but lower rates of employment and higher rates of unemployment in the nonunion labor market. In Figure 4-5, nonunion workers threaten organizing and demanding a higher wage at WU. However, employers provide wages at WT to pacify the threat of organizing. This increase in wages leads to a reduction of employment from E^* to ET and unemployment from EQS – ET in the nonunion labor market occurs. Farber (2005) finds evidence that nonunion wages experience a decrease after a RTW law is implemented, which suggests that nonunion workers may threaten to organize.

Figure 4-5: Threat Effect in the Nonunion Labor Market



Subsection 3: Union Services, RTW Laws, and Free-Riding

To analyze what happens to wages and employment from RTW laws, an understanding of how RTW laws impact unionization is necessary. As found by Ellwood and Fine (1987), RTW laws reduce the amount of organizing within ten years of adoption of a RTW law, which can lead to a permanently lower level of unionization in the long-run. This subsection will analyze demand for union services and collective bargaining, which is the main union service provided. It will discuss how union services have public good characteristics that are suitable to be observed in a median voter framework. Next, it will assess how RTW eliminate excludability for consuming union services, which in turn raises the cost of union services per member.

Demand for Union Services

Union services have individual and collective characteristics that are essential to understand before investigating the impact of RTW laws on union services. For an individual, demand for union services or a composite of union services, such as collective bargaining, can be analyzed as any typical economic good or service. The net benefits of unionization will have an impact on an individual's decision to join a union. However, the costs of unionization need to be considered, since to potential wage gains from higher wage differentials may be offset by job losses. The wage elasticity of demand, the extent of legal protection, and encouragement of union organizing will impact the amount of job losses from higher union wage differentials (Kaufman, 2004).

Demand for union services will be a function of the price of union services, wage differentials provided, nonpecuniary benefits provide in a union work environment, price of union service substitutes, income, and tastes and preferences for unionization, which can be seen in the following equation (Hirsch & Addison, 1986):

$$D^U = f(p_U, wd, n, p_s, I, z) \quad (1)$$

$$D'_{p_U} < 0 \text{ and } U'_{wd} > 0, U'_n > 0, U'_{p_s} > 0, U'_I > 0, U'_z > 0 \quad (2)$$

From these equations, it is assumed that an increase in union fees and dues for union services leads to a fall in quantity demanded for union services. Increases in the wage differentials and nonpecuniary benefits, such as improved working conditions, will shift the demand for union services outwards. However, if the wage premium is too large, then the employee may face unemployment from employer resistance (Kaufman, 2004). For the price of substitutes, competing services for union services considered substitutes instead of complements. Potential competing services include competitive markets, good

management, and favorable labor legislation (Kaufman, 2004). If governments provide social welfare benefits, then low cost provisions can be considered a substitute for union bargained pensions (Hirsch & Addison, 1986). It is also assumed that the union services are normal goods, but it is important to note that it is not theoretically clear or empirically clear if union services are normal goods or inferior goods (Hirsch & Addison, 1986; Kaufman, 2004). Furthermore, increases in tastes for unionization will shift the demand curve for union services outward. Tastes can reflect individual preferences for unionization, along with broader social and cultural norms, such as percentage of blue-collared workers and industry concentration (Kaufman, 2004). RTW laws have potential to decrease bargaining power, which can lead to lower levels of demand for union services.

Collective Bargaining and Median Voter

One of the essential services provided by union services is collective bargaining. Collective bargaining is essential to understand, since it has collective characteristics. With a union security agreement, collective bargaining and many other union services are nonrival and excludable (Zax & Ichniowski, 1990). Collective bargaining is nonrival, since the consumption of collective bargaining by one worker does not prevent the consumption of another. In addition, there are nonrival characteristics of the workplace that are better determined collectively through bargaining instead of individual preference (Hirsch & Addison, 1986). Collective bargaining is excludable due to union shop and agency shop legal frameworks that prevent free-riders from benefiting from the nonrival characteristics of collective bargaining without paying for them. Union shop and agency

shop legal requirements are mandatory union dues and these provisions act as a tax on the provision of collective bargaining (Zax & Ichniowski, 1990). Unlike private goods, where individuals set their marginal benefits of consumption equal to the price or marginal cost of the good, the output of collective bargaining is determined collectively. The aggregate demand of collective bargaining is determined vertically instead of horizontally across covered members in a bargaining unit (Hirsch & Addison, 1986).

The legal context of NLRB elections and union representation corroborate the collective provision of collective bargaining. From the nonrival characteristics of collective bargaining, the median worker instead of the marginal worker determines the level of unionization. The NLRA states that union representation is established by a majority of working voters (Hirsch, 2008). Therefore, the median worker determines the level of the unionization democratically through organizing and maintenance voting instead of the marginal worker preference for the provision of union services (Hirsch, 2008).

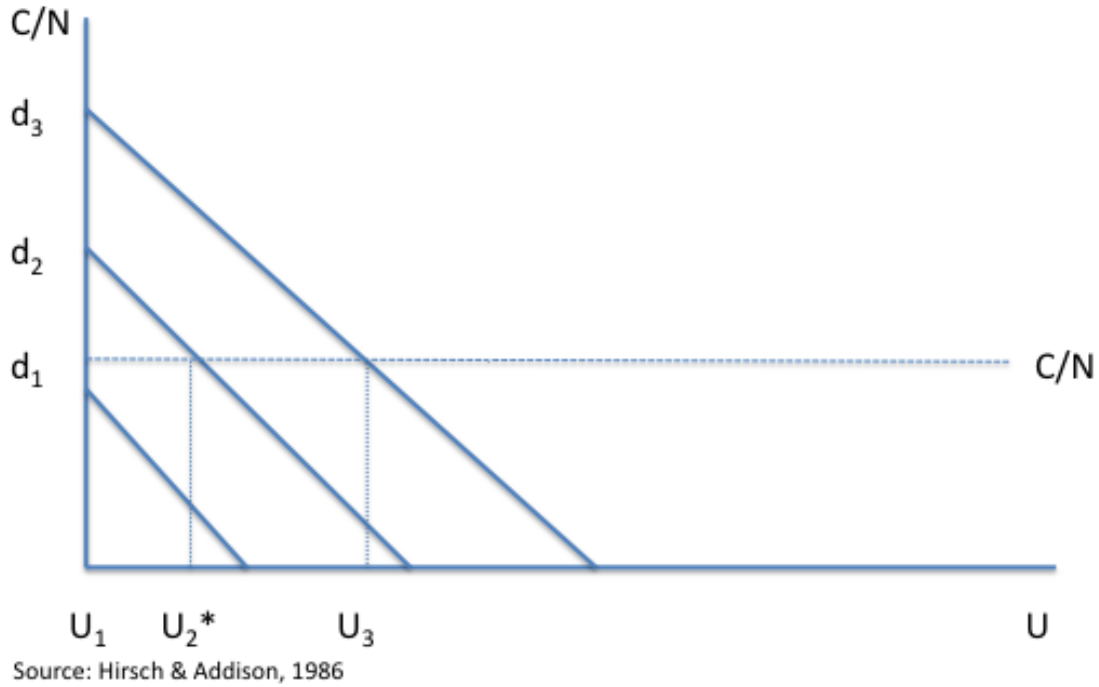
Given the legal context and nonrival provision of collective bargaining, the median voter framework provides a straight-forward analysis of the provision of union services. The median voter theorem is useful in explaining union goals, preferences, and outcomes and union leaders will bundle characteristics of union services, including wage differentials and probability of job loss, to the median voter (Hirsch & Addison, 1986). Under the median voter theorem, union leaders will propose a level of union services that is most preferable to the median voter, since any other package will be defeated in a vote. This theorem is applicable to many union vote situations, such as: union leaders trying to

organize new members; union leaders trying to bargain and retain their elected positions; and union leaders trying to prevent decertification (Hirsch & Addison, 1986).

In Figure 4-6, the demand for union services from the median voter determines the level of union services. The horizontal axis represents the quantity of union services, while the vertical axis represents the cost of union services per member (Hirsch & Addison, 1986). There are three hypothetical workers and their individual demand curves, or willingness to pay, for union services are set by d_1 , d_2 , and d_3 . The cost of union services per worker, C/N , includes dues and expected losses from strikes and is assumed to be equal across member. The quantity demanded for union services for each member is the point where the C/N line intersects the individual demand curves (Hirsch & Addison, 1986).

The median voter sets equilibrium for union services when union services are determined democratically (Hirsch & Addison, 1986). The preference for the first voter at d_1 is for no union services, since the C/N is greater than the individual's willingness to pay for union services. The third voter preference at d_3 is for higher union services. However, the bundle of composite union services is set at the point where the median voter d_2 crosses the C/N line, since the median voter would reject any other level of union services and union services are set at U_2^* .

Figure 4-6: Median Voter and Demand for Union Services



Supply of Union Services

In addition to understanding the demand for union services before analyzing the impact of RTW laws on union services, the supply of union services must be analyzed. Unions face different factor costs to provide union services to members. Many of the factor inputs into providing union services are heavily fixed costs. While unions face budgetary constraints, unions are not viewed in a profit maximizing setting similar to a competitive firm, since they may have different goals (Hirsch & Addison, 1986). The supply of union services will be set up as the following equations:

$$S^U = f(p_U, w, v, OC, BC) \quad (3)$$

$$S'_{p_U} > 0 \text{ and } S'_w < 0, S'_v < 0, S'_{OC} < 0, S'_{BC} < 0 \quad (4)$$

From these equations, it is assumed that an increase in the price of union services will increase the union services provided. It is also assumed that increases in wages and rents

for providing union services will reduce the supply of union services. It is also assumed that increases in bargaining cost or organizing costs will reduce union services provided and shift the supply of union services curve inward.

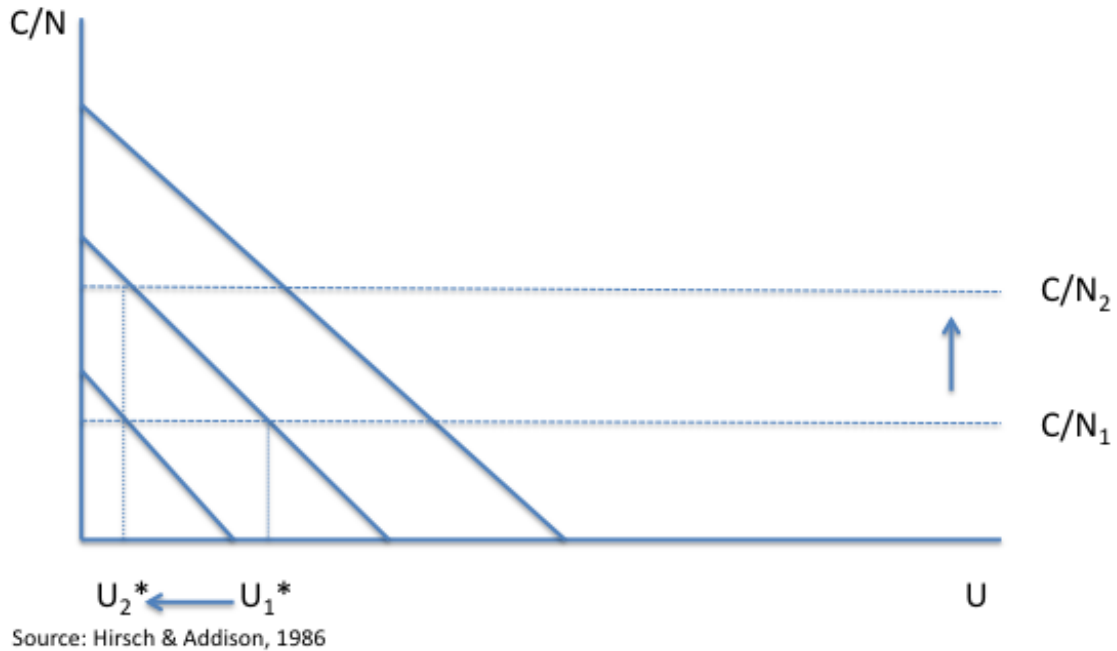
It is helpful, especially when considering RTW laws, to separate organizing costs and bargaining cost from typical supply functions (Hirsch & Addison, 1986). These factors are important for the provision of union services, since they have high fixed costs. When there are larger firms or more employees, the average fixed costs for organizing and providing union services declines substantially and a union faces economies of scale for organizing and bargaining that is similar to a natural monopoly (Kaufman, 2004). Servicing costs include collective bargaining of wages, benefits, and working conditions, grievance filings, and provision of information and the costs of providing services likely will fall over a larger membership (Hirsch & Addison, 1986). Organizing costs also face high levels of fixed costs. It is very costly to organize and establish a vote through the NLRB for union representation and these substantial initial fixed costs have risen sharply over time. In addition, the cost of organizing is also dependent upon the legal structure of governments and organizations (Farber & Western, 2001). RTW laws have potential to impact the high fixed costs of collective bargaining and organizing.

RTW Laws and the Elimination of Excludability

As seen in the demand for union services, the extent of legal protection and encouragement affects the demand for union services. RTW laws reduce the encouragement and bargaining power of unions through the elimination of union security agreements. If the legal exclusion mechanism is eliminated, then a less than optimal

amount of a public good will be provided (Hirsch & Addison, 1986). In addition, collective bargaining and union services after a RTW law is passed makes the provision of union services nonrival and nonexcludable (Zax & Ichniowski, 1990). From the Free-Rider Hypothesis, RTW laws eliminate the exclusion mechanism and members incur higher costs per member for the same benefits they receive due to free riders (Farber 1984; Zax & Ichniowski, 1990). In Figure 4-7, the increase in organizing and bargaining cost from free-riders reduces the demand for union services by increasing the cost per members. Cost per member rises from $C/N1$ to $C/N2$, which causes the median voter to select a lower level of union services. From the Bargaining Power Hypothesis, the lower level of union services from the median voter reduces the resources available to bargain and reduces the ability of unions to bargain for wage differentials (Addison & Belfield, 2004; Kaufman, 2004). This reduce further reduces the demand for union services and collective bargaining and makes unionization less attractive for workers (Ellwood & Fine, 1987).

Figure 4-7: Elimination of Excludability on Demand for Union Services



Subsection 4: Unionized Labor Market with a RTW Law

When the excludability is no longer viable from the elimination of a union security agreement, unionized labor markets lose market power from free-riders. Free-riders reduce the expenditures for striking, maintaining, and organizing union workers. Bargaining power is a form of market power for unions to obtain higher wages, but RTW laws should reduce this market power from higher costs per member from free-riders and lower expenditures from lowered demand for union services from the median voter (Addison & Belfield, 2004). The loss of bargaining power from free-riders is consistent with the free-rider hypothesis and the bargaining hypothesis. Without a formal excludability union security agreement from RTW laws, unions lose market power and bargaining power (Ellwood & Fine, 1987). This subsection will discuss what happens to

wages and employment in unionized labor markets when unions have their market power erode from RTW laws.

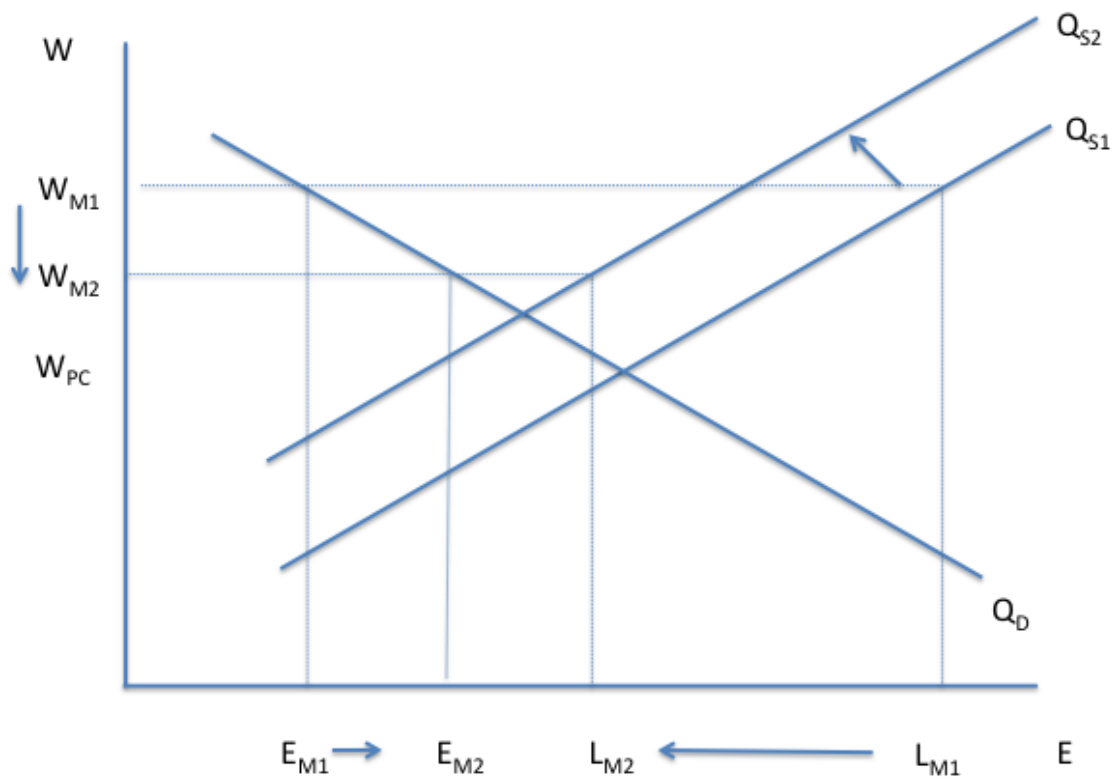
Short-Run

In the short-run, it is expected that RTW laws will put downward pressure on wages after adoption. From the monopolist-union model, unions face lower bargaining power after RTW laws increase cost per member and reduce revenues. These reductions will prevent unions from bargaining for wage differentials, since the threats of striking and bargaining are reduced (Ellwood & Fine, 1987). Therefore, from the union-monopolist model, it is expected that wages for union members will fall. In Figure 4-8, the loss of bargaining power leads to a decline in the ability to bargain for high wages that causes union wages to fall from WM1 to WM2. It is also possible that wage for workers in a bargaining unit might not be affected due by RTW laws in the very short-run after adoption, since contracts for the bargaining unit might already be established for a contractual period (Farber, 2005).

There are a few important notes for the impact of RTW laws on union employment in the union-monopolist model. First, for unionized employment, it is possible that the employment of union labor can expand. In Figure 4-8, after a fall in union wages, employment is expanded from EM1 to EM2. Second, there is expected to be a fall in the queuing of union jobs and increased spillover into the nonunion labor market. If reductions in bargaining power reduce the demand for union services, then it is expected that workers will opt out of the union labor market and decrease the supply of unionized labor from QS1 to QS2. From Figure 4-8, it is expected that the union job

queuing for union jobs will fall given the reduction in unionized labor. Assuming if all unemployed union members wait for union jobs, then job queuing is expected to fall from $LM1 - EM1$ to $LM2 - EM2$, since more union members opt out of the union labor market to the nonunion labor market. This will decrease unemployment in the union sector, but it is expected that more workers will spill over into the nonunion sector given the lower chance of obtaining a union job. Overall, from the union-monoplist model, it is possible that wages are reduced from loss of bargaining power, employment will slightly increase, job queuing will fall, and more unemployed union members will opt out of the union labor market into the nonunion labor market.

Figure 4-8: Loss of Bargaining Power in Union-Monoplist Model



From the efficient contracts model, it is expected that unions will lose bargaining power shortly after a RTW law is adopted. From Figure 4-3, a loss of bargaining power will shift the agreement point away from B2 towards B1, which will increase profits for the firm and decrease utility for the union. The firm has a stronger incentive to wait and bargain closer to the labor demand curve or renege on the contract, since the union loses resources to bargain for better outcomes (Kaufman, 2004). After a RTW law, the loss of bargaining power can lead to losses in union wages and potentially union employment. This result is slightly different from the union-monopolist model, since declines in wages and employment occur as the firm has stronger bargaining power to push wages and employment down the contract curve toward the labor demand curve. Furthermore, inward shifts from the unionized labor supply would impact wages and employment in the efficient contract model, as well. It should be noted that contracts may be sticky in the short-run and union members may not experience losses in wages and employment immediately after a RTW is adopted (Farber, 2005).

From the voice model, it is expected that the free-rider effect will reduce the collective voice of unions in the workplace. Free-riding reduces union services to less than optimal level, union services for collective voice will be reduced (Freeman & Medoff, 1984; Kaufman, 2005). This is expected to increase the turnover rate and number of quits (Freeman & Medoff, 1984). Furthermore, if unions do increase productivity in the short-run, then it is possible that the increase in job turnover and losses in firm-specific knowledge can reduce productivity and wages in the short-run. The voice model corroborates with the other two models prediction of losses in union wages in the short-run.

While wages are expected to fall for unionized workers in all three models, it is possible that wages might not be affected in the very short-run. It is possible that wages for workers in a bargaining unit might not be affected by RTW laws in the very short-run after adoption, since contracts for the bargaining unit might already be established for a contractual period (Farber, 2005). However, if a firm sees that an adoption of a RTW law reduces the market power of a union it has a contract with, it could have an incentive to renege on the contract, since punishment of infringement depends on bargaining power of the union (Malcomson, 1983). This would lead to declines in union wages and union employment.

Long-Run

It is expected that union will further lose bargaining power in the long-run after a RTW law is adopted. This will further reduce the demand for union services and reduce the supply of unionized employment. However, while wages might be lower in the long-run in an RTW state, it is possible that wage premiums, or union wages to nonunion wages, might have to be evaluated with lower levels of unionization in RTW states in the long-run. Wage premiums may be higher, since they are a relative measure (Ehrenberg & Smith, 2015). After a RTW law is adopted, absolute wages for unions and nonunions are expected to fall, but nonunion wages may fall faster than union wages due to shifts into the nonunion labor market. Empirical evidence shows that wage premiums are usually higher in RTW states as a way to entice membership, but this is compatible with overall wages for union and nonunion workers (Moore & Newman, 1985).

Another factor that might affect the ability to raise the wage premium in the long-run is the firm's ability to bargain against work rules. If work rules are diminished by the loss of bargaining power, firms will be able substitute unionized labor for capital or nonunion labor. This will make the wage elasticity of demand more elastic for unionized labor and make any wage differentials more costly in terms of employment losses.

As the bargaining power of union firms falls in the long-run after adoption of a RTW law, it is expected that there will be further decreases in the supply of unionized labor. Given that the lack of ability for organizing and maintaining union services, the demand falls and more workers opt out for employment opportunities in the nonunion sector. This can be seen in the overall decline in the unionization for private sector workers over the last few decades. Increases in global competitive markets, structural shifts in employment to the nonunion sector, and increases in technology have reduced the supply of unionized labor relative to the supply of nonunionized labor (DiNardo & Lee, 2004; Faber & Western, 2001; Hirsch, 2008). It is expected that RTW laws may speed up this process of reducing the supply of unionized labor in the long-run.

Subsection 5: Nonunionized Labor Market with a RTW Law

Without an exclusion mechanism, RTW laws reduce the bargaining power of unions and increase the cost of organizing from higher probabilities of free-riders. This spills over into the nonunion market through the two mechanisms discussed prior. On one hand, the increased cost of organizing reduces the probability of organizing as seen by Ellwood & Fine (1987). As Farber (2005) sees, the threat effect is less likely to happen and wages for nonunion workers fall after Idaho adopts a RTW law. On the other

hand, the unemployment from unionized workers is less likely to happen given that unions lose bargaining power from RTW laws. If there are fewer unemployed workers spilling over into the nonunion labor market, then there is less downward pressure on wages for nonunion workers. It is hard to be a predetermination of the outcome of wages and employment for nonunion workers given the countervailing effects, but it is expected that the spillover effect will dominate the threat effect and wait unemployment will be reduced from lower expectations of obtaining a union job.

Short-Run

The threat effect will be expected to decline, or possibly be eliminated, when an RTW law is adopted. The cost of organizing becomes more expensive per worker from free-riders and Ellwood and Fine (1987) show that organizing is reduced after the adoption of a RTW law. Therefore, it is expected that nonunion workers will no longer be able to threaten for higher wages through organizing and this will put downward pressure on wages. Farber (2005) finds evidence of this for nonunion wages after Idaho's adoption of a RTW law. From Figure 4-5, increases in nonunion employment toward competitive equilibrium are expected after the threat of organizing is reduced.

The spillover effect will be expected to become more dominant in the nonunion sector after a RTW law is adopted. First, it is expected that more union members will no longer wait for union jobs to open. From the union-monopolist model in the union sector, unemployed union members may wait for a union job to open up and they queue for a job opening. However, after a state adopts a RTW, wait unemployment and job queuing for a union job is expected to fall. From Figure 8, bargaining power is reduced

for lower union services and more unemployed union members opt out of the union labor market due to declines in bargaining power, fewer unemployed union members wait for a union job to open (Ehrenberg & Smith, 2015). This can be seen in the shift from LM1 to LM2. Second, even after lower union wages and lower unemployment in the union labor market, not all unemployed union workers will wait for a union job, as well. In Figure 4-8, unemployment in the union labor market is reduced from LM1 – EM1 to LM2 – EM2. However, some of the unemployed members in LM2 – EM2 may wait for a union job or some may spill over into the nonunion labor market like those workers from LM1 to LM2.

Long-Run

It is expected that more previously unionized workers will shift to the nonunion labor market after a RTW law is adopted in the long-run. First, the median voter may keep chiseling away at the bottom union members and the worker may choose to wait for a union job or move to the nonunion labor market (Freeman, 2005). Second, increased competitive pressures, increased capital mobility, and reduced work rules increase wage elasticity of demand for union workers and nonunion workers (Hirsch, 2008). Therefore, any wage differentials that unions bargain for in an elastic labor market will result in employment losses which may spillover into the nonunion market.

It is expected that firms will increase capital investment and relocate production towards states with RTW laws. Since RTW laws reduce union rent-seeking on competitive returns to capital, long-term investment in productivity-enhancing capital is expected to increase the demand for any complement labor (Hirsch, 2008). Increases in

long-term productivity and capital, may create new opportunities for nonunion workers to obtain employment. To corroborate this expectation, Holmes (1998) and Kalenkoski and Lacombe (2006) find that manufacturing employment shares near state borders are larger in RTW states than union shop states.

Section 3: Hypotheses

With the results from the empirical literature review and the expectations of outcomes from the conceptual model, hypotheses for the research questions can be established. Similar to the empirical literature review, the conceptual model shows that there may be evidence to support both proponents' and opponents' stances on RTW laws.

Subsection 1: Short-Run Impacts

Four research questions were established for short-run impacts. First, it is expected that RTW laws will increase the probability of a worker without a job to obtain a job. From the conceptual model, RTW laws reduce unionization and bargaining power, which will reduce the market power of unions and the threat effect of nonunion workers. This will increase employment opportunities in the union and nonunion labor markets for unemployed workers. From the empirical literature review, Newman (1983), Holmes (1998), Kuncce (2006), and Kalenkoski and Lacombe (2006) find RTW laws are associated with higher employment growth and employment shares.

Second, it is expected that RTW laws will increase the probability that a worker with a job will lose a job. From the voice model of unions, unionization can reduce turnover by providing an alternative voice to the exit voice (Freeman & Medoff, 1984).

However, it is expected that the elimination or reduction of the collective voice from a RTW law will increase the probability that a worker will use the exit voice. After a RTW law reduces union services, a worker will have less flexibility to work through a formal grievance system and is more likely to vote by leaving the workplace (Addison & Belfield, 2004).

Third, it is expected that RTW laws will decrease the wages for workers who are currently employed. From the conceptual model, the union-monopolist model and the efficient contracts models show that RTW laws reduce the bargaining power of unions. This will lead to a reduction in the bargaining power of unions. RTW laws also are expected to reduce the threat of organizing, which can reduce the wages of nonunion workers. Furthermore, a reduction in the bargaining power of unions reduces the demand for unions and more workers opt out of the union labor market into the nonunion labor market. This spillover puts downward pressure on nonunion wages. From the empirical literature review, six out of ten studies found that RTW laws reduced wages. Farber (2005) finds that the threat effect is reduced after Idaho adopts a RTW law and Li (2012) finds that union wages are strongly impacted in RTW states after a merger.

Fourth, it is expected that workers will be less likely to be a union member after a RTW law. From the conceptual model, the free-rider hypothesis expects that RTW laws will increase the cost per member and reduce the amount of union services desired by the median voter of the union. The bargaining power hypothesis expects that RTW laws will lower level of union services, since the reductions from higher costs per member will reduce resources available to bargain and reduce the ability to collective bargain and provide a collective voice. From the literature, Ellwood and Fine (1987) find that RTW

laws reduce the amount of union organizing, but they do not find that lower union organizing increases the likelihood of adopting of a RTW law. Zax and Ichniowski (1990) find that public sector RTW were statistically significant in reducing the formation of local public sector bargaining units.

Subsection 2: Long-Run Impact

In the long-run, capital becomes a flexible variable that makes the demand for unionized labor more elastic. This further erodes the union's market power to bargain for wage differentials without conspicuous losses in employment (Kaufman, 2004). It is important to note that Ellwood and Fine (1987) find that reductions in organizing from RTW laws are no longer statistically significant after ten years. However, unionization and union services may permanently be decreased. These theoretical concepts, along with the evidence from the empirical literature review, are utilized to answer the three research questions on long-run impacts.

First, it is expected that RTW laws will increase the number of establishments or establishment shares in manufacturing in a county in the long-run. From the conceptual model, it is found that a RTW law reduces the ability of a union to extract rent from long-term capital (Hirsch, 2008). This gives firms an incentive to reinvest in long-term capital, such as physical structures and research and develop, that can boost productivity and output in the long-run. From the empirical literature review, Holmes (1998) and Kalenkoski and Lacombe (2006) find that employment shares in manufacturing are higher in counties in RTW states along the border.

Second, it is expected that RTW laws will increase county-level employment in the long-run. From the conceptual model, it is expected that overall employment in a county will increase from RTW laws eliminating unemployment. It is possible that unionized employment will decline overall, especially with reductions in bargaining power for wage differentials (Farber & Western, 2001; Hirsch, 2008). In addition, Newman (1983) finds that employment grows for industries that are more labor intensive.

Third, it is expected that RTW laws will decrease average weekly wages in a county. From the conceptual model, labor demand becomes more elastic in the long-run (Hirsch, 2008). RTW laws make bargaining power weaker, which reduces the ability of unions to make demand for unionized labor more inelastic. Any wage differentials that union bargain for are impacted by larger employment losses from more elastic demand. DiNardo and Lee (2004) find that unions are unable to bargain for higher wages in competitive markets. Furthermore, firms in labor-intensive industries are likely interested to be located in RTW states for competitive wages and Newman (1983) finds RTW laws are associated with employment in labor-intensive industries.

Chapter 5: Methodology and Data

This chapter of the dissertation establishes the research design to test the hypotheses developed in the previous chapter and it discusses the methodologies that will be utilized to answer the research questions. To answer the short-run research questions, a difference-in-difference methodology will be utilized. For the long-run analysis, a regression discontinuity design using state borders will be utilized. This methodology is similar to methodology used by Holmes (1998), except it looks at RTW/Union-shop state borders before and after a RTW law is implemented.

These methodologies to be employed will be useful in controlling for heterogeneity and endogeneity issues. The difference-in-difference design can control for time-invariant unobserved heterogeneity (Wooldridge, 2009). The regression discontinuity design can control for unobserved heterogeneity around cutoff points, which can be tested (Jacob, Zhu, Somers, & Bloom, 2012). For endogeneity, Ellwood and Fine (1987) find that RTW laws reduce unionization after implementation, but lower unionization does not increase the probability of implementing a RTW law. Therefore, these methodologies will focus on a pre-post analysis of Right-to-Work (RTW) laws unlike many of the studies reviewed in the empirical literature review.

This chapter also discusses how to operationalize the research questions into dependent and independent variables. Panel data and micro data will be utilized to help control for endogeneity and heterogeneity in the short-run. Panel data are useful since they can control for unobserved heterogeneity that might bias the coefficients over time (Wooldridge, 2009). Micro data are useful for dealing with any potential endogeneity issues, since individuals do not directly affect the implementation of a RTW law (Moore,

1998). This chapter also discusses explanatory variables that will be employed to control for other sources of time-varying unobserved heterogeneity.

This chapter is divided into two sections for each set of research questions discussed in Chapter 4. The first section will go into detail about the short-run impact analysis of RTW laws. Panel data and the difference-in-difference research design will be utilized to answer questions about short-run impacts on individuals in Midwestern states. The second section investigates the long-run impacts of RTW laws by focusing on Oklahoma and surrounding states before and after Oklahoma's RTW law. This research design utilizing county-level data and state-border discontinuities, or regression discontinuity design, is similar to Holmes (1998) and Kalenkoski and Lacombe (2006).

Section 1: Short-Run Impact Analysis

To answer the short-run research questions, a difference-in-difference methodology will be utilized to assess the impact of RTW laws for the Midwestern states of Michigan, Indiana, and Wisconsin in the short-run. A difference-in-difference methodology will assess the average impact of the treatment on employment status, unemployment status, union member status, and weekly earnings. This research design utilizes panel data derived from the Current Population Survey (CPS) in the short-run.

Several Midwestern states have recently adopted RTW laws. Indiana enacted RTW legislation for all private workers in February of 2012, while Michigan enacted the RTW legislation for all public workers, except public safety workers, and all private workers in December of 2012 (Collins, 2014). Wisconsin enacted a RTW law in March of 2015 (NCSL, 2015) for private sector workers. Wisconsin also eliminated union

security agreements and most collective bargaining rights for public sector workers in 2011. To analyze the impact of RTW laws in Indiana, Michigan, and Wisconsin on labor outcomes we will use data from working-age individuals in these states and three neighboring states that did not adopt RTW laws during the time period, Ohio, Illinois, Kentucky, and Minnesota.

Subsection 1: Difference-in-Difference

Difference-in-difference is a quasi-experimental research design that attempts to control for unobserved heterogeneity. This research design is very useful for evaluating the impact of a policy before and after the implementation of the policy. The difference-in-difference estimator attempts to control for unobserved heterogeneity by observing the impact of a policy on a treatment and control group with at least two years of observations (Wooldridge, 2009). The sample can be divided into four groups, which are the following: treatment group before the policy, treatment group after the policy, control group before the policy, and control group after the policy. The average treatment effect will come from the differences between the differences in the treatment group before and after the policy and the difference in the control group before and after the policy (Wooldridge, 2009).

The exogeneity of the policy is an important part of the difference-in-difference estimator. If units of analysis were able to manipulate the treatment, then the estimator would be biased (Murnane & Willett, 2011). Moore (1998) brings this up as a concern when investigating the impact of RTW laws on labor outcomes. However, Ellwood and Fine (1987) show that RTW laws reduce union organizing and levels of unionization,

while union organizing and levels of unionization do not impact the adoption of a RTW law. Furthermore, if the difference-in-difference estimator is matched with microdata, the ability of a unit of analysis to manipulate the treatment is reduced (Moore, 1998).

Panel Difference-in-Difference

The main empirical analysis will utilize a difference-in-difference with individual fixed effects. The labor outcomes assessed will include continuous and dichotomous variables. For the continuous variable, the natural log of weekly earnings will be analyzed. For dichotomous variables, the analysis will include employment, unemployment, and union status. We will estimate three sets of equations, one using data from Michigan and neighboring states, one using data from Indiana and neighboring states, and the other using data from Wisconsin and neighboring states. Let:

$$Y_{ijt} = \alpha + Z'_{ijt}\beta + \gamma Post_t + \eta RTW_j + \delta Post_t RTW_j + \eta \quad (5)$$

Where Y_{ijt} is the outcome variable for individual i in state j during time t . Z'_{ijt} is a vector of time-varying characteristics, for individual i in state j during time t , and β is a vector of coefficients for the corresponding characteristics. $Post_t$ is a dummy variable, which is 0 for before implementation of a RTW law and 1 after the implementation of a RTW law during time t . There is also a dummy variable for a state receiving a RTW treatment, RTW_j , which is 0 for a union shop state and 1 for a RTW state for state j . The $PostRTW_{jt}$ dummy variable is the interaction between the treatment and post dummy variables. The delta coefficient for $PostRTW_{jt}$ is difference-in-difference estimate of adopting a RTW law between time period t and $t+1$ on wage and employment outcomes.

η is a vector of individual fixed effects, where each individual has two observations, which are before and after the RTW law is implemented.

Pooled Difference-in-Difference

Given the relatively small sample sizes of the panel data in the observed states, it will be useful as a sensitivity test to investigate the impact of RTW laws on wage and employment outcomes using data that pools individuals in cross-sectional household surveys from before and after the RTW laws are implemented. Utilizing a richer sample of data on individual characteristics and labor market outcomes, but without panels, a difference-in-difference can be employed to see the impact of RTW laws on workers in Indiana, Michigan, and Wisconsin.

$$Y_{ijt} = \alpha + X'_{ijt}\beta + Z'_{ijt}\eta + \gamma Post_t + \eta RTW_j + \delta Post_t RTW_j \quad (6)$$

Where Y_{ijt} is the outcome variable for individual i in state j during time t . X'_{ijt} is a vector of time-invariant characteristics, for individual i in state j during time t , and β is a vector of coefficients for the corresponding characteristics. Z'_{ijt} is a vector of time-varying characteristics, for individual i in state j during time t , and η is a vector of coefficients for the corresponding characteristics. $Post_t$ is a dummy variable, which is 0 for before implementation of a RTW law and 1 after the implementation of a RTW law during time t . There is also a dummy variable for a state receiving a RTW treatment, RTW_j , which is 0 for a union shop state and 1 for a RTW state for state j . The $PostRTW_{jt}$ dummy variable is the interaction between the treatment and post dummy variables. The delta coefficient for $PostRTW_{jt}$ is difference-in-difference estimate of adopting a RTW law between time period t and $t+1$ on wage and employment outcomes.

Comparison Groups

Individuals in neighboring states will provide comparison group to the individuals experiencing the RTW treatment. Individuals in neighboring Midwestern states as opposed to all states are utilized to satisfy the underlying assumption in difference-in-difference. One of the important assumptions with difference-in-difference methodology is that the underlying trends must be linear between treatment and comparison over the time period examined (Murnane & Willett, 2011). Midwestern states were more likely to experience similar macro economic trends relative to states outside of the Midwest. These states have been historically strongholds of the labor movement and historically have had high manufacturing employment rates. Including states that differ from Indiana, Michigan, and Wisconsin may bias the coefficients of the difference-in-difference estimator, since these states may have underlying economic trends that differ (Eren & Ozbeklik, 2016).

Several Midwestern states are used for comparison groups to the Midwestern states that experience RTW laws. The treatment individuals in Indiana will be compared to individuals in Ohio, Illinois, and Kentucky. Michigan is also included, since individuals in Michigan were not exposed to a RTW law before and after Indiana's RTW law. Individuals in Michigan are compared to neighboring individuals in Ohio, Illinois, and Wisconsin. Individuals in Wisconsin are compared to individuals in neighboring Illinois and Minnesota. It is important to note that Wisconsin public sector employees are impacted by a public sector RTW before Michigan's RTW law, while Wisconsin private sector workers are not impacted by a private sector RTW law until 2015.

Subsection 2: Data

The data that will be used in the short-run difference-in-difference methodology will come from the Current Population Survey (CPS). This subsection will discuss the characteristics of the CPS along with different types of CPS data. In addition, this subsection will discuss the methods to create short-term longitudinal data from the CPS. In addition to panel data from CPS, pooled individuals from the CPS will be utilized in the difference-in-difference methodology.

Current Population Survey

To study the impacts of RTW laws on labor market outcomes with individual fixed effects, it is necessary to have data on the employment and wages for the same workers both before and after the implementation of the RTW laws in both the RTW states and in neighboring states. Panel data provides information on the same unit over time, which can be used to control for unobserved characteristics and assess the importance of lagged or initial characteristics for decisions and changes (Wooldridge, 2009). The CPS is a potential source of micro data to analyze the impact of RTW laws on employment and wage outcomes. The CPS is a household monthly survey of 55,000 to 60,000 households, which is collected by the Census Bureau and provided by the Bureau of Labor Statistics (BLS, 1997). The CPS interviews households in a “4-8-4” pattern for a total of eight interviews that are not consecutive (Bleakley, Ferris, & Fuhrer, 1999). A household is interviewed for four months and then the household is rotated out of the survey for eight months and then interviewed for four additional months (BLS,

1997). A benefit of utilizing CPS data is that it provides microdata to deal with endogeneity issues and time-invariant unobserved heterogeneity (Moore, 1998).

There are two main types of CPS data that are useful for analyzing the dependent variables. One is the Basic CPS, which provides all eight interviews, and the other is the CPS Merged Outgoing Rotation Group (MORG). The Basic CPS provides information on households for all eight interviews. When the CPS basic monthly data are appended together, it is possible to observe month-to-month transitions and four year-to-year transitions for four consecutive months (Madrian & Lefgren, 1999). However, the each monthly interview of the Basic CPS does not contain all of the data on earnings and union status that are necessary (NBER, 2015). The CPS MORG provides data from the respondents in the fourth month and the last month, but the subsample is smaller than the Basic CPS by providing data for 25,000 households. Along with labor force data, the CPS MORG provides weekly wages, hours worked, union status, demographic variables, and other important labor force characteristics (Feenberg & Roth, 2007). By appending CPS MORG files, it is possible to obtain information on households in the same month over a year (Feenberg & Roth, 2007; Schumacher, 1999).

Panels from CPS

In order to test the impact of RTW laws on individual labor dynamics panel data from the Current Population Survey (CPS) will be discussed and developed. It is not initially designed for panel analysis, but panel data and labor market flows can be established (Boon, Carson, Farberman, & Ilg, 2008; Schumacher, 1999). Households can be matched month-to-month or year-to-year from the Basic CPS or year-to-year from the

CPS MORG (Madrian & Lefgren, 1999; Schumacher, 1999). There are three essential variables that can be utilized to match individuals over time. These are a household identifier (HHID), an individual line number within the household (LINENO), and a migration identifier (HHNUM) (Madrian & Lefgren, 1999). A unique identifier variable can be obtained by concatenating these variables, but there are still potential recording errors that need to be investigated before having a final panel. There may be false positive and false negative matches from this identifier. However, checking the identifier with other factors, such as sex, age, education, and race, can reduce or eliminate the false positive and false negative matches (Madrian & Lefgren, 1999).

These short-term panels provide benefits compared to other longer-term panel data. A strong benefit of the CPS is that it provides a representative sample of the entire civilian, noninstitutional population (Fallick & Fleischman, 2004). While National Longitudinal Survey (NLS), the Panel Study of Income Dynamics, or the Survey of Income and Program Participation (SIPP) provide longer panels than matching the CPS, these surveys provide smaller sample sizes over the geographic area of interest (Madrian & Lefgren, 1999). Another benefit is that national statistics on labor force participation is aggregated from the CPS microdata (Fallick & Fleischman, 2004). Another benefit is that important labor characteristics that are relevant to assess RTW laws, such as union status, are available from the CPS.

Pooling CPS Data

By pooling individuals over the time period of interest, the CPS can be utilized to assess the impact of RTW laws. There is potential to increase the sample size that is

limited in the panel data methodology by pooling individuals from the CPS MORG. Utilizing the CPS still provides labor force status, union status, and wage outcomes that can be assessed in a difference-in-difference methodology. The CPS MORG provides weekly earnings, union status, and labor force status as discussed in the prior section. In addition, the CPS provides explanatory variables, such as age, education, race, sex, marital status, industry, occupation, and other explanatory measures (Feenberg & Roth, 2007). This will help control for many factors that the panel data is able to capture with individual fixed effects.

Dependent Variables for Short-Run Impacts

The CPS data can be utilized to obtain values of Y_{ijt} to answer the research questions. From the CPS data, an individual's union status and weekly wages are obtained in the fourth month and eighth month in the CPS (Feenberg & Roth, 2007). In addition, labor force status is provided every month, so the Basic CPS and the CPS MORG can be pooled to obtain labor force status in any given interview month.

Using panel data on the same people before the RTW laws is implemented and after the RTW laws is implemented, this dissertation will construct the following qualitative dependent variables: (1) a dummy variable equal to one if a working-age individual is employed in the labor force, (2) a dummy variable equal to one if a worker with a working-age individual is unemployed in the labor force, and (3) a dummy variable that is one if a worker is a member of a union. To analyze the impact of RTW laws on individual wages, the dependent variable will measure the natural log change in weekly earnings for individual i .

Explanatory Variables for Short-Run Impacts

The explanatory variables to be included in the difference-in-difference models will vary depending on the data utilized. First, the panel data will focus mostly on individual fixed effects and will only assess explanatory variables that change over time. Second, the pooled data will need more covariates to control for many time-invariant factors that the panel data can control.

The most important explanatory variables will be the inclusion of individual fixed effects. These fixed effects allow the methodology to control for observed and unobserved individual characteristics that might impact the outcomes of interest. Furthermore, a vector of time-varying characteristics is included, such as changes in industry and changes in occupation between two time periods. Additional, specifications tests will include time-varying macro trends, such as state unemployment rates to test the robustness of the main specification.

For pooled data, there are many covariates that need to be included in the research design, since individual fixed effects are not utilized. Important explanatory variables that were utilized in the empirical literature were education, experience, age, race, sex, marital status, industry, union status, and occupation (Davis & Huston, 1993). The CPS data can be utilized to create dummies or continuous variables for these variables. The CPS contains age and education, along with other demographic variables. It also contains information on NAICS industries and Occupational Classifications (Feenberg & Roth, 2007). Other covariates include status of labor laws affecting different workers. These binary variables include workers covered by the National Labor Relations Act, The

Railway Labor Act, the Federal Service Labor-Management Relations Statute, and different state labor laws for public sector workers. Binary variables for each state's public sector workers will control for time-invariant bargaining rights for these workers have.

Subsection 3: Limitations

There are several notable limitations with the short-run difference-in-difference research methodology with CPS panel data. The first issue is sample size and statistical power. While the sampling size of the CPS is better than most long-term panel sets, there might be limited statistical power for assessing the impact of RTW laws on individuals in the Midwest. Statistical power increases when the number of participants increases, which can help reduce the chance of falsely accepting a false null hypothesis, or a Type II error (Murnane & Willett, 2011). The CPS Basic provides a larger sample than the CPS MORG, but the CPS MORG provides important data that are not always available for each month in the CPS Basic. Therefore, utilizing both the Basic CPS and CPS MORG will help deal with statistical power.

While difference-in-difference estimators can control time-invariant unobserved heterogeneity, difference-in-difference estimators are still subject to time-varying unobserved heterogeneity or self-selection bias. Difference-in-difference design can be applied to individuals in neighboring states. However, when utilizing states and state policies as a way to separate treatment and control groups, there will be concern about the randomization of treatment and control (DiNardo & Lee, 2010). Individuals in different

states may vary in nonrandom ways and it might not be plausible to believe that all unobserved heterogeneity has been controlled (DiNardo & Lee, 2010).

In relation to unobserved time-varying heterogeneity, political environments may be driving the impacts rather than the actual RTW law. To satisfy the linear parallel assumption, it is important that the comparison states have comparable political environments. Indiana, Michigan, and Wisconsin had Republican governors when their RTW laws were adopted. The comparison state of Ohio had a Republican governor, but it did not adopt a RTW law. Similarly, Illinois had a Republic governor in 2015 when Wisconsin's RTW law was implemented. In addition, it is possible that budget cuts may have contribute to the impacts on labor outcomes. However, Indiana and Michigan experienced budget spending increases when their RTW law was implemented, while Ohio experienced a budget cut (Ballotpedia, 2016a; Ballotpedia, 2016b; and Ballotpedia, 2016c). All treatment and comparison states also have State Budget Stabilization Funds to help smooth out consumption in times of financial stress (NCSL, 2016).

Another potential limitation is the endogeneity issue between RTW laws and labor outcomes. If RTW laws and labor outcomes are endogenous, then the impact of the RTW law on labor outcomes will be biased. However, there are several important factors to consider. First, Ellwood and Fine (1987) found that a RTW law reduced union organizing, but they did not find that low unionization increased the propensity for likelihood of RTW adoption. Second, the research design focuses on individuals and individuals are less likely to impact the adoption of a RTW law. Therefore, microdata alleviates the concern that endogeneity between RTW laws and labor outcomes will bias the difference-in-difference estimate of the impact of RTW laws (Moore, 1998).

The other notable limitation is in regard to measurement error from utilizing data from the CPS. There are several types of measurement error that occur in the CPS, which are nonresponse, mortality, migration, and recording errors (Madrian & Lefgren, 1999). First, nonresponse is problematic with the CPS and the problem has become worse over time (Hirsch & Schumacher, 2004). An interviewer might be unable to get in contact with the respondent after repeated attempts from temporary absence, inability for other reasons, or refusal to cooperate (Madrian & Lefgren, 1999). Another issue with limited nonresponse is in regard to wages. When considering the impact of RTW laws, observing wage dynamics are unfortunately limited due to imputation issues from missing observations compared to other variables, such as labor force status. Union workers are not a “hot-deck” matching variable when computing imputed wages, which creates downward bias in union wages (Hirsch & Schumacher, 2004).

Attrition is a particular measurement error that occurs in the CPS. Attrition occurs not just from nonresponse or dropping out but from migration as well. (Fallick & Fleischman, 2004). Sampling from the CPS comes from a household at a particular address. If an individual moves out of the household, then attrition with the sampling occurs. Madrian and Lefgren (1999) note that statistical tabulations from the CPS show that around fifteen percent of the population report living in a different address from the prior year. The implementation of a RTW law may impact residential movement in nonrandom ways, which would bias the coefficient on the RTW variable. Another potential microdata source that deals with this type of attrition is the Longitudinal Employer-Household Dynamics data, but the micro data are restricted to the Center of Economic Studies at the Census Bureau (Fallick & Fleischman, 2004).

Section 2: Long-Run Impact Analysis

While the short-run analysis assess the impact of RTW on wages and employment shortly after the adoption of RTW laws, this section will examine the impact of RTW laws in the long-run. Ellwood and Fine (1987) find that most of the impact of unionization is within ten year of the adoption of a RTW law. Therefore, the adoption of a RTW law by Oklahoma provides a policy adoption that allows for at least ten years of analysis of RTW laws on wages and employment. Oklahoma adopted a RTW law in 2001, while the surrounding states have had RTW laws on the books for many years, except for Colorado. Oklahoma's RTW work law was applicable to both private workers and state and local public sector workers (Collins, 2014). Utilizing a methodology similar to Holmes (1998) and Kalenkoski and Lacombe (2006), county-level data will assess the impact of RTW laws on employment, wages, and establishments. Unionization is not assessed here, since there is insufficient data on unionization at the county level.

Subsection 1: State Border/Regression Discontinuity Methodology

After a RTW law has been implemented, it may take time for capital and establishments to readjust in different geographic areas. To test the long-run impact of a RTW policy change, a type of regression discontinuity design is utilized. In 2001, Oklahoma adopted a RTW law, which was the only Great Plains state not to have one at the time (Collins, 2014). Before the adoption of Oklahoma's RTW law, Holmes (1998) found that, in 1992, discontinuities exist in manufacturing shares between RTW and

union shop counties along RTW and union shop state borders. He finds that when one moves from a union shop state to a RTW state, the manufacturing share of employment increases by 6.6 percentage points. Kalenkoski and Lacombe (2006) find a similar effect but smaller in magnitude of 2.12 increase in manufacturing employment share after controlling for spatial errors. Oklahoma's adoption of an RTW law provides an opportunity to observe the law's impact in the long-run. Since Oklahoma adopted the law almost 15 years ago, a sufficient amount of time has presumably passed to see if a discontinuity along the state borders still exists. This part of the dissertation is motivated by Holmes' (1998) method and it will use a regression discontinuity design to test for discontinuities between counties in RTW states and union shop states. Also, this research includes average weekly wages, employment, and establishments in manufacturing, whereas Holmes (1998) used employment shares in manufacturing industries as the dependent variable. The dependent variables will include the following measures: 1) manufacturing's share of total employment in a county; 2) the natural log of manufacturing employment in a county; 3) manufacturing's share of total wages in a county; 4) the natural log of average weekly wages in manufacturing in a county; 5) manufacturing's share of total establishments in a county; and 6) the natural log of manufacturing establishments in a county.

Kalenkoski and Lacombe (2006) analysis of state borders differed from Holmes (1998) in two important ways. First, Kalenkoski and Lacombe (2006) attempted to control for spatial errors that are correlated with agglomeration economies, employment centers, and measurement error. Second, the authors attempted to control for county-level characteristics, such as education, population, sex, race, and mean travel time to

work. Controlling for these characteristics, the authors found an impact of RTW laws on employment shares, but they were smaller in magnitude than Holmes (1998).

Oklahoma Analysis

For the long-run analysis, the regression discontinuity design will be tested in two different time periods. Holmes (1998) tested for border discontinuities in 1947, at the adopted of the Taft-Hartley Act, and in 1963. Discontinuities along the RTW-union shop border exist in 1963, 16 years after the adoption of the Taft-Hartley Act, but no discontinuities exist in 1947. This dissertation will estimate two sets of equations to see if there is a discontinuity before the Oklahoma's RTW law and no discontinuity after the law has been in place for many years. In the first regression, a discontinuity in outcomes will be tested for Oklahoma, Arkansas, Kansas, and Texas counties in 2000, one year before Oklahoma adopted a RTW, using counties within appropriate bandwidths of the Oklahoma-Texas, Oklahoma-Kansas, and Oklahoma-Arkansas state border segments. For the second regression, a discontinuity will be tested along the same border segments in 2010, which is nine years after Oklahoma adopted a RTW law. The first equation will test to see if a discontinuity exists. The second equation will see if any potential discontinuity still exists or if the potential discontinuity has closed.

A regression discontinuity design is employed, since there may be differing effects of RTW laws along the border compared to the interior of a state. Holmes (1998) argues that the marginal benefit of moving to a RTW state is costly as one moves closer to the state border. This assumes that the marginal cost of moving is less than the marginal cost of unionized labor as one moves closer to the RTW/union-shop state

border. Near the centers of the states, there is less distinction of RTW policies on labor outcomes (Holmes, 1998). Using distance to the state border between a RTW state and a union shop state, these relationships can be seen in Figure 5-1 and Figure 5-2 where the state border is the cutoff point. In Figure 5-1, a hypothetical discontinuity is seen at the border in labor outcomes between a RTW state and a union shop state in 2000. From Figure 5-2, it is hypothesized that the border gap between the two states has closed after Oklahoma adopted the law. Furthermore, additional counties away from the border can be utilized to increase the sample size and increase the power of the analysis.

Figure 5-1: Oklahoma-Texas Border in 2000 (Before Oklahoma RTW Law)

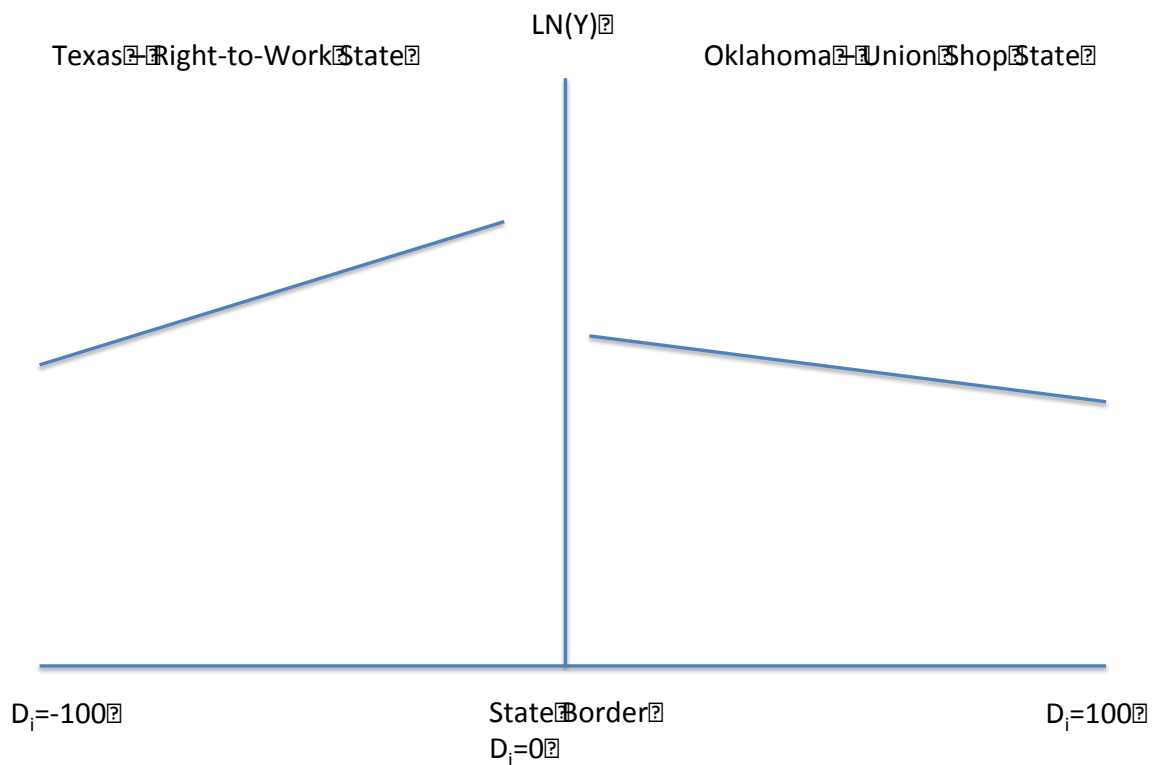
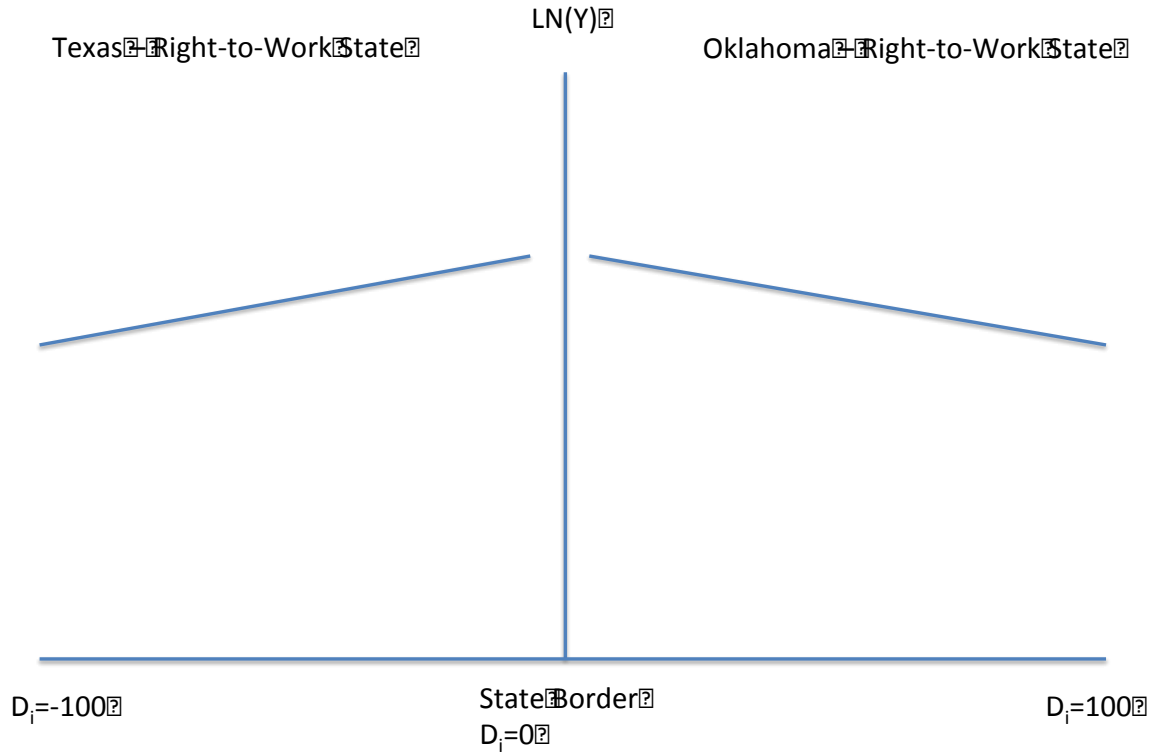


Figure 5-2: Oklahoma-Texas Border in 2011 (10 Years After OK RTW Law)



This dissertation will vary from what Holmes (1998) did in a couple of ways. First, the research design will utilize a linear interaction regression discontinuity design, where policy variable will be interacted with a running variable of distance from the border (Jacob, et al., 2012). Second, a robustness check will include county characteristics will be utilized to control for variations in economic activity along border segments. The inclusion of covariates should not impact the RDD coefficient (Lee & Lemieux, 2009). Similar to Holmes (1998), a set of equations will be tested for manufacturing that historically tend to be more unionized relative to other private sectors (Ehrenberg & Smith, 2015; BLS, 2015). The statistical model to test for state border discontinuities for manufacturing is the following:

$$Y_{ij} = \alpha + \beta RTW_j + f(D_{ij}) + \lambda mile_{ij} + \delta \quad (7)$$

Where Y_{ijt} is the outcome variable for county i in state j . These outcomes will include the different measures of wages, employment, and establishments. D_{ij} is the minimum distance from the population centroid of the county to the RTW-union shop state border segment for county i in state j . $f(D_{ij})$ is the functional form of the continuous running variable that represents the relationship between the minimum distance to the border and different outcomes. Different functional forms will be tested to reduce bias (Jacob, et al., 2012). RTW_j is the treatment variable and is a dichotomous variable that is a 1 for a county in Arkansas, Kansas, or Texas and 0 for a county in Oklahoma and the beta is the local average treatment effect at the state border. $mile_{ij}$ is a continuous variable that captures the distance along the state border moving east to west or north to south. Gamma is a vector of border fixed effects for the Oklahoma-Arkansas border, Oklahoma-Kansas border, and Oklahoma-Texas border. Alpha measures the average value of the outcome for those in the treatment group after controlling for the running variable (Jacob, et al., 2012).

As a robustness check, the equation (7) will be modified to include covariates to test the sensitivity of the RDD impact coefficients. Let:

$$Y_{ij} = \alpha + \beta RTW_j + f(D_{ij}) + \lambda mile_{ij} + \delta + X'_{ij}\gamma \quad (8)$$

X'_{ij} is a vector of county characteristics of population, sex, and race and gamma is the vector of coefficients for the corresponding county characteristics. Beta measures the marginal impact of RTW laws on county wages and employment for county i in state j at the state border (Jacob, et al., 2012).

All-State Analysis

To corroborate the findings from the Oklahoma long-run analysis, a second RDD is utilized to assess the impact of RTW laws along the RTW-union shop state borders that did not experience a RTW policy change. This will include all state used in Holmes (1998) and they will be assessed in 2000 and 2010, which is before and after Oklahoma's RTW law. The regression discontinuity design will be similar as in equations (7) and (8) for the all-state analysis.

The all-state analysis expands the number of counties observed. Holmes (1998) utilizes two broad segments of state borders between RTW states and union shop states. The first segment runs east to west from the Maryland-Virginia border in the eastern beginning to Texas-Oklahoma in the western end. A north-south border segment begins between North Dakota and Minnesota and it runs down through the Kansas-Oklahoma border. The RTW states include North Dakota, South Dakota, Iowa, Nebraska, Kansas, Arkansas, Tennessee, and Virginia. The union shop states will include Maryland, D.C., West Virginia, Kentucky, Missouri, Illinois, Wisconsin, and Minnesota. To assess the impact along the RTW-union shop border without a policy change, the all-state analysis will exclude counties along the Oklahoma-Arkansas, Oklahoma-Kansas, and Oklahoma-Texas state borders. Furthermore, similar to Holmes (1998), western state borders in the Mountain West and Pacific West are excluded due to larger counties and sparse populations.

Subsection 2: Data

This subsection will discuss the data that will be utilized with the RD design. The main source of data for counties comes from the Quarterly Census of Wages and Employment. Minimum distance to the state borders comes from Holmes (2016). Additional covariates will come from the decennial census for county characteristics.

Quarterly Census of Wages and Employment

The long-run analysis will use the Quarterly Census of Employment and Wages (QCEW). The QCEW is a data source from the Bureau of Labor Statistics that provides information about wages and employment for counties and industries on a quarterly basis. The QCEW is based upon state unemployment insurance and it provides a virtual census of all nonagricultural payroll workers along with their wages. Employment from the QCEW represents the number of workers on payroll on the 12th day of the month, which includes all corporate officials, executives, supervisory personnel, clerical workers, wage earners, pieceworkers, and part-time workers (BLS, 1997). Total wages submitted include gross wages and salaries, bonuses, stock options, tips and gratuities, but it does not include other benefits such as health insurance and pension funds (BLS, 1997). The QCEW can be utilized to answer questions about the impact of wages and employment for heavily unionized industries along state borders where a RTW law discontinuity exists in the long-run. Pooling of counties over time along the borders can be included in an analysis to see how aggregate employment and wages have change long after a RTW law has been adopted.

Dependent Variables for Discontinuities

The dependent variables will consist of three sets of outcomes. The first two sets are related to the short-run dependent variables on wages and employment. The third variables tried to test Hirsch (2008) idea that unionization reduces investment in long-run capital.

There are two types of wages that will be assessed in the long-run analysis. First, the natural log of manufacturing average weekly wages is considered (BLS, 1997). Second, the manufacturing's share of total wages in a county is also considered. It is important to note that average weekly wages are sensitive to changes in hourly wage rates and average weekly hours, which are not available from the QCEW. If RTW laws have an impact on hours worked or hourly wage rate as will be investigated in the previous sections, then the long-run analysis will not be able to distinguish between the two effects.

Two measures of employment in manufacturing will be considered. The first one is manufacturing's share of total employment in a county. Holmes (1998) and Kalenkoski and Lacombe (2006) analyze county-level employment shares and this will provide corroborating evidence to their findings. The second dependent variable is natural log of total manufacturing employment in a county. These data include all persons on state unemployment insurance and do not include self-employed or undocumented, off-the-book workers (BLS, 1997).

The third set of variables will focus on measures of establishments. Similar to employment, the first measure of establishments will include the share of total establishments in manufacturing compared to the total number of establishments in a

county. The second measure will include the natural log of manufacturing establishments in a county. Hirsch (2008) suggests that rent-seeking unions reduce capital investment by firms into long-term physical capital and research and development. An establishment is a physical presence of a firm, so it serves as a proxy for physical capital. Newman (1983), Holmes (1998), and Kalenkoski and Lacombe (2006) find some evidence that there is movement of industries to states with a RTW law, especially labor-intensive industries.

Explanatory Variables for Discontinuities

There are two sets of explanatory variables that will be utilized in the RDD methodology. The first set includes geographic variables that are related to the running variables of the RDD. The second set of variables is explanatory variables that will be used as a robustness test for the RDD.

There are three types of geographic variables that are essential the research design. The main explanatory variable is the running variable, which is minimum distance to the border. Holmes (1998) discusses that the minimum distance to the border is the minimum distance from a counties population centroid to the state border. Distance to the border is centered around zero (Jacob, et al., 2012). Another geographic variable is the mile marker as one moves east to west along a state border or north to south along a state border. This variable measures factors that affect the outcomes of interest as one moves farther west and south along state borders. In essence, this measure should capture more rural characteristics in Oklahoma and Texas as the variable increases. Holmes (2016) provides several essential geographic data for the RDD

methodology. The final geographic characteristics include border fixed effects. For the Oklahoma analysis, there will be three types of fixed effects for Oklahoma-Arkansas, Oklahoma-Kansas, and Oklahoma-Texas borders. These fixed effects that will capture time-invariant characteristics for each of the separate borders.

As a robustness test of the RDD methodology, a specification will also include county-level variables to control for factors. First, the inclusion of covariates should not affect the RTW impacts, since the only factors that should vary at the state border are treatment to RTW laws and no treatment of RTW laws (Lee & Lemieux, 2009). Second, the inclusion of explanatory variables should reduce the standard error around the RTW coefficient from the state border regression discontinuity design and provide a robustness check. County characteristics come from the Census Bureau's intercensal estimates and can be utilized to control for population, sex, race, and ethnicity (Census, 2016). These variables include the county population, the mean percentage of females in a county, the mean percentage of non-Hispanic Blacks in a county, the mean percentage of non-Hispanic Asians, the mean percentage of non-Hispanic other races in a county, and the mean percentage of Hispanics in a county. If the RDD methodology is robust, then the inclusion of these characteristics should only reduce the standard error and not impact the RTW estimates.

Subsection 3: Limitations

There are notable limitations with the methodology and data utilized in this section. The RDD methodology only provides local average treatment effect and the estimate of the impact of RTW laws is only valid at state borders (Jacob, et al., 2012). If

there are any impacts from RTW laws at state borders, they cannot be generalized to other parts of the states of interest.

There are other notable limitations with the methodology and data utilized in this section. First, the unit of analysis is the county as opposed to the individuals within a county. Unfortunately, the CPS does not have a sufficient sample size to investigate individuals at the county level, particularly many rural counties along the Oklahoma-Texas, Oklahoma-Kansas, and Oklahoma-Arkansas borders.

A second potential issue is a lack of local randomization at state borders. The running variable of distance from the border should provide a sharp cutoff and provides assignment to the treatment. However, if treatment and control counties on both sides of the border vary in nonrandom ways, then this would bias the coefficient of the policy variables (Murnane & Willett, 2011). Another possible threat to internal validity is including counties too far from the border and incorrectly specifying the functional form of the running variable. Not correctly specifying the functional form can bias the coefficient of the policy (Murnane & Willett, 2011). However, not including enough counties in proximity to the border can reduce the statistical power (Murnane & Willett, 2011).

A third potential problem arises from the lack of unionization or union density at the county level. While certain industries may be prone to unionization than others, without county-specific observation it is hard to control for union density at the county-level. Eren and Ozbeklik (2016) find that private-sector unionization and manufacturing unionization is reduced at the state level six years after Oklahoma's RTW law. Farber (2005) tests the threat effect on Oklahoma and finds that there is no effect on nonunion

wages. The author mentions that there might be a marginal effect to no effect of RTW laws on wages if union density is low and the threat is unlikely.

A fourth potential issue is related to endogeneity between RTW laws and county outcomes. While the units of analysis are not individual-level as in the short-run, counties are still unable to manipulate state policy and this reduces the propensity for endogeneity (Moore, 1998). Ellwood and Fine (1987) look at the impact of RTW laws five to ten years after each of the seven states change policies and find that RTW laws reduce unionization, but low unionization does not increase the likelihood of RTW adoption. Eren and Ozbeklik (2016) use synthetic controls to create comparable counterfactual states that do not have a RTW law to assess the impact of Oklahoma's RTW law. The authors utilize variables that would indicate higher likelihood of Republican voting pattern, which include percentage of population that is white, male, not living in a metropolitan area, and without college degree. Finally, Oklahoma's neighboring states and Oklahoma itself had Republican governors at the time of adoption, which indicates similar political environments in the control and treatment states..

Spatial errors are another potential factor that may limit the results from the methodology. Kalenkoski and Lacombe (2006) find that controlling spatial error can reduce the magnitude of the impact the coefficient of RTW laws. The authors argue that if omitted variables related to RTW laws vary over space, then RTW laws might be bias. The explanatory variables in the robustness tests at the county level attempt to control for factors that are related to spatial errors, such as population in a county.

Chapter 6: Short-Run Analysis

This chapter of the dissertation discusses the results from the empirical findings in the short-run analysis of Right-to-Work (RTW) laws. The short-run analysis focuses on the impact of RTW laws in Indiana, Michigan, and Wisconsin. Overall, the empirical findings show that RTW laws are associated with lower likelihoods for being union members and lower wages. The evidence on employment and unemployment is consistent but not always statistically significant among the states analyzed.

This chapter of the dissertation consists of four sections. The first section reviews the short-run research questions. The second section discusses the descriptive statistics and graphical pre-post analyses of RTW laws in the short-run. The third section discusses the main empirical findings of the panel difference-in-difference design of the impact of RTW laws. The fourth section discusses sensitivity tests, such as falsification tests and pooled difference-in-difference analyses.

Section 1: Short-run Research Questions

This section will briefly restate the research questions of interest for the short-run analysis of RTW laws in the Midwest. First, do RTW laws decrease the likelihood that an individual will be a member of a union in the short-run? Second, do RTW laws increase or decrease the probability that an individual will be employed in the short-run? Third, do RTW laws increase or decrease the probability that an individual will be unemployed in the short-run? Finally, are RTW laws associated with an increase or decrease in wages or earnings in the short-run? The findings of these research questions are discussed in the subsequent subsections.

Section 2: Descriptive Statistics and Pre-Post Analysis

The first step in the empirical assessment of RTW laws will be a descriptive analysis of the states of interest utilizing descriptive statistics of the treatment and comparison states, along with pre-post analyses of the treatment and comparison states. The descriptive statistics discusses the observable covariates of treatment and comparisons states before implementation of the RTW laws. The pre-post analysis focuses outcomes for each of the dependent variables of interest before and after the state law is implemented.

Subsection 1: Descriptive Statistics

The descriptive statistics of the treatment and comparison states provide a statistical assessment of the dependent variables of interest along with observable explanatory variables. This provides an assessment of the comparability of states. The merged outgoing rotation group from the Current Population Survey (CPS) provides the main source of data for the analysis of RTW laws as discussed in the previous chapter. Only individuals subject to the law are assessed, such that federal workers, management workers, domestic workers, self-employed workers, and air and rail line workers under the RLA are not included. Also, individuals younger than 16 are not included in the CPS merged outgoing rotation group. The variables of interest are discussed for each treatment state along with the comparison groups for each treatment state.

Indiana and Comparison States

Indiana is compared to several neighboring states that have similar characteristics. The data on individuals are pulled from the CPS for the twelve months before February 2012 in Indiana, Illinois, Kentucky, Michigan, and Ohio. The individual data from Illinois, Kentucky, Michigan, and Ohio are aggregated together to form the comparison group. Table 6-1 provides means and standard deviations for individuals in Indiana, the treatment group, and the comparison group. The sample sizes ranged from 5,863 for union status and weekly earnings to 11,555 for labor force characteristics to 14,028 for education and demographic characteristics.

From March 2011 to February 2012, the means and standard deviations of Indiana and the comparison states are relatively similar for the dependent variables. From Table 6-1, the rate of unionization is slightly lower for Indiana relative to the comparison states. Indiana has a unionization mean of 0.125, while the comparison states have a unionization mean of 0.163. Employment and unemployment means are similar between treatment and comparison states. Indiana has an employment mean of 0.508 and an unemployment mean of 0.043, while the comparison states means are 0.500 and 0.050, respectively. The mean weekly earnings in Indiana are slightly lower at 724.2 dollars per week compared to 746.6 dollars per week for the comparison states. The employment, unemployment, and weekly earnings means were not statistically different between the treatment and comparison groups, while the union status difference was statistically significant using a t-test.

Table 6-1 shows that there are some differences between the treatment state and comparison states in the independent variables. Observing important human capital

variables of experience, education, and SES characteristics, there are a few notable differences in the means. First, Indiana has a statistically significant and slightly more experienced workforce on average with potential experience mean at 32.22 years, while the comparison states mean potential experience is 32.48 years. Indiana has a workforce that is more likely to have a high school diploma and less likely to have secondary education. Indiana also has a workforce that is slightly less racial and ethnically diverse than the comparison states. Indiana has a slightly higher number of women, but the difference is not statistically significant. In addition, there are more married individuals in Indiana compared to the comparison states.

Table 6-1: Descriptive Statistics of Indiana and Comparison States

All Individuals Not Exempt	March 2011-February 2012			
	Indiana		Comparison	
Dependent Variables	Mean	SD	Mean	SD
Union Density	0.125	0.331	0.163	0.369
Employed	0.508	0.500	0.505	0.500
Unemployed	0.043	0.203	0.050	0.218
Weekly Earnings	724.2	523.8	746.6	556.8
Independent Variables	Mean	SD	Mean	SD
Potential Experience	33.22	18.99	32.48	18.70
High School Dropout	0.138	0.345	0.140	0.347
High School Degree	0.380	0.486	0.325	0.468
Some College	0.180	0.384	0.189	0.392
Associate's Degree	0.073	0.260	0.086	0.280
Bachelor's Degree	0.155	0.362	0.166	0.372
Advanced Degree	0.074	0.261	0.094	0.292
Female	0.552	0.497	0.549	0.498
White, Non-Hispanic	0.872	0.334	0.802	0.399
Black, Non-Hispanic	0.071	0.257	0.102	0.303
Asian, Non-Hispanic	0.012	0.110	0.028	0.164
Hispanic	0.035	0.184	0.058	0.234
Other, Non-Hispanic	0.009	0.096	0.010	0.100
Married	0.615	0.487	0.577	0.494
Manufacturing, NAICS 31-33	0.115	0.319	0.096	0.294
Construction, NAICS 23	0.034	0.181	0.028	0.165
Education Sector, NAICS 61	0.057	0.233	0.062	0.242
Utilities, NAICS 22	0.007	0.082	0.005	0.072
Transportation, NAICS 48-49	0.019	0.137	0.020	0.140
Public Administration, NAICS 92	0.021	0.143	0.020	0.141

Along with human capital inputs, selected 2-digit NAICS sectors were assessed between Indiana and the comparison states for industries with traditionally higher rates of unionization. The likelihood of individuals in manufacturing was statistically different where mean likelihood of an individual in Indiana was 11.5 percent and 9.6 percent for individuals in the comparison states. Similar to manufacturing, the mean likelihood for

an individual to be in construction was statistically different in Indiana compared to the comparison states. Indiana had a mean of 0.034 and the comparison states had a mean of 0.028. The mean likelihoods of individuals in the education, utilities, transportation and warehousing, and public administration sectors were not statistically different between Indiana and the comparison states.

Michigan and Comparison States

Michigan is also compared to similar Midwestern states to assess the impact of Michigan's RTW laws. The data on individual are pulled from the CPS for April 2012 to March 2013 in Illinois, Michigan, Ohio, and Wisconsin. Indiana is not included in the analysis, since Indiana had already adopted and implemented a RTW law. The mean and standard deviations of the dependent variables of interest along with notable explanatory variables for Michigan and comparison states can be found in Table 6-2. The sample size for Michigan and comparison states ranges from 5,383 for union status and weekly earnings to 10,221 for labor force characteristics to 12,440 for education characteristics.

Michigan dependent variables of interest provide a slightly different assessment compared to Indiana. Similar to Indiana, the union status likelihood is statistically different. However, Michigan has a higher mean likelihood for union status than the comparison states at 0.187 compared to 0.160. Employment status is also statistically significantly different, where Michigan's mean is 0.483 compared to 0.54 for comparison states. Unemployment status was not statistically significantly different and Michigan's unemployment mean was 0.045 compared to 0.046 in the comparison status. Weekly earnings were slightly lower in Michigan with a mean of 763.9 dollars per week

compared to a mean of 772.5 dollars per week in comparison states, but the difference was not statistically significant.

The human capital explanatory variables show relative comparability between the treatment and comparison states. Potential experience in Michigan had a mean of 32.76 years of experience, while the comparison states had a mean of 32.80 year of experience. Only two of the educational categories were statistically significant utilizing a t-test, which were the likelihoods of having a high school degree and some college. The mean probability of an individual having a high school degree was slightly lower in Michigan relative to the comparison states with a mean of 0.308 compared to 0.322 in comparison states. Individuals in Michigan had a slightly higher mean of having some college with a mean of 0.205 compared to the comparison states' mean of 0.176. Individuals, who are high school dropouts, have an associates degree, bachelor degree, or graduate degree, were not statically different between the treatment and comparison states.

The demographic variables between Michigan and the comparison states had some slight statistical differences. Michigan's workforce had a slightly higher rate of non-Hispanic whites with a mean of 0.823 compared to 0.801. Michigan had a slightly higher rate of non-Hispanic blacks with a mean of 0.117 compared to 0.096 in the comparison states. Hispanics were more likely to be in the comparison states than Michigan, where Michigan's mean was 0.027 and the comparison states' mean was 0.065. Non-Hispanic Asians and non-Hispanics in other racial groups were not statistically different. Similarly, females in the population of interest were not statistically different between Michigan and the comparison states. However, the mean

likelihood that an individual is married was statistically different with Michigan's mean of 0.593 and the comparison states' mean of 0.570.

Table 6-2: Descriptive Statistics of Michigan and Comparison States

All Individuals Not Exempt	April 2012-March 2013			
	Michigan		Comparison	
Dependent Variables	Mean	SD	Mean	SD
Union Density	0.187	0.390	0.160	0.367
Employed	0.483	0.500	0.540	0.498
Unemployed	0.045	0.208	0.046	0.210
Weekly Earnings	763.9	559.3	772.5	570.1
Independent Variables	Mean	SD	Mean	SD
Potential Experience	32.76	18.71	32.80	18.57
High School Dropout	0.115	0.319	0.123	0.329
High School Degree	0.308	0.462	0.322	0.467
Some College	0.205	0.404	0.176	0.381
Associates Degree	0.106	0.308	0.099	0.299
Bachelor's Degree	0.173	0.378	0.187	0.390
Advanced Degree	0.094	0.291	0.093	0.290
Female	0.551	0.497	0.547	0.498
White, Non-Hispanic	0.823	0.382	0.801	0.399
Black, Non-Hispanic	0.117	0.322	0.096	0.295
Asian, Non-Hispanic	0.022	0.147	0.022	0.145
Hispanic	0.027	0.163	0.065	0.247
Other, Non-Hispanic	0.010	0.100	0.015	0.122
Married	0.593	0.491	0.570	0.495
Manufacturing, NAICS 1-33	0.103	0.304	0.097	0.297
Construction, NAICS 23	0.018	0.132	0.030	0.172
Education Sector, NAICS 61	0.061	0.239	0.065	0.246
Utilities, NAICS 22	0.007	0.084	0.005	0.072
Transportation, NAICS 48-49	0.013	0.111	0.023	0.150
Public Administration, NAICS 92	0.021	0.143	0.019	0.137

Selected industry groups were also compared between Michigan and the comparison states. There were only two 2-digit NAICS sectors that were statistically

different between the treatment and comparison groups. The construction sector, NAICS 23, in Michigan had a lower mean likelihood of 0.018, while the comparison states had a mean likelihood of 0.030. Individuals in Michigan had a lower mean likelihood of 0.013 of being employed in transportation and warehousing, NAICS 48-49, while the comparison states had a mean likelihood of 0.023. Manufacturing, education, utilities, and public administration sectors were not statistically different between treatment and comparison states.

Wisconsin and Comparison States

Wisconsin is also compared to other Midwestern states, but there are fewer comparison states given that nearby Midwestern states, such as Iowa and Michigan, have already adopted RTW laws. Therefore, Wisconsin is only compared to Illinois and Minnesota. Data on individuals from Wisconsin, Illinois, and Minnesota are pulled from the CPS from April 2014 to March 2015. The sample size compared to Indiana and Michigan is smaller, since there are only three states included. The sample size for this analysis ranges from 2,115 for union status and weekly earnings to 3,763 for labor force characteristics.

Similar to Indiana, the dependent variables of interest are not statistically different from one another except for union status. The likelihood of union status is lower in Wisconsin than it is in Illinois and Minnesota. The mean likelihood of union status in Wisconsin is 0.118, while the comparison states have a mean likelihood of 0.180. The lower mean likelihood may be due to Wisconsin Act 10 of 2011, which eliminated collective bargaining rights for state and local workers (Wisconsin State Legislature,

2011). Employment status, unemployment status, and weekly earnings were not statistically different between the treatment and comparison states. The mean likelihood of employment status in Wisconsin was 0.578, while the mean likelihood of employment status in the comparison states was 0.557. In Wisconsin, the mean likelihood of unemployment status was 0.029, while the mean likelihood of unemployment status in comparison states was 0.036. Wisconsin had a mean weekly earnings of 867.3 dollars per week, while the comparison states had a mean weekly earnings of 856.1 dollars per week.

While human capital inputs were relatively similar, there were a few notable statistical differences. Individuals in Wisconsin had higher mean years of potential experience at 34.01, while the comparison states had a mean years of potential experience at 31.68. Individuals without a high school degree and individuals with a high school diploma, some college, and advanced degrees were also statistically different between Wisconsin and comparison states. Individuals in comparison states had a higher mean likelihood of being a high school drop out compared Wisconsin with mean likelihoods of 0.122 and 0.104, respectively. Individuals in Wisconsin were more likely to have a high school diploma with a mean likelihood of 0.321, while the comparison states had a mean likelihood of 0.263. Individuals in Wisconsin were less likely to some college or an advanced graduate degree. Wisconsin had a mean likelihood of some college of 0.161, while the mean likelihood of some college was 0.191 for comparison states. Individuals in Wisconsin had a mean likelihood of 0.096 for an advanced graduate degree compared to 0.114 for comparison states. Individuals with associate's degree or bachelor's degrees were not statistically different.

Table 6-3: Descriptive Statistics of Wisconsin and Comparison States

All Individuals Not Exempt	April 2014-March 2015			
	Wisconsin		Comparison	
Dependent Variables	Mean	SD	Mean	SD
Union Density	0.118	0.015	0.180	0.010
Employed	0.578	0.017	0.557	0.009
Unemployed	0.029	0.006	0.036	0.003
Weekly Earnings	867.3	27.70	856.1	15.38
Independent Variables	Mean	SD	Mean	SD
Potential Experience	34.01	0.625	31.68	0.352
High School Dropout	0.104	0.011	0.122	0.006
High School Degree	0.321	0.016	0.263	0.008
Some College	0.161	0.013	0.191	0.007
Associates Degree	0.123	0.011	0.116	0.006
Bachelor's Degree	0.195	0.014	0.193	0.007
Advanced Degree	0.096	0.010	0.114	0.006
Female	0.546	0.017	0.531	0.009
White, Non-Hispanic	0.872	0.012	0.730	0.008
Black, Non-Hispanic	0.060	0.008	0.097	0.005
Asian, Non-Hispanic	0.019	0.005	0.049	0.004
Hispanic	0.037	0.007	0.112	0.006
Other, Non-Hispanic	0.012	0.004	0.012	0.002
Married	0.570	0.017	0.541	0.009
Manufacturing, NAICS 1-33	0.121	0.011	0.075	0.005
Construction, NAICS 23	0.030	0.006	0.031	0.003
Education Sector, NAICS 61	0.068	0.009	0.075	0.005
Utilities, NAICS 22	0.010	0.003	0.004	0.001
Transportation, NAICS 48-49	0.019	0.005	0.023	0.003
Public Administration, NAICS 92	0.032	0.006	0.028	0.003

There were also statistical difference in demographic variables between Wisconsin and the comparison states. Demographics of race and ethnicity were statistically different between Wisconsin and the comparison states. Individuals in Wisconsin were more likely to be non-Hispanic white with a mean of 0.872 compared to

0.730 in the comparison states. The mean likelihood for an individual to be non-Hispanic black in Wisconsin was 0.060 compared to 0.097 in the comparison states. Wisconsin's mean likelihood for Hispanics was lower at 0.037 compared to 0.112 in comparison states. The mean likelihood for an individual to be non-Hispanic Asian was statistically lower in Wisconsin at 0.019 while the mean likelihood in comparison states was 0.049. Individuals of non-Hispanic other racial groups were not statistically different between Wisconsin and the comparison states. The mean likelihood for an individual to be female was not statistically different between treatment and comparison, as well.

In the descriptive analysis of highly unionized industries, there were a few notable differences between Wisconsin and comparison states. Individuals in Wisconsin were more likely to be employed in manufacturing than the comparison states with a mean likelihood of 0.121 compared to 0.075 in comparison states. Individuals were also more likely to be employed by the utilities sector in Wisconsin with a mean likelihood of 0.010 compared to 0.004 in comparison states. There were no statistical differences in prevalence of employment between treatment and comparison for construction, transportation and warehousing, education, and public administration.

Subsection 2: Pre-Post Analysis

An assessment of pre-post outcomes provides a descriptive analysis of the outcomes of interest for treatment and comparison states. Unconditional pre-post analyses were performed for each state for each research question of interest. These analyses include the average outcome for union status, employment status, unemployment status, and the natural log of weekly earnings before and after the

respective state's RTW law is implemented. The pre-law means include all individuals outcomes one to twelve months before the law is implemented, while the post-law means include all individual outcomes one to twelve months after the law is implemented.

Indiana Pre-Post

The variables of interest for Indiana show conspicuous trends before and after Indiana's RTW law was enacted. The governor implemented this law on February 1, 2012 (NCSL, 2015). All pre-observations are individuals in the CPS before this month, while all post-observations are the same individuals twelve months later. These data observations are only for individuals intended to be affected by the law, so these data exclude management, farm workers, domestic workers, self employed workers, and workers covered by the RLA.

There are four tables that show the unconditional changes in union status, employment status, unemployment status, and weekly earnings for Indiana and the comparison states of Illinois, Kentucky, Michigan, and Ohio. From Table 6-4, it appears that the rate of unionization in Indiana relative to the comparison states fell before and after the Indiana RTW law. The mean likelihood of union status in comparison states is relatively stable before and after the law, while union status in Indiana experiences a conspicuous decline.

Table 6-4: Union Status Before and After RTW Law in Indiana

Indiana			
Unionization	Before	After	After-Before
Control	0.163	0.162	-0.001
Treatment	0.126	0.109	-0.017
Treatment-Control	-0.037	-0.053	-0.016
Employment	Before	After	After-Before
Control	0.505	0.508	0.003
Treatment	0.508	0.491	-0.017
Treatment-Control	0.003	-0.016	-0.019
Unemployment	Before	After	After-Before
Control	0.050	0.045	-0.005
Treatment	0.043	0.042	-0.001
Treatment-Control	-0.007	-0.003	0.004
Logged Weekly Earnings	Before	After	After-Before
Control	6.319	6.353	0.034
Treatment	6.309	6.338	0.029
Treatment-Control	-0.010	-0.015	-0.005

The employment and unemployment status show notable trends before and after the law in the states of interest. From Table 6-4, employment status in treatment and control states have similar means before the law, but the employment status mean likelihood slightly increases in the comparison states while the mean likelihood in Indiana notably falls. From Table 6-4, both treatment and comparison states display declines in the mean likelihood of unemployment. However, the comparison states show a greater decline in the likelihood compared to Indiana.

The weekly earnings between Indiana and the comparison group show a different trend relative to the other graphs. The natural log of weekly earnings used to compare earnings between treatment and comparison states given the skewness of earnings. From Table 6-4, the natural log of weekly earnings appears to have similar trends of growth between the treatment and comparison groups before and after the law.

Michigan Pre-Post

The variables of interest for Michigan provide insight into an important concept of RTW laws. RTW laws do not affect workers covered by collective bargaining agreements that are still in effect after the law is implemented. Workers wanting to drop membership need to wait until after the collective bargaining agreement end before dropping membership. While Michigan's laws were signed by the governor on December 12, 2012, the two bills for private and public sector workers did not go into effect until March 28, 2013 (NCSL, 2015; NRTWLDF, 2016a). The CPS data will look at individuals twelve months before April 2013 and twelve months after April 2013. These observations do not include individuals not intended to be affected by the law, such as self-employed and domestic workers.

The figures from the pre-post analysis of Michigan's RTW law show that there is not much of an impact on the dependent variables of interest relative to Indiana. From Table 6-5, the pre-post analysis of the RTW law at the point of implementation shows that the mean likelihood of union status in Michigan goes up slightly while the mean likelihood of union status in comparison states slightly falls. Employment status is relatively similar to that of union status. From Table 6-5, the pre-post analysis of employment status shows flat trends for both the treatment state and the comparison states. The mean likelihoods of employment status do not change much between the two time periods.

The other two dependent variables of interest do show differences in the pre-post analysis. From Table 6-5, the mean likelihood of unemployment status for Michigan

increased between the twelve months prior to the law and twelve month after the law, while the mean likelihood of unemployment status fell for the comparison states. From Table 6-5, the mean natural log of weekly earnings grew in Michigan between the two time periods, while the mean natural log of weekly earnings from the comparison states fell between the two time periods.

Table 6-5: Union Status Before and After the Law in Michigan

Unionization	Before	After	After-Before
Control	0.160	0.154	-0.006
Treatment	0.187	0.191	0.005
Treatment-Control	0.027	0.038	0.011
Employment	Before	After	After-Before
Control	0.540	0.538	-0.002
Treatment	0.483	0.483	0.000
Treatment-Control	-0.057	-0.055	0.001
Unemployment	Before	After	After-Before
Control	0.046	0.042	-0.004
Treatment	0.045	0.049	0.004
Treatment-Control	-0.001	0.008	0.009
Logged Weekly Earnings	Before	After	After-Before
Control	6.361	6.369	0.008
Treatment	6.340	6.375	0.035
Treatment-Control	-0.021	0.006	0.027

The analysis of Michigan's RTW laws brings an important point, since RTW laws do not affect worked covered by collective bargaining agreements are still in place after the law is implemented. The covered workers must be obliged to the dictates of the previously bargained agreement and must wait until it ends before dropping membership (NRTWLDF, 2016b). There may have been many previously bargained agreements in place in Michigan, since 2014 was the first year of a notable drop in union membership in Michigan (Oosting, 2015). In addition, Michigan largest state worker union had

previously bargained contract agreements in place until December 2013 (Eggert, 2012). Therefore, the Michigan RTW law will be tested around December of 2013, as well.

Wisconsin Pre-Post

The dependent variables of interest show notable trends before and after the Wisconsin RTW law that has similarities and differences compared to Indiana. The Wisconsin law was implemented on March 9, 2015 (NCSL, 2015). Individuals from the CPS are obtained twelve months before March 2015 and the remaining months of 2015. The mean likelihood of union status, employment status, and unemployment status, along with the mean natural log of weekly earnings, is compared between the treatment state, Wisconsin, and the comparison states of Illinois and Minnesota.

The pre-post analysis of union status is similar to that of Indiana's RTW law pre-post assessment of union status. From Table 6-6, it appears that the law unconditionally impacts unionization. The mean likelihood of union status declines between the pre-post periods in Wisconsin, while the mean likelihood of union status in the comparison states remained relatively flat.

There appears to be differences in employment status and unemployment status between the treatment and control states in Wisconsin and comparison states. From Table 6-6, it appears that the treatment and comparison states had similar trends in employment before and after the law. The mean likelihood of employment status in Wisconsin grew after the enactment of the law, while the mean likelihood of employment status in the comparison states slightly increased. This increasing trend in mean likelihood appears to be a different trend compared to Indiana before and after its RTW

law. From Table 6-6, the treatment and comparison states have different trends in unemployment. The mean likelihood of unemployment status in Wisconsin experiences an increase, while the mean likelihood of unemployment status in the comparison states drops conspicuously.

Table 6-6: Union Status Before and After the Law in Wisconsin

Wisconsin			
Unionization	Before	After	After-Before
Control	0.180	0.179	-0.001
Treatment	0.117	0.098	-0.020
Treatment-Control	-0.062	-0.081	-0.019
Employment	Before	After	After-Before
Control	0.557	0.559	0.002
Treatment	0.580	0.586	0.008
Treatment-Control	0.021	0.027	0.006
Unemployment	Before	After	After-Before
Control	0.036	0.026	-0.010
Treatment	0.019	0.031	0.002
Treatment-Control	-0.007	0.005	0.012
Logged Weekly Earnings	Before	After	After-Before
Control	6.465	6.490	0.025
Treatment	6.526	6.489	-0.012
Treatment-Control	0.036	-0.001	-0.037

There are also differences in trends for weekly earnings for Wisconsin and the comparison states. From Table 6-6, the mean natural log of weekly earnings increases notably in the comparison states, while the mean natural log of weekly earnings in Wisconsin appears to gradually decline.

Section 3: Main Findings

This section presents the main empirical findings from the difference-in-difference research design utilizing panel data. The main empirical findings assess the impact of RTW laws for individuals the law intends to treat. Table 6-7 shows the main empirical findings of the post-treatment interaction. As an additional specification, the RTW laws are assessed for all workers in each treatment state.

Utilizing a linear probability model, the main findings utilize panel data and a difference-in-difference methodology. The CPS data utilized is from the CPS MORG extract provided by NBER (2016). These data are extracted from the CPS from interview 4 and interview 8, which occurs a year after interview 4. The specification utilizes individual fixed effects on the same individual before and after the law that control for many observed and unobserved factors which may affect the individual's outcome status for unionization, employment, or unemployment. For the natural log of weekly earnings, an OLS difference-in-difference is utilized to assess the impact of RTW laws on earnings. The CPS data include all individuals before the law and the same individuals twelve months later, who are not self-employed, management, farm workers, domestic workers, or air and rail workers.

Additional specification issues are taken into account for the difference-in-difference methodology. For each panel difference-in-difference equation, a Hausman test was conducted to test for fixed effects or random effects. The Hausman tests showed that random effects were not appropriate. In addition to individual fixed effects, industry and occupation dummy variables are included in the specification. These dummy variables reflect changes in individual moves between industries and occupations before

and after the law. Furthermore, errors are clustered at the state level to account for correlation of errors within states. Conley and Taber (2011) show that this is an appropriate step when comparing states with a difference-in-difference methodology, since standard errors can be inconsistent with a small number of policy changes. Each state is discussed separately, but notable and consistent findings appear in the main empirical findings from the panel data and difference-in-difference methodology.

Table 6-7: Main Findings from the Panel Difference-in-Difference

States RTW Observations Around	Indiana Feb-12	Michigan Mar-13	Michigan* Dec-13	Wisconsin Mar-15
Union Status	-0.0154** (0.00493)	0.00698 (0.00829)	-0.0135* (0.00468)	-0.0149** (0.00164)
N	11,672	10,752	10,626	4,244
Employed	-0.0225*** (0.00380)	-0.00201** (0.000478)	-0.00751 (0.00531)	0.00341 (0.00134)
N	13,125	12,003	11,814	4,635
Unemployed	0.0153*** (0.00230)	0.00789 (0.00366)	0.0126 (0.00538)	0.00227 (0.00204)
N	13,125	12,003	11,814	4,635
LN(Weekly Earnings)	-0.0203** (0.00723)	0.0148 (0.00736)	-0.0268** (0.00551)	-0.0422* (0.0121)
N	11,663	10,736	10,613	4,237

Variables include Treatment, Post, Treatment*Post, Individual Fixed Effects, and Industry and Occupation Binary Variables

All utilize XTEG balancing on DD and initial intervention and final interview (before and after)

*Michigan State and Local Workers Bargaining Agreement ended in Dec 2013

Subsection 1: Indiana Main Findings

Individuals in Indiana appear to have notable outcomes from the Indiana RTW law. After controlling for individual fixed effects, along with industry and occupational changes, the impact of RTW laws in Indiana in the short-run becomes apparent. From Table 6-7, the Indiana RTW law is associated with a reduction in the likelihood of being

a union member by 1.54 percentage points. This impact is statistically significant at the 5-percent level. This is a notable impact considering that the mean likelihood of union status in Indiana before the law was 12.5 percent.

The Indiana RTW law is also associated with changes of the likelihood of being employed and unemployed. The Indiana RTW law lowers the likelihood of being employed by 2.25 percentage points and this impact is statistically significant at the 1-percent level. It is a notable impact but the mean likelihood of being employed before the law was 50.8 percent. Along with lower employment status likelihood, the Indiana RTW was statistically associated with an increase in unemployment status. The law increased the likelihood of unemployment status relative to the comparison states by 1.53 percentage points and this is statistically significant at the 1-percent level. This is also a notable impact considering the mean likelihood of unemployment in Indiana was 4.3 percent before the law was enacted.

The Indiana RTW law was associated with reductions in the weekly earnings for individuals covered by the RTW law in Indiana. The RTW law reduced weekly earnings by 2 percent after the law was enacted relative to the comparison states. This was statistically significant at the 5-percent level. Overall, individuals in Indiana experienced worsen labor outcomes after the law was enacted relative to the comparison states.

Subsection 2: Michigan Main Findings

Michigan's enactment of RTW laws shows that the collective bargaining laws in effect matter for the impact of RTW laws. From Column 2 in Table 6-7, when the impact of Michigan RTW law is focused on the date of implementation, there appears to be no

impact of RTW laws on unionization, unemployment, and wages. However, if the impact of the RTW law is centered on the end of the collective bargaining agreement for states workers in December of 2013, as seen in Column 3 in Table 6-7, marginal impacts similar to Indiana emerge.

Looking at the impact of RTW laws at the time of implementation in March 2013, the Michigan RTW laws only affect employment. The coefficients of the difference-in-difference interaction are not statistically different from zero for unionization, unemployment status, and weekly earnings. The Michigan RTW law is associated with a slight reduction in employment status, but the effect is small. The Michigan RTW law is associated with a 0.2 percent reduction in employment status that is statistically significant at the 5-percent level. However, the mean likelihood of employment status before the law in Michigan was 48.3.

If the analysis of the RTW law moves to the point when state workers' collective bargaining agreement ends and workers are able to drop union membership, similar marginal impacts to Indiana are apparent. Focusing on the end of state workers' collective bargaining agreement in December 2013, the Michigan RTW laws are associated with a reduction in the likelihood of union status. The Michigan RTW laws reduce union status by 1.35 percentage points, but this is statistically significant at the 10-percent level. It is also a notable impact since the mean likelihood of union status before the Michigan RTW laws was 18.7 percent.

Unlike the Indiana RTW law, the Michigan RTW laws, after the end of state workers' collective bargaining rights, are not associated with employment status and

unemployment status. The coefficients of the difference-in-difference interaction were in the similar direction as Indiana, but these were not statistically different from zero.

Similar to the Indiana RTW law, the Michigan RTW laws were associated with wages after focusing on the end state workers' collective bargaining agreement. The Michigan RTW laws reduce wages by 2.7 percent, which is relatively similar to Indiana's RTW impact of 2.0 percent. This impact was statistically significant at the 5-percent level. Michigan's RTW laws provide intriguing insight into the impact of RTW laws and collective bargaining, which will be discussed in more detail in the chapter 8.

Subsection 3: Wisconsin Main Findings

Similar impacts appear in the short-run difference-in-difference analysis of Wisconsin RTW law. Individuals experienced similar marginal impacts on union status and wages from Wisconsin's RTW law relative to Indiana and Michigan. From Table 6-7, the Wisconsin RTW law is associated with a reduction in union status. The law reduces union status by 1.5 percentage points, which was statistically significant at the 5-percent level. This impact is notable considering that the mean likelihood of unionization in Wisconsin before the law was 11.8 percent.

Similar to Michigan after the end of state worker's collective bargaining agreement, the Wisconsin RTW law was not associated with employment and unemployment status. For employment status, the treatment-post interaction term was not statistically different from zero relative to the comparison states. Furthermore, the coefficient of the difference-in-difference term for Wisconsin RTW law was not statistically associated with unemployment status.

Similar to Indiana and Michigan, the Wisconsin law is associated with reductions in weekly earnings. The Wisconsin RTW law reduces weekly earnings by 4.2 percent relative to the comparison states, which was statistically significant at the 10-percent level. Summarizing the results, the Wisconsin RTW reduced individual labor outcomes on average in the short-run. Across each treatment state, RTW laws were statistically associated with reductions in wages and union status in the short-run.

Subsection 4: All Worker Specification Test

In addition to the main empirical findings, which focus on individuals that the law intends to treat, an additional specification includes the impact of RTW laws on all workers in the state. This specification still utilizes the CPS MORG, but it includes management, federal workers, air and rail workers, self-employed, domestic workers, and farm workers. It is important to note that many construction managers are union members. One benefit of this specification is that it increases the sample size relative to the main empirical findings. The additional individuals included in this specification do not change the signs of the coefficient on the difference-in-difference interactions, but it did change magnitude and standard errors of the coefficients.

Table 6-8: Additional Specification from Panel Difference-in-Difference

States RTW	Indiana	Michigan	Michigan*	Wisconsin
Observations Around	Feb-12	Mar-13	Dec-13	Mar-15
Union Status	-0.0215*** (0.00401)	0.000183 (0.00573)	-0.0131* (0.00462)	-0.0210** (0.00251)
N	14,432	13,192	12,981	5,352
Employed	-0.0237*** (0.00477)	0.00151 (0.00249)	-0.0111* (0.00465)	0.00488 (0.00409)
N	17,836	16,287	15,867	6,465
Unemployed	0.0154*** (0.00240)	0.00300 (0.00301)	0.0129** (0.00316)	0.000620 (0.00419)
N	17,836	16,287	15,867	6,465
LN(Weekly Earnings)	-0.0150 (0.0102)	0.00477 (0.00738)	-0.0323*** (0.00517)	-0.0310** (0.00662)
N	14,414	13,168	12,961	5,341

Variables include Treatment, Post, Treatment*Post, Individual Fixed Effects, and Industry and Occupation Binary Variables

All Utilize XREG balancing on DD and Initial Intervention and Final Interview (Before and After)

All workers in the state 16 years or older

*Michigan State and Local Workers Bargaining Agreement ended in Dec 2013

Indiana has relatively similar impacts with one notable difference. From Table 6-8, the impact of RTW laws on unionization is still negative on union status, employment status, and wages, but the impact of Indiana's RTW law is no longer statistically significant for wages. However, the impact of Indiana's RTW law reduces the likelihood of being a union member by 2.15 percentage points and this is statistically significant at the 1-percent level. For employment, the Indiana RTW law is associated with a reduction in likelihood of employment status of 2.37 percentage points, which is statistically significant at the 1-percent level. The Indiana RTW law is still associated with increased likelihood of unemployment status and the marginal impact on the likelihood of unemployment status goes up by 1.54 percentage points, which is statistically significant

at the 1-percent level. For wages, the coefficient of the difference-in-difference interaction term is still negative, but it is not statistically different from zero.

Michigan still shows differences in the impact of its RTW laws between the date of implementation and the date of the end of state workers' collective bargaining rights. At the time of implementation, the Michigan RTW laws are not statistically different from zero for all dependent variables of interest. This is slightly different compared to the main specification, since Michigan's RTW law is no longer associated with employment status at the time of implementation.

Under the alternative specification, Michigan's RTW laws become associated with employment and unemployment status when state workers' collective bargaining right end. From Table 6-8, Michigan's RTW laws still reduce union status by a similar margin and it is statistically significant at the 10-percent level with this specification. However, Michigan's RTW laws are associated with a reduction in employment status and an increase in unemployment status. The interaction term of the difference-in-difference shows that the Michigan RTW laws decrease the likelihood of employment status by 1.11 percentage points, which is statistically significant at the 10-percent level. For unemployment status, Michigan's RTW laws increase the likelihood of unemployment by 1.29 percentage points relative to the comparison states, which is statistically significant at the 5-percent level. The impact of Michigan's RTW laws on wages are slightly larger in this specification, where Michigan's RTW laws reduce wages by 3.23 percent and this is statistically significant at the 1-percent level.

Under the new specification, the impact of Wisconsin's RTW law is relatively similar. The Wisconsin RTW law is associated with a reduction in the likelihood of

union membership by 2.1 percentage points and this is statistically significant at the 5-percent level. Similar to the prior specification, Wisconsin's RTW law is not associated with employment status. As before with unemployment status, the interaction term of the difference-in-difference is not statistically different from zero. The impact of Wisconsin's RTW law on wages is slightly less than the prior specification. Under this specification, Wisconsin's RTW law reduces wages by 3.1 percent and this impact is statistically significant at the 5-percent level.

While there were some slight changes in statistical significance between the two specifications, the impacts were relatively similar. Across the treatment states, RTW laws reduce the likelihood of union status. For Michigan and Wisconsin, the RTW laws were associated with reductions in wages between the two specifications. The impact of Indiana's RTW law was consistent between the two specifications on the likelihood of employment and unemployment, while Wisconsin's RTW law remained unassociated with employment and unemployment status. However, there was a difference between Michigan's RTW laws between the two specifications. After including all workers in the states, the impact of Michigan's RTW laws became statistically significant on employment and unemployment. The marginal impacts were relatively similar to those impacts in Indiana.

Subsection 5: Time-Varying Robustness Check

A specification with state unemployment rates was tested with the main empirical specifications. There was concern that observable time-varying factors, such as state unemployment rates, may have impacted the likelihood of labor outcomes. The main

difference-in-difference specification for NLRA and state and local workers is modified to include monthly state unemployment rates. From Table 6-9, it appears that the outcomes are fairly robust, but the standard errors for difference-in-difference estimates for individuals in Michigan do increase. The difference-in-difference across all three states do not change impacts much compared to Table 6-7, which provides a robustness test when including time-varying data.

Table 6-9: Main Specification Including State Unemployment Rates

States RTW Observations around	Indiana Feb-12	Michigan Mar-13	Michigan* Dec-13	Wisconsin Mar-15
Union Status	-0.0185** (0.00588)	0.00692 (0.00954)	-0.0133 (0.00639)	-0.0148** (0.00264)
N	11,672	10,752	10,626	4,244
Employed	-0.0214** (0.00509)	-0.00154 (0.00235)	-0.00667 (0.00527)	0.00334 (0.00152)
N	13,125	12,003	11,814	4,635
Unemployed	0.0157*** (0.00290)	0.00937* (0.00396)	0.0123 (0.00591)	0.00252 (0.00263)
N	13,125	12,003	11,814	4,635
LN(Weekly Earnings)	-0.0246*** (0.00527)	0.0140 (0.00820)	-0.0193 (0.0122)	-0.0400** (0.00925)
N	11,663	10,736	10,613	4,237

Variables include Treatment, Post, Treatment*Post, Individual Fixed Effects, Industry and Occupation Binary Variables, and Monthly State Unemployment Rates
 All Utilize XTREG balancing on ID and Initial Intervention and Final Interview (Before and After)
 *Michigan State and Local Workers Bargaining Agreement ended in Dec 2013

Section 4: Other Findings and Tests

In addition to the main empirical findings, additional empirical tests were done to assess the strength and weaknesses of the main empirical model. These additional tests include falsification tests and utilizing difference-in-difference methodologies with

pooled data and panel data from a larger sample of the CPS. The first pooled test utilizes the panel data in the main findings, but does not control for individual fixed effects. The next two specifications utilize the CPS basic data instead of the merged outgoing rotation group. The first one of these specifications includes a panel of matched individuals who are not management, self-employed, domestic workers, or air and rail workers, while the latter specification includes all working-age individuals.

Subsection 1: Falsification Tests

Without a RTW law implemented, individuals should not be able to free-ride and there is an expectation that a centering of the treatment-post interaction around an arbitrary date before the RTW laws should lead to null findings. An arbitrary date of January 2010 was selected for each treatment state and for their comparison states. This date is prior to any RTW law or reductions in collective bargaining. The data and methodology are the same as the main empirical findings in Table 6-7, but the time period has been centered on January 2010.

Table 6-10: Union Falsification Tests

Panel 1: NLRA and State Workers			
States/TW	Indiana	Michigan	Wisconsin
Observations around	Jan-10	Jan-10	Jan-10
Union Status	0.00345 (0.00458)	0.0101 (0.00575)	-0.00244* (0.000741)
N	12,479	11,627	8,775
Panel 2: All Workers			
States/TW	Indiana	Michigan	Wisconsin
Observations around	Jan-10	Jan-10	Jan-10
Union Status	0.00106 (0.00273)	0.00163 (0.00368)	-0.00380*** (0.000333)
N	15,130	14,094	10,850

Variables include Treatment, Post, Treatment*Post, Individual Fixed Effects, Industry Fixed Effects, and Occupation Fixed Effects. All utilize XTEG balancing on ID and Initial Interview and Final Interview (Before and After).

From Table 6-10, it is seen that there is little to no effect of January 2010 on the union status for the states of interest. Using the same cohort of individuals from Table 6-7, which are workers that are not federal, management, domestic workers, self-employed, rail and airline workers, or farm workers, panel 1 shows that the impact of the difference-in-difference on union workers is statistically insignificant. The treatment-post interaction term is statistically significant at the 10-percent level, but the marginal impact is only 0.24 percentage points. Panel 2 shows a similar situation when other workers are included. However, panel 2 shows that the impact is slightly more and more statistically significant when other workers, such as federal workers. There may have been a change in union association related to federal workers, air or rail workers, or construction

management that may have spilled over into NLRA and state and local workers. The impacts those are notable smaller than the larger reductions observed after a RTW law.

Subsection 2: Pooled CPS MORG

The importance of unobserved individual characteristics is seen when panel data are pooled instead of being utilized with a first-difference to control for time-invariant unobserved heterogeneity. Utilizing pooled data instead of panel data understates the impact of RTW laws on union status, employment status, unemployment status, and wages. Pooled data cannot control for individual unobserved heterogeneity, such as tastes and preferences for unionization, and the impacts are mostly downward biased.

The CPS MORG data were pooled to get a larger sample size of individuals, since not all individuals are observed between the two time periods. Similar to the main empirical findings, the pooled design does not include individuals who are not covered by the NLRA law or state bargaining laws, such as federal workers, domestic workers, self-employed, management, air and rail workers. Comparing Table 6-11 to Table 6-7, the sample sizes increase around a few thousand observations. However, the pooled data difference-in-difference specifications include individual-level, time-invariant explanatory covariates, since these cannot be controlled through a pooled design compared to a panel design. Individual explanatory variables include bargaining laws fixed effects, such as if an individual is covered by the NLRA or a state-specific bargaining law for state and local workers. Human capital variables, such as potential experience, education, racial and ethnic identity, sex, and marital status were included. Industry and occupational fixed effects were included, as well. As in main empirical

findings, standard errors are clustered by states to account for correlation of errors within states.

The impacts of RTW laws are notably different in the pooled difference-in-difference design for most dependent variables of interest compared to the panel difference-in-difference design. From Table 6-11, the impact on unionization is still negative for Indiana and Wisconsin, but it is no longer statistically significant for Michigan in either period. Using pooled data, RTW laws reduce the likelihood of unionization by 0.78 percentage points compared to 1.54 percentage points using panel data. However, the Wisconsin RTW law appears to overstate the impact of the Wisconsin RTW law on unionization. Using the pooled findings, the Wisconsin RTW law reduces the likelihood of unionization by 4.56 percentage points, while the main empirical findings shows a reduction of 1.49 percentage points. For Michigan, the impact of RTW laws after the state collective bargaining agreement ends in December of 2013 was not statistically significant compared to the panel data which found that RTW laws reduced unionization by 1.35 percentage points.

Utilizing pooled data instead of panel data, the impact of RTW laws on employment and unemployment status is different when not controlling for individual observed heterogeneity. From Table 6-11, Indiana's RTW laws still has an impact on the likelihood of employment and unemployment status, but the marginal impact is diminished from negative 2.25 percentage points to negative 1.08 percentage point reduction for employment and from 1.53 percentage points to 0.39 percentage points for unemployment. Conversely, for Michigan's RTW laws, the marginal impacts on employment and unemployment in Table 6-11 become statistically significant at the 10-

percent level utilizing pooled data, while the marginal impacts were statistically insignificant when controlling for individual heterogeneity in Table 6-7. While the signs on the coefficients were the same direction as the panel data, the statistical significant does change. For Wisconsin, the marginal impact of RTW laws on employment switches directions. However, both specifications were not statistically different from zero. For unemployment status in Wisconsin, the pooled specification showed that Wisconsin's RTW law increased unemployment status by 0.69 percentage points, while the panel design showed that Wisconsin's RTW laws was not associated with unemployment status.

Utilizing pooled data from the analysis of RTW laws shows that RTW laws had no statistical impact on wages. This is quite different from the panel design results, which showed that RTW laws were associated with wages and that the marginal impacts of each state's RTW law reduced wages. While the direction of the impact is negative for each state using the pooled design, none was statistically significant.

Table 6-11: Pooled Difference-in-Difference Findings

States	Indiana	Michigan*	Wisconsin
Observations	Feb-12	Dec-13	Mar-15
Union Status	-0.00785* (0.00324)	0.00218 (0.00563)	-0.0456*** (0.00375)
N	16,057	15,003	7,454
Employed	-0.0108** (0.00238)	-0.0182* (0.00630)	-0.00522 (0.00708)
N	18,240	16,818	8,169
Unemployed	0.00385** (0.00120)	0.0122* (0.00481)	0.00689* (0.00168)
N	18,240	16,818	8,169
LN(Weekly Earnings)	-0.00217 (0.0108)	-0.00254 (0.0144)	-0.0112 (0.0133)
N	16,038	14,974	7,442

Variables include Treatment, Post, Treatment*Post, Individual Fixed Effects, *Michigan State and Local Workers Bargaining Agreement ended in Dec 2013

One possible issue with the comparison of the panel design to the pooled design is nonrandom attrition. In the panel specification, individuals who were not matched between the survey 4 and survey 8 were dropped from the panel analysis. These individuals may have been dropped in systematic ways that may have biased the results from the main empirical findings from the panel difference-in-difference. However, there are many unobserved individual factors that may have biased the pooled results, such as tastes, ability, and motivation.

Subsection 3: Panel Basic CPS

The CPS interviews households in a 4-8-4 design, which shows that individuals can have four potential year-to-year transitions. The CPS MORG provides the survey 4 and survey 8 year-to-year transition, but the Basic CPS can construction three additional

year-to-year transitions for survey 1 and survey 5, survey 2 and survey 6, and survey 3 and survey 7. The main focus of the Basic CPS is the employment and unemployment status, since these outcomes are collected in each interview, while union status and weekly earnings are only collected in survey 4 and 8.

The methodology is the same between Table 6-7 and Table 6-12, but the data include more year-to-year transitions for the same individual. The methodology nets out individual fixed effects that may be observed or unobserved. In addition, occupation and industry dummy variables account for changes in industry and occupation. As in main empirical findings, standard errors are clustered by states to account for correlation of errors within states.

The sample size is notable increased for employment and unemployment status using the CPS basic. From Table 6-12, the specification includes individuals that the RTW law is intended to treat in panel 1 and all workers in the state in panel 2. The impacts of Indiana's RTW law on employment status and unemployment status are still in the same direction using observing more year-to-year transitions for individual. However, the marginal impacts have been reduced, but are still statistically significant. For Michigan, the signs of the coefficients for the treatment-post interaction do not change on employment and unemployment status between Table 6-7 and Table 6-12, but the impacts are still statistically insignificant. For Wisconsin, marginal impacts of the RTW law on employment status remain statistically insignificant in Panel 1 and Panel 2. For unemployment status, Wisconsin's RTW law remains unassociated with unemployment status for both specifications. The marginal impacts of the RTW laws on union status and weekly earnings in Table 6-12 are similar to Table 6-7 and Table 6-8,

since union status and earnings are only available in the outgoing rotation group or survey 4 and survey 8.

Table 6-12: Panel Basic CPS Findings

	Panel 1: NLRA and State and Local			Panel 2: All Worker		
States RTW Observations Around	Indiana Feb-12	Michigan* Dec-13	Wisconsin Mar-15	Indiana Feb-12	Michigan* Dec-13	Wisconsin Mar-15
Union Status	-0.0139** (0.00500)	-0.0136* (0.00446)	-0.0178** (0.00263)	-0.0198*** (0.00415)	-0.0132* (0.00448)	-0.0203*** (0.00194)
N	11,902	10,684	4,273	14,686	13,024	5,376
Employed	-0.00838** (0.00237)	-0.00366 (0.00414)	0.00195 (0.00208)	-0.0115*** (0.00140)	-0.00456 (0.00349)	0.00350 (0.00134)
N	51,443	44,136	18,132	69,698	59,309	25,210
Unemployed	0.00627** (0.00210)	0.00491 (0.00411)	-0.000464 (0.00215)	0.00912*** (0.000907)	0.00491 (0.00322)	-0.00258 (0.00128)
N	51,443	44,136	18,132	69,698	59,309	25,210
LN(Weekly Earnings)	-0.0206** (0.00705)	-0.0250** (0.00623)	-0.0403* (0.0118)	-0.0137 (0.0105)	-0.0335*** (0.00484)	-0.0317* (0.00740)
N	11,893	10,671	4,266	14,668	13,004	5,365

Variables include treatment, post-treatment*post, individual fixed effects, industry fixed effects, and occupation fixed effects. All utilize XTEG balancing on D and initial intervention and final interview (before and after). *Michigan State Workers Bargaining Agreement ended in Dec 2013

Since the same individual may have four different transitions, a concern is that the standard errors may be correlated within person and that there may systematic differences for the number of times a person is observed in the CPS. Standard errors were clustered within state to control for within-state correlation of errors (Conley & Taber, 2009). However, if the same individual is surveyed multiple times, standard errors may be correlated within state and within individuals. Therefore, there may be issues of biased standard errors when looking at multiple, independent year-to-year transitions for individuals. There also may be an issue if there are systematic differences between the outcomes and the number of times of year-to-year transitions is observed. The CPS

MORG has one transition for each individual, but the CPS Basic may have up to four year-to-year transitions for each person. Systematic differences in the number of transitions observed may have reduced the impacts of RTW laws between Table 6-7 and Table 6-12. However, the direction of the impacts did not change between Table 6-7 and Table 6-12.

Section 5: Conclusions

The short-run analysis finds support for the opponents of RTW laws and little to no support for proponents of RTW laws in Indiana, Michigan, and Wisconsin. Consistent across Indiana, Michigan, and Wisconsin, RTW laws lowered the likelihood for an individual to be a member of a union, which is consistent with the free-rider hypothesis and bargaining power hypothesis. Furthermore, controlling for individual fixed effects allow the model to control for the taste hypothesis. RTW laws lowered weekly earnings across Indiana, Michigan, and Wisconsin. These impacts were in-line with reduced threat effect and reduced union-monopolist and efficient contract models. Interestingly, the impact of RTW laws on likelihood of being employed or unemployed were consistent, but not always statistically significant, across the states of interest. The impacts were statistically significant in Indiana, but not for Wisconsin. The impact of Michigan's RTW law on employment and unemployment was sensitive to the population included, such that RTW law lowered employment likelihoods and increased unemployment likelihoods when all workers are considered but not for NLRA and state and local workers.

While one of the specifications controls for state unemployment rates, state budgets are another potential factor that may influence the impacts of RTW laws. If the state budgets are set to enact “pro-business” policies that are correlated with adoption of RTW laws, such as reducing state deficits or cutting taxes, then these can impact state and local worker employment and unemployment. There are several important factors to consider with this. First, the comparison groups were Midwestern states that all went through notable reductions in manufacturing employment and other budgetary constraints after the Great Recession. In addition, Indiana, Michigan, and Ohio all had Republican governors during the time period examined. If the linear trends in state budget reductions adversely affected each state similarly, then the linear trend will be captured in the difference-in-difference design. Second, the individual CPS data are on a monthly basis, whereas most state budgets are conducted annually. There may be insufficient observations and variations in state budgets to explain changes at the individual level on a monthly basis. Finally, the outcomes are fairly consistent and robust across different specifications. The results were consistent after including the state unemployment rate, which should have a much greater impact on individual employment. However, the impact of state expenditures on employment is still a valid concern.

Chapter 7: Long Run Analysis

This chapter of the dissertation focuses on the impact of RTW laws in the long-run. This analysis shifts away from the Midwestern Great Lake states with its historically strong labor unions and advocates to the Great Plains with lower rates of unionization. The analysis focuses on the impacts of RTW laws around Oklahoma's state borders before and after Oklahoma's RTW law. Similar to Holmes (1998), the analysis finds while RTW laws are associated with increased manufacturing employment, along with establishment and total wages shares at the county level. However, Oklahoma's adoption of a RTW law provided little evidence that RTW laws directly improve manufacturing employment and wages along Oklahoma's state borders with its neighboring RTW in the long-run. It is likely that there are other factors, including other "pro-business" state policies besides RTW laws, causing these discontinuities.

The long-run analysis of Oklahoma's RTW laws focuses on its state borders with neighboring RTW states of Arkansas, Kansas, and Texas before and after Oklahoma adopted its RTW law. Surrounded by RTW states, Oklahoma implemented its RTW law in September 28, 2001 (NRTW, 2015). Using a regression discontinuity design, the analysis fails to find that Oklahoma's outcomes of interest converged with its neighboring RTW states after it adopted a RTW law. The analysis finds that RTW laws have a statistically significant impact on shares of employment and wages in manufacturing between Oklahoma and surrounding RTW states at state borders in manufacturing before Oklahoma's RTW law in 2000. However, in 2010, nine years after Oklahoma adopted a RTW law, these discontinuities along state borders between Oklahoma and RTW states remain. When covariates are included, average weekly wages

converge at state borders and differences in 2000 are eliminated by 2010. However, the impact coefficients are sensitive to the inclusion of covariates, which reduces the validity of the RD design. The impact of RTW laws on total employment, total establishments, and share of establishments in manufacturing was not statistically different between Oklahoma and RTW states in 2000 and 2010.

Comparison tests were utilized to assess the impact of RTW laws outside of Oklahoma's state borders on the outcomes of interest. Similar to Holmes (1998), the impact of RTW laws for all counties along the union shop and RTW state borders were analyzed in 2000 and 2010. This analysis found that statistically significant impacts of RTW laws on measures of employment, wages, and establishments along state borders in 2000 remained statistically significant in 2010, but these impacts were slightly reduced.

While this corroborates Holmes (1998) findings that RTW laws matter, it also points to a few issues as to why Oklahoma's outcomes did not converge with its neighboring RTW states after it adopted a RTW law. First, Oklahoma's RTW law may have not had enough time to eliminate differences between Oklahoma and its neighboring RTW states. Second, unionization rates in manufacturing in Oklahoma and in neighboring RTW states were fairly low before Oklahoma's RTW law, but its RTW did reduce unionization in manufacturing (Eren & Ozbeklik, 2016). If RTW laws impact wages and employment through unionization, then initially low unionization in manufacturing might explain the lack of convergence with Oklahoma's neighbors. Third, there are other policies or factors causing employment and wage discontinuities along Oklahoma's state borders, such as taxation rates in comparison RTW states or mix of

types of manufacturing. Finally, local randomization might not have been available along Oklahoma's state borders.

This chapter will be broken into four sections. First, the long-run research questions will be reviewed. Second, descriptive analyses of employment, establishments, and wages along state borders will be analyzed. In addition, graphical analyses of discontinuous jumps at state borders provide important insight into the specification of the models to assess the impact of RTW laws at state borders. Third, the main empirical findings from the regression discontinuity design are discussed for different outcome measures. The industry analyzed is manufacturing, since manufacturing is historically more union-intensive than service industries. Finally, the impacts of RTW laws are analyzed using more states that have not changed their RTW laws. This is done to help determine if there are any secular trends along other union shop-RTW state borders outside of Oklahoma's state borders.

Section 1: Long-run research questions

The impact of RTW laws on wages and employment may differ from the impacts in the short-run. The long-run analysis does not focus on the impact of RTW laws on unionization in the long-run. In addition, the industry of analysis for these research questions is manufacturing, since manufacturing is historically more union-intensive. First, what happens to employment in manufacturing the long-run? Second, what happens to wages in manufacturing in the long-run? Third, what happens to establishments in manufacturing in the long-run?

Section 2: Descriptive and Graphical Analysis

This section of the long-run analysis delves into the descriptive and graphical analyses for Oklahoma and surrounding RTW states of comparison. First, a descriptive statistical analysis of measures of employment, wages, and establishments in manufacturing for counties in the states of interest is assessed. Second, a graphical analysis of minimum distance against outcome measures in manufacturing is utilized to inform the regression discontinuity design functional form.

Subsection 1: Descriptive Statistics

The descriptive statistics of outcomes variables of interest provide insight into state border discontinuities. The Quarterly Census of Employment and Wages (QCEW) provide the main data source to assess the impact of RTW laws in the long-run. The main unit of analysis is the county and the QCEW provides measurements of employment, wages, and establishments for each county in the states of interest. Descriptive analyses assess Oklahoma and the RTW states of Arkansas, Kansas, and Texas in 2000 and 2010.

Notable trends appear between Oklahoma and surrounding RTW states in 2000. From the Table 7-1, counties in Oklahoma show differences in the outcomes measures in manufacturing relative to the counties in RTW states. Manufacturing employment is lower in Oklahoma with a mean of 2,944 than its neighboring RTW states with a mean of 5,893. Looking at the share of total manufacturing employment in the county relative to all non-farm employment, the RTW states have a mean share of 19.2 percent in 2000, while Oklahoma counties had a mean share of 14.4 percent. Looking at manufacturing

share of total wages, Oklahoma had a mean share 18.3 percent and RTW counties had a higher mean of 19.2 percent. Since total wages is a function of total employment, it is important to look at weekly wages. In Oklahoma, the mean weekly wages in manufacturing is lower at 535.59 dollars per week and RTW counties had a higher mean of 580.60 dollars per week. For measures of manufacturing establishments, Oklahoma counties had a lower mean of 79.34, while RTW counties had a mean of 111.5. There is does not appear to be a big difference between Oklahoma counties and RTW counties for the share of establishments in manufacturing.

Table 7-1: Descriptive Statistics Oklahoma and RTW States in 2000

Manufacturing 2000	Oklahoma		RTW States	
	Mean	SD	Mean	SD
Employment	2,944	7,421	5,893	20,225
Employment Share	0.144	0.0866	0.192	0.1177
Weekly Wages	535.59	153.38	580.60	190.60
Total Wage Share	0.183	0.1181	0.236	0.1411
Establishments	79.34	219.57	111.5	415.32
Establishment Share	0.053	0.0194	0.058	0.0211
N	85		115	

Nine years after the implementation of Oklahoma's RTW law, outcome variables of interest show little difference with its fellow RTW states. From Table 7-2, the differences between Oklahoma counties, or former union shop counties, and RTW counties in 2010 do not seem notably different, except for average weekly wages. However, all counties appear to have secular reductions in employment and establishments in manufacturing between 2000 and 2010. Oklahoma counties had a mean employment in manufacturing of 1,888, while neighboring RTW states had a mean employment of 3,986. Counties in Oklahoma had a mean employment share in

manufacturing of 8.3 percent and counties in RTW states had a mean employment share of 9.1 percent. For total wage share in manufacturing, the difference in means between Oklahoma and comparison states is only remains about 5 percentage points. However, the difference in mean weekly wages in Oklahoma counties is smaller compared to its RTW neighbors in 2010. Mean weekly wages in Oklahoma was 752.19 dollars per week, while mean weekly wages in comparison counties was 781.33 dollars per week in 2010. For establishments, Oklahoma counties still have a lower mean at 66.4 compared to the RTW counties with a mean of 91.1. There remains to be little difference between Oklahoma counties and RTW counties in 2010 for establishment shares in manufacturing.

Table 7-2: Descriptive Statistics Oklahoma and RTW States in 2010

Manufacturing 2010	Oklahoma		RTW States	
	Mean	SD	Mean	SD
Employment	1,888	5,147	3,986	13,096
Employment Share	0.087	0.0646	0.135	0.1020
Weekly Wages	752.19	190.34	781.33	237.68
Total Wage Share	0.112	0.0883	0.167	0.1270
Establishments	66.40	179.45	91.11	316.32
Establishment Share	0.045	0.0179	0.048	0.0168
N	97		122	

Subsection 2: Graphical Analysis

A graphical analysis of the minimum distance to the state border provides important insight into the analysis Oklahoma before and after its RTW law. A regression discontinuity design is based upon a treatment assignment along some continuous variable, or running variable, that has a cutoff point. The units of analysis are assigned

on one side of the cutoff point, which generates a discontinuity (Jacob, Zhu, Somers, & Bloom, 2012). The treatment is assignment to RTW status, while the running variable is the minimum distance to the border. Holmes (1998) explains that minimum distance to the border represents the cost of moving a business from a union shop state to a RTW state. The farther away a firm is from the state border means that a firm is less likely to move. Treatment and the running variable are visually assessed with outcome variables of interest to provide guidance when assessing a regression discontinuity. First, graphical analyses provide a visual inspection of discontinuous jumps at cutoff points along a defined running variable. This is done to see if a discontinuity actually exists at cutoff points. The running variable is minimum distance to state borders for a county population centroid and it is important to note that minimum distance to state borders was provided by Holmes (2016). Second, graphical analyses provide guidance for the specification of the model between the running variable and the outcome variables of interest (Jacob, et. al., 2012). For the graphical analyses, bins were established in ten-mile intervals from the state borders and average outcomes were obtained for each 10-mile bin interval. Linear trends were included as well to assess the need of interaction terms between minimum distance to the border, or the running variable, and the treatment variable.

The treatment variable is having a RTW law on the books. For the states of interest, Oklahoma is the only state with a union shop law in 2000, while Arkansas, Kansas, and Texas are RTW states. After Oklahoma adopts a RTW law, it becomes a RTW state, as well. The regression discontinuity design tests to see if a discontinuity still exists between Oklahoma and its neighboring RTW states before and after its RTW law.

Therefore, Oklahoma's state borders can be considered former union shop counties in 2010, even though it has a RTW law.

The first set of labor outcomes visually inspected is employment outcomes. The natural log of total employment in manufacturing is considered first. Another employment measure of interest is shares of employment in manufacturing relative to total employment in the county. This is done, since it was one of the primary outcomes of interest in Holmes' (1998) study of RTW laws.

A visual inspection of manufacturing employment shares in Oklahoma counties, or union shop counties, and manufacturing employment shares in Arkansas, Kansas, and Texas, or RTW counties, show notable trends. From Figure 7-1, the natural log of manufacturing employment shows a discontinuity along Oklahoma's borders or when minimum distance to the border is equal to 0. RTW counties show a higher mean employment at Oklahoma's borders in 2000, but the jump is diminished by 2010. The slopes show that manufacturing employment declines as one moves closer to Oklahoma's state borders in both years. From Figure 7-2, in 2000, there is a conspicuous jump in manufacturing employment shares between Oklahoma counties and RTW counties at the state border. At the state border, counties in a RTW state have higher average employment share in manufacturing. In addition, the running variable shows that the slopes of the running variable to the outcome variable different between union shop counties and open shop counties. This provides guidance to consider a treatment-running variable interaction term. In 2010, there still appears to be a difference between former union shop counties in Oklahoma and counties in RTW states. However, the jump seems

to be much smaller than in 2000. Both sides of the border also show different slopes, which indicate to consider a treatment-running variable interaction term.

Figure 7-1: Employment in Manufacturing

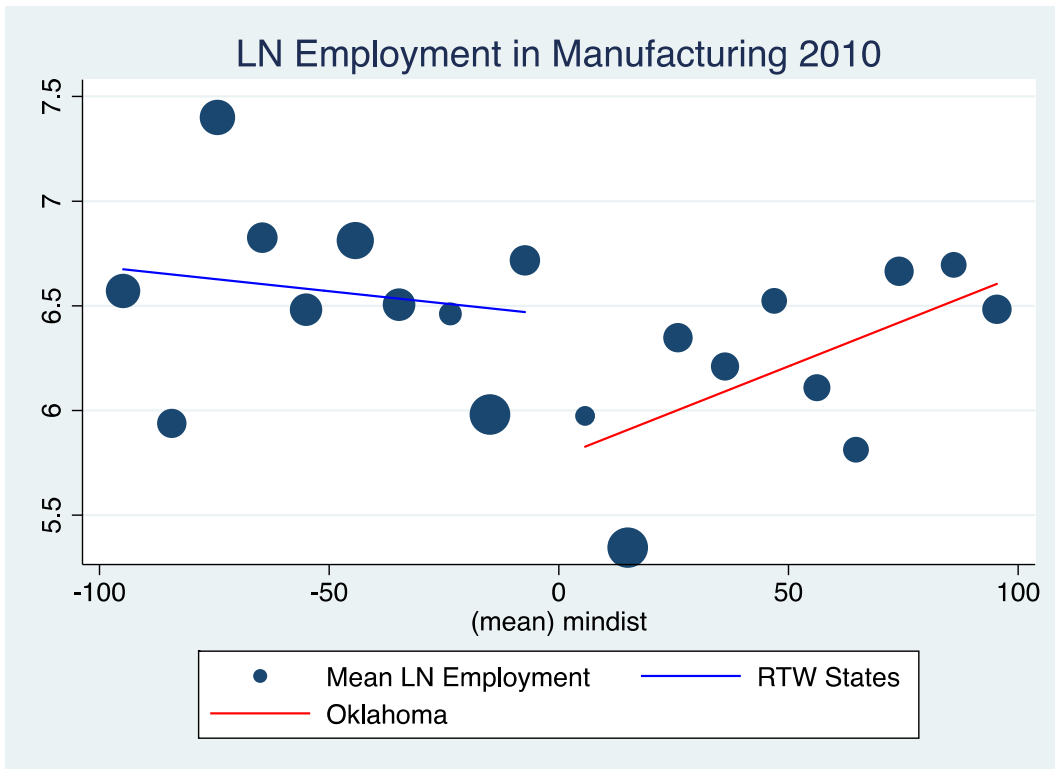
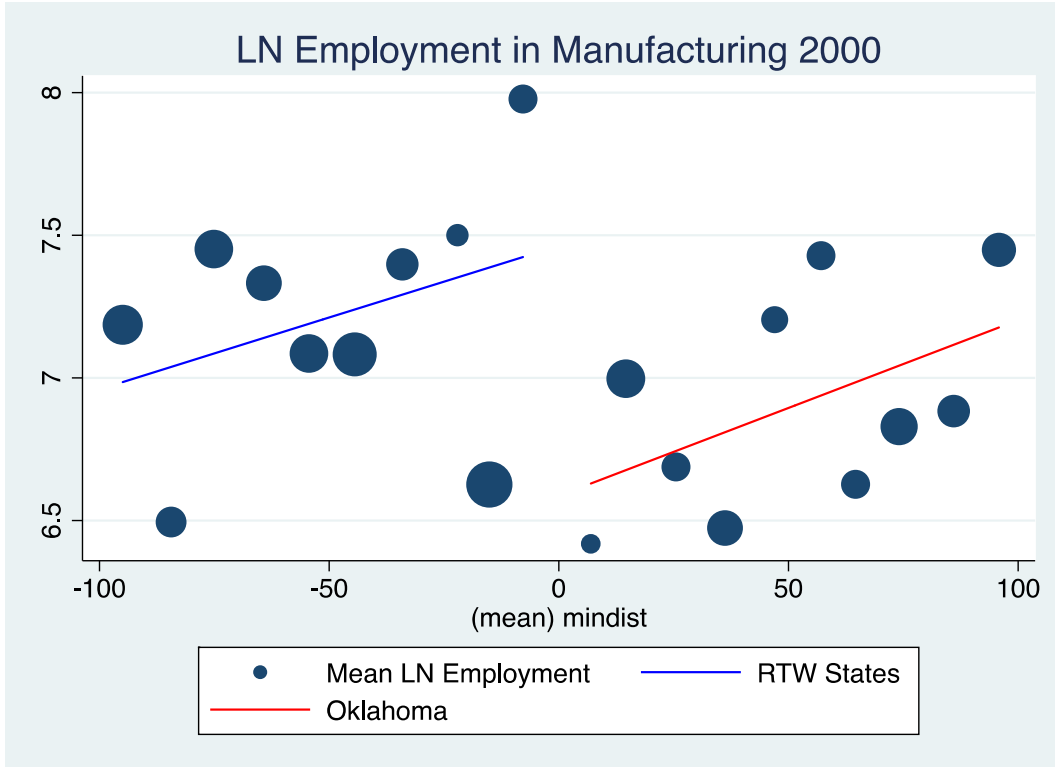
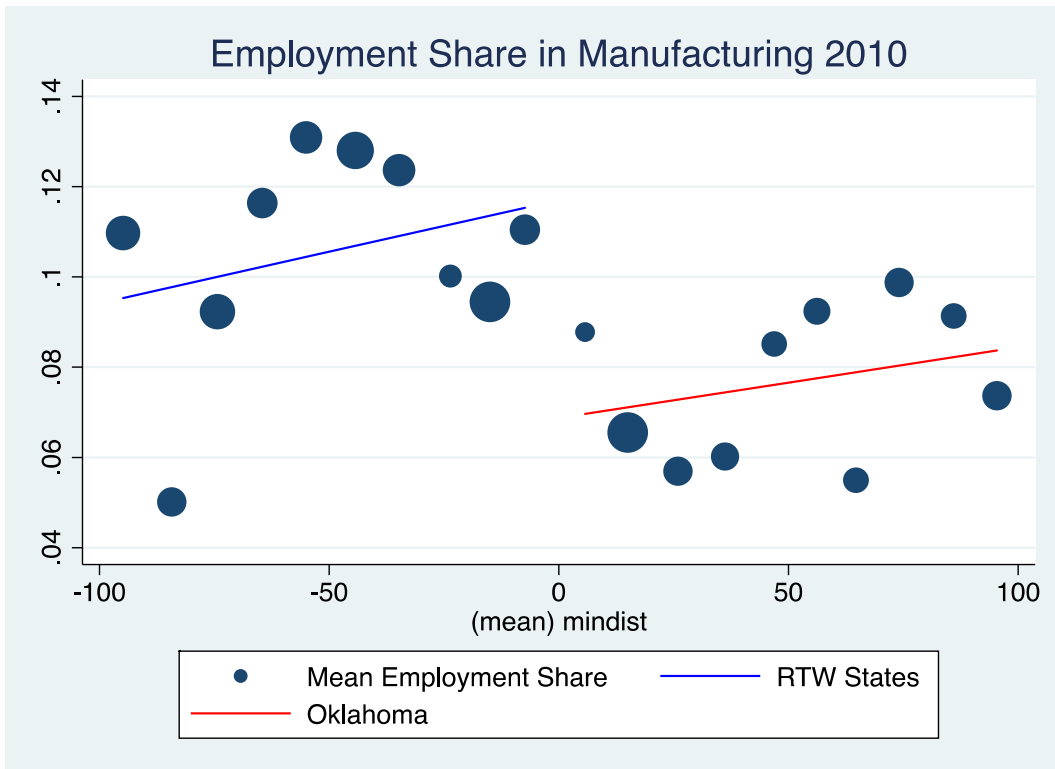
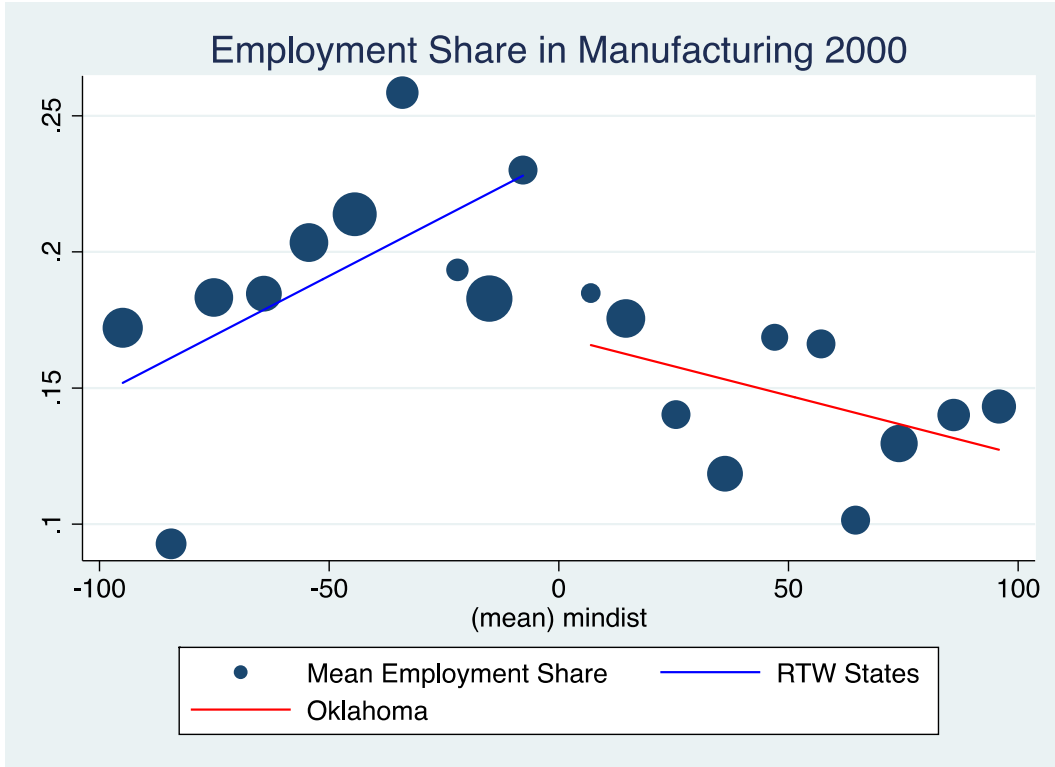


Figure 7-2: Employment Shares in Manufacturing



A graphical analysis of wage outcomes provides important insight into the analysis of wages at state borders. The first measure of wage outcomes is total wage of manufacturing as a share of non-farm total wages in a county. The second measure of wages in a county is the natural log of average weekly wages in manufacturing. Average weekly wages includes average hourly wages and average weekly hours, while total wages implicitly includes a measure of total employment. Therefore, it is expected that total wage shares might look similar to total employment shares.

The visual inspection of wage measures also show interesting trends. For total wages shares in manufacturing, there is a similarity in discontinuities and linear slopes with total employment shares. From Figure 7-3, in 2000, there is a large discontinuity at the state border, or at the cut-off point. The average total wage shares are higher in RTW counties than Oklahoma counties at the cutoff point. The linear trend in bins also show different slopes, which indicate that treatment-running variable interaction terms should be considered. In 2010, similar to total employment shares there is still a discontinuity, but it appears to have increased. When former union shop counties in Oklahoma are compared to RTW counties, the total wage shares still appear to be larger before and after Oklahoma's RTW law. The linear slopes appear to also differ, which suggest an interaction term for the post-period specification.

The natural log of average weekly wages appears to slightly converge after Oklahoma's RTW law is implemented. Average weekly wages in manufacturing in 2000 show a notable discontinuity between Oklahoma counties and RTW counties. However, unlike total wage shares, the discontinuity appears smaller in 2010. From Figure 7-4, there is a potential discontinuity in the natural log of average weekly wages at the state

border in 2000. The natural log of average weekly wages in Oklahoma counties appears to have a lower weekly wages than counties in RTW states at the state borders. The discontinuity appears to be a notable jump at the cutoff point in 2000. Also, average weekly wages appear to converge between union shop counties and open shop counties as someone moves away from the border in 2000. Therefore, a treatment-running variable interaction term should be considered for this model specification. In 2010, nine years after Oklahoma adopted their RTW law, average weekly wages were higher in RTW counties than in Oklahoma, but with a much smaller discontinuity. Also, Oklahoma and comparison states show differ linear trends away from state borders, which also indicates using an interaction term in the 2010 specification of weekly wages.

Figure 7-3: Total Wage Shares in Manufacturing

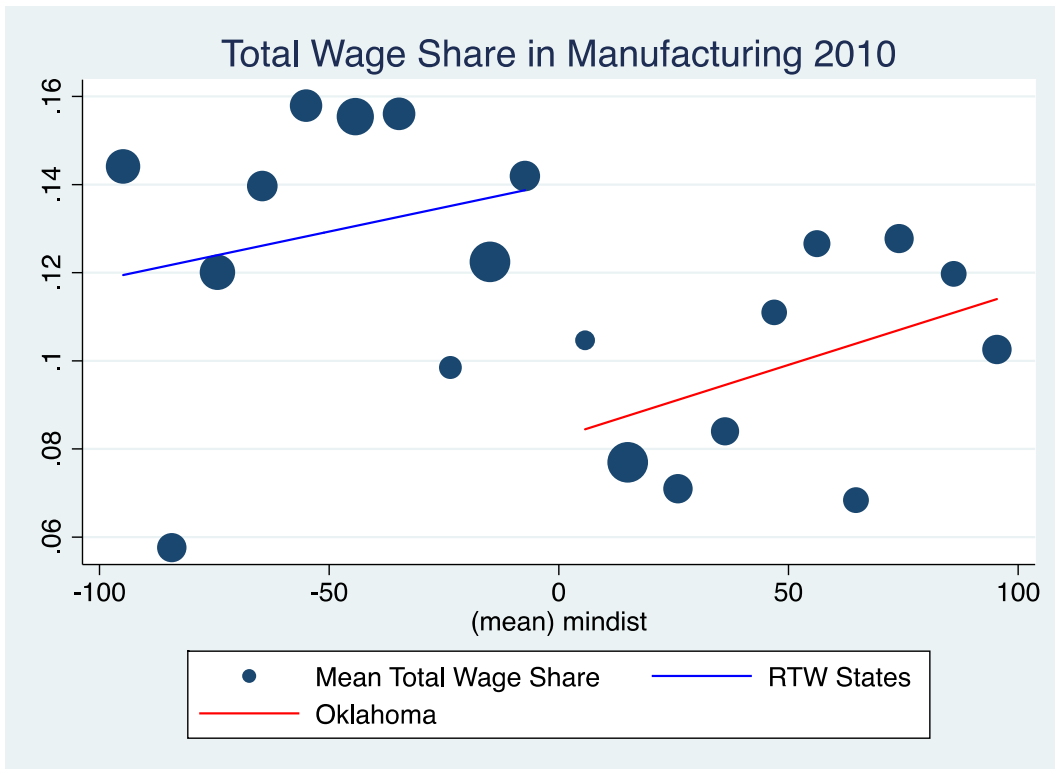
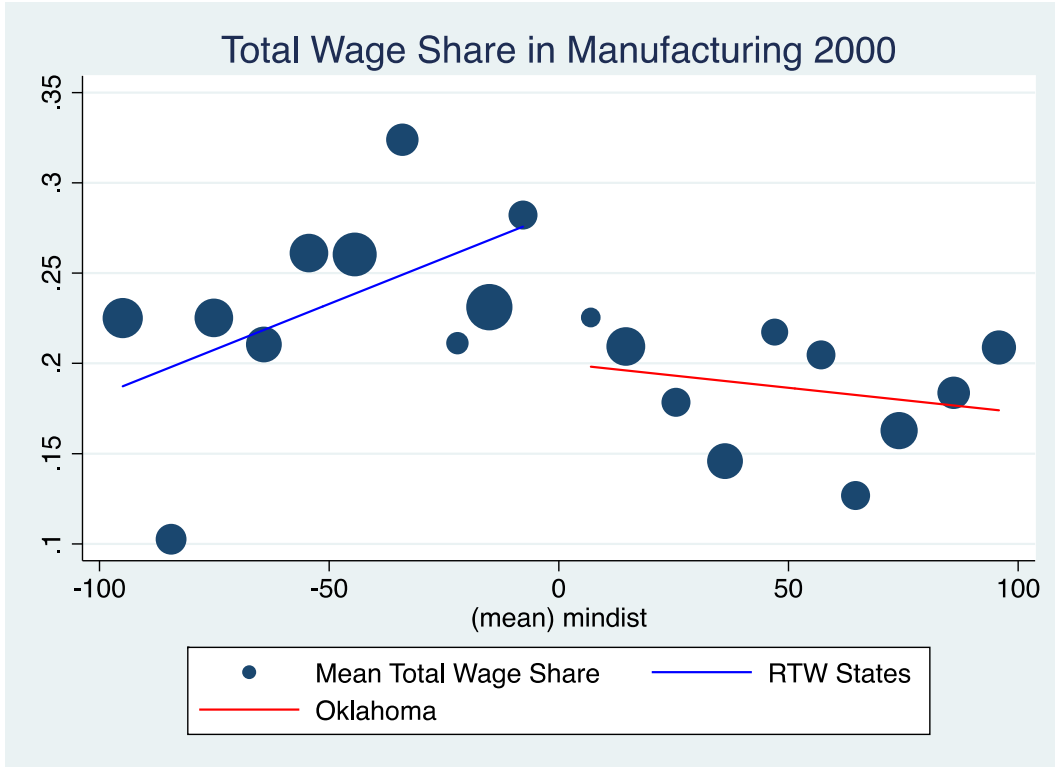
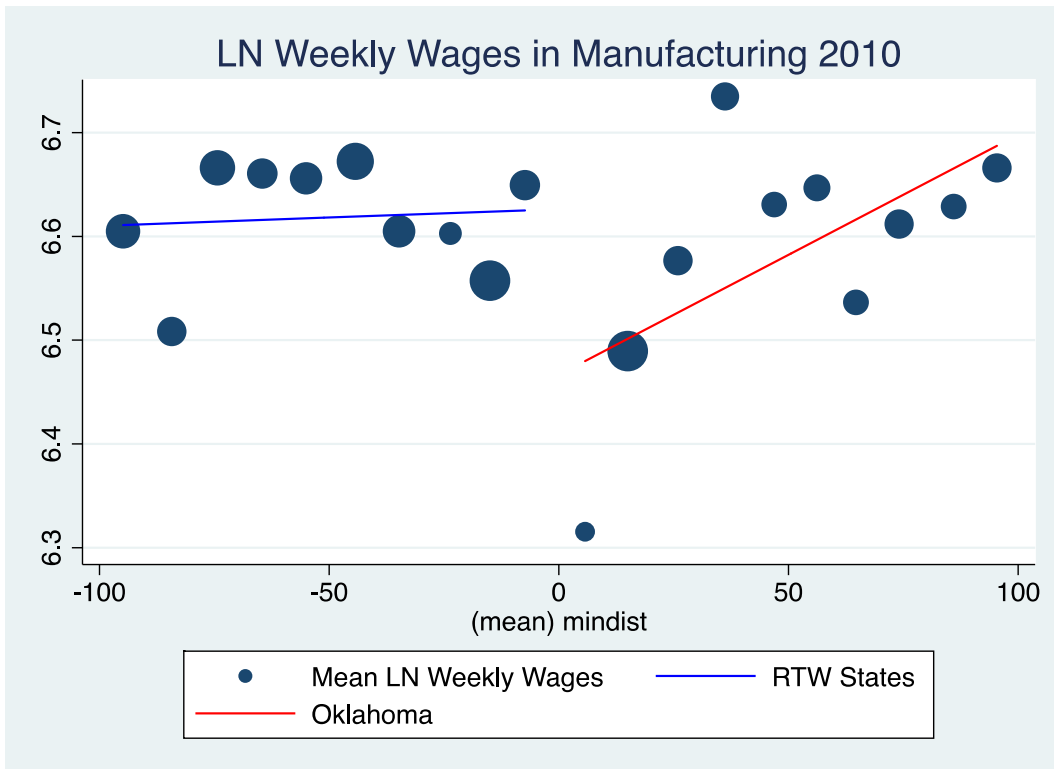
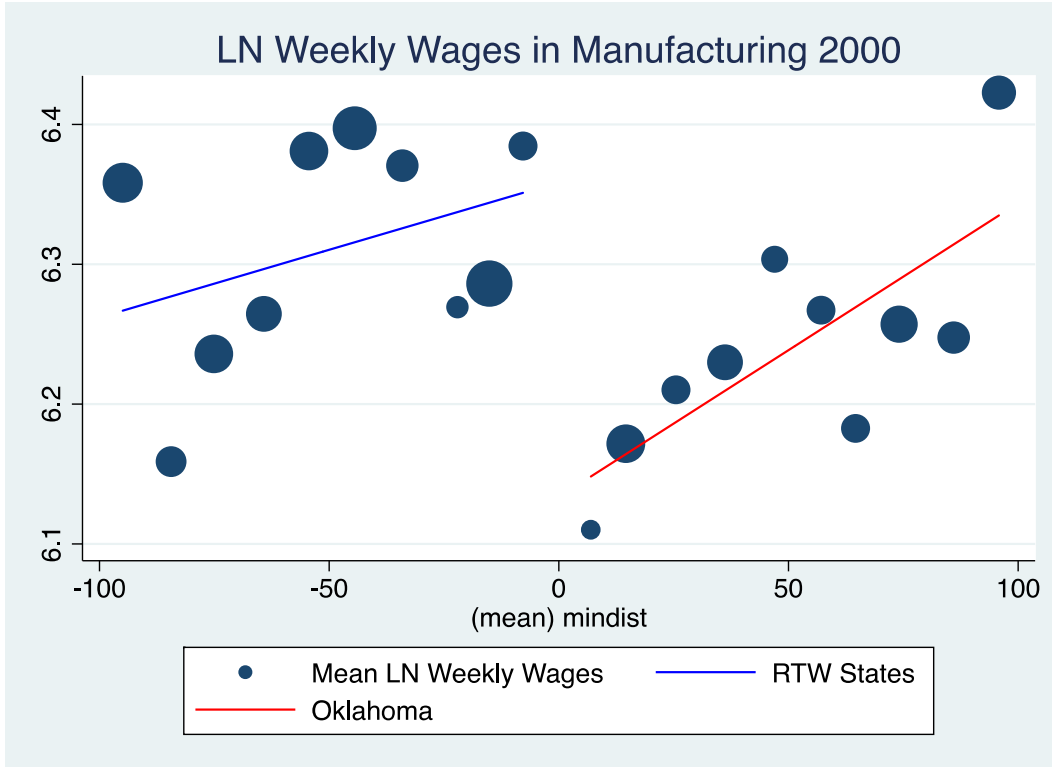


Figure 7-4: LN Weekly Wages in Manufacturing



A visual analysis of manufacturing establishment may provide insight into the movement of capital between union shop and open shop states and what happens after a RTW law is adopted. There are two measures of establishments that are considered. First, the natural log of total establishments in manufacturing is considered between Oklahoma and neighboring RTW states. Second, total establishments in manufacturing are compared to total non-farm establishments to get a measure of establishment share in manufacturing for each county.

The natural log of total establishments showed familiar discontinuities between Oklahoma counties and neighboring counties in RTW states counties along Oklahoma's state borders before and after Oklahoma's RTW law. The discontinuity was similar to the discontinuity in the natural log of employment. From Figure 7-5, in 2000, there is a discontinuity jump at the state border, where counties in RTW states have a higher measure of average natural log of total establishments than union shop counties in Oklahoma. In addition, average natural log of establishments appear to increase on both sides of the border as someone moves away from the state border, which is indicative of including an interaction term between treatment and minimum distance. In 2010, nine years after Oklahoma adopted its RTW law, there still appears to be a discontinuous jump at state borders and the discontinuity appears to be little changed. Average natural log of establishments appear to be converging as one moves away from Oklahoma's state borders, so a treatment-running variable interaction should be utilized.

Measures of establishment shares show convergence at state borders after Oklahoma's RTW law. From Figure 7-6, shares of establishments in manufacturing do not appear to have conspicuous discontinuity at the state border in 2000. The average

share of establishments for RTW counties seems to be around 0.005 percentage-points higher than counties in Oklahoma in 2000. In addition, the slopes decline on both sides of the border as the mileage away from the border increases. This indicates an interaction term might be necessary for the model specification. In 2010, a discontinuity does appear to have disappeared between counties in Oklahoma and neighboring RTW states. In addition, the slopes increase on both sides of the border as the mileage away from the border increases, which indicates an interaction term in the specification.

Figure 7-5: Establishments in Manufacturing

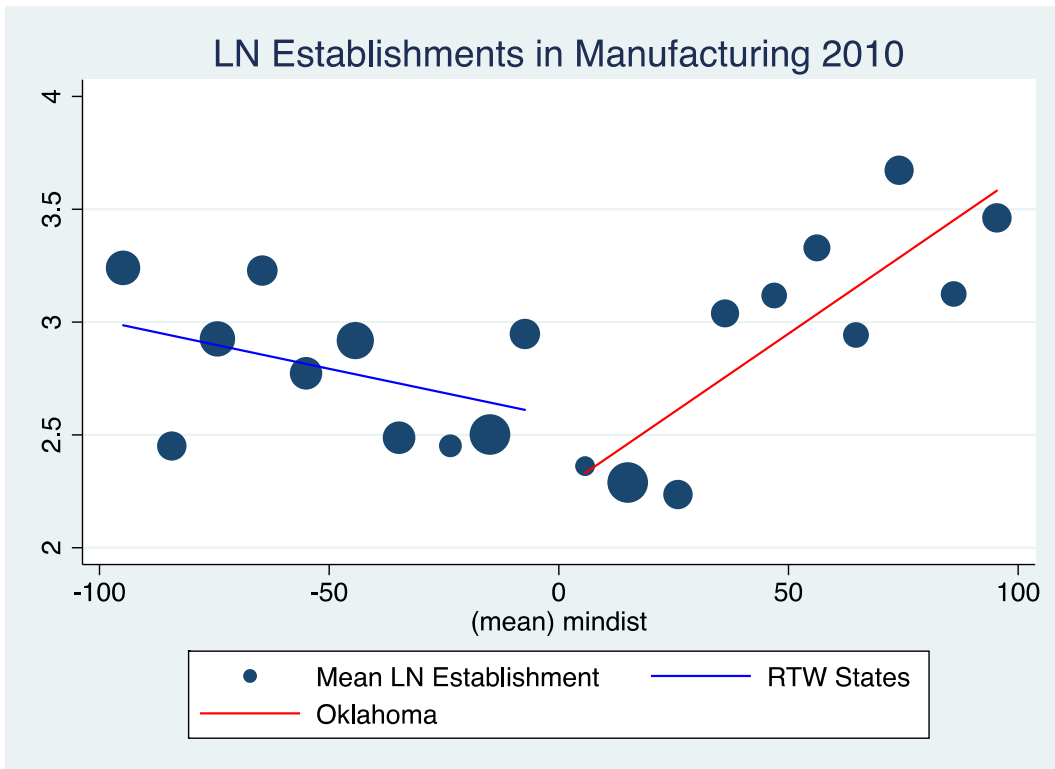
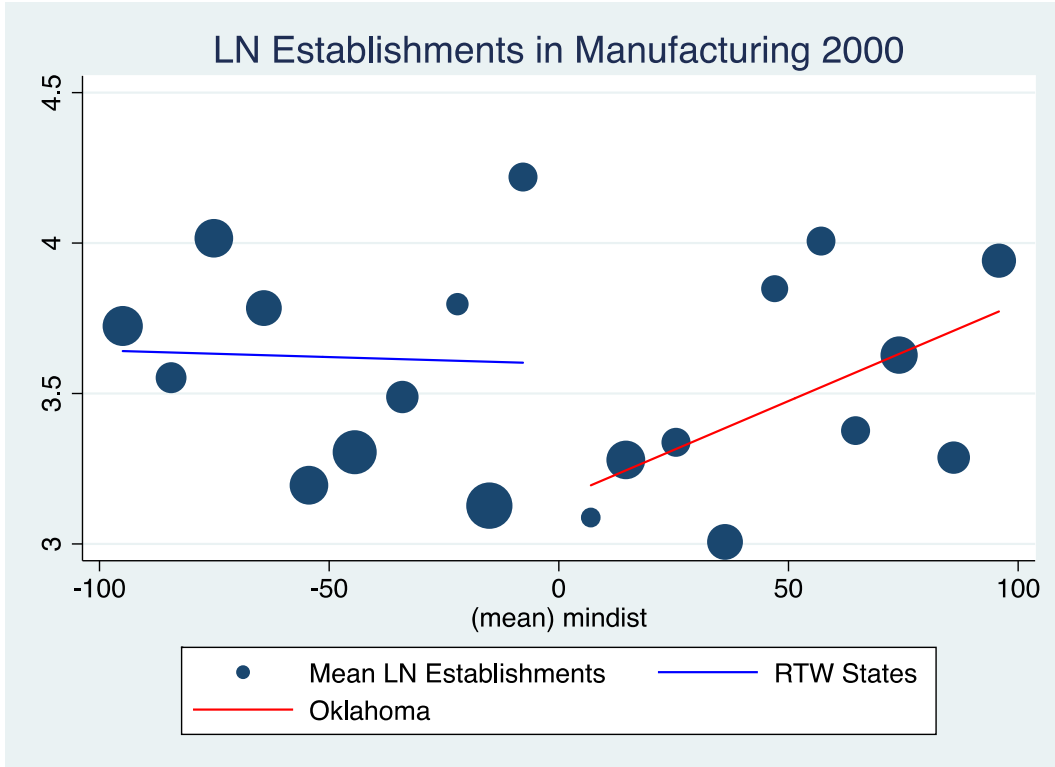
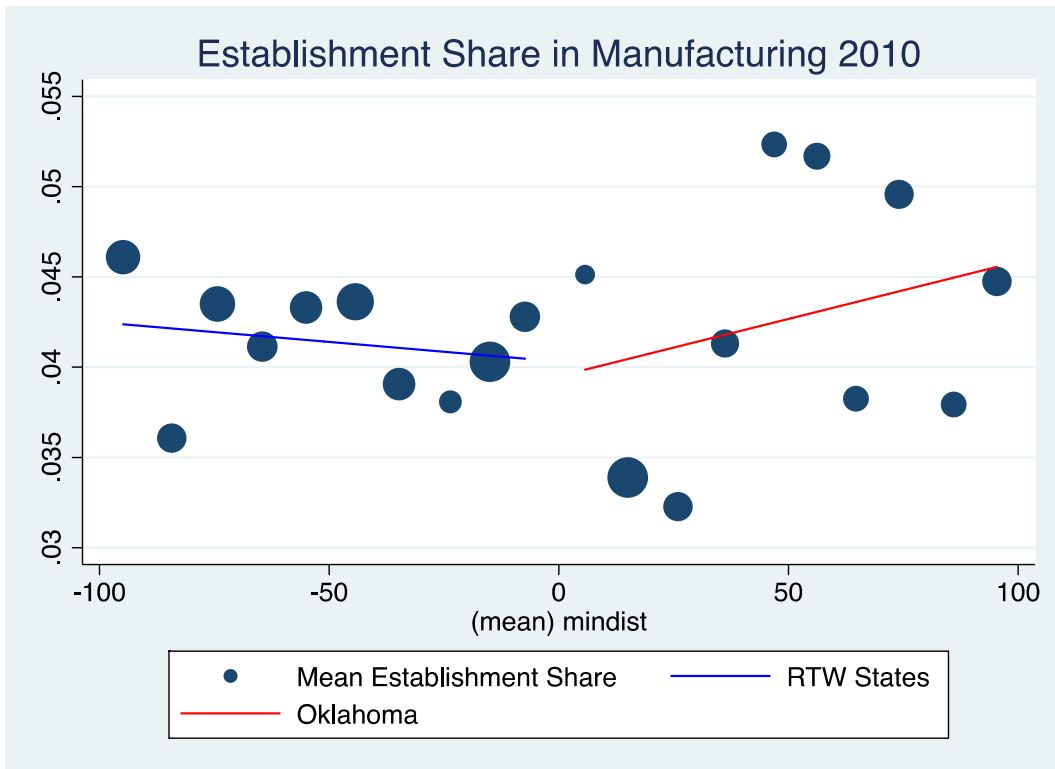
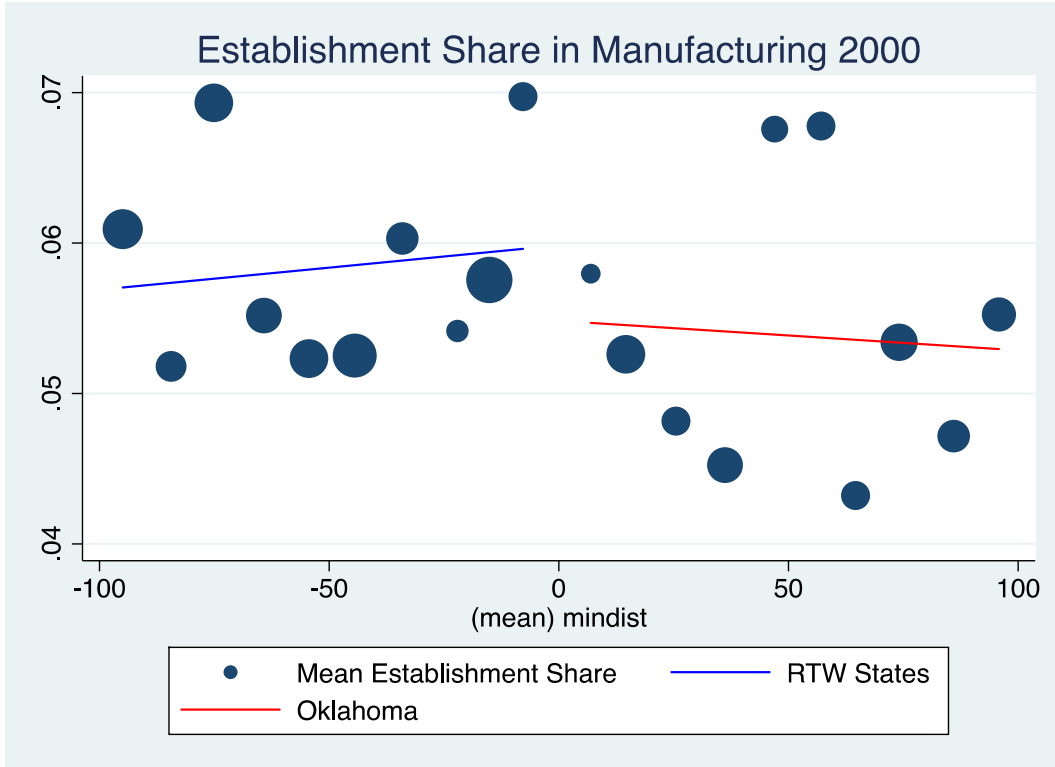


Figure 7-6: Establishment Shares in Manufacturing



Section 3: Main Oklahoma Borders Findings

This section discusses the main empirical findings from the regression discontinuity design to assess the impact of RTW laws in the long-run. The analysis focuses on local average treatment effects of RTW laws along Oklahoma's state borders before and after its RTW law. It shows that employment and wage discontinuities along Oklahoma's state borders with neighboring RTW states remain after Oklahoma adopts its RTW law. However, these discontinuities at state borders are reduced slightly nine years after Oklahoma adopts a RTW law. When including covariates, RTW states had higher average weekly wages before Oklahoma's RTW law, but the impact is eliminated after Oklahoma adopts its RTW law. This may imply that average weekly wages between Oklahoma and neighboring RTW states may have converged after Oklahoma adopts its RTW law. However, since the impact coefficients are sensitive to the inclusion of covariates, the validity of the RD design on average weekly wages is reduced (Lee & Lemieux, 2010). Impacts of RTW laws on measures of total employment, total establishments, and establishment shares were not statistically significant before or after Oklahoma's RTW law.

There are two parts of the long-run analysis. First, the impact of RTW laws from Arkansas, Kansas, and Texas is compared to Oklahoma's union shop status in 2000 along Oklahoma's state borders. Second, Oklahoma adopts a RTW law in 2001 and the impact of RTW laws from Arkansas, Kansas, and Texas are reassessed along Oklahoma's state borders in 2010. Therefore, the impact of RTW laws is compared to another RTW state and it is expected that prior discontinuities along Oklahoma's state would be diminished or eliminated.

The main empirical findings investigate multiple outcome variables at the state border for establishments in manufacturing. Two types of county employment outcomes are the natural log of total manufacturing employment and employment shares of total employment in manufacturing. Two types of county wage outcomes are total wage shares in manufacturing and the natural log of average weekly wages in manufacturing. Two types of county establishment outcomes are the natural log of total number of manufacturing establishments and establishment shares of total establishments in manufacturing.

The main specification for the regression discontinuity design is a linear-interaction specification. The linear-interaction specification includes the treatment variable, the running variable, and a treatment-running variable interaction. This is done for several reasons. First, the graphical analysis provides a starting point for the main model specification. Most of the graphical analyses suggested a treatment-running variable interaction specification. Second, the Akaike information criteria (AIC) approach can be used as a measure of goodness of fit for the specification. The linear-running variable specification had the smallest AIC and should be more appropriate among a set of specifications (Jacob, et. al., 2012). Finally, an F-test approach is utilized for specification tests. This is done to see if the specification fits the data used in the graphical analysis. The linear-running variable specification test failed to reject the null hypothesis, which means that the specification is appropriate (Jacob, et. al., 2012).

Subsection 1: Employment

The long-run analysis of RTW laws along Oklahoma's state borders show that employment discontinuities are statistically significant before Oklahoma's RTW law and these discontinuities remain nine years after Oklahoma adopts a RTW law. From Table 7-3, in 2000, the impact of RTW laws increased employment share in manufacturing relative in RTW states by 7.39 percentage points compared to Oklahoma union shop counties, which was statistically significant at the 5 percent level. In 2010, Oklahoma has a RTW law, but the impact of neighboring states RTW law on employment shares in manufacturing is exists. Even though Oklahoma has a RTW law, the impact of neighboring RTW laws is a 6.99 percentage point increase in employment shares in manufacturing, which was statistically significant at the 1 percent level. The impact of RTW laws on total manufacturing employment was not statistically significant before Oklahoma's RTW law, but it becomes statistically significant after Oklahoma adopts a RTW law.

Covariates are added to the model for specification tests of the RD design on employment. These are done for robustness checks to see if covariates have a notable impact on the treatment (Jacob, et. al., 2012). These county covariates include population, mean female population, mean non-Hispanic black population, mean non-Hispanic Asian population, mean Hispanic population, and mean non-Hispanic other race population. The impacts of RTW laws on employment shares remain fairly robust with the inclusion of covariates, even though the impacts are slightly reduced before and after Oklahoma's RTW law. From Table 7-3, the impact of RTW laws on employments shares in 2000 was an increase of 6.65 percentage points, which was statistically

significant at the 10 percent level. In 2010, after Oklahoma adopts a RTW law, the impacts remained and neighboring RTW laws increased employment shares in manufacturing by 5.95 percentage points along Oklahoma’s state borders, which was statistically significant at the 5 percent level. The impacts of RTW laws on total manufacturing employment are sensitive to the inclusion of covariates and the impacts were statistically insignificant.

Table 7-3: Employment Outcomes around Oklahoma

Oklahoma Analysis	Without Covariates	
2000	Employment Share	LN Employment
RTW Coefficient	0.0739** (0.0292)	0.627 (0.467)
N	200	200
R Squared	0.223	0.099
2010	Employment Share	LN Employment
RTW Coefficient	0.0699*** (0.0228)	0.861* (0.486)
N	213	213
R Squared	0.222	0.171
Oklahoma Analysis	With Covariates	
2000	Employment Share	LN Employment
RTW Coefficient	0.0665* (0.0345)	-0.0164 (0.431)
N	200	200
R Squared	0.277	0.491
2010	Employment Share	LN Employment
RTW Coefficient	0.0595** (0.0265)	0.225 (0.465)
N	213	213
R Squared	0.269	0.474

*All specifications include treatment binary, minimum distance, and milemarker on state border

**All specifications include border fixed effects for OK-TX, OK-KS, and OK-AR

***Covariates include county population, county female, county black, county asian, county other race, county hispanic

Subsection 2: Wages

The impact of RTW laws on measures of wages along Oklahoma's state borders showed similar trends to employment. Focusing on the total wage share in manufacturing, the impact of Oklahoma's union status and subsequent RTW law show similar trends to total employment share. From Table 7-4, in 2000, the impact of RTW laws is positive and statistically significant. RTW laws increase total wage shares in manufacturing by 9.93 percentage points compared to Oklahoma's union shop counties, which was statistically significant at the 1 percent level. In 2010, the impact of neighboring RTW laws is still statistically significant, but the impact is reduced to 8.93 percentage points nine years after Oklahoma adopts a RTW law.

It is important to note that employment is implicitly measured within total wages, so an additional measure of wages is tested. Average weekly wages in manufacturing capture county level average weekly hours and hourly wages. From Table 7-4, the impact of RTW laws along Oklahoma's state borders on average weekly wages in 2000 was statistically significant at the 5 percent level and increased average weekly wages by 23.1 percent. In 2010, after Oklahoma's adoption of a RTW law, the impact of neighboring RTW laws on average weekly wages remain statistically significant at the 10 percent level along Oklahoma's state borders and increases average weekly wages by 14.1 percent.

Covariates are added to the model specifications for a robustness test. The impact of RTW laws on total wage shares remains fairly robust after the inclusion of covariates. From Table 7-4, after including covariates, RTW laws are associated with a 10.5 percentage point increase in total wages shares in manufacturing along Oklahoma's state

borders in 2000, which is statistically significant at the 5 percent level. In 2010, there is still an impact of RTW laws on total wage shares in manufacturing along state borders. After Oklahoma adopts a RTW law, the impact of neighboring RTW laws was reduced to a 9.2 percentage-point increase in total wage shares in manufacturing, which was statistically significant at the 1 percent level.

The impact of the RTW laws on average weekly wages showed differences when covariates were added. When covariates are added, the impact of RTW laws increased average weekly wages by 17 percent at state borders in 2000. This was statistically significant at the 5 percent level. However, the state border discontinuities are no longer statistically significant in 2010 after Oklahoma adopts its RTW law. When covariates are included, there is slight support that Oklahoma's RTW law may have helped converge average weekly wages with its RTW neighbors. However, this test showed that the average weekly wage model without covariates was not as robust as total wage shares and may invalidate the RD design (Lee & Lemieux, 2010).

Table 7-4: Wage Outcomes around Oklahoma

Oklahoma Analysis	Without Covariates	
2000	Total Wage Share	LN Weekly Wages
RTW Coefficient	0.0993*** (0.0375)	0.208** (0.0856)
N	200	200
R Squared	0.167	0.052
2010	Total Wage Share	LN Weekly Wages
RTW Coefficient	0.0893*** (0.0296)	0.132* (0.0773)
N	213	213
R Squared	0.185	0.053
Oklahoma Analysis	With Covariates	
2000	Total Wage Share	LN Weekly Wages
RTW Coefficient	0.105** (0.0442)	0.157* (0.0927)
N	200	200
R Squared	0.233	0.261
2010	Total Wage Share	LN Weekly Wages
RTW Coefficient	0.0920*** (0.0345)	0.0804 (0.0852)
N	213	213
R Squared	0.228	0.203

*All specifications include treatment binary, minimum distance, and milemarker on state border

**All specifications include border fixed effects for OK-TX, OK-KS, and OK-AR

***Covariates include county population, county female, county black, county asian, county other race, county hispanic

Subsection 3: Establishments

To test the impact of RTW laws on movement in capital, measures of establishments in manufacturing were assessed. There was less evidence that RTW laws are associated with manufacturing establishments along Oklahoma's state borders. From Table 7-5, the impact of RTW laws on the shares of establishments in manufacturing was not statistically significant in 2000 between RTW states and union shop counties in

Oklahoma. In 2010, there was still no difference in discontinuities at state borders nine years after Oklahoma adopted a RTW law.

Total establishments showed a similar outcome compared to establishment shares. From Table 7-5, for total manufacturing establishments in 2000, there were not statistical differences along Oklahoma's state borders between Oklahoma and its neighboring RTW counties. In 2010, after Oklahoma adopts its RTW law, the specification remains statistically insignificant.

When covariates are added to the specifications of the model, the impact of RTW laws on establishment shares and total establishment per capital were sensitive to the inclusion of covariates. The impacts of RTW laws at Oklahoma's state borders on total establishment remained statistically insignificant before and after Oklahoma's RTW law. However, the impact of neighboring RTW laws on establishment share becomes statistically significant in 2010, but the coefficients are fairly sensitive to the inclusion of covariates, which reduces the validity of the design.

Table 7-5: Establishment Outcomes around Oklahoma

Oklahoma Analysis	Without Covariates	
	Establishment Share	LN Establishments
2000		
RTW Coefficient	0.00801 (0.00554)	0.284 (0.336)
N	200	200
R Squared	0.221	0.086
2010		
RTW Coefficient	0.00649 (0.00431)	0.438 (0.327)
N	213	213
R Squared	0.241	0.177
Oklahoma Analysis	With Covariates	
	Establishment Share	LN Establishments
2000		
RTW Coefficient	0.00367 (0.00661)	-0.204 (0.280)
N	200	200
R Squared	0.266	0.579
2010		
RTW Coefficient	0.00930* (0.00505)	0.00481 (0.280)
N	213	213
R Squared	0.276	0.581

*All specifications include treatment binary, minimum distance, and milemarker on state border

**All specifications include border fixed effects for OK-TX, OK-KS, and OK-AR

***Covariates include county population, county female, county black, county asian, county other race, county hispanic

Section 4: State Border Impacts for All States

To corroborate the results from the Oklahoma state border analysis, more state borders between RTW states and union shop states are utilized using QCEW data in 2000 and reassessed in 2010. This is a comparison test to check if there was a secular trend affecting all state borders between union shop and RTW states. The specification of state

borders is similar to that of Holmes (1998), where all RTW-union shop state borders east of the Mountain West are included. Furthermore, Oklahoma's state borders are excluded to see how other state border impacts varied over time. Also, unlike Holmes (1998), minimum distance to the border is interacted with RTW treatment for the functional form, which is consistent with the Oklahoma analysis.

It is important to test all state border discontinuities between RTW states and union shop states, since there may be a secular trends in the impact of RTW laws along all state borders between 2000 and 2010. If state border discontinuities between union shop states and open shop states are reduced or remain nearly the same, then Oklahoma's RTW law will likely have had no effect on outcomes of interest. However, if the state border discontinuities along other state borders are increasing, then Oklahoma's RTW law might have helped reduced impacts from this secular trend.

The results support the idea of a secular trend among all states and that Oklahoma's RTW law may not have slightly reduced employment and wage discontinuities along Oklahoma's state borders. The results show that state border discontinuities between RTW states and union shop states for manufacturing employment shares were statistically significant in 2000 and in 2010 for all states outside of Oklahoma's state borders. However, the impact of RTW laws on employment shares is reduced over the time period. The same remained true for total wage shares in manufacturing and establishment shares in manufacturing. The impacts of RTW laws on average weekly wages in manufacturing were statistically significant in 2000 and 2010 with the gaps between union shop states and RTW states growing in the time period. There seems to be a general convergence in employment shares between 2000 and 2010

when all states were affected by declining manufacturing employment, but it also shows a divergence between RTW states and union shop states between 2000 and 2010 for average weekly wages in manufacturing with RTW having higher wages at state borders.

The main specification was the linear with interaction to be consistent with the Oklahoma state border analysis. The local average treatment effects of RTW laws at state borders is assessed in 2000 and reassessed in 2010. All states borders that were included in Holmes' (1998) analysis were included, such that state borders in the rural Mountain West and Western counties were excluded. However, Oklahoma state borders and counties along Oklahoma state borders were excluded. The RTW states include Arkansas, Iowa, Kansas, Nebraska, North Dakota, South Dakota, Tennessee, and Virginia. The union shop states include District of Columbia, Illinois, Kentucky, Maryland, Minnesota, Missouri, West Virginia, and Wisconsin. While Arkansas and Kansas counties that border Missouri are included, their border counties with Oklahoma are not included.

From Table 7-6, using this specification, the impact of RTW laws at state borders increased employment shares in manufacturing by 5.58 percentage points in 2000, which was statistically significant at the 1 percent level. From Holmes' (1998) specifications, the impact of RTW laws on employment shares in manufacturing ranged between 6.4 to 6.6 percentage points in 1992. In 2010, the impact of RTW laws at state borders was associated with an increase in employment share by 3.95 percentage points, which was statistically significant at the 1 percent level. From Table 7-6, including covariates, the impact of RTW laws on employment shares remains fairly robust. While RTW laws remain positively associated with manufacturing employment shares, the impact appears

to have declined more along state borders outside of Oklahoma where no RTW laws were implemented.

Total manufacturing employment was tested along state borders. It appears that RTW laws were statistically significant and positive on the natural log of total manufacturing employment in 2000 or in 2010. When covariates are included, the RTW impacts along state borders remain statistically significant and positive. However, the impacts are very sensitive to the inclusion of covariates, which reduces the validity of the RD design to assess the impact of RTW laws on total employment.

Table 7-6: Employment Outcomes by All States

All-State w/o DK	Without Covariates	
	Employment Share	LN Employment
2000		
RTW Coefficient	0.0558*** (0.0132)	0.275* (0.161)
N	984	984
R Squared	0.198	0.087
2010		
RTW Coefficient	0.0395*** (0.0106)	0.489*** (0.178)
N	1,044	1,044
R Squared	0.155	0.088
All-State w/o DK	With Covariates	
	Employment Share	LN Employment
2000		
RTW Coefficient	0.0496*** (0.0126)	0.481*** (0.127)
N	984	984
R Squared	0.287	0.443
2010		
RTW Coefficient	0.0352*** (0.0101)	0.531*** (0.142)
N	1,044	1,044
R Squared	0.258	0.427

*All specifications include treatment binary, minimum distance, and milemarker on state border

**All specifications include border fixed effects for all RTW-Union shop borders except for DK borders

***Covariates include county population, county female, county black, county Asian, county other race, county Hispanic

Testing the impact of RTW in all states showed that RTW laws at state borders were associated total wage shares and average weekly wages. Interestingly, the impact on average weekly wages possibly shows a divergence between 2000 and 2010. From Table 7-7, the impact of RTW laws at state borders statistically significant at the 1 percent level and increased total wage shares by 7.12 percentage points in 2000. Ten year later, the impacts of RTW laws are still statistically significant at the 1 percent level,

but the impact was reduced to an increase of 5.64 percentage points. The impacts on total wage shares remain fairly robust when covariates are included. RTW laws were associated with a 6.2 percent increase in average weekly wages in 2000 and, in 2010, RTW laws were associated with a 12.4 percent increase in average weekly wages. When covariates are included the impacts in 2010 are fairly robust, while the impacts in 2000 are more sensitive. This does question if there was a divergence in average weekly wages between union shop and RTW states along state borders.

Table 7-7: Wage Outcomes by All States

All-State w/o DK	Without Covariates	
	Total Wage Share	LN Weekly Wages
2000		
RTW Coefficient	0.0712*** (0.0163)	0.0597* (0.0312)
N	984	984
R Squared	0.189	0.143
2010		
RTW Coefficient	0.0564*** (0.0138)	0.117*** (0.0334)
N	1,044	1,044
R Squared	0.158	0.107
All-State w/o DK	With Covariates	
	Total Wage Share	LN Weekly Wages
2000		
RTW Coefficient	0.0658*** (0.0157)	0.0920*** (0.0275)
N	984	984
R Squared	0.265	0.344
2010		
RTW Coefficient	0.0528*** (0.0133)	0.124*** (0.0294)
N	1,044	1,044
R Squared	0.236	0.324

*All specifications include treatment binary, minimum distance, and milemarker on state border

**All specifications include border fixed effects for all RTW-Union shop borders except for DK borders

***Covariates include county population, county female, county black, county Asian, county other race, county Hispanic

Unlike the analysis of RTW laws along Oklahoma state borders, the all-state analysis shows that RTW laws are associated with the share of establishments in manufacturing. From Table 7-8, RTW laws along state border increased the share of establishments in manufacturing by 0.83 percentage points in 2000. In 2010, the impact of RTW laws remained statistically significant at the 1 percent level and was associated with a 0.7 percentage point increase in the establishment shares. The RTW impacts on

establishment shares remained fairly robust when covariates were included. For total establishments, the impacts of RTW laws were not statistically significant without covariates in 2000, but RTW laws were associated with a 23.1 percent increase in total establishments in 2010. However, the impacts were very sensitive to the inclusion of covariates, which reduces the validity of the design of the impact of RTW laws on total establishments along state borders.

Table 7-8: Establishment Outcomes by All States

All-State w/o DK	Without Covariates	
	Establishment Share	LN Establishments
2000		
RTW Coefficient	0.00831*** (0.00278)	0.0402 (0.120)
N	984	984
R Squared	0.240	0.078
2010		
RTW Coefficient	0.00702*** (0.00225)	0.208* (0.120)
N	1,044	1,044
R Squared	0.228	0.091
All-State w/o DK	With Covariates	
	Establishment Share	LN Establishments
2000		
RTW Coefficient	0.00685** (0.00273)	0.224*** (0.0829)
N	984	984
R Squared	0.283	0.569
2010		
RTW Coefficient	0.00657*** (0.00220)	0.245*** (0.0837)
N	1,044	1,044
R Squared	0.275	0.567

*All specifications include Treatment Binary, Minimum Distance, and Milemarker on State Border

**All specifications include Border Fixed Effects for All RTW-Union Shop Borders except for DK borders

***Covariates include County Population, County Female, County Black, County Asian, County Other Race, County Hispanic

Section 5: Conclusions

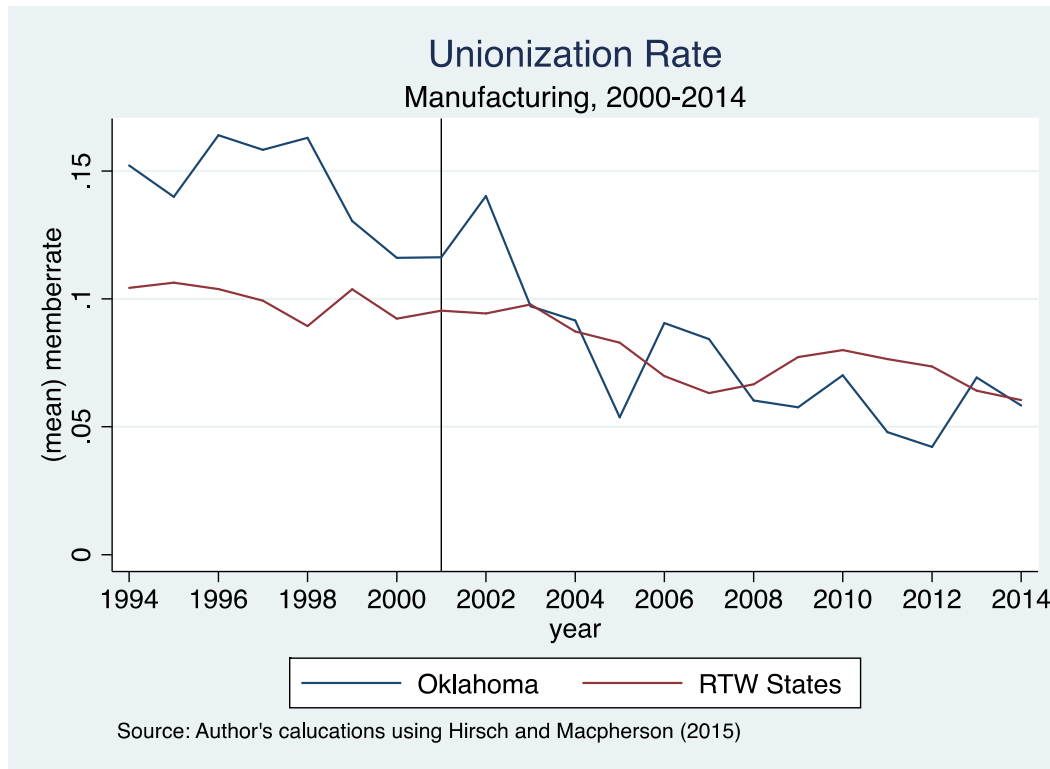
The analysis shows that will RTW laws are positively associated with higher employment, total wage, and establishment shares in manufacturing along state borders. In addition, RTW laws may be possibly associated with increased average weekly wages in manufacturing. However, there is insufficient evidence that RTW laws directly cause these discontinuous impacts along state borders. From the Oklahoma analysis, state border discontinuities in employment and total wage shares remained fairly constant before and after Oklahoma adopted a RTW law. It was expected that wage and employment discontinuities found in 2000 would converge, or be eliminated, after Oklahoma adopted its RTW law. While the state border discontinuities do decline, they do not decline as much as other state borders which remained union shop-RTW borders. From the all-state analysis of RTW laws, the impact of RTW laws remained statistically significant between 2000 and 2010, but the impacts were reduced ten years later and these reductions were usually more than what was observed along Oklahoma state borders after it adopted a RTW law.

There is insufficient evidence that RTW laws directly impact employment, wage, and establishment shares for several reasons. Given that Oklahoma adopted a RTW law, it was expected that any discontinuity between Oklahoma and its RTW neighbors would disappear. However, this was not observed and there may be other policies and factors that were discontinuous along state borders. Such factors may include different taxation rates, different tax breaks or incentives, or other “pro-business” policies. It is important

to note that Holmes (1998) uses RTW laws as proxy variables for “pro-business” policies and does not directly test RTW laws.

Another notable factors here is that Oklahoma had a low rate of unionization just before it adopted a RTW law. It is possible that RTW laws may not affect wages and employment in a sector that already has low rates of unionization. Since unionization for manufacturing is unavailable at the county level, it was not possible to test Oklahoma’s RTW law on county level unionization in manufacturing. However, from Figure 7-7, the rate of unionization in Oklahoma in manufacturing is compared to Arkansas, Kansas, and Texas is not much higher in 2000. Rates of unionization in manufacturing appeared to be converging even before Oklahoma’s RTW law was implemented. Figure 7-7 does show that unionization rates in manufacturing do converge after Oklahoma’s RTW law, though. Eren and Ozbeklik (2016) find that Oklahoma’s RTW law does reduce overall private unionization and manufacturing unionization. Given possible other state policies and factors, along with Oklahoma’s low rate of unionization in manufacturing, Oklahoma’s RTW law might not been strong enough to converge employment and wage share discontinuities given. Farber (2005) concludes that the marginal effect of RTW laws might be small if unionization is low initially and this research corroborates with this conclusion.

Figure 7-7: Unionization Rate in Manufacturing



Subsection 1: Limitations

One of the most notable limitations comes from the sensitivity of the impact coefficients on the inclusion of covariates. In a RD design, impacts at cutoff should not be sensitive to the inclusion or exclusion of covariates (Lee & Lemieux, 2010).

Furthermore, the inclusion of covariates should only reduce the standard error of the impact coefficient (Jacob, et al., 2012). The sensitivity of the impact coefficient on absolute measure of employment and establishments reduces the reliability of these outcomes. However, the RD design using employment, total wage, and establishment shares outcomes were much more robust and more reliable.

To assess the internal validity of the regression discontinuity design for the analysis of Oklahoma's RTW law, non-outcome variables are tested at state borders. Non-outcome variables should not be discontinuous at state borders (Jacob, et. al., 2012). While this is mainly a test for manipulation at the cutoff, it can provide important insight into confounding variables. These county covariates include population, mean female percentage mean non-Hispanic white percentage, mean non-Hispanic black percentage, mean non-Hispanic Asian percentage, mean non-Hispanic other race percentage, and mean Hispanic percentage. A test of these covariates in the specifications showed that non-Hispanic whites and non-Hispanic other race were two covariates that showed discontinuous jumps at state borders. These differences are not surprising with history of Native Americans being forced to relocate to Oklahoma. While including covariates in the model may improve precision, they seem to be appropriate for a more valid assessment of Oklahoma's union shop status (Jacob, et al., 2012).

Chapter 8: Discussion, Policy Significance, and Future Research Questions

This chapter concludes the work done on the impact of Right-to-Work (RTW) laws on wages and employment. In support of opponents, lower unionization, wages, and employment, along with higher unemployment propensities, were found for individuals in Midwestern states in the short-run, while no positive labor outcomes were observed. However, there is very limited evidence for proponents in the long-run. RTW laws were positively associated with employment, total wage, and establishment shares in manufacturing along state borders. However, there was no evidence that RTW law cause these impacts, since employment and wage gaps along Oklahoma's state borders did not improve employment and total wage shares after Oklahoma adopted a RTW law. For a policy recommendation, policymakers should be hesitant for adopting RTW laws as a panacea for labor markets.

This chapter consists of two sections that integrate a discussion of the results, a policy recommendation, and future research questions. The first section will discuss the results and how they relate to the findings in the literature and theoretical conceptual models. The second section provides a policy recommendation for policymakers from the empirical findings and prior research. In addition, a discussion of future research questions will help provide a direction forward to a better understand of the impacts of RTW laws.

Section 1: Discussion of Empirical Findings

This dissertation attempted to answer the research questions for the impact of RTW laws on employment and wages, along with other outcomes, such as unionization and establishments. In the short-run, the results appear to support the arguments of opponents of RTW laws, where individuals experienced lower wages and employment. In the long-run, though, while RTW laws are positively associated with employment, total wage, and establishment shares in manufacturing, there are likely other state policies driving these associations. From the Oklahoma analysis, there is a lack of direct evidence that RTW laws improve employment and wage discontinuities in manufacturing along state borders.

This section will be divided into three subsections that discuss the empirical results. The first subsection discusses the findings from individuals impacted shortly after Midwestern implementation of RTW laws. The second subsection will discuss the results of Oklahoma before and after its RTW law on county labor outcomes. The third section will discuss the limitations of the results.

Subsection 1: Short-Run

There is some evidence for opponents of RTW laws in the short-run. It was found that RTW laws were associated with lower likelihood of unionization and lower wages in the Midwestern states analyzed. However, the opponent's argument of "Right-to-Fire" was only seen in Indiana, such that individuals were more likely to become unemployed and less likely to become employed. Under a more inclusive specification of all workers

in the states, Michigan's RTW law was associated with lower likelihood of becoming employed and higher likelihood of becoming unemployed.

Overall, the short-run results were consistent with the findings in the conceptual model and the literature. The results also helped corroborate concepts that might have had alternative plausible outcomes. For unionization, the free-rider and bargaining power hypotheses seem plausible after controlling for individual tastes and preferences. In addition, the threat effect seemed more plausible than the spillover effect for nonunion wages. The impacts of RTW laws on employment and unemployment still appear vague without more statistical power. Each outcome measure will be discussed below.

Unionization

The first research question focused on the probability that an individual will be a union member after the adoption of a RTW law. Consistently across Indiana, Michigan, and Wisconsin, the likelihood of unionization was lower after each state's RTW law was implemented. These results were consistent with the free-rider hypothesis and bargaining power hypothesis. In addition, the methodology utilizes individual fixed effects to control for the taste hypothesis. Along with conclusions in Moore (1998), this provides additional evidence that RTW laws have actual effects on unionization instead of just being symbolic laws.

The impact of RTW laws on unionization likelihoods was fairly robust across the three Midwestern states. The coefficient impacts were the following: -1.54 percentage points in Indiana; -1.35 percentage points in Michigan; and -1.49 percentage points in Wisconsin. It is important to note that these impacts are quite notable considering that

the mean unionization likelihoods for these states were 12.5 percent for Indiana, 18.7 percent for Michigan, and 11.8 percent for Wisconsin. Furthermore, the direction of these results were consistent with the findings from Ellwood and Fine (1987), Hundley (1988), Farber (1984), Garofalo and Malhotra (1992), Eren and Ozbeklik (2016), and Zax and Ichniowski (1990), which all found that RTW laws are associated with lower union membership and organizing.

Lower unionization may have implications for other labor outcomes related to worker voice. This may lead to a collective good problem where workplace conditions and preferences are not democratically aligned to the median worker (Freeman & Medoff, 1984). In addition, reductions in unionization may lead to reductions in worker voice participation in the workplace, since the loss of collective voice may increase worker retaliation from whistleblowing (Addison & Belfield, 2004). Furthermore, loss of worker voice leaves workers with only an exit voice (Addison & Belfield, 2004). Other factors may include the following: not aligning benefits and preferences to the median worker; reductions in firm specific training; or changes in workplace safety (Freeman & Medoff, 1984). These additional outcomes will be further discussed in the future research questions section.

Another factor was the delayed impact of Michigan's RTW law. Around the time of implementation, there was no statistically significant impact on unionization. However, when the analysis refocuses to the end of state worker bargaining agreement several months after the official implementation of Michigan's RTW law, the RTW law reduces unionization for individuals in Michigan. There are several different thoughts for why this happened. One possible idea for this is that collective bargaining strength in

Michigan for all unions was higher relative to union in Indiana and Wisconsin. These unions may have renegotiated new collective bargaining agreements before the implementation of Michigan's RTW law. Another possible idea is weakened collective bargaining rights for state and local workers in Indiana and Wisconsin preceding their RTW laws may have weakened demand for unionization initially. Their RTW law may have been a catalyst to push more workers to give up their membership.

Employment

The second research question wanted to assess the impact of RTW laws on employment. The impact of RTW laws on employment was consistently negative, but not always statistically significant, relative to wages or unionization. The likelihood of employment decreased only for individuals in Indiana. These individuals were 2.25 percentage points less likely to be employed after Indiana's RTW law. However, under a specification including all workers in the state, individuals in Michigan were less likely to be employed after the RTW law. Individuals in Michigan were 1.11 percentage points less likely to be employed after Michigan's RTW law. While there was no short-run evidence that RTW laws improve employment likelihood, these laws may not worsen employment likelihoods as much as opponents may claim.

It is not too surprising that employment outcomes were consistent but not always statistically significant, since there were several channels for RTW laws to indirectly affect employment. For union workers, the union-monopolist model expects union employment to increase as bargaining power decreases and union wages approach equilibrium, while the efficient contracts model can show decreases in union employment

(Ehrenberg & Smith, 2015). For nonunion workers, employment is expected to increase with weaker ability to threaten organizing, but there may be less spillover from the union market and lower nonunion employment (Ehrenberg & Smith, 2015). With the lack of power to distinguish between nonunion and union, it is hard to tell which channels are impacting employment from lower ability to bargain for higher wages.

Employment outcomes having negative, but not always statistically significant, are in contrast when compared with the literature. From the literature review, several studies found that RTW laws were associated with increases in manufacturing employment, but Li (2012) found that union employment was worsen with the presence of a RTW law. These studies differed from the current study, which focused on all NLRA and state and local workers. Furthermore, while the methodology controlled for changes in industry, the methodology lacked the power to focus on manufacturing and other subgroup analyses by industry.

Unemployment

The third research question looks at the impact of RTW law on the probability of an individual being unemployed after the law. Similar to employment outcomes, the impact of RTW laws on the probability of being unemployed was consistently positive, but not always statistically significant. The likelihood of being unemployed increased only in Indiana after it implemented its RTW law. Individuals in Indiana were 1.53 percentage points more likelihood to be unemployed after its RTW law. This is a notable impact, since the mean likelihood for being unemployed before the policy in Indiana was 4.3 percent. Under the specification that includes all workers in the state, not just NLRA

and state and local workers, individuals in Michigan were 1.29 percentage points more likely to be unemployed after Michigan's law was implemented and state worker bargaining agreement ended.

Unemployment can impact workers in different ways. For union workers under the efficient contracts model, if the bargaining power is reduced, then firms may fire workers to bring wages and employment closer to the labor demand curve (Ehrenberg & Smith, 2015). For nonunion workers, bargaining power reduction is only expected to increase employment, but it may reduce spillover from the union labor market (Ehrenberg & Smith, 2015). However, reduction in bargaining power of union may increase employee turnover (Freeman & Medoff, 1984). Without statistical power, it is hard to assess the impact of RTW laws through these channels.

The impact on unemployment in the literature was consistent, but not always statistically significant. While Li (2012) did find that union workers were more likely to decrease employment, Stevans (2009) and Carrol (1983) found that RTW laws were statistically insignificant with state unemployment rates. These results show that looking at individuals instead of state unemployment rates can provide important insight into the impact of RTW laws on unemployment.

Wages

The final short-run research question considers the association between RTW laws and wages. Similar to the impact of RTW laws on unionization, the coefficients of the difference-in-difference terms were fairly robust across Indiana, Michigan, and Wisconsin. Individuals in these states experience lower wages on average after their

RTW laws. Individuals in Indiana experienced a decrease in wages of about 2.01 percent, while individuals in Wisconsin experienced a decrease of about 4.13 percent. Individuals in Michigan, after state bargaining agreements ended in December of 2013, experienced lower wages by 2.64 percent on average.

These results are consistent with several channels that RTW laws can reduce bargaining power of unions. For union workers, both the union-monopolist model and efficient contracts model expect that union workers will have lower wages after a reduction in bargaining power (Ehrenberg & Smith, 2015). RTW laws reduce bargaining power and the lack of bargaining power is expected to prevent unions from bargaining for higher than equilibrium wages. For nonunion workers, there were two opposing channels for the impact of reduced unionization on nonunion wages. First, the threat effect implies that a lack of threat of organizing means that firms do not have to compensate above wage equilibrium to prevent workers from organizing (Ehrenberg & Smith, 2015). Second, the spillover effect implied that reductions in bargaining power meant that union workers would stop spilling over into the nonunion labor market and give upward pressure on wages (Ehrenberg & Smith, 2015). From the results, it seems that the threat effect might be more likely. However, given the lack of statistical power, it is unclear whose wages were more impacted by the implementation of these RTW laws.

These results are also consistent with several findings in the literature. Several studies found that RTW laws were negatively associated with wages. From Table 3-3, for wages levels, of the ten studies included, six of the studies found that RTW laws were associated with lower wages. Carrol (1983), Farber (1984), Farber (2005), Garofalo and Malhotra (1992), Li (2012), and Stevans (2009) found that RTW laws were negatively

associated with wages. Farber (2005) finds that the threat effect occurs in Idaho using a difference-in-difference method. Li (2012) find that union wages were adversely affected in RTW states after a merger using a triple difference method.

Subsection 2: Long-Run

While no evidence for the arguments of proponents of RTW laws was found in the short-run, there was very limited evidence in favor of their arguments in the long-run for manufacturing employment. Unlike the short-run analysis, which focused on individuals, the long-run analysis focused on counties and manufacturing within these counties. The industry of focus was manufacturing, since it historically has had higher rates of unionization than other private sector industries and is more likely to be affected by RTW laws.

The long-run analysis found that RTW laws are associated with higher employment, total wage, and establishment shares in manufacturing. Holmes (1998) found that there were employment share discontinuities in manufacturing between RTW states and union shop states along state borders. This analysis applied a similar methodology, but it also included a change in policy around Oklahoma's state borders before and after its RTW law. Before the Oklahoma RTW law, the impacts of RTW laws were positive and statistically significant on employment and total wage share discontinuities in manufacturing along Oklahoma state borders. However, nine years after Oklahoma's RTW law, employment and total wage discontinuities remained, even though Oklahoma had a RTW law.

To test for secular trends among all RTW-union shop state borders, the long-run analysis included an all-state analysis of the impacts of RTW laws in 2000 and 2010. The all-state analysis found that the impacts of RTW laws on employment, total wage, and establishment share discontinuities were statistically significant and positive in 2000. Similar to the analysis of RTW laws around Oklahoma's state borders, these impacts remained statistically significant and positive in 2010. However, the impacts around all state borders declined more than the impacts around Oklahoma state borders even though Oklahoma adopted a RTW law.

If Oklahoma's RTW law helped improve employment and wages, then it was expected that employment and total wage shares would converge with its neighboring RTW states. This was not observed and reduces evidence that RTW laws directly improve employment and total wage shares in manufacturing in the long-run. Eren and Ozbeklik (2016) find that Oklahoma's RTW law does reduce overall unionization and manufacturing unionization. Given possible other state policies and factors, along with Oklahoma's low rate of unionization in manufacturing, Oklahoma's RTW law might not been strong enough to converge employment and wage share discontinuities given. Farber (2005) concludes that the marginal effect of RTW laws might be small if unionization is low initially and this research corroborates with this conclusion.

Employment

The first long-run research question focused on the impact of RTW laws in more union-intensive industries. The strongest support for proponents' arguments came from RTW laws association with employment shares in manufacturing along RTW-union shop

state borders. From the all-state analysis, RTW laws were associated with an increase in employment share of 5.58 percentage points in 2000 and 3.95 percentage-points in 2010. However, there was a lack of evidence that RTW laws are responsible for this discontinuity. There was no evidence that Oklahoma's RTW law helped converge manufacturing employment shares along its state borders with its neighboring RTW states of Arkansas, Kansas, and Texas. In 2000, before the Oklahoma RTW law, the impact of RTW laws around Oklahoma's state borders was a 7.39 percentage-point increase in manufacturing employment share for Oklahoma's RTW neighbors of Arkansas, Kansas, and Texas. However, nine years after Oklahoma adopts its RTW law, the discontinuity remained with a slightly lower impact of 6.99 percentage points and statistically significant between Oklahoma and neighboring RTW states along Oklahoma's state borders. For total manufacturing employment, there were no impacts of RTW laws observed before or after the Oklahoma RTW law.

Convergence in manufacturing employment shares was expected to occur with its RTW neighbors after Oklahoma adopted a RTW law. As bargaining power falls, the ability to retain wage premiums and work rules falls and employment is expected to increase (Ehrenberg & Smith, 2015). The wage elasticity becomes more elastic and declines in wages can lead to larger employment gains in the long-run. However, the impacts of RTW laws along RTW-union shop borders that did not observe a change in RTW laws fell more than the ones that did.

There were possible several reasons for a lack of convergence. First, unionization in manufacturing in Oklahoma was relatively low when Oklahoma adopted a RTW law. If RTW laws affect employment and wages by lowering unionization, then Oklahoma's

RTW law might have been ineffective on employment and wage since unionization was low when Oklahoma adopted a RTW law. Another factor is that manufacturing employment may vary by types of manufacturing industry groups. These mixes were not observed and could have impacted manufacturing employment shares. In addition, there are possibly other state policies that are discontinuous along Oklahoma's state borders that explain increased employment shares, such as taxation breaks or taxation preferences. Holmes (1998) did use RTW laws as a proxy variable for "pro-business" policies and did not directly test RTW laws separate from other pro-business laws.

These long-run outcomes are similar to the findings in the literature. Holmes (1998), Kuncce (2006), and Kalenkoski and Lacombe (2006) find that RTW laws are positively associated with employment or employment shares in manufacturing. Holmes (1998) and Kalenkoski and Lacombe (2006) find that employment shares are higher at state borders in RTW states compared to union shop states by 6.6 percentage points and 2.12 percentage points, respectively. This analysis corroborates their findings for employment shares in manufacturing by looking at RTW-union shop state borders outside of Oklahoma's state borders.

By focusing on Oklahoma's policy change, not much evidence was observed that RTW laws improve employment share and may be other state policies are more important to explain state border discontinuities. Eren and Ozbeklik (2016) find a lack of impacts of Oklahoma RTW law on manufacturing employment and employment-population ratios utilizing synthetic controls methods. While Eren and Ozbeklik (2016) find that Oklahoma's RTW law lowers unionization and unionization in manufacturing, the impact on already low unionization might not have spilled over into employment.

Wages

The second long-run research question wanted to assess the impact of RTW laws on wages in more union-intensive industries. There was mixed evidence that wages are impacted by RTW laws in the long-run for counties. Two different wage concepts were tested in the analysis: total wage shares in manufacturing and average weekly wages per worker in manufacturing. Total wages shares are heavily influenced by total employment, while average weekly wages per worker nets out the influence of employment.

The impact of RTW laws on total wage shares is fairly similar to the results seen for employment shares. For RTW-union shop state borders outside of Oklahoma's state borders, RTW laws were associated with a 7.12 percentage-point increase in total wage shares in manufacturing in 2000 and a 5.64 percentage-point increase in 2010. For Oklahoma, the impact of neighboring RTW laws in 2000 was 9.93 in 2000. In 2010, the discontinuity along Oklahoma's state borders remained. Its impact on total wage share in manufacturing was an increase of 8.93 percentage points. The impact of RTW laws on total wage shares fell more along RTW-union shop state borders with no policy change than Oklahoma's state borders with a policy change.

For average weekly wages, there was mixed effects of RTW laws. From the all-state analysis, RTW laws were associated with a 6.2 percent increase in average weekly wages in 2000 and this impact grows to 12.4 percent in 2010. The impact of RTW laws in 2000 appeared to be sensitive to the inclusion of covariates while the impact in 2010 appeared to be more robust. For Oklahoma state borders, the impact of RTW laws was

statistically significant and associated with an increase of 23.1 percent in 2000 and an increase of 14.1 percent in 2010. The 2010 impact seemed to be sensitive to the inclusion of covariates. If the validity of the RD design is true, then this is an intriguing outcome. Oklahoma appears to be slowly be converging with RTW neighbors after its RTW policy, while other RTW states appear to be diverging with neighboring union shop states. It was expected that average weekly wages in manufacturing would be higher in union shop counties, since unions would try to bargain for higher wages.

It was expected that average weekly wages in Oklahoma before its RTW law would be higher due to higher wages premiums. However, Reed (2003) focused on wages and controlling for initial conditions and found that RTW states experienced convergence with wages. Furthermore, Hanley (2010) found that RTW laws were associated with reduced income inequality from wage convergence. It is possible that Oklahoma's private sector unionization in manufacturing were not strong enough to bargain for higher wages even before Oklahoma's RTW law. However, average weekly wages outside of Oklahoma's state borders appear to be diverging. It was expected that wages would converge with the other states in the long-run. Since wage elasticity of demand will become more elastic in the long-run, especially for a competitive industry such as manufacturing (Farber & Western, 2001). It is possible that unions do not have strong enough strength to bargain for higher wages in the face of a more elastic and competitive manufacturing sector or unions have to push for higher wage premiums to entice membership.

Establishments

The final long-run research question looks at the impact of RTW laws on capital in more union-intensive industries. To see how capital reacts to RTW laws, measures of manufacturing establishments were utilized to answer the research question. These measures of manufacturing establishments included the share manufacturing establishments of the total establishments and the total number of establishments. From the all-state analysis, RTW laws were associated with higher shares of establishments in manufacturing along RTW-union shop state borders by an increase of 0.83 percentage points in 2000 and an increase of 0.7 percentage points in 2010. However, there was not much evidence that manufacturing establishments were different at state borders between Oklahoma and comparison states before and after Oklahoma's RTW law.

For manufacturing establishments, RTW laws were seen to be associated with shares of manufacturing establishments, but there was a lack of evidence that RTW laws directly impact shares of establishments. Given the argument by Holmes (1998) that establishments within a certain distance of a RTW border would move out of a union shop state in an RTW state, it was expected that establishments would be higher in RTW states along state borders. While this was observed, there was no difference before and after Oklahoma changed from a union shop state to a RTW state.

This analysis was slightly different from many of the outcomes of interest in the literature. Most of the outcomes focused on wages, wage growth, employment, or employment growth. However, movement of capital is important, since union rules about capital and labor may prevent substituting union labor with capital or nonunion labor (Kaufman, 2004). Additional research on the evaluation the impact of RTW law on

different measures of capital, such as foreign direct investment, would provide more insight.

Subsection 3: Limitations

This subsection will discuss several important limitations to the research. While this research attempts to look at the impact of RTW laws in the short-run and long-run, there are several notable limitations that need to be discussed. There are internal validity and external validity concerns for the short-run and long-run analyses, which may reduce the validity of the research.

There are several notable external validity concerns between and within the short-run and long-run analyses. While the two methods are meant to corroborate one another, the results are not directly comparable. The short-run and long-run analysis focus on two different regions, surveys, and units and populations of interest, such that various factors that may differ in observed and unobserved ways. The regions are quite different from one another politically, economically, and socially. What happens in the short-run in Indiana, Michigan, and Wisconsin might not be generalizable to Oklahoma. Conversely, what happens to Oklahoma in the long-run might not be generalizable to Indiana, Michigan, and Wisconsin in the long-run. Furthermore, the measure of employment and wages differs between the two analyses. The short-run analysis is an analysis of individuals from a household survey, while the long-run analysis focuses on establishment data from unemployment insurance. The short-run population of interest was all NLRA and state and local workers, while the population of interest in the long-run analysis was all employment covered by unemployment insurance in manufacturing.

In addition, individuals were the focus in the Midwestern short-run analysis, while counties were the unit of analysis in Oklahoma and surrounding Great Plains states.

For the short-run analysis, it is important to note that this analysis focuses on Midwestern states. If the internal validity of the short-run analysis holds, it can assess the impact of the RTW laws on individuals in Indiana, Michigan, and Wisconsin. It does not provide an assessment of the impact of Georgia's RTW law, Utah's RTW law, or any other state that previously adopted a RTW law. These states implemented their RTW laws during different time periods with different economic conditions. Overall, it is important to note that most of the research here focuses on the states of interest as opposed to a national overall impact of RTW laws.

For the long-run analysis, these results are not generalizable beyond its state borders to the rest of Oklahoma. If internal validity holds for the discontinuity, they are only valid at state borders. The regression discontinuity design is able to focus the impact of a policy around the cutoff point, or state borders, and what is valid at state borders may not be valid at other points within the treatment and comparison states (Jacob, et al., 2012). The mean impact of Oklahoma's RTW law is only identified locally at state borders (Jacob, et al., 2012). Therefore, the long-run impacts in 2000 and 2010 for the entire state of Oklahoma are not identified.

There are several notable factors that need to be discussed, which may affect the internal validity of the research. While this research attempts to minimize unobserved heterogeneity and selection bias, these issues can still plague quasi-experimental research designs.

For the short-run analysis, a difference-in-differences methodology with panel data was chosen to observed outcomes from individuals in Indiana, Michigan, Wisconsin, and surrounding states. While the difference-in-differences methodology with individual fixed effects can control for many unobservable factors, there are still selection bias and unobserved heterogeneous factors that could have biased the results. The difference-in-differences methodology is violated when factors that vary over time between treatment and control are not controlled (DiNardo & Lee, 2010). Furthermore, if individuals in treatment and comparison states experience different pre-post trends, then the causal inference from difference-in-difference methodology is reduced (Murnane & Willett, 2011). To test this, state unemployment rates were included in a robustness check. The results were fairly robust, but some of the standard errors of the difference-in-difference estimators increased.

The evidence of the long-run analysis of Oklahoma's RTW law is limited due to a couple of important points. When the assumptions of regression discontinuity design holds, its validity can be as strong as experimental design (Lee & Lemieux, 2009). First, the impact estimates in the RD design should be robust when covariates are added to the model (Lee & Lemieux, 2009). This was not observed when covariates were added to the model for several of the outcomes of interest including average weekly wages. These impact estimates were sensitive to the inclusion of covariates. Second, a threat to the internal validity of the regression discontinuity design occurs when the treatment variable is not the only discontinuous variable at the cutoff point (Jacob, et al., 2012). For Oklahoma, county data on non-Hispanic whites and non-Hispanic other races were discontinuous at the state border. While an analysis included these covariates, it reduces

the validity that only the state policies vary between Oklahoma and comparison states in 2000. Third, the regression discontinuity design is not a difference-in-difference in discontinuities over time. While the all-state analysis is meant to corroborate with the analysis of RTW laws around Oklahoma state borders, it is not possible to attribute the discontinuity to RTW laws alone with the current research. In addition, it is not possible to attribute the discontinuities along Oklahoma's state borders solely to its union shop and subsequent RTW law, since other state policies impacting manufacturing employment may vary along Oklahoma's state borders. Holmes (1998) uses RTW laws as a proxy for states with "pro-business" laws and he is unable to directly state the effect of RTW laws as opposed to "pro-business" laws. Finally, Kalenkoski and Lacombe (2006) account for spatial errors, which reduced the discontinuity at state borders. The effect of these spatial errors could reduce the discontinuities observed at Oklahoma's state borders before and after the RTW law.

Section 2: Policy Recommendation and Future Research Questions

This section will discuss policy recommendations based upon the work done in this dissertation. The first subsection will discuss the policy recommendation based upon the empirical findings and supporting literature. The second subsection will contemplate future research questions for further research into the impact of RTW laws.

Subsection 1: Policy Recommendation

Based upon the empirical findings, along with the supporting literature, state policymakers should be hesitant to adopt and implement a RTW law as a panacea for

labor markets in their states. However, if policymakers are more concerned about ways to improve manufacturing shares of employment, then there is some very limited support for this at the potential expense of lower likelihood of employment and wages for their constituents in other sectors. Furthermore, the outcomes of interested focused on workers rather than firms. It is important to consider that firms may benefit from lower unionization and lower wages, especially in labor-intensive industries that have elastic demand for their products.

The empirical results from the short-run findings show no support for the arguments of proponents of RTW laws that these laws improve labor outcomes in the short-run. In fact, the arguments of opponents had more support than the proponents. These results were seen across three Midwestern states that adopted RTW law. While supporting literature found results that RTW laws improve employment in manufacturing, the results here found that NLRA and state and local workers had lower wages, employment, and unionization in the short-run after the implementation of RTW laws.

Another reason policymakers should be hesitant to adopt RTW laws is due to other potential factors that can affect individuals related to the voice mechanisms of unionization. While many policymakers may or may not welcome reductions in the monopoly-face of unionization, there may be unintended consequences for public good characteristics in the workplace from reductions in the voice face of unions. With weakened collective voice in the workplace, the exit voice may become the only means for workers to voice their dissatisfaction (Addison & Belfield, 2004). In addition, there is potential for reductions in firm-specific training, which may impact productivity

(Freeman & Medoff, 1984). Another issue is that workplace injuries, illnesses, and retaliations may increase due to a lack of collective voice (Kaufman, 2005).

Furthermore, erosion of collective voice creates a market failure, since it reduces the democratic participation in the workplace and results in less than socially optimal level of employee voice (Kaufman, 2005).

The policy recommendation is not a complete rejection of RTW laws, since the long-run findings did show support for employment in manufacturing. The all-state analysis did find that RTW laws are associated with higher manufacturing employment, total wage, and establishment shares. This finding is similar to other studies in the literature review, which finds that manufacturing employment is affected by the presence of a RTW law. However, the Oklahoma analysis shows that states adopting these laws may not eliminate or reduced employment or total wage share discontinuities along state borders in manufacturing and there may be other state policies that are more effective. There was also some mixed evidence that RTW laws were associated with higher average weekly wages along the border, but these results were diminished due to sensitivity to the inclusion of covariates.

Subsection 2: Future Research Questions.

While policy makers should be hesitant to adopt RTW laws as panacea for state labor markets or the national labor market, it is clear that additional research on RTW laws is needed. While individuals in the Midwest were adversely affected by the implementation of their respective state RTW laws in the short-run, there were mixed results from the long-run analysis. The all-state analysis shows that RTW laws are

positively associated with employment, total wage, and establishment shares in manufacturing at state borders, but there was a lack of evidence in Oklahoma that adopting a RTW law improves employment and total wage shares in manufacturing.

The short-run and long-run analyses were different regions with different units of analysis from different surveys. While they meant to corroborate one another, these different analyses were not to be generalized between one another. It would be useful to see what happened to Oklahoma in the short-run, especially since unionization in manufacturing was low before it adopted a RTW law. Did Oklahoma experience any short-run impacts from its RTW law? Given that Oklahoma is surrounded by RTW states, its potential comparison states are also treatment states, which may be problematic for a sufficient research design. What about the impacts of RTW laws in Indiana, Michigan, and Wisconsin in the long-run? Do border discontinuities emerge with their neighboring states are new RTW-union shop borders? Additional time is necessary for a long-run analysis of the impact of these Midwestern RTW laws. Also, the long-run analysis focused on counties instead of individuals. What is the impact of RTW laws on individuals over a long-period of time?

There are other unionization questions that appear from the results of these analyses. There was support for the free-rider hypothesis and bargaining power hypothesis. However, it was difficult to tell which mechanism was more prominent than the other. Furthermore, did free-riding in bargaining units increase after Indiana, Michigan, and Wisconsin adopt their RTW laws? While unionization, wages, and employment did fall for individuals, did reductions in unionization directly lead to lower wages and employment or were these declines correlational? There is also a question

about the theoretical impacts of RTW laws on unionized labor markets and non-unionized labor markets. Do RTW laws have a greater impact on wages and employment in union labor markets or non-union labor markets? Subgroup analyses would require more statistical power than the current research.

There are also additional RTW law research questions related to public and private unions. Given that the impact of Michigan's RTW law was almost a year delayed while the impacts in Indiana and Wisconsin were swift, do weak public sector collective bargaining rights portend quicker impacts of RTW laws? There are also questions about which union workers are affected more. If a state passes a public and private RTW law, are private or public sector union membership more adversely affected? Some states had no public sector union security agreement, but allowed for private sector union security agreement under the NLRA. If this state adopts a private sector RTW law, does it prompt additional public sector workers to opt out of their union membership?

From the policy recommendation section, there are concerns that RTW laws may affect other outcomes not studied in this research. With regard to the loss of the collective voice, are workers more likely to experience more labor market churn? Are workers more likely to have shorter tenure? If collective voice is lost in the workplace, then safety may become a greater issue. Are injuries, illness, and workplace accidents more likely to occur with the presence of a RTW law? Are injuries, illnesses, and workplace accidents less likely to be reported with the presence of a RTW law? If the loss of collective voice is associated with a loss in firm-specific training, do RTW laws reduce firm-level productivity? Do RTW laws increase job churn, since the exit voice is their only way to vote in the labor market?

Given that there is some support that RTW laws are positively associated with manufacturing employment, there is a question about what kind of manufacturing is impacted by RTW laws. Given that all states experienced declining shares of employment in manufacturing between 2000 and 2010, did RTW states or union shop states experienced bigger declines in manufacturing employment during this time period? Are there particular subsectors of manufacturing that are more sensitive to the presence of a RTW law? Are low-tech, labor-intensive manufacturing industries more interested in the presence of a RTW given a more elastic demand for wages from these firms? Are firms in high-tech sectors less concerned about the presence of a RTW law and are unions able to capture higher wages from more inelastic demand in these sectors?

In the short-run and long-run, there was a lack of direct support for proponents of RTW laws. However, these research questions focused on individual workers, along with manufacturing wages and employment at the county level, instead of firms and establishments. It is possible that firms may lobby for RTW laws, since it might improve their profitability due to reduced labor costs. Do RTW laws increase the profitability of firms? Do RTW laws reduce labor costs and associated costs for production of goods and services?

There are also other questions about the interaction of RTW laws and the movement of capital. The all-state analysis found that RTW laws are positively associated with manufacturing establishment shares. As global expansion of manufacturing has occurred over the past several decades, manufacturing firms may use RTW laws as a proxy for states and places for doing business. Do states with RTW laws

experience more foreign direct investment? More specifically, do states with RTW laws have more foreign direct investment in manufacturing establishments and capita?

The research done in this dissertation has seen that RTW laws are not just symbolic and they have a real effect on individuals. RTW laws reduce unionization and union organizing. From the literature review and the research, there is support that RTW laws reduce wages, but were associated with higher manufacturing employment. However, there are still many more questions to be answered, so that policymakers and researchers can have a better idea of the true impacts of RTW laws on individuals and states.

Appendix: Systematic Review Process

Section 1: Search Strategy

A search strategy was employed to provide a more thorough and exhaustive search of the literature of RTW laws on wages and employment during the Spring Semester of 2014. The search strategy included electronic databases searches and citation search. This subsection will discuss the databases that were searched, the keywords used for the databases, and the filters that were used. The results of the search strategy will be mentioned as well.

Subsection 1: Databases Electronically Searched

There were two major databases that were used in the search of the literature. The first one is a meta-database search provided from the Albin O Kuhn Library from the University of Maryland, Baltimore County. This database searches through several different databases from EBSCO. The second major database that was searched was JSTOR, which was provided from the Department of Labor.

The databases searched from Albin O Kuhn Library, or AOK One Click, included many different databases across different disciplines from EBSCO. A search of the different databases across disciplines was utilized so relevant literature was not excluded. In addition, AOK One Click provided references to databases outside of EBSCO databases including Science Direct. This helped reduce biases in the search strategy.

A citation search through some of the relevant articles found in the AOK One Click search prompted another database search. The second major database search was

JSTOR, which provides access to articles that may not have been assessable through AOK One Click. While this electronic search provided additional references, this search provided fewer references than the AOK One Click.

Subsection 2: Keywords

The electronic search through AOK One Click search and JSTOR search required keywords to find relevant references. The keywords for each were slightly different, but they were searched in the same manner. The keywords included criteria for an intervention and outcomes, but additional keywords were tested as well.

The first criterion keywords were for the intervention. For the AOK One Click search, the initial keyword for the intervention criterion was “RTW”. This keyword criterion was modified, since many citations included “Right-to-Work” as a human right instead of the RTW law. Also, AOK One Click provided information on the intervention keywords. Detailed records showed that “Right-to-Work Law” might be filed as “Closed & Open Shop”. Therefore, the final intervention criterion was used as “Right-to-Work Law” OR “Closed & Open Shop” for the AOK One Click search. The intervention keyword for the JSTOR search only included “Right-to-Work law”. This provided a sufficient number of citations in the JSTOR databases.

The second criterion keywords were for the outcomes. These outcome keywords were set to find eligible studies that were relevant to labor outcomes for this dissertation. The keywords were the same for this criterion in the AOK One Click search and the JSTOR search. These keywords included wages, income or employment and the second criterion was searched as the following: “Wages” OR “Income” OR “Employment”.

A third criterion keyword was used for the AOK One Click search, but not the JSTOR search. Using only the intervention and outcome criteria from AOK One Click, resulted in more than four thousand citations. Many of the citations included law case studies, which were not relevant to this systematic review. Therefore, another criterion was included to reduce the number of law case studies citations. For the AOK One Click search, the keyword of “Analysis” was used as the third criterion. The JSTOR search did not provide an overabundance of law case studies, so the third criterion keyword was not included.

Subsection 3: Filters

To find eligible studies that will be discussed in the Inclusion and Exclusion Criteria subsection, initial filters were included in the AOK One Click search and the JSTOR search. First, for both electronic searches, the searches were restricted to include studies from 1947 to present. These years were chosen, since 1947 was the year of the Taft-Hartley Act that provided states the option to adopt RTW laws. The second filter was for English language. While it is possible that there are eligible studies that analyze RTW laws in the United States, which are written in a non-English language, the probability appears to be very low. Also, the author’s lack of knowledge of languages besides English also used as a reason for this filter.

Additional filters were included with the AOK One Click search that was not used in the JSTOR search. The AOK One Click search included a filter for academic journals. AOK One Click search includes studies from the grey literature, but focus of the search strategy was on published citations. It is possible that excluding citations from the grey

literature may bias the results of this systematic review. However, it is hoped that excluding the grey literature will reduce studies of lower quality. Another filter used in the AOK One Click was for peer-reviewed articles. This filter is similar to the academic journal filter, since it was used to exclude citations of lower quality.

Subsection 4: Results

The results of the electronic database searches provided many results to find eligible citations for the impact of RTW laws on wages and employment. The AOK One Click search provided 1,840 relevant citations, while the JSTOR search provided 425 citations. The next subsection will discuss the inclusion and exclusion criteria that were used to find eligible studies for the systematic review.

Section 2: Inclusion and Exclusion Criteria

Inclusion and exclusion criteria were applied to the electronic database searches to help find eligible studies. First, the inclusion and exclusion criteria are discussed. Next, results of the inclusions and exclusion criteria are analyzed. The inclusion and exclusion criteria were applied in two different rounds. The first round focused on a review of the titles and abstracts to find eligible studies. The second round focused on a more in-depth review of the studies that were included after the title and abstract review.

Subsection 1: Inclusion and Exclusion Criteria

The inclusion and exclusion criteria included five major criteria to determine the eligibility of the citations. These criteria included the following: the intervention of the study, the outcome or outcomes of the study, the research design of the study, and the data and target population used in the study. Other minor inclusion and exclusion criteria will be discussed as well.

The first major criterion focuses on the intervention studies in the citation. Articles that focused on the impact of RTW laws were considered eligible for inclusion into the systematic review. However, studies that were not empirical analysis RTW laws or other kinds of studies not related to RTW laws were excluded from the study. These types of studies were law case studies of RTW laws, theoretical analyses of RTW laws, other types of background related articles on RTW laws, and studies with an intervention other than RTW laws. A study needed to assess the impact of RTW laws to be eligible for inclusion and were excluded if they failed this criterion.

The second major criterion focused on the outcomes analyzed in the studies. To be eligible for inclusion in the systematic review, a study need to assess the impact of RTW laws on wages or employment, which are the main labor outcomes of interest for this systematic review. Wage outcomes that were considered for inclusion were the following: levels of wages, growth of wages, or a related wage concept, such as income inequality. Employment outcomes that were considered for inclusion were the following: levels of employment, employment shares, growth in employment, or unemployment rates. Other types of outcomes were excluded from the study. Many RTW law studies focused on the impact of RTW laws on unionization, the number of strikes, stock prices,

and adoption of such laws. These outcomes were excluded, since the impact on unionization and free-riding has been discussed in the prior section and the other outcomes are out-of-scope.

The third major criterion was the type of research design that was used to assess the impact of RTW laws on wages and employment. The research design is a critical component for assessing the internal validity and studies with poor research design were to be excluded from the systematic review. These studies are excluded, since poor research design typically have biased results (Kennedy, 2008). Biases results in the studies will bias the outcomes of this systematic review. To be eligible for inclusion in the systematic review the research designs of the studies needed to address the following: time-invariant unobserved heterogeneity, endogeneity, or both. Studies with a research design or identification strategy that attempted to control for at least one of these factors will be included. These types of research designs include the following: Random Controlled Trials research design, Instrumental Variable research design, Difference-in-Difference research design, Fixed Effects research design, Reduced Form research design, or Regression Discontinuity research design.

There are several types of research designs that will be excluded from this systematic review. First, qualitative studies will be excluded. Even though qualitative studies are important to understand the context of an issue or problem, these studies do not provide internal validity (Trochim & Donnelly, 2008). Second, studies with a simple Ordinary Least Squares (OLS) analysis of RTW laws will be excluded. These research designs are excluded because the coefficients of interest will be biased, since there will be time-invariant and time-varying unobserved heterogeneity and endogeneity. For

example, there will be unobserved heterogeneity, since many unmeasured time-invariant characteristics, such as state preferences or secular trends, will not be controlled. Also, there may be endogeneity, since cross-sectional studies, especially studies utilizing aggregate data, may not distinguish between tastes and RTW laws.

The fourth type of inclusion-exclusion criterion is the data. Studies that focus on the impact of RTW laws need to look at wage and employment data from the United States. Eligible studies include data that focuses on individuals, counties, metropolitan areas, or states within the United States. Studies that focus on open and closed shops outside of the United States, mainly Canada and the United Kingdom, will be excluded. Since the labor outcomes focus on RTW laws enacted from the Taft-Hartley Act of 1947, the impact of open and close shops outside of the United States will be considered out-of-scope and therefore excluded.

Other inclusion and exclusion criteria are filters used in the search strategy. Filters were used to exclude grey literature, non-English literature, and literature from 1947 to present. The grey literature was excluded, since it is perceived that the grey literature may not have the same quality as the literature published in academic journals. One of the issues related to this is peer-reviewed academic journals. These exclusion filters hope to exclude literature that may be of lower quality and more likely to have biased results. This exclusion criterion may bias the results, since excluding eligible grey literature may bias the systematic review outcome. Literature that is not published in English was excluded, since the author has a lack of knowledge of languages outside of English. This exclusion criterion may bias the results, since eligible literature may be excluded. However, there were very few non-English search results found and the

likelihood of bias from this exclusion criterion seems minimal. The last minor exclusion criterion looks to exclude studies before 1947, which was the year of the enactment of the Taft-Hartley Act. The likelihood of bias from this exclusion criterion seems minimal, since most empirical analyses of RTW laws began in the early 1980's.

Subsection 2: Results of Abstract and Title Review

The first round of inclusion and exclusion was a review of the titles and abstracts found from the search strategy. The initial exclusion criteria used the filters and a brief review of the titles and abstracts to bring the number of citations for the systematic review to a more manageable level. The intervention and outcome criteria were the two major criteria applied in the title and abstract review. If the title or abstract contained any information about the intervention and the desired outcomes, it was moved to the second round for more in-depth review.

For the AOK One Click search, the number of potential citations after the academic journal, peer-reviewed, 1947 to present, and English filters was 1,840. The abstracts and citations of the 1,840 results were reviewed to see if they contained information about the intervention, RTW laws, and the desired labor outcomes. Out of the 1,840 citations, 94 citations were deemed to be relevant to the intervention and outcomes. However, 3 of these citations were duplicates and were removed. Therefore, a total of 91 citations were exported into Endnote for further review.

For the JSTOR search, the number of potential studies for inclusion after using the academic journals and 1947 to present filters was 425. The titles and abstracts for each of the 425 were reviewed to assess the eligibility of the studies. 12 out of 425

citation abstracts or titles contained information on both the intervention and outcomes. However, 3 of these studies were duplicates with the AOK One Click search and were removed. A total of 9 citations were exported from JSTOR for a more in-depth review.

Subsection 3: Results of In-Depth Review

The second round of inclusion and exclusion was a more in-depth review of the citations from the title and abstract review. Each of the studies exported were reviewed in a more thorough criteria process. The title and abstract review looked to see if the intervention and outcomes were present, but the in-depth review applied all of the criteria. This included reviewing the methods and data sections to see if the intervention, outcomes, research designs, data and other criteria were satisfied. From the two searches, 10 studies from the AOK One Click search were kept for the systematic review, while 6 studies from the JSTOR search for the systematic review. (See Table A1 and Table A2)

From the AOK One Click search, the 91 citations for in-depth review resulted in 10 studies being utilized for the systematic review. After reviewing the data and methods sections, 20 out of 91 studies did not have the correct intervention. These studies were excluded, since these studies looked at right-to-work as a human right or open and closed shops outside of the RTW law. Next, 40 out of 91 studies were excluded, since these papers were considered background papers. These papers included law studies, theoretical studies, and non-empirical analyses of RTW laws. 14 out of 91 studies were deemed to have the incorrect outcomes. These studies were excluded, since the outcomes were typically rates of unionization. 6 out of 91 studies were excluded, since the research design was insufficient. These studies were usually simple OLS regressions of the

impact of RTW laws on wages. 1 out of 91 studies was excluded, since it was a qualitative study of RTW laws on an individual's wage. 2 out of 91 studies were literature reviews. These literature reviews provided citations checks to see if other eligible studies were missing, but they were not included in the systematic review. The final result of the AOK One Click search was 10 studies deemed eligible to be included in the systematic review.

From the JSTOR search, the 9 citations for in-depth review resulted in 6 studies being included in the systematic review. The data and methods were reviewed for these papers to assess the study's eligibility. 2 out of 10 studies assessed the impact of RTW laws on outcomes beyond the scope of this systematic review. One study assessed the impact on stock prices, while the other assessed the impact on rates of unionization. 1 out of the 10 studies was deemed to have an insufficient research design. While the study looked the impact of RTW laws on wages, the impact was analyzed using a simple OLS model. That final result from the JSTOR search was 6 studies deemed eligible to be included in the systematic review.

Table A1: Inclusion-Exclusion Process of AOK One Click Search

AOK One Click/EBSCO Search			
Step	Process	Discarded	Result
Search Strategy	AOK One Click Search	-	1840 citations
Abstract and Title Review	Examine title and abstracts for intervention and outcomes	1746	94 citations
Duplicates	Endnote searches for duplicates	3	91 citations
Wrong Intervention	Exclude non-RTW studies	20	71 citations
Wrong Outcomes	Exclude non-wages or non-employment studies	14	57 citations
Poor Research Design	Exclude studies with insufficient research designs	6	51 citations
Poor Research Design	Exclude qualitative studies	1	50 citations
Background Papers	Exclude law case studies and non-statistical studies	38	12 citations
Literature Reviews	Exclude literature review (kept for citation searches)	2	10 citations

Table A2: Inclusion-Exclusion Process of JSTOR Search

JSTOR Search			
Step	Process	Discarded	Result
Search Strategy	JSTOR Search	-	425 citations
Abstract and Title Review	Examine title and abstracts for intervention and outcomes	413	12 citations
Duplicates	Endnote searches for duplicates	3	9 citations
Wrong Intervention	Exclude non-RTW studies	0	9 citations
Wrong Outcomes	Exclude non-wages or non-employment studies	2	7 citations
Poor Research Design	Exclude studies with insufficient research designs	1	6 citations
Poor Research Design	Exclude qualitative studies	0	6 citations
Background Papers	Exclude law case studies and non-statistical studies	0	6 citations

Section 3: Study Quality

This subsection covers the methodology of how the quality of the studies was assessed. There were two major factors of the studies that were analyzed to determine the quality, which were the study's research design and the data used to analyze the impact of RTW laws on wages or employment. The quality of the studies was ranked from 1 to 5, where a 1 was a low-quality research design and a 5 was a high-quality research design. There were no studies that were assessed as a 5 and only one study was assessed as a 4. There were seven 3s, four 2s, and four 1s. (See Table 3-1).

Subsection 1: Research Design

The research design is an essential component of an empirical study. A strong research design can control for observed and unobserved factors, which will provide strong internal validity. The gold standard of research designs is the random controlled trial (RCT) or a randomized experiment (Trochim & Donnelly, 2008). Any study that used randomized experiments to assess the impact of RTW laws would provide unbiased coefficients for the sample studied. Unfortunately, RTW laws are likely adopted nonrandomly and none of the studies is able to provide a randomized experiment. Therefore, research designs utilized to study the impact of RTW laws were quasi-experiments.

Given that all of the research design utilized in the eligible studies were quasi-experimental, it is important to assess the quality of the research design to find potential biased results. Some of the biggest empirical issues for assessing the impact of RTW laws are care of unobserved heterogeneity and endogeneity (Moore, 1998). Therefore, there were three main factors that were used to determine the quality of the research

design, which were the following: the ability to control for time-invariant unobserved heterogeneity, the ability to control for time-varying unobserved heterogeneity, and the ability to control for endogeneity. Ellwood and Fine (1987) lessened the worry about endogeneity, but without a control for endogeneity or simultaneity test, endogeneity needs to be considered.

Subsection 2: Unobserved Heterogeneity

Unobserved heterogeneity is a major problem in quasi-experimental research designs. Unobserved heterogeneity will bias the coefficients of interest, since the omitted variables will be correlated with the errors (Kennedy, 2008). There are two types of unobserved heterogeneity that will be assessed for research design quality. The first type is time-invariant unobserved heterogeneity and the second is time-varying unobserved heterogeneity. The first type is variability that does not change over time, while the second type is variability that does change over time.

Subsection 3: Endogeneity and Simultaneous Equations

Another major issue with assessing the impact of RTW laws using a quasi-experimental design is endogeneity. Endogeneity will bias the coefficients of interest, since the regressors, or explanatory variables, included in the model will be correlated with the error term (Kennedy, 2008). Endogeneity occurs due to simultaneous equations, where x causes y and y causes x . This is especially conspicuous when utilizing state-level data, where it might be harder to distinguish between preferences for anti-unionism and RTW laws (Moore, 1998). However, Ellwood and Fine (1987) showed that

endogeneity might not be as big of a problem as Newman and Moore (1985) state. Ways to deal endogeneity is with an Instrumental Variable design, which can estimate the endogenous variable through an instrument (Kennedy, 2008).

Subsection 4: Assessment of Research Designs

There are some quasi-experimental research designs that can be utilized to control for unobserved heterogeneity. Better quality research design, such as Difference-in-Difference, Instrumental Variable, and Regression Discontinuity, can be utilized to control for unobserved heterogeneity (Angrist & Pischke, 2010). These research designs can better control for omitted variables that may bias the coefficient of interest. The time-invariant unobserved heterogeneity could be controlled using fixed effects methodology (Kennedy, 2008). There may be time-invariant factors of states that might affect wages that will bias the impact of RTW laws if fixed effects are omitted.

The ranking of studies was assessed given the quasi-experimental research design employed. In regard to the ranking, a study that utilizes a regression discontinuity design as designated as a 5. Since regression discontinuity design are considered “close cousins” of RCT and these designs provide a strong research design for controlling unobserved heterogeneity, it is assessed at a higher quality (DiNardo & Lee, 2010). If a study has a strong instrument that provides random assignment, then an IV study will be assessed at a 5. However, if a weak instrument is utilized, then the instrument will be unreliable and the IV design will be assessed at a 2 (Kennedy, 2008). A Difference-in-Difference model will be assessed at a 4, since it looks at control and treatment groups before and after treatment and controls for time-invariant unobserved heterogeneity.

However, self-selection or unobserved heterogeneity may still plague this design (DiNardo & Lee, 2010). A Fixed-Effect design is assessed as a 3. This design is similar to Difference-in-Difference, such that it controls for time-invariant unobserved heterogeneity, but the Fixed-Effects method may not look at a pre-post test of an intervention (Kennedy, 2008). An OLS regression that attempts to control for unobserved factors and endogeneity will be assessed as a 1. While simple OLS models were rejected in the inclusion-exclusion criteria, some studies tried to identify the impact of RTW laws through reduced form equations. However, there are many self-selection problems with these research designs, which makes their assessment low (Kennedy, 2008).

Subsection 5: Data

The data utilized in the studies is an important factor to consider when assessing study quality. There are three types of data that are assessed for quality purposes, which are panel data, cross-sectional data, and pooled data. Studies that utilized panel data were given higher weight, while pooled data and cross-sectional data receive lower weights. Usually, panel data are derived from microdata, which increases their usefulness.

More recent studies utilize panel data for the analysis of RTW laws. Panel data are useful, since these data collect information about the same units of analysis over time. This provides a way to control for unobserved heterogeneity in micro units of analysis (Kennedy, 2008). Furthermore, panel data provide more variability that reduces multicollinearity, where multicollinearity increases the standard error of a coefficient of

interest (Kennedy, 2008). Therefore, panel data are assessed at a higher quality than other types of data.

Cross-sectional data are utilized in earlier studies of RTW laws. Unfortunately, unobserved heterogeneity may plague cross-sectional data sets, since there are many factors that cannot be controlled (Kennedy, 2008). Furthermore, cross-sectional data cannot tell anything about what happens to the units of analysis over time. Also, cross-sectional data cannot provide information about dynamics, such as growth in wages and employment (Kennedy, 2008). Therefore, the assessment of cross-sectional data is the lowest.

Pooled data are another source data used in the analysis of RTW laws. Pooling cross-section across time can potentially provide better data for analysis than cross-sectional, but these data do not follow the same micro units of analysis over time (Kennedy, 2008). Therefore, pooling data is given a higher quality assessment than cross-sectional but a lower quality assessment than panel data.

Subsection 6: Study Quality Assessment

The quality ranking was assessed using research design and data assessments in the prior sections. There were no studies that utilized a Regression Discontinuity design, which provides provided very high level of internal validity for quasi-experiments. Therefore, no studies were given a 5 ranking. One study was assessed as a 4 quality, while seven studies were assessed at a 3 quality. There were four studies assessed at a 2 ranking and there were four studies assessed to have a 1 ranking of quality. (See Table 3-1).

There were eight studies that had relatively higher quality rankings. The lone 4 ranking was given to Li (2012), since this study utilized a Difference-in-Difference research design and panel data. There were seven 3 rankings given. While Farber (2005) uses a difference-in-difference method, pooled data are used instead of panel data, which weakens the study quality from a 4 to a 3. Some of these studies utilized a fixed effect or random effects research design and panel or pooled data (Kunze, 2006; Newman 1983; and Schumacher, 1999). Stevens (2009) utilizes an instrumental variable approach with a weak instrument, which is assessed at a 2. However, Stevens (2009) exploits panel data to take care of time-invariant unobserved heterogeneity, which puts the study quality at a 3. Kalensoski and Lacombe (2006) and Holmes (1998) utilize an identification strategy to control for geographical characteristics. While these designs border upon a Regression Discontinuity design, they do not attempt to control for RTW laws separate from other “pro-business” laws. Therefore, the exact impact of RTW laws cannot be precisely assessed.

There were eight other studies, which were typically lower in quality than the previously discussed studies. The four 2 rankings given and these research design typically utilized an Instrumental Variable design, but the instrument was not arguable exogenous and the instruments were considered weak. In addition, none of these studies utilizes panel data (Carroll, 1983; Moore, Dunlevy, & Newman, 1986; and Wessels, 1981). Hanley (2010) uses a Fixed Effect design, but the author uses pooled data instead of panel data. This reduced the assessment from a 3 ranking to a 2 ranking. All of the 1 rankings used a OLS to estimate the impact of RTW laws on wages or employment. However, these studies typically estimated reduced-form equations to control for

endogeneity. However, this design is still weak and, furthermore, these studies utilized cross-sectional data. Therefore, the likelihood of bias is high and the quality was assessed as low.

Subsection 7: Abstracted Information

This section includes abstracted information from studies included in the systematic review, but not included in the main wage findings. They were important, but less relevant to the wage and employment findings from the analysis chapters.

Farber (1984)

Farber (1984) looked at the impact of RTW laws on unionization and wages for all workers. The author utilizes an earning function with an OLS regression based on a structural equation with microdata from the 1977 CPS to test the research question. While Farber (1984) found that RTW laws are associated with lower wages, RTW laws were also associated with larger in union wage premiums, since nonunion wages fell more than union wages.

To investigate the impact of RTW laws on wages, Farber (1984) considers an earnings function. The author utilizes a sample size of 28,827 individuals in 1977 from the CPS to assess an earnings function. The author does attempt to exclude workers not affected by NLRA and RTW laws, such as managerial, sales, and self-employed workers. The author assesses the impact of the natural log of wages for individuals based upon a RTW dummy variable, union status dummy for wage differentials, and an interaction of union status and RTW status between RTW states and non-RTW states. In addition,

Farber (1984) utilizes a vector of individual characteristics that includes years of education, years of experience, female, marital status, race, and industry dummies.

Farber (1984) finds that RTW laws are associated with an 8.2 percent decline in wages for all worker wages, 4.8 percent decline in wages for union workers, and a 7.7 percent decline in nonunion workers. Farber utilizes microdata, which helps deal with multicollinearity when assessing RTW laws. However, the author does not mention the exclusion of public sector workers, who are subject to different laws, which downward biases the impact of RTW. This research design was fairly weak, since it does not provide a decent design to control for unobserved heterogeneity.

Moore, Dunlevy, and Newman (1986)

Moore, Dunlevy, and Newman (1986) provided a rebuttal to Carroll (1983) finding that RTW laws are negatively associated with wages. Using the data and the methodology that Carroll (1983) utilizes, the authors focus on a sensitivity analysis of the two-stage process. They find that RTW laws are not associated with wages.

The authors' rebuttal to Carroll (1983) is essentially a sensitivity analysis of Carroll's (1983) 2SLS analysis of RTW laws. From Carroll's (1983) data set, which includes observations from fifty states from 1964-1978, the dependent variable is the real average hourly wage rate in a state. They include an additional explanatory variable of AFL-CIO score index of congressional voting records to account for tastes for the first stage of predicted unionization. Additional explanatory variables for predicting unionization include job variations in a state, regional dummies, time dummies, and a RTW dummy variable. After the first stage estimation, real average hourly wages of

states are regressed upon a RTW law dummy, along with predicted unionization, job mixes, region dummies, and time dummies.

Moore, et al., (1986) find that the impact of RTW laws on real average wages for a state becomes statistically insignificant. However, similar with Carroll (1983), the first-stage process uses a nonrandom instrument to predict unionization. However, the authors provide important insight into the sensitivity of the models when a proper research design is not considered.

Newman (1983)

Newman (1983) investigates industry migration and manufacturing growth in the southern portion of the United States. Employment growth is observed to be the growth above the national average growth for a particular state. The author uses pooled data from the Employment and Earnings report from the Bureau of Labor Statistics (BLS) on manufacturing industries to assess the impact of RTW, along with OLS including time fixed effects. The author finds that RTW laws are positively associated with state employment growth relative to national employment growth

Newman (1983) analyzes the impact of state policies on manufacturing employment growth by state relative to national manufacturing employment growth. Specifically, the author attempts to see the impact of the growth in corporate income taxes, the growth in unionization, and the presence of a RTW law on the dependent variable, employment. The RTW law variable is included to be a proxy for business climate. To assess the impact of these policies on manufacturing employment, the author obtains employment data from thirteen 2-digit SIC industries from the BLS report on

Employment and Earnings from 1957 to 1965 and 1965 to 1973. The author does two types of pooled regressions, where one specification includes the state policy explanatory variables along with time fixed effects and the other adds state policies interacted with capital-to-labor ratios to account for capital intensity.

Newman (1983) finds that RTW laws are associated with 0.347-point increase in employment growth relative to national employment growth. In addition, more labor-intensive manufacturing employment growth is associated with RTW laws. However, the study could have been improved by utilizing industry fixed effects along with time fixed effects. Furthermore, the study does not look at policy changes that would have strengthened the research design.

Schumacher (1999)

Schumacher (1999) studies wage differentials between union members and covered nonmembers. The author assesses the impact by utilizing pooled data micro data from the CPS MORG from 1983 to 1997, along with a fixed effects method. The results found that a lower union wage differential and free-riding is prevalent in RTW states.

Schumacher (1999) investigates the change in wage outcomes for individuals utilizing a fixed effect method. To analyze the change in wage outcomes for nonunion workers, union members, and free-riders, the author pools CPS MORG data from January 1983 to October 1997. Given the few covered nonmembers in union shop states, the author restricts the sample to RTW states. The author looks at the natural log of private sector weekly earnings as the dependent variable. For the explanatory variables, the author looks at union member status, covered nonmember status, and personal and job

characteristics including potential experience, experience-squared, years of schooling, MSA size dummies, sex, race, Hispanic ethnicity, part-time status, region dummies, industry dummies, occupation dummies, and year dummies. For a second specification, the author looks at changes in the natural log in weekly earnings along with changes in union and covered nonmember status.

The author finds that free-rider wages are 0.14 log points lower than union members in RTW states. However, the author finds that the free-rider penalty is reduced to -0.094 log points when allowing the coverage coefficient to vary by industry and occupation. These findings allow the author to conclude that the free-rider penalty is driven in part by union wage effects across industries and occupations and that free-riders may cluster into covered jobs where union power is low. However, the author does not imply causation and says that free-riding may weaken bargaining power of unions or weak bargaining power may increase the propensity to free-ride. The author concludes that RTW laws may reduce bargaining power and organizing, which may lead to more free-riders and smaller union-nonunion differentials.

While the author does not directly assess the impact of RTW laws, the author provides important insight into the impact of free-riders on wages. One potential issue with the use of wage data on unions and nonunions from the CPS is the prevalence of imputed wage (Hirsch & Schumacher, 2001). Increases in union wage imputation based upon nonunion wages could potentially bias the union wages downward and reduce the union wage premium. However, the method to utilize pooled data to analyze labor outcomes is a strong candidate for analyzing short-term outcomes for this dissertation. Furthermore, the author discusses the construction of longitudinal panels from the CPS

MORG to assess changes in wages, which will be a method utilized in this dissertation to assess short-run impacts of RTW laws.

References

- Addison, J. T. & Belfield, C.R. (2004). Union voice. *Journal of Labor Research*. 25(4) p. 563-596
- Angrist, J.D. & Pischke, J.S. (2010). The credibility revolution in empirical econometrics: How better research design is taking the con out of econometrics. *Journal of Economic Perspectives*. 24(2) p. 3-30
- Ballotpedia (2016a). Indiana state budget 2011-2012. Retrieved on November 29, 2016 from: [https://ballotpedia.org/Indiana_state_budget_\(2011-2012\)](https://ballotpedia.org/Indiana_state_budget_(2011-2012))
- Ballotpedia (2016b). Michigan state budget 2012-2013. Retrieved on November 29, 2016 from: [https://ballotpedia.org/Michigan_state_budget_\(2012-2013\)](https://ballotpedia.org/Michigan_state_budget_(2012-2013))
- Ballotpedia (2016c). Ohio state budget 2011-2013. Retrieved on November 29, 2016 from: [https://ballotpedia.org/Ohio_state_budget_\(2011-2013\)#Budget](https://ballotpedia.org/Ohio_state_budget_(2011-2013)#Budget)
- Baumgartner, F.R., Berry, J.M., Hojnaski, M., Kimball, D.C., & Leech, B.L. (2009) *Lobbying and policy change: Who wins, who loses, and why*. Chicago: The University of Chicago Press
- Blanchflower, D.G. & Bryson, A. (2004). What effect do unions have on wages now and would freeman and medoff be surprised? *Journal of Labor Research*. 25(3) p. 383-414
- Bleakley, H., Ferris, A.E., & Fuhrer, J.C. (1999). New data on worker flows during business cycles. *New England Economic Review*. July/August 1999 p. 49-76
- Brown, C. & Medoff, J. (1978). Trade unions in the production process. *Journal of Political Economy*. 86(3), 35-378.
- Boon, Z., Carson, C.M., Faberman, R.J., & Ilg, R.E. (2008). Studying the labor market using bls labor dynamics data. *Monthly Labor Review*. 131(2) p. 3-16
- Budd, J.W. (2005). The effect of unions on employee benefits: Updated employer expenditure results. *Journal of Labor Research*. 26(4) p. 669-676
- Bullocks, K. (2007). Official time as a form of union security in federal sector labor-management relations. *Air Force Law Review*. 59 p. 153-229
- Bureau of Labor Statistics (2015). BLS handbook of methods. Retrieved on December 20, 2015 from: <http://www.bls.gov/opub/hom/pdf/homch1.pdf>
- Bureau of Labor Statistics (1997). BLS handbook of methods. Retrieved on November 18, 2014 from: <http://www.bls.gov/opub/hom/pdf/homch5.pdf>

- Carroll, T.M. (1983). Right-to-work laws do matter. *Southern Economic Journal*. 50(2), pp. 494-509
- Census Bureau (2016). Intercensal estimates. Retrieved on April 11, 2016 from: <https://www.census.gov/popest/data/intercensal/>
- Collins, B. (2014). Right-to-work laws: Legislative background and empirical research. Congressional Research Service. Retrieved on March 13, 2015 from: <http://fas.org/sgp/crs/misc/R42575.pdf>
- Conley, T.G. & Taber, C.R. (2011). Inference with “difference-in-differences” with a small number of policy changes. *The Review of Economics and Statistics*. 93(1) 113-125
- Davey, M. (2015). Union suffer latest defeat in midwest with signing of Wisconsin measures. *The New York Times*. Retrieved on December 24, 2015 from: http://www.nytimes.com/2015/03/10/us/gov-scott-walker-of-wisconsin-signs-right-to-work-bill.html?_r=0
- Davis, J.C. & Huston, J.H. (1993). Right-to-work laws and free riding. *Economic Inquiry*. 31(1) p. 52-58
- Davis, J.C. & Huston, J.H. (1995). Right-to-work laws and union density: New evidence from micro data. *Journal of Labor Research*. 26(2) p. 223-229
- Department of Labor. (2015). In *Glossary*. Retrieved on September 13, 2015 from: <http://www.dol.gov/dol/aboutdol/history/glossary.htm>
- Dercon, S. & Shapiro, J.S. (2007). Moving on, staying behinds, getting lost: Lesson on poverty mobility from longitudinal data. In D. Narayan & P. Petesch (Eds.), *Moving out of poverty: Cross-disciplinary perspectives on mobility* (p. 77-126). New York, NY: Palgrave Macmillan. Washington, D.C.: World Bank.
- Devinatz, V.G. (2011). The continuing controversy over right-to-work laws in the early twenty-first century. *Employee Responsibilities and Rights Journal*. 23(4) p. 287-293
- DiNardo, J. & Lee, D.S. (2010). Program evaluation and research designs. (NBER Working Paper 16016). Cambridge, MA: National Bureau of Economic Research. Retrieved from: <http://www.nber.org/papers/w16016.pdf>
- DiNardo, J. & Lee, D.S. (2010). Program Evaluation and Research Design (Working Paper No. 16016). Retrieved from the National Bureau of Economic Research website: <http://www.nber.org/papers/w16016>

- DiNardo, J. & Lee, D.S. (2004). Economic impacts of new unionization on private sector employers: 1984-2001. *The Quarterly Journal of Economics*. 119(4) p. 1383-1441
- Doucouliagos, C. & Laroche, P. (2003). What do unions do to productivity: A meta-analysis. *Industrial Relations*. 42(4), 650-691.
- Eggert, D. (2012, Dec. 13). AG Bill Schuette: Right-to-work law applies to state employees, will withstand lawsuits. *MLive*. Retrieved on May 22, 2016 from: http://www.mlive.com/news/index.ssf/2012/12/ag_schuette_right-to-work_law.html
- Ehrenberg, R.G. & Smith, R.S. (2015). *Modern labor economics: Theory and public policy* (12th ed.). Harlow, England: Pearson Education Limited
- Ellwood, D.T. & Fine, G. (1987). The impact of right-to-work laws on union organizing. *Journal of Political Economy*. 95(2) p. 250-273
- Eren, O. (2009). Does membership pay off for covered workers? A distributional analysis of the free rider problem. *Industrial and Labor Relations Review*. 62(3) p. 367-380
- Eren, O. & Ozbeklik, S. (2016). What do right-to-work laws do? Evidence from a synthetic control method. *Journal of Policy Analysis and Management*. 35(1) p. 173-193
- Fallick, B. & Fleischman, C.A. (2004). *Employer-to-employer flows in the u.s. labor market: The complete picture of gross worker flows*. Federal Reserve Board Finance and Economic Discussion Series Working Paper 2004-34. Retrieved on December 18, 2015 from: <http://www.federalreserve.gov/pubs/feds/2004/200434/200434pap.pdf>
- Farber, H.S. (2005). Nonunion wage rates and the threat of unionization. *Industrial and Labor Relations Review*. 58(3) p. 335-352.
- Farber, H.S. (1984). Right-to-work laws and the extent of unionization. *Journal of Labor Economics*. 2(3) p. 319-352
- Feenberg, D. & Roth, J. (2007). CPS labor extracts. National Bureau of Economic Research. Retrieved on December 18, 2015 from: <http://www.nber.org/morg/docs/cpsx.pdf>
- Fields, G.S., Duval, R.H., Freije, S. & Sanchez Puerta, M.L. (2007). Intragenerational income mobility in latin america. *Economia*. 7(2) p. 101-143
- Freeman, R.B. (2005). What do union do? The 2004 m-brane stringtwister edition. *Journal of Labor Research*. 26(4) p. 641-668.

- Freeman, R.B. (1986). Unionism comes to the public sector. *Journal of Economic Literature*. 24(1) p. 41-86
- Freeman, R.B. (1976). Individual mobility and union voice in the labor market. *American Economic Review*. 66(2), 361-368
- Freeman, R.B., & Medoff, J.L. (1984). *What do unions do?* New York, NY: Basic Books, Inc., Publishers
- Freeman, R.B. & Valletta, R.G. (1988). The effects of public sector labor laws on labor market institutions and outcomes. In R.B. Freeman & C. Ichniowski (Eds.), *When public sector workers unionize* (p. 81-106). Chicago, IL: University of Chicago Press
- Garofalo, G. A., & Malhotra, D. M. (1992). An integrated model of the economic effects of right-to-work laws. *Journal of Labor Research*. 13(3) p. 293-305
- Hanley, C. (2010). Earnings inequality and subnational political economy in the United States, 1970-2000. *Research in Social Stratification & Mobility*. 28(2) p. 251-273
- Hegji, A. (2012). Federal labor relations statutes: An overview. Congressional Research Service. Retrieved from <https://www.fas.org/sgp/crs/misc/R42526.pdf>
- Hirsch, B.T. (2008). Sluggish institutions in a dynamic world: Can unions and industrial competition coexist? *Journal of Economic Perspectives*. 22(1) p. 153-176
- Hirsch, B.T. (2004). What do unions do for economic performance?. *Journal of Labor Research*. 25(3) p. 415-455
- Hirsch, B.T. & Addison, J.T. (1986). *The economic analysis of unions: New approaches and evidence*. Boston, MA: Allen & Unwin, Inc.
- Hirsch, B.T. & Macpherson, D.A. (2015): Union membership and coverage database from the current population survey. Retrieved on July 28, 2015 from: <http://www.unionstats.com>
- Hirsch, B.T. & Macpherson, D.A. (2003). Union membership and coverage database from the current population survey: Note. *Industrial and Labor Relations Review*. 56(2), pp 349-54
- Hirsch, B.T. & Schumacher, E.J. (2004). Match bias in wage gap estimates due to earnings imputations. *Journal of Labor Research*. 22(3) p. 689-722
- Hirsch, B.T. & Schumacher, E.J. (2001). Privates sector union density and the wage premium: Past, present, and future. *Journal of Labor Research*. 22(3) p. 488-518

- Holmes, T.J. (2016). The state border data set. Retrieved on July 23, 2016 from: <http://users.econ.umn.edu/~holmes/data/BorderData.html>
- Holmes, T.J. (1998). The effect of state policies on the location of manufacturing: Evidence from state borders. *Journal of Political Economy*. 106(4) p. 667-705
- Hundley, G. (1988). Who joins unions in the public sector? The effect of individual characteristics and the law. *Journal of Labor Research*. 9(4) p. 301-323
- Ichniowski, C. & Zax, J.S. (1991). Right-to-work laws, free riders, and unionization in the local public sector. *Journal of Labor Economics*. 9(3) p. 255-275
- Indiana House Bill 1001, I.C. § 22-6 (2012). Retrieved on September 16, 2015 from: <http://www.in.gov/apps/lisa/session/billwatch/billinfo?year=2012&session=1&request=getBill&doctype=HB&docno=1001>
- Indiana House Bill 1001, I.C. 4-15-17 § 5(4). Retrieved on January 4, 2015 from: <http://www.in.gov/legislative/bills/2011/HE/HE1001.1.html>
- Jacob, R., Zhu, P., Somers, M., & Bloom, H. (2012). *A practical guide for regression discontinuity*. MDRC. Retrieved on April 4, 2015 from: http://www.mdrc.org/sites/default/files/regression_discontinuity_full.pdf
- Kalenkoski, C. M., & Lacombe, D. J. (2006). Right-to-Work Laws and Manufacturing Employment: The Importance of Spatial Dependence. *Southern Economic Journal*. 73(2) p. 402-418
- Kaufman, B.E. (2005). What do unions do? Evaluation and commentary. *Journal of Labor Economics*. 26(4) p. 555-595
- Kaufman, B.E. (2004). What unions do? Insights from economic theory. *Journal of Labor Economics*. 25(3) p. 351-382
- Kennedy, P. (2008). *A guide to econometrics* (6th ed.). Malden, MA: Blackwell Publishing
- Kunce, M. (2006). What factors affect a state's manufacturing employment? Evidence from 1974-1994 state panel data. *Review of Urban & Regional Development Studies*. 18(1) p. 1-10
- Lafer, G. (2013). The legislative attack on American wages and labor standards, 2011-2012. Economic Policy Institute. Union and Labor Standards Report. Retrieved on November 29, 2014 from: <http://www.epi.org/publication/attack-on-american-labor-standards/>

- Lee, D.S. & Lemieux, T. (2010). Regression discontinuity design in economics. *Journal of Economic Literature*. 48(2) 281-355.
- Lee, D.S. & Lemieux, T. (2009). Regression discontinuity design in economics. (NBER Working Paper 14723). Cambridge, MA: National Bureau of Economic Research. Retrieved from: <http://www.nber.org/papers/w14723>
- Lewis, H.G. (1963). *Unionism and relative wages in the united states*. Chicago: University of Chicago Press
- Li, X. (2012). Workers, unions, and takeovers. *Journal of Labor Research*, 33(4) p. 443-460
- Madrian B.C. & Lefgren, L.J. (1999). A note on longitudinally matching current population survey (cps) respondents. National Bureau of Economic Research Technical Working Paper 247. Retrieved on December 18, 2015 from: <http://www.nber.org/papers/t0247>
- Malcomson, J.M. (1983). Trade unions and economic efficiency. *The Economic Journal*. 93(369a) p 51-65
- Michigan House Bill 4003, M.C.L. §423.209 (2012). Retrieved on September 16, 2015 from: [http://www.legislature.mi.gov/\(S\(y12tgj1r1tfa1005ixdgvkvw\)\)/mileg.aspx?page=GetObject&objectname=2011-HB-4003](http://www.legislature.mi.gov/(S(y12tgj1r1tfa1005ixdgvkvw))/mileg.aspx?page=GetObject&objectname=2011-HB-4003)
- Michigan Senate Bill 116, M.C.L. §423.14 (2012). Retrieved on September 16, 2015 from: [http://www.legislature.mi.gov/\(S\(c4dee4b2jyzrq2k4mrpk3kp0\)\)/mileg.aspx?page=GetObject&objectname=2011-SB-0116](http://www.legislature.mi.gov/(S(c4dee4b2jyzrq2k4mrpk3kp0))/mileg.aspx?page=GetObject&objectname=2011-SB-0116)
- Moore, W.J. (1998). The determinants and effects of right-to-work laws: A review of the recent literature. *Journal of Labor Research*. 19(3) p. 445-469
- Moore, W. J. (1980). Membership and wage impact of right-to-work laws. *Journal of Labor Research*. 1(2) p. 349-368
- Moore, W.J., Dunlevy, J.A., & Newman, R.J. (1986). Do right to work laws matter? Comment. *Southern Economics Journal*. 53(2) p. 515-524
- Moore, W.J. & Newman, R.J. (1985). The effects of right-to-work laws: A review of the literature. *Industrial and Labor Relations Review*. 38(4), pp. 571-585
- Murnane, R.J. & Willett, J.B. (2011). *Methods matter: Improving causal inference in educational and social science research*. New York: Oxford University Press.

- National Bureau of Economic Research. (2016). CPS merged outgoing rotation groups. Retrieved on May 31, 2016 from: <http://www.nber.org/data/morg.html>
- National Bureau of Economic Research. (2015). Current population survey (cps) basic monthly data at nber. Retrieved on December 19, 2015 from: http://www.nber.org/data/cps_basic.html
- National Conference of State Legislatures. (2015). Right-To-Work Resources. Retrieved on September 15, 2015 from: <http://www.ncsl.org/research/labor-and-employment/right-to-work-laws-and-bills.aspx>
- National Conference of State Legislatures. (2016). State budget stabilization funds. Retrieved on November 29, 2016 from: <http://www.ncsl.org/research/fiscal-policy/state-budget-stabilization-funds-spring-2008.aspx>
- National Labor Relations Act of 1935, 29 U.S.C. § 151 (2015a). Retrieved on September 12, 2015 from: <https://www.law.cornell.edu/uscode/text/29/151>
- National Labor Relations Act of 1935, 29 U.S.C. § 153 (2015b). Retrieved on September 12, 2015 from: <https://www.law.cornell.edu/uscode/text/29/153>
- National Labor Relations Act of 1935, 29 U.S.C. § 157 (2015c). Retrieved on September 13, 2015 from: <https://www.law.cornell.edu/uscode/text/29/157>
- National Labor Relations Board. (1997). Basic guide to the national labor relations act. Retrieved on September 24, 2015 from: <https://www.nlr.gov/sites/default/files/attachments/basic-page/node-3024/basicguide.pdf>
- National Right to Work Legal Defense Foundation (2016a). Know your right: Michigan's right to work law. Retrieved on May 22, 2016 from: <http://www.nrtw.org/en/blog/know-your-rights-michigan-right-work-03212013>
- National Right to Work Legal Defense Foundation (2016b). Special legal notice to most public-sector workers in Michigan. Retrieved on May 22, 2016 from: <http://www.nrtw.org/en/special-legal-notice-michigan-public-workers-rights>
- National Right to Work Legal Defense Foundation, Inc. (2016c). Special notice to West Virginia workers: What West Virginia's right to work law means for your rights. Retrieved on March 14, 2016 from: <http://www.nrtw.org/WV-RTW>
- National Right to Work Legal Defense Foundation (2015). Right to work states: Oklahoma. Retrieved on July 19, 2016 from: <http://www.nrtw.org/c/okrtwlaw.htm>
- Newman, R.J. (1983). Industry migration and growth in the south. *The Review of Economics and Statistics*. 65(1), 76-86.

- Nicholson, W. (2005). *Microeconomic theory: Basic principals and extensions*. Mason, OH: Thomas Southwestern
- Oddi, M. (2011). Indiana government “budget legislation codifies collective bargaining ban”. The Indiana Law Blog. Retrieved on January 4, 2016 from: http://indianalawblog.com/archives/2011/06/ind_govt_budget_2.html
- Oklahoma Constitution Article 23 §1A. Retrieved on September 17, 2015 from: http://www.oklegislature.gov/ok_constitution.html
- Oklahoma State Legislature. (2001). State Question 695. Retrieved on September 17, 2015 from: http://oksenate.gov/publications/legislative_briefs/Legis_Brief_2002/state_questions_2002.html
- Oosting, J. (2015, Jan. 23). Michigan union membership dropped significantly in 2014, first full year under right-to-work law. *MLive*. Retrieved on May 22, 2016 from: http://www.mlive.com/lansing-news/index.ssf/2015/01/michigan_union_membership_down.html
- Palomba, N.A., & Palomba, C.A. (1971). Right-to-work laws: A suggested economic rationale. *Journal of Law and Economics*, 14(2), pp. 475-483
- Railway Labor Act of 1926, 45 U.S.C. § 151a (2015a). Retrieved on September 13, 2015 from: <https://www.law.cornell.edu/uscode/text/45/151a>
- Railway Labor Act of 1926, 45 U.S.C. § 152 (2015b). Retrieved on September 13, 2015 from: <https://www.law.cornell.edu/uscode/text/45/152>
- Reed, R.W. (2003). How right-to-work laws affect wages. *Journal of Labor Research*. 24(4) p. 713-730
- Sanes, M. (2014). Barriers to freedom of contract in the public sector. Center for Economic and Policy Research. Retrieved on September 15, 2015 from: <http://www.cepr.net/documents/public-rtw-2014-10.pdf>
- Sanes, M. & Schmitt, J. (2014). Regulation of public sector collective bargaining in the states. Center for Economic and Policy Research. Retrieved from <http://www.cepr.net/documents/state-public-cb-2014-03.pdf>
- Schumacher, E.J. (1999). What explains wage differences between union members and covered nonmembers. *Southern Economics Journal*. 65(3) p. 493-512
- Slon, P. & Zuniga, E. (2006). Poverty dynamics in costa rica with panel data from cross-section. *Cepal Review*. 89 p. 165-178

- Shimabukuro, J.O. (2011). Collective bargaining and employees in the public sector. Congressional Research Service. Retrieved from: http://digitalcommons.ilr.cornell.edu/cgi/viewcontent.cgi?article=2241&context=key_workplace
- Sobel, R.S. (1995). Empirical evidence of the union free-rider problem: Do right-to-work laws matter? *Journal of Labor Research*. 16(3) pp. 347-365
- Stevens, L.K. (2009). The effects of endogenous right-to-work laws on business and economic conditions in the united states: A multivariate approach. *Review of Law and Economics*. 5(1) p. 595-614
- Taft-Hartley Act of 1947, 29 U.S.C. § 158 (2015a). Retrieved on September 13, 2015 from: <https://www.law.cornell.edu/uscode/text/29/158>
- Taft-Hartley Act of 1947, 29 U.S.C. § 164(b) (2015b). Retrieved on September 13, 2015 from: <https://www.law.cornell.edu/uscode/text/29/159>
- Taylor, B.J. & Whitney, F. (1992). *U.S. labor relations law: Historical development*. Englewood Cliffs, NJ: Prentice Hall
- Trochim, W.M. & Donnelly, J.P. (2008). *The Research Methods Knowledge Base* (3rd ed.). Cengage Learning: Mason, OH.
- Valletta, R.G. & Freeman, R.B. (1988). Appendix b the nber public sector collective bargaining law data set. In R.B. Freeman & C. Ichniowski (Eds.), *When public sector workers unionize* (pp. 399-419). Chicago, IL: University of Chicago Press
- Vedder, R. (2010). Right-to-work laws: Liberty, prosperity, and quality of life. *Cato Journal*. 30(1), pp. 171-180
- Wessels, W.J. (1981). Economic effects of right to work laws. *Journal of Labor Research*. 2(1), pp 55-75
- West Virginia Legislature, Senate Bill 1 (2016). Retrieved on March 14, 2016 from: http://www.legis.state.wv.us/Bill_Text_HTML/2016_SESSIONS/RS/pdf_bills/SB1%20eng.pdf
- Wisconsin Act 1, § 991.11, Wisconsin Statutes (2015). Retrieved on September 16, 2015 from: <https://docs.legis.wisconsin.gov/2015/related/acts/1>
- Wisconsin Act 10, § 35.095(3)(a), Wisconsin Statutes (2011). Retrieved on September 15, 2015 from: <https://docs.legis.wisconsin.gov/2011/related/acts/10>

Wisconsin State Legislature. (2011). Summary of provisions of 2011 act 10. Retrieved on September 15, 2015 from: <http://legis.wisconsin.gov/lfb/publications/budget/2011-13-budget/documents/act32/act%2010.pdf>

Zax, J.S. & Ichniowski, C. (1990). Excludability and the effects of free riders: Right-to-work laws and local public sector unionization. *Public Finance Quarterly*. 19(3) p. 293-315

