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THE EFFECTS OF "RIGHT-TO-WORK" LAWS:
AN INTERRUPTED TIME-SERIES ANALYSIS

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ABSTRACT

This paper begins with a theoretical discussion and literature review of “right-to-work” (RTW) laws’ effects, including arguments made for and against them. The analysis section of this paper uses Oklahoma’s 2001 RTW law as a case study, with neighboring non-RTW states Colorado, Missouri, and New Mexico also analyzed for comparison. An interrupted time-series analysis is performed to determine if the RTW law affects three dependent variables: unemployment, median household income, and workplace illness and injury rates. This paper adopts the hypotheses of RTW opponents: unemployment will be unaffected by the law, income will decrease relative to union-shop states, and occupational illness and injury rates will increase relative to other states. The results tentatively support this paper's hypothesis regarding unemployment, but not for median household income or occupational illness and injury, which had very mixed results.

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Effects of "Right-to-Work" Laws: An Interrupted Time Series Analysis

“Closed-shop” union arrangements - in which workers are required to join and pay dues to a union - have long been controversial. Opponents object to mandatory union membership, and claim that unions hinder job growth by demanding more of employers. Supporters of union-shops point to the “free-rider problem,” referring to workers' reluctance to pay dues if they will receive benefits won by the union anyway, which reduces unions' funds and, thus, their ability to organize and make demands. “Right-to-work” (RTW) laws banning union shops have earned the nickname “right to work for less” laws among opponents, who cite the generally higher wages of union members. Unions have increased wages and benefits and improved working conditions for many. These metrics can vary significantly between states; this paper aims to determine if the presence of “right-to-work” laws can partially explain this variance.

For many years, the RTW issue seemed static, with states enacting them being mostly concentrated in the Southeast and Great Plains. Many states enacted the laws soon after the Taft-Hartley Act of 1947 made it legal for them to do so, and other states enacted them only intermittently over the following decades. However, the 2012 enactment of RTW laws in Michigan and Indiana shows that these laws are not a dead issue, and should renew interest in researching their effects. The significance of RTW laws' passage in these states is enormous. They are the nexus of the American auto industry - traditionally a heavily unionized sector - and Michigan was the site of the Flint Sit-Down Strike of 1936-37, one of the most significant victories of organized labor in the 20th century. Trying economic times frequently lead people to accept diminished protections for workers, in the hopes of gaining favor from industry. It is important to evaluate these sacrifices afterward: did a reduction in wages or benefits occur and, if so, were enough new jobs created to compensate for these losses?

This paper will begin by reviewing academic literature on unions and RTW laws. This will include both theoretical examinations of RTW laws and empirical studies of their effects. This paper's own theories about the effects of RTW laws will follow, including discussion of possible extraneous variables that must be considered, and culminating with a hypothesis that can be tested with an interrupted time series analysis in the next section, which will examine changes in metrics that both opponents and supporters say can be affected by RTW laws. The conclusion will evaluate the correctness of the hypotheses, and identify future research topics that could build on this paper.

Literature Review

Much of the existing research on RTW laws focuses on whether they actually reduce union membership. This is certainly worthwhile; metrics like wages or insurance benefits are more likely to be affected by a drop in union membership than by the mere existence of a RTW law. Moore and Newman (1985) and Moore (1998) wrote fairly comprehensive reviews of literature on the effects of RTW laws. They name three prominent theories as to why a RTW law should affect or not affect union membership. The “free-rider hypothesis” and the “bargaining power hypothesis” say that RTW laws should hinder unionism. The free-rider hypothesis refers to the problem of workers receiving benefits advanced by the union even if they do not join it, thus reducing their incentive to join and pay dues. The related bargaining power hypothesis says that labor organizing will be discouraged by the lack of closed shops under the RTW law because unions will be less effective in making demands: if they don't represent the entirety of a workforce, their power to strike will be limited. The hypothesis that downplays effects of RTW laws is the “taste hypothesis.” This says that enactment of a RTW law merely expresses the feelings people in that state have toward unions, and that the law itself is insignificant. In other

words, only states where people dislike unions will pass RTW laws, so the laws are purely symbolic, as unions were not powerful in that state to begin with. This is probably true in some states, although it is unlikely that the taste hypothesis applies to Michigan's recent RTW law, which was followed by large protests.

The results of empirical tests of these three hypotheses have been mixed. There is a broad range of results: Davis and Huston (1985) say that RTW state residents are 8.2 percent less likely to unionize, while Farber (1984) says that RTW laws have no independent effects, and that taste or other factors explain any differences. The methodology used has a strong influence on the findings. Moore (1998) has a detailed technical comparison of these methodologies, and concludes that the reduction in union membership is likely between 3 and 8 percent.

This paper will not address the question of how much RTW laws affect union membership, for two reasons. From a practical perspective, if the question has not been definitively answered by decades of research, it is unlikely to be settled here. Secondly, there are some effects of RTW laws that do not strictly depend on changes in unionization - particularly, industry relocation. Whether businesses will relocate from union shop states to RTW states depends only on whether business leaders *believe* labor there will be cheaper – which, as Moore and Newman (1985) point out, does not necessarily reflect reality. Thus, the question of RTW effects on employment can still be explored without knowing precisely how the laws affect unionization.

The effects of RTW laws on wages have been most often studied in terms of the union wage premium – that is, the difference in wages between a union-member and non-member working in the same position. Theoretical arguments about the effects of RTW laws on wages are more difficult to make than one might imagine (Moore 1998). Many suggest that the union

wage premium must be higher in RTW states, or else no one at all would join unions; this has been supported in some empirical studies (Farber 1984). The average wage for all workers is lower in RTW states (Moore 1980, Carroll 1983), but Wessels (1981) argues that the RTW law itself is not causal. Its effect varies notably between studies, depending on how the RTW dummy variable is treated.

The effects of RTW laws on employment are less studied than their effects on union membership or wages. Much of the literature in this area focuses on the rise of industry in the southern states: can this be explained, largely or in part, by RTW laws, or are other factors more useful in explaining this? Moore's 1998 survey reversed the conclusion he and Newman reached previously (1985). The earlier literature review found no significant effects, while the more recent one suggests that RTW laws may encourage industrial development in a state. However, he also writes that separating RTW laws from a state's overall "business climate" is incredibly difficult. RTW laws are most often passed in states whose laws favor business in other ways: low taxes, lax regulations, etc. This makes it hard to determine how much of the effect is due to the RTW law independently. However, the effect of "business climate" in general may not be as large as is generally thought. In 1975, the industrial group Fantus rated states on a variety of factors, including RTW laws, and although researchers (Cobb 1982, Plaut and Pluta 1983) found a statistically significant effect, it was only about a 0.5 percent correlation with industrial location. In other analyses (Carlton 1979, 1983) no statistically significant effect for the Fantus ratings was found at all.

Other studies also show mixed results. Newman (1983) found that RTW laws encouraged employment growth in 11 of the 13 industries studied, but his later study (1984) found that the effects gradually decreased, so states that passed RTW laws soon after Taft-Hartley no longer

saw any effects by the mid-1970s. Woodward and Glickman (1991) find that RTW laws are negatively correlated with foreign investment, and related employment, which they attribute to union shops' higher productivity. However, Holmes's (1995) study of regions along the borders of RTW and non-RTW states found that manufacturing employment was higher in the RTW states. Thus, drawing firm, universally applicable conclusions about RTW effects on employment is difficult.

Due to the highly mixed results of the research cited above, studying RTW effects on income and unemployment in this paper can still be useful. State-comparison interrupted time-series were not cited in either of the literature reviews mentioned above, so perhaps this method could reveal new information about these topics. Issues that are not strictly economic - like workplace safety and employer-provided health insurance - have been most neglected in the literature. Safety issues were a major factor in motivating the early formation of unions, and continue to be a concern. Although it is possible that the creation of the Occupational Safety and Health Administration (OSHA) in 1970 has resulted in fewer differences in safety between union and non-union shops, this should not be assumed. An empirical study examining any change in the number of safety incidents after a RTW laws is passed would be valuable. Another issue that appears not to have been studied by any paper cited in the two literature reviews is any change in the percentage of uninsured people in RTW states. Securing health insurance for members is a common union goal, and it would be valuable to know whether RTW states have fewer people with private insurance. If so, this would be important for public policy: if fewer people have private insurance, it could put a greater burden on government programs like Medicaid.

Theory

This paper proposes that RTW laws can partially explain the variance in household income, unemployment, and occupational illness and injuries between states. A study of the effects of RTW laws should empirically test the primary arguments both for and against these laws: respectively, that their enactment reduces unemployment, or that it reduces wages and harms safety. It is, of course, entirely possible that both of these claims are correct to some degree, in which case a value judgment must be made that weighs the significance of these effects. Although there is significant conflict in the literature, enough studies have shown measurable effects of RTW laws to make this paper's theory plausible.

The hypothesis of this paper is that of RTW opponents: specifically, that enactment of a RTW law will reduce wages in that state, and that any reduction in unemployment – if it occurs at all – will be statistically insignificant or mostly attributable to other factors. That unionized workers usually make more money and have better benefits than nonunionized workers in an equivalent position is a fact. Collective bargaining is more effective than individual bargaining, because an individual worker is far easier to replace than an entire workforce. RTW laws effectively cripple unions, because of the aforementioned free rider problem: if unions lack the dues obtained from compulsory membership, they will not be able to effectively advocate for workers. Additionally, the RTW reality that not all workers are union members will take away unions' leverage in bargaining with industry. With each worker left to fight for him or herself, their wages will be reduced relative to those in wholly unionized, closed shop enterprises. Thus, if this theory is correct, the epithet "right to work for less" will be justified.

Aside from voicing philosophical objections to requiring union membership, supporters of RTW laws most often argue that they reduce unemployment by encouraging industrial

growth. They concede the point that union members earn better wages, and claim that this reduces the number of people that a company with a unionized workforce can hire. The thinking is that the company would like to hire more workers, but either cannot afford to because of the high wages, or chooses not to in order to maximize profit. Thus, they say, if a state bans closed shop arrangements, then companies will be able to hire more workers - an ability that proponents assume they will exercise - and new industries will be attracted to the state, thus lowering unemployment. However, the RTW claim that the prevalence of unions in a state reduces employment is tenuous, as shown by the mixed results of previous research cited above. To begin with, companies usually resist hiring new workers unless consumer demand requires it. The hiring binge that proponents assume will occur after a RTW law is enacted is probably nonexistent. Additionally, the unemployment rate has a wide variety of other, probably more significant variables affecting it aside from unionization.

Because wages and unemployment constantly fluctuate for reasons entirely unrelated to a state's labor laws, it is best to compare a newly-minted RTW state with nearby, demographically similar states that permit union shops. This way, changes in the RTW state's data can be put into a broader context, allowing for evaluation of outside factors that could be affecting the dependent variables. For example, the passage of a state's RTW law could coincide with the beginning of a national or regional period of economic growth, stagnation, or recession. If that state were studied in isolation, changes in the dependent variables could be wrongly attributed to the passage of the law.

Other factors particular to a new RTW state could also have an effect. For example, RTW states are most heavily concentrated in the southeast. The southeast has experienced industrial development over the past several decades, but as noted above, explanations for this differ.

Although some people attribute industrial growth in the south to RTW laws, it could also be because land is far cheaper than in the more densely populated northeast, or for other reasons. If examining a southern RTW state, then, it should be compared with Missouri or Kentucky – the southernmost states in the east to allow closed shops. Likewise, the RTW Wyoming would be best compared with its union-shop neighbors Montana and Colorado, as they share economic and demographic features common to the Rocky Mountains, and would be more likely to experience the same broader economic trends.

Perhaps the passage of a RTW law could be part of a broader spree of business-favoring legislation in the state government, potentially masking the effects of the RTW law. Studies of this kind cannot demand that the independent variable of interest be enacted in isolation, but efforts to control for extraneous variables should be made, and other significant business- and labor-related legislation coinciding with the RTW law should be considered.

Figures 1-3 below use imaginary data to represent the hypotheses of this paper, by the solid lines, and of RTW-supporters, by the dashed lines. They represent 20-year time spans, when a RTW law is enacted in year 10. The single line in years 1-9 represents the imaginary actual statistics for this state, and the divergence in lines after year 10 represents the difference in hypotheses between this paper and RTW supporters. To reiterate, these outcomes for “right-to-work” states will be compared with the same measures performed on union shop states, to control for broader economic fluctuation.

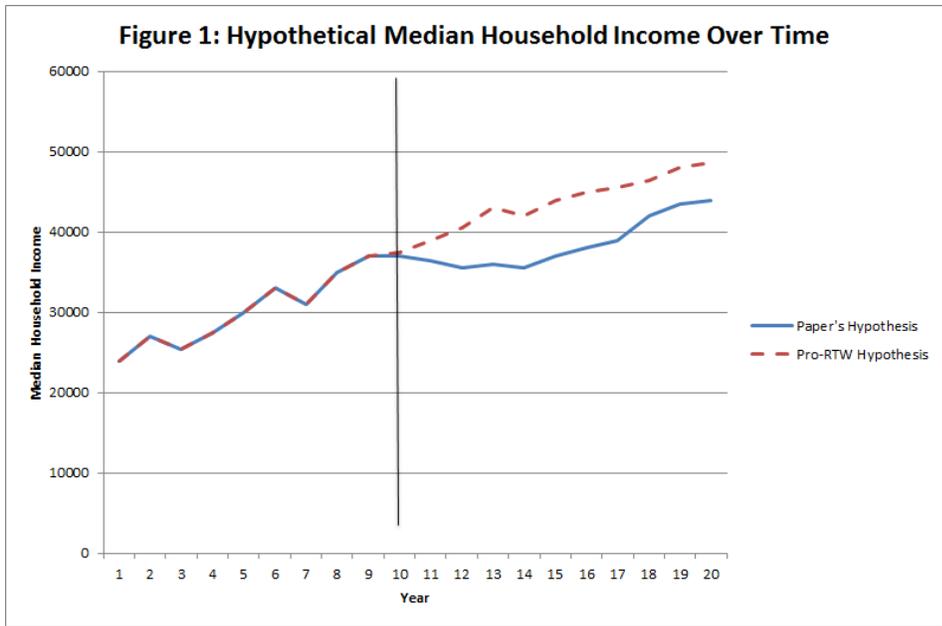


Fig. 1. The solid line after the RTW law represents the paper's hypothesis: wages will fall after the passage of the law due to reduced union bargaining power. The dashed line represents supporters' claims: wages will rise or stay the same after a RTW law. Year 10 represents RTW passage.

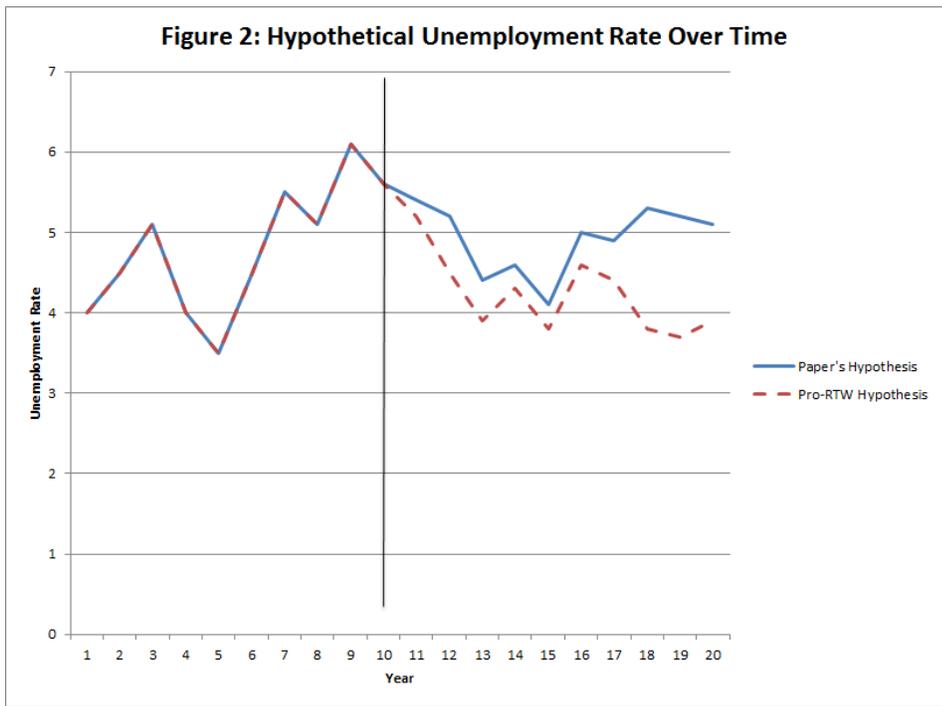


Fig. 2. The solid line after the RTW law represents this paper's hypothesis: unemployment will be unaffected by a RTW law. The dashed line represents RTW supporters' hypothesis: that unemployment will decrease after the RTW law. Year 10 represents RTW passage.

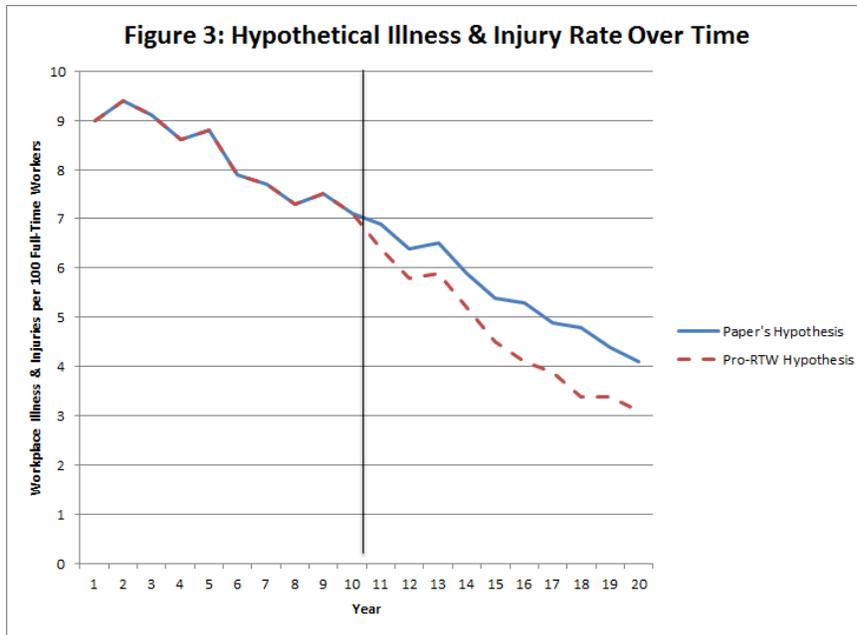


Fig. 3. The solid line represents the paper's hypothesis: illness & injury rates will be relatively higher after RTW passage. The dashed line represents the pro-RTW hypothesis that RTW laws will not affect illness & injury rates. Year 10 represents RTW passage.

This study will use Oklahoma, which became a RTW state in 2001, as its subject for analysis. The same statistics will be measured for Oklahoma's three union-shop neighbors: Missouri, Colorado, and New Mexico. The unemployment rate, average wages, and occupational illness and injury rate for each state will be studied with five variations of the following model, each of which will be explained in detail in the analysis section:

$$y = a + b_1\text{Trend} + b_2\text{RTWd} + b_3\text{RTWc} + b_4\text{Oklahomad} + b_5\text{Coloradod} + b_6\text{Missourid} + b_7\text{NewMexicod} + b_8\text{Oklahoma} + b_9\text{Colorado} + b_{10}\text{Missouri} + b_{11}\text{NewMexico}$$

Where y = the unemployment rate, average wages, or occupational illness and injury rate,
a = a constant

b₁Trend = the change in the variable for all four states combined

b₂RTWd = a dummy variable for Oklahoma's RTW law

b₃RTWc = a counter variable for Oklahoma's RTW law

b(State)d = a dummy variable indicating the specified state

b(State) = the trend for the specified state

The dependent variables will use data from the Bureau of Labor Statistics (BLS) and Census Bureau, because, as federal agencies, standards for data are consistent between states. The BLS provides unemployment data by state for each month. This paper uses the average for a year, beginning 10 years before the passage of Oklahoma's RTW law and ending in 2012, the most recent full year for which data is available. This paper's hypothesis predicts that unemployment will not be significantly affected by the RTW law, meaning that the relationship between the four states will be relatively consistent before and after 2001. Illness and Injury rates will use data from the Survey of Occupational Illness and Injury (SOII), which began in 1996 and provides data up to 2011. Because Oklahoma did not report public-sector illnesses and injuries until 2006, only private sector data will be used. This paper expects that illnesses and injuries will be slightly higher relative to its previous relationship with the other states after 2001. Effects on income will be measured using the Census Bureau's data for median household income by state, beginning in 1991 - a decade before the RTW law - and ending in 2011, the most recent year for which data is available. This paper predicts that median household income in Oklahoma will be lower relative to other states after the RTW law is passed than it had been before.

Analysis

Oklahoma is a good subject of analysis for several reasons. Most important is that - aside from Michigan and Indiana, whose RTW laws are too recent to have had an effect - Oklahoma's law is the most recently-passed, in 2001. Louisiana was the last state before Oklahoma, in 1976. The Bureau of Labor Statistics changed the way it measures unemployment in that same year, so unemployment rates before and after Louisiana passed its law are not easily comparable. This is not a problem with Oklahoma.

This recency, as well as Oklahoma's location in the Great Plains, is helpful for another reason: it avoids the problems with analyzing the southeastern states that enacted RTW laws soon after Taft-Hartley. As mentioned in the literature review section, there are a wide variety of explanations for the rise in industry in the southeast beginning in the late 20th century. Statistically controlling for all these other explanations would be enormously difficult, if not entirely impossible. Thus, Oklahoma's position outside of the southeast, and its RTW law's enactment well after industry began to leave the northeast, means that its RTW law is easier to isolate. One might wish that other states in the column from North Dakota to Texas could be used for comparison, as they might be more demographically and economically similar to Oklahoma, but unfortunately they all passed RTW laws well before Oklahoma did. Missouri, Colorado, and New Mexico are still similar enough to be useful, though, and - given the clustered nature of RTW states - one will not find a better subject for this type of state-comparison, interrupted time series analysis.

Unemployment and income are important because they are most directly related to the claims of RTW supporters and opponents, respectively. Much of the previous research has focused on union wage premiums rather than income. This paper will study general income for two reasons. First, the union wage premium has been exhaustively studied already, so - barring some radically new and different method of analysis - further study of it is unlikely to be useful. Second, this paper is interested in very broad economic effects of RTW laws, so median household income is useful to study.

As stated previously, there is virtually no empirical research on the effects of RTW laws on worker safety - a priority of unions. Thus, studying any difference in the rates of occupational illness and injury will be perhaps this paper's most important contribution.

Figures 4-6 below show the changes in unemployment, illness and injury, and median household income in each state over time, before and after 2001, when Oklahoma passed its RTW law. The unemployment and illness and injury graphs use data from the BLS, and the median household income data is from the Census Bureau. No significant changes in Oklahoma’s numbers relative to other states’ after 2001 are immediately visible in any of the graphs. This suggests that this paper's hypothesis regarding unemployment is correct, as it is not visibly affected by the RTW law. Oklahoma does have lower unemployment after the 2008 recession relative to other states, but if this is a change from the states' previous relationships, it is likely due to other factors that determine how states are affected by economic crises. Regarding median household income and illness and injury rates, though, this paper's hypotheses appear to be incorrect, as no change relative to other states is clearly visible in either. Regression, though, will show whether statistically significant changes did in fact occur.

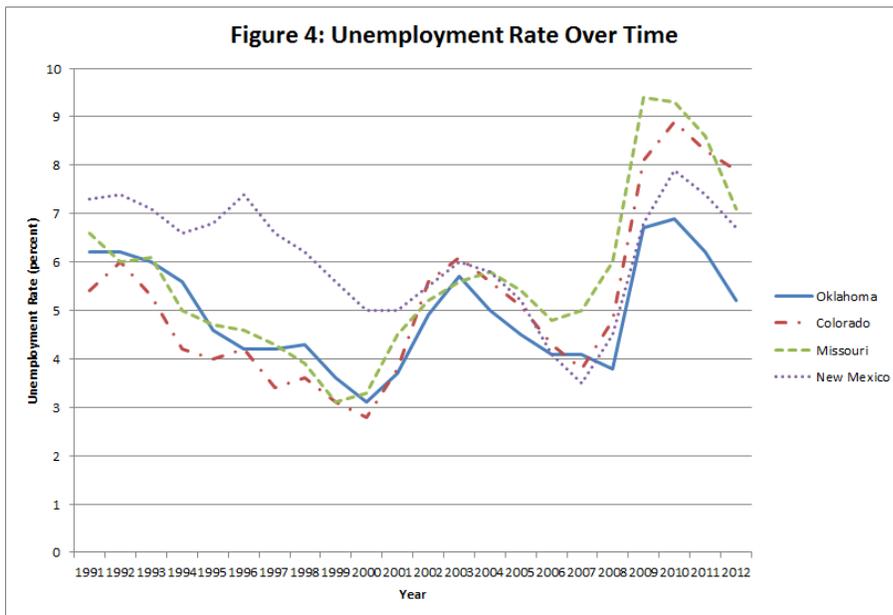


Fig. 4 charts the BLS unemployment rates for each of the four states. It begins ten years before Oklahoma’s RTW law, and ends in 2012, the most recent full year for which data is available.

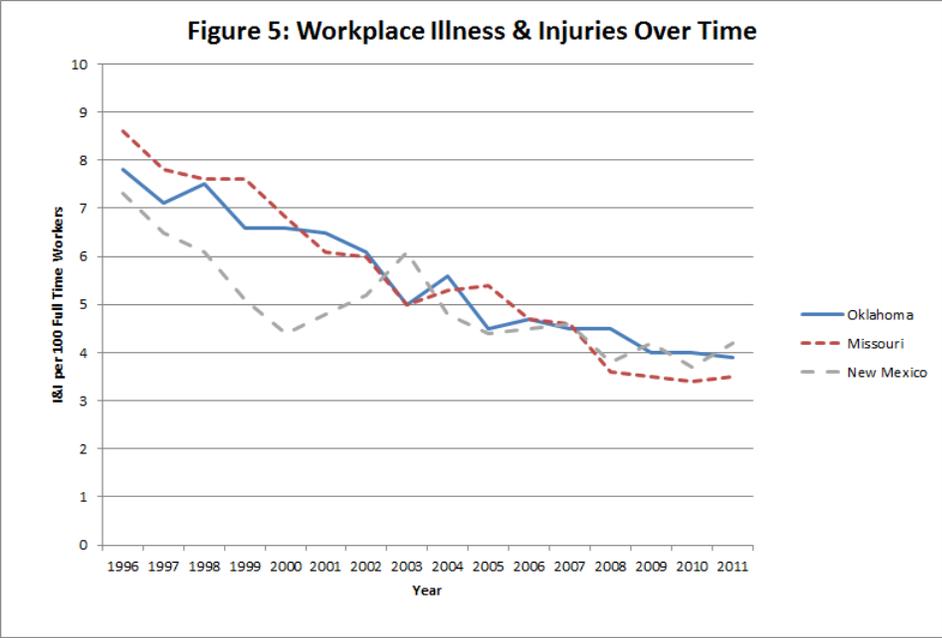


Fig. 5 charts the reported BLS Occupational Illness & Injury rates per 100 full time workers for all years available. The BLS does not provide SOII data for Colorado. Only private sector incidents are plotted, as public sector data was not reported by Oklahoma until 2006.

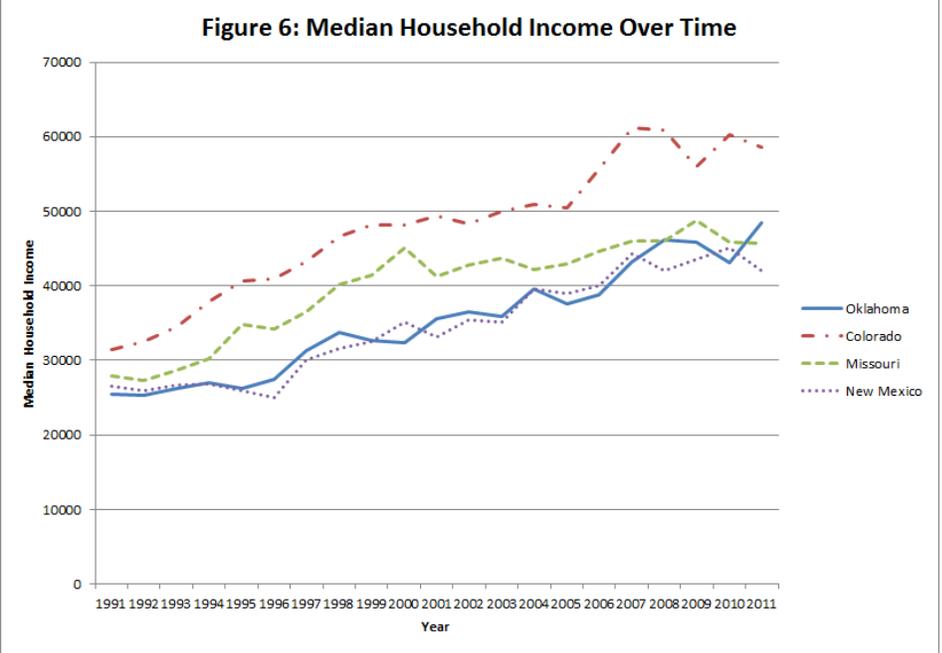


Fig. 6 charts median household income for the four states being studied, as reported by the Census Bureau.

Each of the three dependent variables - unemployment rate, median household income, and illness & injury rate - was tested with the same five ordinary least squares regression models. Model 1 uses a binary RTW dummy variable that shows the RTW law effective in Oklahoma only. It shows the control states' dummy variables and the trend variable in relation to Oklahoma's dummy variable. Model 2 is the same, but uses a counter variable instead of a binary dummy variable, again testing the RTW law for Oklahoma only. Model 3 uses both the binary and counter variables for Oklahoma's RTW law. Together, these three models will show whether any effect from the RTW law has a one-time permanent effect, or if its effects changed over time. Model 4 tests Oklahoma's data only, with all other states omitted from the regression. The trend variable and the binary and counter RTW variables are used. Lastly, Model 5 omits Oklahoma, and treats the control states *as if* they had passed RTW laws, by using both binary and counter RTW dummy variables saying that Colorado, Missouri, and New Mexico passed a RTW law in 2001. The Colorado dummy variable is omitted from the regression, so the other variables' coefficients show their baseline difference from Colorado. Models 4 and 5 will be useful in determining if any effect attributed to the RTW law is truly because of the law; if it is, the RTW coefficients in Model 5 should be close to zero, as the RTW laws are fictional for this regression. If the coefficients are not near zero, other factors must have a determinant effect on the dependent variable. Models 4 and 5 thus help to account for history effects that might otherwise lead us to incorrectly attribute too strong an effect to the RTW law.

Table 1 below shows the five regression models with the unemployment rate as the dependent variable. 22 years of data were used in these regressions. Model 1 shows that, relative to Oklahoma's dummy variable, which served as the baseline referent, the trend variable had a coefficient of 0.068, Colorado's coefficient was -0.013, Missouri's was 0.44, New Mexico's was

0.9, and the binary RTW variable for Oklahoma had a -0.48 coefficient. The trend variable's coefficient was the only statistically significant of these. It means that, for every additional year, unemployment will increase by 0.068 percent. However, this is likely because the 2008 recession occurred near the end of the dataset. The constant was 4.4, and the r-square was 0.15, meaning that only 15 percent of variance in the unemployment rate can be explained by these variables. Other factors evidently have much stronger effects on unemployment than RTW laws.

In Model 2 for the unemployment dependent variable, which again uses Oklahoma as the reference state, the constant is 4.2 and the r-square is 0.15. This means that, if all independent variables were zero, the unemployment rate would be 4.2 percent. The trend coefficient is 0.06; Colorado's coefficient is 0.23; Missouri's is 0.69; New Mexico's is 1.1; and the RTW counter variable for Oklahoma has a -0.0033 coefficient. Only Colorado's dummy variable and the RTW counter variable are statistically significant (using the common $p < 0.05$ test), and the low r-square further suggests that there is not a large effect here.

In Model 3 for the unemployment rate, which uses both a binary and counter RTW variable, the constant is 4.4 and the r-square is 0.16, meaning that only 16 percent of variation in unemployment can be explained by the independent variables in this model. The trend coefficient is 0.063; Colorado's is 0.012; Missouri's is 0.46; New Mexico's is 0.92; the binary RTW dummy is -0.99; and the RTW counter variable is 0.086. Because the binary RTW variable is negative (-0.99, meaning that a RTW law should decrease unemployment by 0.99 percent) but the counter RTW variable is positive (0.086), this suggests that an RTW law might temporarily decrease unemployment, but that this effect would vanish over time. However, the p-values for both RTW variables are above 0.05 - the generally accepted threshold for reliability - so this conclusion is not very reliable.

Model 4 for the unemployment rate, which uses only Oklahoma's data, shows a trend coefficient of -0.35; a binary RTW coefficient of 0.9; and a counter RTW coefficient of 0.5. The constant is 6.7 and the r-square is 0.56. This suggests that RTW laws increase unemployment, but because other states are not included for comparison, this may not account for the 2008 recession. The trend variable and the counter RTW variable are statistically significant, while the binary RTW variable is not.

In Model 5, which treats the control states as if they passed RTW laws in 2001, and whose coefficients are relative to Colorado's dummy variable, the trend coefficient is -0.31; Missouri's is 0.45; New Mexico's is 0.91; the binary RTW dummy's is 0.31; and the RTW counter variable's is 0.61. The constant is 6.4 and the r-square is 0.5. Recall that these dummy variables are fictional, as these are union-shop states. The trend variable, New Mexico's dummy variable, and the counter RTW variable are all statistically significant. Models 4 and 5 support the paper's hypothesis against RTW laws, because the increase in unemployment for these states (with the counterfactual RTW law) is smaller than the increase in Oklahoma. These models are also more reliable for this dependent variable, because more variables are statistically significant in these two than in models 1-3.

Table 1: RTW Laws Effect on Unemployment, 22 Years, OLS Regression					
Dependent Variable: Unemployment Rate (percentage)					
Ind. Variable	Model 1 Binary RTW	Model 2 Counter RTW	Model 3 Binary & Counter RTW	Model 4 Oklahoma Only	Model 5 Other States Only
Trend	0.068 *** 0.026 2.5	0.060 ** 0.027 2.2	0.063 ** 0.027 2.3	-0.35 *** 0.085 -4.1	-0.31 *** 0.074 -4.2
Colorado	-0.013 * 0.56 -0.022	0.23 *** 0.52 0.45	0.012 * 0.57 0.022	--	--
Missouri	0.44 * 0.56 0.77	0.69 ** 0.52 1.3	0.46 ** 0.57 0.81	--	0.45 ** 0.35 1.2
New Mexico	0.90 ** 0.56 1.5	1.1 ** 0.52 2.1	0.92 ** 0.57 1.6	--	0.91 *** 0.35 2.5
Binary RTW Oklahoma Only	-0.48 * 0.67 -0.70	--	-0.99 ** 1.0 -0.99	0.90 ** 0.66 1.3	--
Counter RTW Oklahoma Only	--	-0.0033 *** 0.083 -0.039	0.086 * 0.12 0.70	0.50 *** 0.10 4.7	--
Binary RTW Other States	--	--	--	--	0.31 * 0.57 0.54
Counter RTW Other States	--	--	--	--	0.61 *** 0.093 6.5
Constant	4.4	4.2	4.4	6.7	6.4
R-Squared	0.15	0.15	0.16	0.56	0.5
Observations (n)	88	88	88	22	66
Unstandardized coefficients are listed first, with standard error below and t-values at bottom. * = p < 0.5, ** = p < 0.2, *** = p < 0.01, One-Tail Test					

Table 2 below shows the five regression models with the median household income as the dependent variable. 21 years of data were used in these regressions. Unlike the two other dependent variables, a two-tail test was used for the p-values, because an argument could be made for an effect in either direction: this paper hypothesizes that income will be reduced because RTW laws diminish bargaining power, while RTW-proponents could argue that median

household income will increase because of new jobs that would supposedly be created due to the law. Model 1 shows that, in relation to Oklahoma's dummy variable, the trend variable had a coefficient of 1161, Colorado's coefficient was 12710, Missouri's was 4652, New Mexico's was -618, and the binary RTW variable for Oklahoma had a -0.418 coefficient. The trend's coefficient and Colorado and Missouri's dummy variable's coefficients were statistically significant. In substantive terms, for example, Colorado's coefficient means that its median household income is \$12,710 higher than Oklahoma's, at a baseline where the other independent variables are zero. The constant was 22408, and the r-square was 0.936, meaning that 93.6 percent of variance in median household income can be explained by these variables. The constant means that, if all other variables were zero, the median household income would be \$22,408 per year. The RTW dummy variable does not have a statistically significant effect in this model.

In Model 2 for the median household income, the trend coefficient is 1154; Colorado's coefficient is 12860; Missouri's is 4806; New Mexico's is -464; and the counter RTW variable for Oklahoma is 48.1. The constant is 22408 and the r-square is 0.936. The RTW counter variable does not have a statistically significant effect in this model, although all other variables except for New Mexico's dummy variable do.

In Model 3 for the median household income, the trend coefficient is 1156; Colorado's is 12730; Missouri's is 4670; New Mexico's is -592; the binary RTW dummy is -610; and the RTW counter variable is 110. The constant is 22430 and the r-square is 0.936. Because the binary RTW dummy variable has a negative coefficient but the counter RTW variable has a positive one, this suggests that a RTW would initially reduce median household income by \$610, but that the effect gradually decreases over time. However, the p-values for both RTW variables in Model 3 are large, making this conclusion tenuous.

Model 4 for the median household income, which uses only Oklahoma's data, shows a trend coefficient of 1006; a binary RTW coefficient of 63; and a counter RTW coefficient of 259. The constant is 23250 and the r-square is 0.955. The binary RTW dummy has a statistically significant effect, while the counter variable does not.

In Model 5, which treats the control states as if they passed RTW laws in 2001, and whose coefficients are relative to Colorado's dummy variable as a baseline, the trend coefficient is 1660; Missouri's is -8060; New Mexico's is -13330; the binary RTW dummy's is -1510; and the RTW counter variable's is -687. The constant is 32550 and the r-square is 0.941. All variables except the binary RTW variable have statistical significance. Models 1-3 for median household income are probably best ignored, due to the high p-values of RTW variables. Models 4 and 5 would seem to support RTW proponents, as Oklahoma has had small increases in income since 2001 while the other states have suffered. However, problems with this interpretation will be discussed in the conclusion.

Table 2: RTW Laws Effect on Median Household Income, 21 Years, OLS Regression					
Dependent Variable: Median Household Income					
Ind. Variable	Model 1 Binary RTW	Model 2 Counter RTW	Model 3 Binary & Counter RTW	Model 4 Oklahoma Only	Model 5 Other States Only
Trend	1160 ### 47.6 24.3	1150 ### 48.3 23.8	1150 ### 49 23.5	1006 ### 186 5.4	1660 ### 149 11.1
Colorado	12700 ### 953 13.3	12800 ### 882 14.5	12700 ### 959 13.2	--	--
Missouri	4650 ### 953 4.87	4806 ### 882 5.44	4670 ### 959 4.87	--	-8060 ### 726 -11
New Mexico	-618 # 9530 -0.649	-464 # 882 -0.525	-592 # 959 -0.617	--	-13300 ### 726 -18.3
Binary RTW Oklahoma Only	-0.418 # 1150 -0.000360	--	-610 # 1730 -0.35	63 ### 1470 0.042	--
Counter RTW Oklahoma Only	--	49.1 # 155 0.316	110 # 233 0.471	259 # 246 1.05	--
Binary RTW Other States	--	--	--	--	-1510 ## 1180 -1.27
Counter RTW Other States	--	--	--	--	-687 ### 198 -3.47
Constant	22408	22330	22430	23250 ##	32550
R-Squared	0.936	0.936	0.936	0.955	0.941
Observations (n)	84	84	84	21	63
Unstandardized coefficients are listed first, with standard error below and t-values at bottom. #=p<1, ##=p<0.5, ###=p<0.01, Two-Tail Test					

Table 3 shows the five regression models with the illness and injury rate per 100 full-time workers as the dependent variable. 16 years of data were used in these regressions. Data for Colorado was unavailable for this dependent variable, so its dummy variable is not included as an independent variable in these regressions. Model 1 shows that, in relation to Oklahoma's dummy variable, the trend variable had a coefficient of -0.26, Missouri's was -0.092, New

Mexico's was -0.7, and the binary RTW variable for Oklahoma had a -0.18 coefficient. The trend variable and the RTW dummy were statistically significant. The constant was 7.9, and the r-square was 0.84, meaning that 84 percent of variance in the illness & injury rates can be explained with these independent variables, and that if all independent variables were zero, there would be 7.9 occupational illnesses and injuries for every 100 full-time workers in a year. The trend variable and the RTW dummy variable are statistically significant, while the state dummy variables are not.

In Model 2 for the illness and injury rate, the constant is 7.8 and the r-square is 0.84. The trend coefficient is -0.26; Missouri's is 0.026; New Mexico's is -0.58; and the counter RTW variable for Oklahoma is -0.0027. In this model, too, the trend variable and RTW counter variable are statistically significant, but not the state dummy variables.

In Model 3 for the illness and injury rate, the constant is 7.91 and the r-square is 0.84. The trend coefficient is -0.26; Missouri's is -0.076; New Mexico's is -0.68; the binary RTW dummy is -0.29; and the RTW counter variable is 0.021. These three models suggest, puzzlingly, that the occupational illness and injury rate temporarily decreases (by .29 incidents for each 100 workers), but that this effect disappears, and that incidents will later increase (by 0.21 incidents for 100 workers for every year after the RTW law is passed). However, neither of the RTW variables have a statistically significant effect in this model, so this conclusion is unreliable.

Model 4 for the illness and injury rate, which uses only Oklahoma's data, shows a trend coefficient of -0.29; a binary RTW coefficient of -0.24; and a counter RTW coefficient of 0.048. The constant is 7.9 and the r-square is 0.94. No variable has a statistically significant effect.

In Model 5, which treats the control states as if they passed RTW laws in 2001, and whose coefficients are relative to Missouri's dummy variable, the trend coefficient is -0.55; New

Mexico's is -0.61; the binary RTW dummy's is 0.25; and the RTW counter variable's is 0.32. The constant is 8.7 and the r-square is 0.83. Only the trend variable and New Mexico's dummy are statistically significant. Thus, Models 4 and 5 also show very odd results for this dependent variable, suggesting that RTW laws slightly decrease the rate of occupational illness and injury. However, the lack of significance for the RTW variables in both models discourages faith in this conclusion.

Table 3: RTW Laws Effect on Illness & Injury Rate					
16 Years, OLS Regression					
Dependent Variable: Illness & Injury Rate per 100 full-time workers					
Ind. Variable	Model 1	Model 2	Model 3	Model 4	Model 5
	Binary RTW	Counter RTW	Binary & Counter RTW	Oklahoma Only	Other States Only
Trend	-0.26 *** 0.019 -13	-0.26 *** 0.021 -12	-0.26 *** 0.021 -12	-0.29 ** 0.11 -2.6	-0.55 *** 0.13 -4.02
Missouri	-0.092 * 0.30 -0.30	0.026 * 0.27 0.096	-0.076 * 0.31 -0.24	--	--
New Mexico	-0.70 ** 0.30 -2.2	-0.58 ** 0.27 -2.1	-0.68 ** 0.31 -2.1	--	-0.61 *** 0.21 -2.8
Binary RTW	-0.18 *** 0.34 -0.55	--	-0.29 * 0.45 -0.65	-0.24 * 0.35 -0.68	--
Counter RTW	--	-0.0027 *** 0.044 -0.06	0.021 * 0.058 0.37	0.048 * 0.11 0.41	--
Binary RTW	--	--	--	--	0.25 * 0.43 0.58
Counter RTW	--	--	--	--	0.32 ** 0.14 2.3
Constant	7.9	7.8	7.9	7.9	8.7
R-Squared	0.84	0.84	0.84	0.94	0.83
Observations (n)	48	48	48	16	32
Unstandardized coefficients are listed first, with standard error below and t-values at bottom.					
* = p < 0.5, ** = p < 0.2, *** = p < 0.01, One-Tail Test					

Conclusion

This paper sought to determine whether the presence of "right-to-work" laws could partially explain the variance in unemployment, median household income, and workplace illness and injury rates between states. Supporters and opponents of RTW laws have thought that the laws could affect economic metrics like these. Supporters say they will lower unemployment by attracting new businesses, which could increase income. Opponents say a decrease in unemployment will be minimal or nonexistent, and that the lower wages of non-union workers mean median income will fall, or increase more slowly. This paper's hypothesis adopted the view of RTW opponents: median household income will suffer, and unemployment will barely change or be unaffected. This paper also hypothesized that occupational illnesses and injuries could increase after a RTW law. These hypotheses were tested with an interrupted time-series analysis, examining Oklahoma before and after its 2001 RTW law in comparison with its union-shop neighbors: Colorado, Missouri, and New Mexico. The paper used five least squares regression models to most thoroughly analyze the data. Three analyzed all the states, with Oklahoma's RTW law measured by a binary dummy variable, a counter variable, or both. The fourth regression included only Oklahoma's data, and the fifth regression measured the other states *as if* they had passed RTW laws in 2001. The regressions were repeated for all three dependent variables. The results were mixed. Regarding unemployment, the first three models suggested that RTW laws slightly decreased unemployment initially, but that this effect vanished over time, as shown by the counter variable. The second two models support this paper's hypothesis, because unemployment increases more in Oklahoma (0.9 percent per year with a binary variable, and 0.5 percent per year with a counter variable) than in the other states after 2001 (0.31 percent and 0.61 percent annually, with, respectively, a binary and counter variable.) The second two models are probably a more useful measure in this instance, as they better account for history

effects by showing the union-shop states' changes after 2001, which suggest that RTW laws are not useful in reducing unemployment, and possibly even worsen it. The regressions for median household income were also mixed. The first three models suggest that median household income initially decreased in Oklahoma after the RTW law, but increased as time passed. The large p-values for these models cast doubt on this conclusion, though. The fourth and fifth models seem to support RTW proponents. Median household income increased by \$63 (binary RTW) or \$259 (counter RTW) annually in Oklahoma after 2001, but decreased by \$1510 (binary) or \$697 (counter) annually in the other states. However, this should not be immediately taken as evidence to support RTW laws. The 2008 recession, which affects the last four years of the dataset, could have a confounding effect on this dependent variable especially. Perhaps Oklahoma has other unique features aside from a RTW law that make it weather recessions more easily. More research must be done into possible history effects. Finally, the illness and injury rate regressions are very puzzling: they show Oklahoma's law decreasing the state's illness and injury rate, while other states' rates increase relative to Oklahoma's. It is difficult to theoretically account for this; I am not aware of any RTW supporters who have claimed that the laws will make workplaces safer. Perhaps these regressions are best ignored: the sample size is much smaller than for the other dependent variables (because the Bureau of Labor Statistics only provides illness and injury data back to 1996, and not for Colorado) and the p-values for the RTW variables are quite high. Thus, these results tentatively support this paper's hypothesis regarding unemployment, but not its hypotheses regarding median household income or occupation illness and injury rates.

As shown by the literature review, this paper is hardly alone in having highly mixed and inconclusive results. It is useful, though, in employing a new method of analysis - a state-

comparison interrupted time-series - to address this question. None of the cited literature used this specific method. Although this paper's results for illness and injury rates were likely an aberration, it is also useful to introduce analysis of this dependent variable into scholarly study of RTW laws, as it is not addressed by most literature. The results for median household income, while appearing to support RTW supporters' hypothesis, are useful in highlighting the need for more research into how RTW states are affected by abnormal economic conditions like recessions, and for more research into history effects.

RTW laws have renewed importance in policy, considering Michigan and Indiana's recently-passed laws. These laws were largely inspired by high unemployment after the 2008 recession. This paper shows, though, that the pro-RTW claim that the laws will lower unemployment by attracting new jobs is dubious. Encouraging job growth should certainly be a top priority of state governments in this post-recession era, but these results show that states considering passing RTW laws to accomplish this would do better to look elsewhere for policy solutions.

A major limitation of this paper - in fact, of nearly all studies of RTW laws - is that Oklahoma, while certainly having organized labor, was never especially noteworthy as a union stronghold. The same is true of most RTW states. This makes it difficult to confidently attribute changes in economic variables as being primarily caused by RTW laws. The three states used for comparison, while having many similarities, are also not perfect analogues for Oklahoma. We might wish that Nebraska and Kansas could be used instead, but their RTW laws make this impossible. RTW laws are mostly concentrated in blocs, which is problematic when trying to account for regional differences in economic and demographic structure. Additionally, the dataset, especially for illness and injury rates, does not include as many years as would be ideal.

This is difficult to address: data before the RTW is limited by availability, and data after the law is limited to the number of years that have passed. A researcher who located more past data and waited several years for more future data might have more conclusive results. The limited scope of this paper also means that history is not as fully accounted for as many would desire; this is a particular problem with the median household income regressions, which demand more research into the recession's varying effects between states.

This paper suggests many possibilities for future research. RTW effects on occupational illness and injury should be further studied. A study with a larger dataset would be useful, as would a study using data that predates OSHA, which might have reduced unions' role in ensuring safety. Relatedly, the percentage of workers with employer-provided health insurance would be a useful dependent variable to study in relation to RTW laws. A study similar to this one that more fully accounts for history effects on median household income should also be performed.

Although the income regression seems to support RTW laws, it is very difficult to believe that an effect that large could be caused by RTW laws alone. Different states are likely more- or less- effected by economic crises, and this *must* be accounted for before these results are used as evidence in favor of RTW laws. Finally, although Michigan's new RTW law is probably not the best public policy, it could (if not repealed) solve the problem of RTW states being places without strong histories of organized labor. If someone in 10 to 15 years were to conduct an analysis very similar to this one, but instead comparing Michigan with union-shop states in the Great Lakes region like Ohio, Wisconsin, Illinois, and Pennsylvania, it may prove to be one of the most valuable research papers in all the RTW literature. Thus, I believe the best research in this important field is yet to come.

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