How Right-to-Work Laws Affect Wages

W. ROBERT REED*
University of Oklahoma, Norman, OK 73019

I examine the wage effects of Right-To-Work (RTW). Using state-level data, I estimate that, ceteris paribus, RTW states have average wages that are significantly higher than non-RTW states. This result is robust is across a wide variety of specifications. An important distinctive of this study is that it controls for state economic conditions at the time states adopted RTW. States that adopted RTW were generally poorer than other states. Failure to control for these initial conditions may be the reason that previous studies have not identified a positive wage impact for RTW.

I. *Introduction*

In September 2001, Oklahoma voters made that state the nation's twenty-second "right-to-work" (RTW) state. One of the most hotly contested claims in the weeks leading up to that election was how RTW affected wages. RTW proponents claimed that RTW would increase wages in the state; opponents argued the opposite. The conventional wisdom from the economics literature, typified by the following quote from Moore (1998, p. 445), is that RTW has had little effect on wages.[1]

> "I review the recent literature on the determinants and effects of right-to-work (RTW) laws. The focus is primarily on the econometric studies published since the early 1980s. Five major areas of impact are assessed: unionization, free riding, union organizing activities and successes in NLRB elections, wage structure, and state industrial development. While individual findings are quite sensitive to model specification, *the accumulated evidence indicates that RTW laws have at least a significant short-run impact on all of these areas except perhaps wages*."

This result is somewhat surprising because previous research has identified a number of other effects of RTW that have been significant: RTW has been estimated to significantly affect union organizing activities, plant location decisions, manufacturing employment, and the rate of business formation.[2] Strictly speaking, the wage research is not inconsistent with this other research, since the theoretical impact of RTW on wages is ambiguous. However, the lack of a significant impact on wages stands out from RTW research in other areas.

I use state-level data to investigate the relationship between RTW and wages. My work differs from previous research in a number of respects. Most importantly, I control for the influence of past economic conditions on states' current wages. While some convergence in states' incomes has occurred (Bernat, 2001), the economic past still casts a long shadow on the economic present. I show that once one controls for the influence of economic conditions at the time states originally adopted RTW, a positive and significant impact of RTW is identified.

Section II provides a framework for organizing existing theoretical and empirical research on the consequences of RTW for wages. Section III presents my empirical findings; Section IV provides explanations for reconciling my research with previous studies; and Section V concludes.

II. *A Review and Synthesis of the RTW Literature Pertaining to RTW's Effect on Wages*

A Framework for Analyzing the Overall Effect of RTW on Wages. Although there exists a large literature on the general subject of RTW and wages, most of this research does not directly address RTW's overall impact on wages.

Let the average wage in a state be given by $\overline{w}$,

$$\overline{w} = w_U S_U + w_N S_N , \tag{1}$$

where w and S represent the average wage and percentage share of total employment in the union (*U*) and nonunion (*N*) sectors. By making use of the fact that $S_U + S_N = 1$, it is straightforward to demonstrate that the effect of RTW on the average wage in a state can be represented by the equation,

$$\partial\overline{w}/\partial RTW = [S_U \ \partial(w_U - w_N)/\partial RTW] + [(w_U - w_N)\partial S_U/\partial RTW] + \partial w_N/\partial RTW . \tag{2}$$

Equation (2) decomposes the overall effect of RTW on wages into three separate components. The first component, $S_U \ \partial(w_U - w_N)/\partial RTW$, is how RTW affects the union wage premium. Ceteris paribus, an increase (decrease) in the union wage premium results in an increase (decrease) in overall wages. The second component, $(w_U - w_N)\partial S_U/\partial RTW$, is how RTW affects the size of the unionized sector. Assuming that $w_U > w_N$, an increase (decrease) in the size of the unionized sector will, ceteris paribus, result in an increase (decrease) in overall wages. The last component, $\partial w_N / \partial RTW$, is how RTW affects wages in the nonunion sector. Ceteris paribus, an increase (decrease) of wages in the nonunion sector will result in an increase (decrease) in overall wages.

Theoretical Predictions of How RTW Affects Each of the Three Components. RTW may encourage free riding by workers since employees can benefit from union-negotiated wage gains without paying dues. With fewer financial resources and a smaller membership, unions may be less successful in pressuring employers for increased wages. With respect to the union wage premium component, this reasoning predicts that $S_U \ \partial(w_U - w_N)/\partial RTW < 0$. On the other hand, RTW forces unions to recruit members. One way that unions can demonstrate their overall value to potential members is by securing tangible benefits, such as large wage gains. Accordingly, some argue that RTW might encourage unions to be more aggressive in negotiating wage increases, so that $S_U \ \partial(w_U - w_N)/\partial RTW > 0$. Thus, theory is ambiguous about the sign of $S_U \ \partial(w_U - w_N)/\partial RTW$. More detailed discussions, as well as other theories, concerning the hypothesized effects of RTW on the union wage premium are found in Gallaway (1966), Bennett and Johnson (1980), Wessels (1981), Farber (1984), Moore and Newman (1985), Garofalo and Malhotra (1992), and Moore (1998).

There are several possible avenues through which RTW may affect the share of workers employed in the unionized sector of the economy, S_U. As discussed above, RTW influences both the incentives and the ability of unions to negotiate benefits. In turn, the size of union-associated benefits affects workers' demand for unionized employment. In addition, RTW raises the cost to unions of both gaining and keeping members. Most researchers conclude that, overall, RTW theoretically reduces the strength of unions and the size of the union sector.[3] If RTW results in a reallocation of workers from the relatively high-paying union sector to the relatively low-paying nonunion sector, the prediction for the second component is $\left(w_U - w_N\right) \partial S_U / \partial RTW < 0$. Ceteris paribus, the result will be a decrease in the state's average wages. Further discussions of this effect, and other relevant theories, are found in Carroll (1983), Elliot and Huffman (1984), Farber (1984), Moore and Newman (1985), Ellwood and Fine (1987), Garofalo and Malhotra (1992), and Moore (1998).

Union activity should have "crowding in" and "threat" effects on the nonunion wage level, w_N. A potential consequence of aggressive union behavior is that unionized firms will decrease employment which results in more workers seeking employment in the nonunion sector, depressing wages there (the "crowding in" effect). To the extent that RTW curbs aggressive union behavior, the prediction for the third component is $\partial w_N / \partial RTW > 0$. Others have argued that aggressive union behavior causes nonunion firms to raise wages in order to provide less incentive for their workers to organize (the "threat" effect). If RTW caused unions to behave less aggressively, nonunion firms might not have the same incentive to increase wages, and $\partial w_N / \partial RTW < 0$. Another argument is that RTW may encourage the creation and in-migration of new businesses, which increases the general demand for labor, so $\partial w_N / \partial RTW > 0$. Thus, theory is ambiguous about the sign of RTW's impact on the nonunion wage. Cobb (1982), Schmenner (1982), Newman (1984), Neumark and Wachter (1995), and Moore (1998) discuss these issues in greater detail.

Empirical Research Pertaining to the Effect of RTW on Wages. Table 1 uses the preceding framework to relate previous empirical research to RTW's impact on wages. I identified only two studies that directly estimate the effect of RTW on a representative measure of average wages ($\partial \overline{w} / \partial RTW$). Both Moore (1980) and Farber (1984) use nationally representative micro-data sets (Michigan Income Dynamics Panel and the Current Population Survey, respectively) to estimate the relationship between individuals' wages and residence in a RTW state. Both find negative effects, though only Farber's estimate achieves significance.[4]

Far more common is the estimation of RTW's effect on the wages of manufacturing production workers, represented in Table 1 by $\partial \tilde{w} / \partial RTW$. Because these studies oversample the union sector and because wages in the union and nonunion sectors may respond differently to RTW, these studies are of limited value in determining the overall impact of RTW on wages. Most of these studies estimate a negative effect for RTW. Only Carroll (1983) reports a statistically significant effect.[5]

Other studies reported in Table 1 pertain to how RTW affects the three individual components. With respect to how RTW affects the union wage premium, the empirical literature is mixed. Only Farber (1984) reports a statistically significant effect.

A large number of studies estimate the relationship between RTW and union membership. As the quote from Moore in Table 1 states, the empirical literature indicates that RTW negatively affects union membership. It bears emphasizing, however, that union membership is not synonymous with the number of workers in the unionized sector. For example, RTW could increase the number of workers who free ride. This would reduce union membership but not the number of workers receiving union wages. It is the latter element that is relevant for the overall impact of RTW on wages. Thus, while the literature indicates a negative effect of RTW on union membership, it is unclear how to interpret this with respect to RTW's effect on wages.

Finally, I found only two studies that estimate RTW's impact on nonunion wages. Both the Moore (1980) and Farber (1984) studies report a negative impact, though only Farber finds a significant effect.

In summary, these findings generally corroborate Moore's conclusion: "Theory does not indicate how RTW laws affect wages. The empirical evidence accumulated in the 1970s and 1980s indicates that RTW laws do not have strong lasting effects on wages. Most researchers find that RTW laws have no impact on union wages, nonunion wages, or average wages in either the private or public-sector" (1998, p. 460). My only qualification is that only two studies directly estimate RTW's effect on overall wages.

III. *Empirical Investigation of the Effect of RTW on Wages*

Sample Selection and Methodology. I estimate the difference in current wages between RTW and non-RTW states, controlling for initial economic conditions that existed at the time of adoption. My dependent variable is *LNW2000*, the log of average wages in 2000, defined as "Wage and Salary Disbursements by place of work" divided by "Wage and Salary Employment by place of work."[6]

A complication immediately arises in selecting data to measure "initial economic conditions." RTW laws were not all adopted in the same year. Years of adoption range from 1944 to 2001, with 1947 being the modal year.[7] I use two different years for determining "initial economic conditions." 1945 is prior to the adoption of RTW by most of the states in the sample and thus provides a "before RTW" snapshot of economic conditions. However, 1950 is closer to the adoption dates of the RTW states used in my sample.[8] The results using 1945 as the benchmark year are more conservative and are reported herein. The 1950 results are presented in a supplementary Appendix that may be downloaded from the internet.[9]

Wyoming (1963), Louisiana (1976), Idaho (1986) and Oklahoma (2001) adopted RTW significantly after 1945/1950. I handle these states as follows: I count Oklahoma as a non-RTW state because it adopted RTW after the end of the sample period. I drop Idaho and Louisiana because they changed their RTW status in the middle of the sample period. Wyoming is a more difficult call. I decide to omit Wyoming from the subsequent analysis.[10] Finally, I eliminate Alaska and Hawaii, as is common in state-level analyses. Accordingly, my sample consists of 45 observations: 18 RTW states and 27 non-RTW states.[11] The following preliminary results motivate the importance of controlling for initial economic conditions.[12]

Preliminary Results. A simple regression of the log of 2000 average wages on a RTW dummy variable results in the following estimated equation[13]:

$$LNW2000 = 3.5044 - \underset{(-3.21)}{0.1380}\, RTW \tag{3}$$

$R^2 = 0.1698$, $n = 45$.

The *RTW* coefficient is large, negative, and statistically significant (the *t*-statistic is reported in parentheses below the coefficient and is based on robust (White) standard errors). This simple regression has a relatively low R^2 of 0.1698. Thus, without controlling for the influence of other variables, RTW is negatively and significantly associated with lower wages.

RTW states, however, are not a random sample of all states. In particular, RTW states were generally poorer than other states when they adopted RTW. Thus, at the very least, empirical analysis of the effect of RTW on wages should control for this "initial condition." Unfortunately, data limitations make it impossible to calculate state-level average wages prior to 1958.

My solution is to proxy average wages with Per Capital Personal Income (PCPI), which is available for 1945. As evidence of the value of this proxy, I note that for years in which both average wages and PCPI are available, simple correlations between the two variables are typically about 0.90. Accordingly, my first attempt to control for initial economic conditions is to add the log of the state's PCPI in 1945 (*LNP1945*) to equation (3). The corresponding regression results are reported below.

$$\underset{}{LNW2000} = 0.2458 - \underset{(-0.80)}{0.03343\, RTW} + \underset{(6.13)}{0.4577\, LNP1945} \tag{4}$$

$R^2 = 0.4737$, $n = 45$.

Equation (4) yields two insights with respect to estimating RTW's effect on wages. First, it provides evidence that initial economic conditions are an important determinant of average wages over 50 years later. States with higher (lower) incomes in 1945 had higher (lower) average wages in 2000. The relative importance of initial economic conditions is attested by the fact that the R^2 increases from 0.1698 to 0.4737 when *LNP1945* is added.

Second, it suggests that the negative effect of RTW reported in previous empirical studies may be due to the omission of initial economic conditions. A comparison of equations (3) and (4) shows that the inclusion of *LNP1945* causes the coefficient on the RTW dummy to become substantially less negative and statistically insignificant.

This preliminary analysis demonstrates the importance of controlling for the influence of states' economic conditions at the time they adopted RTW. The next step is to expand the list of control variables to account for other possible determinants of states' average wages in 2000.

Final Results. I now consider the following control variables for possible inclusion in the empirical analysis:

> *TEMP*: a variable that measures the state's average annual temperature to address concerns that the introduction of air conditioning after 1945 may have had a nonrandom effect on RTW states.[14]
>
> *FARM1945*: a variable that measures farming's share of total state earnings in 1945 to address concerns that agriculture-specific demand and supply factors may have had a nonrandom effect on RTW states.[15]
>
> *MANU1945*: A variable that measures manufacturing's share of total state earnings in 1945 to address concerns that factors specific to demand and supply in the manufacturing sector may have had a nonrandom effect on RTW states.[16]
>
> *DENS1945*: a variable that measures the state's population density in 1945 to address concerns that the influence of rural/urban development factors may have disproportionately affected RTW states.
>
> *SOUTH*: a dummy variable that identifies southern (Confederacy) states.
>
> *EDUC1945*: a variable that measures state-level educational attainment in 1945. Education is widely viewed to be an important contributor to economic growth.[17]
>
> *RTW*LNP1945*: an "interaction" term generated by multiplying the *RTW* dummy variable by *LNP1945*. This interaction allows RTW to have a different effect depending on whether the state was relatively poor or rich at the time it adopted RTW.

The subsequent empirical exercise consists of identifying the best equation for "forecasting" a state's average wages in 2000, given that state's initial economic conditions in 1945.

At this point, model specification becomes a concern. Moore et al. (1986) demonstrate the importance of model specification in estimating the impact of RTW. Accordingly, I use a computer algorithm for selecting the "best" model specifications. This both (i) alleviates concern that model selection is biased by my personal policy (or other) preferences and (ii) provides a way to confirm the robustness of my empirical results.

I use both the Akaike Information Criterion (AIC) and the Schwarz Information Criterion (SIC) to determine model specifications. Both are widely employed in model selection (Grasa, 1987; Diebold, 2001). The seven variables above, together with the variables *RTW* and *LNP1945*, produce a total of 511 possible variable combinations. I estimate all possible variable combinations, calculate the corresponding AIC and SIC values, and then choose the ten best models as determined by lowest AIC/SIC value.

Table 2 presents the empirical results. The top half of the table identifies the 10 model specifications with the lowest AIC values. All ten of these models include the variables *RTW*, *RTW*LNP1945*, *LNP1945*, and *FARM1945*. The other five variables, *DENS1945*, *EDUC1945*, *MANU1945*, *SOUTH*, and *TEMP* each appear in at least one of these models.

The bottom half of Table 2 presents the 10 model specifications that produce the lowest SIC values. There are many similarities. The top six SIC models are identical to the top 6 AIC models, and follow in the same order. However, four of the SIC models (the ones with the highest SIC values) omit the variables *RTW* and *RTW*LNP1945*.

I proceed by determining the estimated effect of RTW on wages for each of these model specifications. The estimated effect is calculated by

$exp\left(\hat{b}_{RTW} + \left(\hat{b}_{RTW*LNP\,1950} \times LNP1945\right)\right) - 1$, where *LNP1945* is set equal to the mean value of *LNP1945* for the 18 RTW states in the sample (=6.891345). The variables *RTW* and *RTW*LNP1945* are added to the four model specifications in which these variables were not originally included (SIC models #7, #8, #9, and #10). For each model specification I also perform a Wald test of the null hypothesis that the RTW effect on wages is zero. These results are reported in Table 3.

The inclusion of additional, initial economic-condition variables dramatically changes the estimate of RTW's effect on wages. In every specification, the estimated effect is positive and reasonably large in absolute value, ranging from 6.64 to 8.35 percent. Seven of the ten estimated RTW effects are significant at the 10 percent level.

The results based on 1950 as the benchmark year are even more striking. The corresponding RTW estimates range from 8.42 to 11.40 percent.[18] All but one of the RTW effects are significant at the 10 percent level. These results are strongly consistent with those of equation (4): They suggest that the negative RTW effect estimated by previous studies reflects omitted variable bias caused by failing to control for economic conditions at the time states adopted RTW.

The next step pursues this subject further by examining the best model--as determined by both the AIC and the SIC--in greater detail. The corresponding regression is:

$$\begin{aligned} LNW2000 = -0.6852 + \underset{(3.58)}{3.0546}\, RTW - \underset{(-3.58)}{0.4322}\, RTW*LNP1945 \\ + \underset{(5.84)}{0.5989}\, LNP1945 - \underset{(-5.08)}{0.006804}\, FARM1945\,; \end{aligned} \tag{5}$$

$R^2 = 0.7555$; $AIC = 0.0082158$; $n = 45$.

Estimated Effect = 7.93%.

Wald Test of $H_0 : \boldsymbol{b}_{RTW} + \left(\boldsymbol{b}_{RTW*LNP\,1945} \times LNP1945\right) = 0$:
$\boldsymbol{c}^2 = 3.539047$ (p-value=0.0599)

RESET Specification Test $(\hat{y}^2, \hat{y}^3)$: $\boldsymbol{c}^2 = 4.369863$ (p-value=0.1125).
RESET Specification Test $(\hat{y}^2, \hat{y}^3, \hat{y}^4)$: $\boldsymbol{c}^2 = 6.692562$ (p-value=0.0824).

Equation (5) shows that an important determinant of states' average wages in 2000 is the size of their agricultural sector in 1945 (*FARM*). Ceteris paribus, states with a historical dependence on agriculture have lower current wages. This has an important consequence for estimates of the effect of RTW on wages. RTW states are disproportionately states that have had large agricultural sectors. Accordingly, failure to control for this influence will cause studies to incorrectly attribute the lower current wages of RTW states to their RTW status, instead of their agricultural legacy.

Once initial economic conditions are taken into account, the model specification above estimates that 2000 average wages are 7.93 percent higher in RTW states compared to non-RTW states. The null hypothesis that the RTW effect equals zero is rejected at the 10 percent level.

A diagnostic check of equation (5) produces good news and bad news. On the positive side, the associated R^2 is high, with approximately 75 percent of the variance in states' 2000 average wages "explained" by the model. On the negative side, there is some concern that misspecification may be a problem. One of the RESET specification tests rejects the null hypothesis of no misspecification at the 10 percent level.

Because of concern with misspecification, I repeat the analysis above, replacing *LNP1945* with the untransformed value of state PCPI in 1945 (*PCPI1945*). The resulting sets of "Top 10" model specifications chosen by the AIC and the SIC were both (i) very similar to each other (again), and (ii) very similar to the models reported in Table 2. The best model--again chosen separately by both the AIC and the SIC--substitutes *PCPI1945* for *LNP1945* in equation (5). The corresponding regression results are reported in equation (6).

$$LNW2000 = 2.9344 + \underset{(3.64)}{0.4346}\, RTW - \underset{(-3.78)}{0.000367} RTW * PCPI1945 \qquad (6)$$

$$+ \underset{(7.30)}{0.000511}\, PCPI1945 - \underset{(-5.03)}{0.006613}\, FARM1945\,;$$

$R^2 = 0.7641;$ $AIC = 0.0079277;$ $n = 45.$

Estimated Effect = 6.68%.

Wald Test of $H_0 : \boldsymbol{b}_{RTW} + \left(\boldsymbol{b}_{RTW*PCPI1945} \times PCPI1945\right) = 0$ **:**
$\boldsymbol{c}^2 = 3.000343$ (*p*-value=0.0832)

RESET Specification Test $(\hat{y}^2, \hat{y}^3)$: $\boldsymbol{c}^2 = 2.214229$ (*p*-value=0.3305).
RESET Specification Test $(\hat{y}^2, \hat{y}^3, \hat{y}^4)$: $\boldsymbol{c}^2 = 3.323023$ (*p*-value=0.3444).

The specification tests look much better. The *p*-values associated with the tests of the "no misspecification" null hypothesis increase from 11 and 8 percent in equation (5), to 33 and 34 percent in equation (6).

In addition, a histogram of the residuals suggests that they are normally distributed. The Jarque-Bera test of normality in the residuals produces a *p*-value of 39 percent. These results provide evidence that equation (6) is robust and not subject to disproportional influence from one or a few outlying observations (states).

Equation (6) is my "final equation." My corresponding best estimate is that--holding constant economic conditions in 1945--average wages in 2000 are 6.68 percent higher in RTW states than non-RTW states.[19] This difference is significant at the 10 percent level. It should also be noted that equation (6), like equation (5), estimates a statistically significant coefficient for the interaction variable *RTW*PCPI1945 (t*-value = -3.78). This suggests that not all states fared equally well under RTW. The negative sign of this coefficient indicates that the gain associated with being a RTW state was greatest for those states that were the poorest in 1945.

The state-specific estimates of the RTW wage effects are (in descending order): Mississippi (22.6 percent), Arkansas (17.7 percent), South Carolina (17.1 percent), Alabama (15.8 percent), North Carolina (14.2 percent), Georgia (11.8 percent), Tennessee (10.5 percent), Virginia (8.9 percent), North Dakota (5.2 percent), Texas (4.7 percent), South Dakota (3.6 percent), Iowa (3.4 percent), Utah (2.3 percent), Arizona (2.2 percent), Kansas (0.7 percent), Florida (0.3 percent), Nebraska (-0.1 percent), Nevada (-14.5 percent).[20] Sixteen of the eighteen states are estimated to have higher 2000 average wages as a result of their RTW status. Nevada's negative estimated effect is due to its unusually large state income in 1945: Nevada's PCPI in 1945 was $1,611, the second largest value in the sample (New York had a PCPI in 1945 of $1,644). This compares to an average PCPI for the RTW states of $1,007, and an average PCPI value of $1,157 for all the states in the sample. The fact that all the RTW states--save two--have positive estimated wage effects is further evidence that these results are not driven by the peculiar circumstances of a small subset of observations (states).

IV. *Reconciling with Previous Empirical Work and Addressing Potential Concerns*

My finding of a positive and statistically significant RTW effect on wages contradicts previous empirical research. Section II suggests one explanation for this difference: Namely, that the discrepancy may be overdrawn since few studies directly estimate the overall impact of RTW on wages. To the best of my knowledge, Moore (1980) and Farber (1984) are the only researchers who do this. Accordingly, they deserve a closer look. Table 4 reports some relevant details.

Both studies use nationally representative micro-data sets from different, but near, time periods (1970 for Moore, 1977 for Farber). While both studies report negative RTW effects on wages, Moore's estimate of –3.71 percent is substantially less negative than Farber's estimate of –6.90 percent. Note that Moore's study includes far greater control for a state's location, occupation, and industry characteristics. This is reflected in the larger R^2 value of his equation.

My empirical results suggest that as important state characteristics are included in the regression equation, the estimated RTW effect on wages will become less negative/more positive (cf. equations (3) and (4)). The fact that Moore estimates a smaller negative RTW effect than Farber is consistent with this.

Neither Moore (1980) nor Farber (1984) controls for economic conditions that were present at the time that RTW was adopted by the states. Thus neither study is in direct conflict with my findings. Had Moore and Farber included controls for states' initial economic conditions, they too may have found that RTW laws have a positive effect on wages. The same analysis applies to the other studies in Table 1 that estimate a RTW effect on various components of wages.

Interestingly, the relationship between initial economic conditions and RTW adoption has received attention in the literature. For example, Moore (1998, p. 449) notes, "Poorer states (measured in terms of economic development, wages, or income) are more likely to adopt a RTW law." Nevertheless, previous empirical work has not exploited this relationship with respect to estimating RTW's effect on wages.

Two potential criticisms of my analysis deserve discussion: (i) the supposed inferiority of state-level versus individual-level data,[21] and (ii) the omission of contemporaneous control variables. My empirical analysis is based on a data set of 45 observations. Micro-based studies typically have thousands of observations--Farber (1984) used over 28,000 observations.[22] In fact, despite their seemingly greater number of observations, individual-level data do not provide more information when it comes to analyzing differences at aggregated levels. Neumark and Wachter (1995, p. 23) emphasize this point in their study of the wage effects of differences in industry-level characteristics: "Certainly, because the identifying information comes from industry-level shifts in variables … there is no added information in the individual-level data."[23] On the dimension of information, the advantage actually lies with state-level data, since state-level data summarize wage information from a far larger number of observations than is contained in micro-based samples. The advantage of individual-level data lies not in their greater number of "observations," but that they frequently contain data on more variables. This leads to a second potential concern with my research.

My research emphasizes the role of state initial conditions as a determinant of current wages. I omit contemporaneous data on state characteristics, such as current values of education, demographic variables, and industry/occupational mix. To the extent these variables provide additional explanatory power and are correlated with RTW status, my results suffer from omitted-variable bias.

The fundamental issue here is the degree to which contemporaneous state characteristics provide additional, *exogenous* information beyond that contained in states' initial economic conditions. One concern driving my methodology is that changes in states' characteristics may be endogenous to changes in states' wages. For example, increases in a state's wages may lead to higher levels of education over time because prosperity makes education more affordable. In addition, states with higher average wages may attract more educated workers from other states. A similar migration argument leads to the possibility that wages may influence a state's observed demographic characteristics: Different demographic groups (e.g., the young, unmarrieds) may face different benefits/costs that affect their decisions to relocate and take advantage of more attractive earnings opportunities in other states. Likewise, firms in different industries and workers in different occupations may face different benefits/costs that affect their willingness to migrate where labor costs are lowest or earnings are highest. Thus, wages may also influence a state's observed industrial and occupational characteristics.

One advantage of my reliance on initial economic conditions is that it eliminates endogeneity problems. Holding constant wages at the times that states adopted RTW (as proxied by state PCPI), there is no possibility that higher wages in 2000 affected state economic conditions in 1945. Nevertheless, to the extent that current state characteristics are an additional, *exogenous* determinant of current state wages, there is a legitimate concern that my empirical results may be subject to omitted-variable bias.

On a related note, my research does not identify specific channels by which RTW may have increased wages. Unfortunately, in this regard, existing research is also not particularly helpful. Previous studies relating RTW to the union wage premium or nonunion wages are limited in that they do not control for states' initial economic conditions. Furthermore, while numerous studies examine the effect of RTW on union membership, it is the number of workers covered by union contracts--not union membership--that matters for the wage effect (cf. equation (2)). RTW may negatively impact union membership (by increasing free-riding behavior) without affecting the number of workers covered by union contracts.

On the other hand, the fact that RTW has been positively related to plant location, the rate of business formation, manufacturing employment, and other dimensions of state economic development (Tannenwald, 1997; Moore, 1998; Holmes, 1998) is consistent with my finding that wages are higher in RTW states, ceteris paribus. Clearly, further research is needed to identify the specific channels through which RTW affects wages.

V. *Conclusion*

I find that after accounting for the influence of economic conditions that were present when states adopted Right-to-Work (RTW), RTW states have significantly higher wages than would otherwise be expected. This finding is robust across a wide variety of model specifications. Perhaps surprisingly, past economic conditions "explain" a large amount of the variation in current state wages. For example, the addition of just two variables to an equation containing a RTW dummy variable--Per Capital Personal Income in 1945 (*PCPI1945*) and agriculture's share of total state earnings in 1945 (*FARM1945*)--along with an interaction term, increases the R^2 of that equation from 17.0 percent to 76.4 percent (cf. equation (3) with equation (6)). The economic past still casts a long shadow on the economic present.

My work also shows that failure to include past state economic conditions substantially biases downward the estimates of RTW's effect on wages. I show how previous research on RTW's effect on wages can be reconciled with my findings once this--and other factors--is taken into account. Nevertheless, additional research investigating (i) the role of explanatory variables not included in this study, and (ii) the channels by which RTW influences wages, is needed.

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NOTES

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[1] It is important to note that just because Right-to-Work did or did not have certain consequences in the past does not imply that it will have the same consequences if implemented in the present. This study addresses the former subject. It makes no attempt to address the latter.

[2] Robert Tannenwald (1997, p. 88f.) writes, "I identified 11 studies that estimate the impact of right-to-work laws on either plant location, the rate of business formation, employment, or some other manifestation of economic development…Eight of them find that the existence of a right-to-work law exerts a positive, statistically significant impact on economic activity."

[3] Moore and Newman (1985, p. 575) state, "…there are good reasons to believe that RTW laws may have a negative effect on the extent of unionism." However, it is important to recognize that most discussions of the "extent of unionism" focus on union membership, which is related, but not identical, to the number of workers employed in the unionized sector.

[4] Moulton (1990) demonstrates that the use of micro-data sets to estimate the impact of state-level policies results in standard errors that are too small. All of the studies in Table 1 that employ micro-data, including the Moore and Farber studies, are subject to this criticism.

[5] Note that Moore et al. (1986) replicate Carroll's work and demonstrate that his finding of significance vanishes when either (i) additional industry mix variables are included in the regression, or (ii) the hypothesized endogeneity of RTW is incorporated in the analysis.

[6] At the time of this writing, 2000 is the most recent year for which wage data are available.

[7] The states with RTW laws and their years of adoption are (in descending order): Oklahoma (2001), Idaho (1986), Louisiana (1976), Wyoming (1963), Kansas (1958), Utah (1955), Mississippi (1954), South Carolina (1954), Alabama (1953), Nevada (1951), Arizona (1947), Arkansas (1947), Georgia (1947), Iowa (1947), North Carolina (1947), North Dakota (1947), Tennessee (1947), Texas (1947), Virginia (1947), South Dakota (1946), Nebraska (1946), and Florida (1944). The last three states had constitutional amendments prohibiting union shops prior to the passage of the Taft-Hartley Act in 1947. Sources differ slightly in their reported adoption dates. These dates are taken from Newman (1984).

[8] 1950 is "closer" to the adoption years of the 18 RTW states both in terms of mean absolute value of the differences ($\sum_{i=1}^{18} \left|AY_i - 1950\right| / 18 = 3.67,\ \sum_{i=1}^{18} \left|AY_i - 1945\right| / 18 = 4.22$) and mean sum of squares ($\sum_{i=1}^{18} \left(AY_i - 1950\right)^2 / 18 = 15.56,\ \sum_{i=1}^{18} \left(AY_i - 1945\right)^2 / 18 = 31.67$), where "*AY*" represents "Adoption Year."

[9] The Appendix is posted on my research website, located at "http://faculty-staff.ou.edu/R/William.R.Reed-1/Papers".

[10] The inclusion/omission of Wyoming has little bearing on the final results.

[11] Indiana adopted RTW in 1957 then repealed it in 1965. I count that state as a non-RTW state.

[12] Unless otherwise noted, all data were electronically obtained from the Bureau of Economic Analysis (BEA) website, www.bea.doc.gov/bea/regional/spi.

[13] Descriptive statistics for the variables used in this study are reported in the supplementary Appendix described in Note #9.

[14] State temperatures were electronically obtained from the Southern Regional Climate Center at www.srcc.lsu.edu/ccd/nrmmax.html.

[15] I calculate *FARM1945* by dividing "Farm Earnings" in 1945 by "Total Earnings" in 1945.

[16] I calculate *MANU1945* by dividing "Private Earnings: Manufacturing" in 1945 by "Total Earnings" in 1945.

[17] The education attainment variable is the average of "Median School Years Completed by Persons 25 Years Old and Over" from the 1940 and 1950 U.S. Census (source: *Statistical Abstract of the U.S.*).

[18] As mentioned above, these results are more fully presented in a supplementary Appendix (cf. Note #9).

[19] Using 1950 as the benchmark year, the corresponding best estimate is that average wages in 2000 are 9.24 percent higher in RTW states. The *p*-values associated with the respective hypotheses of no misspecification are 44.36 and 65.08 percent.

[20] The estimated RTW effect is calculated as $\exp\left(\hat{\boldsymbol{b}}_{RTW} + \left(\hat{\boldsymbol{b}}_{RTW*PCPI1945} \times PCPI1945\right)\right) - 1$, where *PCPI1945* is set equal to the state-specific value and the estimated coefficient values are taken from Equation (6) in the text.

[21] For example, Mishel (2001, p. 5) criticized an earlier version of my research by stating, “Most important, though, is that Reed bases his analysis on aggregate data for 45 states, whereas my analysis is a representative sample of more than 150,000 workers.”

[22] The large number of observations in Farber’s study is primarily responsible for his obtaining statistically significant results where others have failed to do so.

[23] Moulton (1990) is also a good reference on this point.

Table 1

Categorization of Previous Empirical Research on RTW and Wages

Effect	*Study*	*Sign of Effect (Statistical Significance)* [a]
$\partial \overline{w} / \partial RTW$	Moore (1980,Table 3,Column "Total Sample"):	Negative (insignificant)
	Farber (1984,Table 5,Column "All"):	Negative (significant) [b]
$\partial \widetilde{w} / \partial RTW$	Wessels (1981,Table 1,Column "Wages"): [c]	Negative (insignificant)
	Wessels (1981,Appendix Table 1,Column "Wages"): [c]	Negative (insignificant)
	Carroll (1983,Table IV,Column (3a)): [c]	Negative (significant)
	Carroll (1983,Table V,Column (3)): [c]	Negative (significant)
	Moore et al. (1986,Table II,Column (3)): [c]	Negative (insignificant)
	Moore et al. (1986,Table II,Column (4)): [c]	Positive (insignificant)
	Garofalo and Malhotra (1992,Table 3,Column "RTW Effect on Price of Labor-Total Effect"): [d]	Negative (indeterminate)
$\partial (w_U - w_N) / \partial RTW$	Farber (1984,Table 5,Column "All"):	Positive (significant)
	Hundley (1993,Table 5, all columns):	Positive (insignificant)
	Moore (1980,Table 3,Columns "Union Sample" and "Nonunion Sample"):	Positive (insignificant) [e]
	Carroll (1983,Table IV, Columns (3) and (3a)):	Negative (indeterminate) [f]
	Carroll (1983,Table V, Columns (3) and (3a)):	Negative (indeterminate) [f]
$\partial S_U / \partial RTW$	Too many studies to list.	"The available evidence suggests that RTW laws may reduce the extent of unionization in the long run by 3 to 8 percent (Moore, 1998, page 463)" [g]

Effect	*Study*	*Sign of Effect (Statistical Significance)* [a]
$\partial w_N / \partial RTW$	Moore (1980,Table 3,Column "Nonunion Sample"): Farber (1984,Table 5,Column "Nonunion"):	Negative (insignificant) Negative (significant)

Notes: (*a*) Statistical significance is set at the 5 percent, two-tailed level. (*b*) The RTW effect enters through two variables, a *RTW* dummy variable and a *RTW*Union* interaction term. The combined effect must be calculated. Although Farber does not report a test of significance for the combined effect, the data in Table 5 strongly suggest that the total effect is significant. (*c*) The dependent variable in these regressions is "log of hourly wages for production workers on manufacturing payroll." (*d*) The dependent variable in this regression is "the payroll of workers plus total supplemental labor costs divided by hours worked." (*e*) The union wage premium can be calculated as the difference between the *RTW* dummy variable coefficients in the "Union Sample" and "Nonunion Sample." Although Moore does not report a test of the significance of the difference between these two coefficients, the data in Table 3 strongly suggest that the difference is insignificant. (*f*) The effect of RTW on the union wage premium is inferred by the difference in the coefficients for the variables *XPU* in Column (3) and *YPU* in Column (3a). The data in the respective tables are not sufficient to determine significance. (*g*) Note that there is an important distinction between the effect of RTW on union membership, and the effect of RTW on the size of the union sector. Most of the studies summarized by Moore (1985) directly address the former subject. From the perspective of RTW's effect on overall wages, it is the latter subject that is of interest.

Table 2

Top 10 Models Using AIC and SIC Model Selection Criteria

TOP 10 AIC MODELS

Model	*Variables*	*AIC Value*
1	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945	0.0082158
2	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) EDUC1945	0.0084835
3	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) SOUTH	0.0085551
4	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) MANU1945	0.0085671
5	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) DENS1945	0.0085700
6	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) TEMP	0.0085826
7	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) EDUC1945, (6) MANU1945	0.0087976
8	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) EDUC1945, (6) SOUTH	0.0088545
9	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) EDUC1945, (6) TEMP	0.0088610
10	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) EDUC1945, (6) DENS1945	0.0088666

TOP 10 SIC MODELS

Model	*Variables*	*SIC Value*
1	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945	0.010042
2	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) EDUC1945	0.010794
3	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) SOUTH	0.010885
4	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) MANU1945	0.010901
5	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) DENS1945	0.010904
6	(1) RTW, (2) RTW*LNP1945, (3) LNP1945, (4) FARM1945, (5) TEMP	0.010920
7	(1) LNP1945, (2) FARM1945	0.010990
8	(1) LNP1945, (2) FARM1945, (3) SOUTH	0.011019
9	(1) LNP1945, (2) FARM1945, (3) EDUC1945	0.011218
10	(1) LNP1945, (2) FARM1945, (3) EDUC1945, (4) SOUTH	0.011538

Table 3

Estimated RTW Effects and Corresponding Significance Tests

TOP 10 AIC MODELS

Model	*Estimated Effect* [a]	*Wald Test of* $H_0: \boldsymbol{b}_{RTW} + (\boldsymbol{b}_{RTW*LNP1950} \times LNP1950) = 0$ [a]
1	7.93%	$\chi^2 = 3.539047$ (p-value=0.0599)
2	7.69%	$\chi^2 = 3.461576$ (p-value=0.0628)
3	6.64%	$\chi^2 = 1.746846$ (p-value=0.1862)
4	7.60%	$\chi^2 = 2.813118$ (p-value=0.0935)
5	7.88%	$\chi^2 = 3.415896$ (p-value=0.0645)
6	8.35%	$\chi^2 = 3.019164$ (p-value=0.0823)
7	7.02%	$\chi^2 = 2.388622$ (p-value=0.1222)
8	6.86%	$\chi^2 = 2.030151$ (p-value=0.1542)
9	8.14%	$\chi^2 = 2.981550$ (p-value=0.0842)
10	7.68%	$\chi^2 = 3.357375$ (p-value=0.0669)

TOP 10 SIC MODELS

Model	*Estimated Effect* [a]	*Wald Test of* $H_0: \boldsymbol{b}_{RTW} + (\boldsymbol{b}_{RTW*LNP1950} \times LNP1950) = 0$ [a]
1	*same as Model 1-AIC*	*same as Model 1-AIC*
2	*same as Model 2-AIC*	*same as Model 2-AIC*
3	*same as Model 3-AIC*	*same as Model 3-AIC*
4	*same as Model 4-AIC*	*same as Model 4-AIC*
5	*same as Model 5-AIC*	*same as Model 5-AIC*
6	*same as Model 6-AIC*	*same as Model 6-AIC*
7 [b,c]	*same as Model 1-AIC*	*same as Model 1-AIC*
8 [b,d]	*same as Model 3-AIC*	*same as Model 3-AIC*
9 [b,e]	*same as Model 2-AIC*	*same as Model 2-AIC*
10 [b,f]	*same as Model 8-AIC*	*same as Model 8-AIC*

Notes: (*a*) The *RTW* effect is estimated by $exp\left(\hat{\boldsymbol{b}}_{RTW} + \left(\hat{\boldsymbol{b}}_{RTW*LNP1945} \times LNP1945\right)\right) - 1$, where *LNP1945* is set equal to the mean value of *LNP1945* for the 18 RTW states in the sample (=6.891345). (*b*) Since the original model did not include the variables *RTW* and *RTW*LNP1945*, these variables were added to the equation in order to estimate and test the

RTW effect. (*c*) The addition of the variables *RTW* and *RTW*LNP1945* to Model 7-SIC produces the same variable specification as Model 1-AIC. (*d*) The addition of the variables *RTW* and *RTW*LNP1945* to Model 8-SIC produces the same variable specification as Model 3-AIC. (*e*) The addition of the variables *RTW* and *RTW*LNP1945* to Model 9-SIC produces the same variable specification as Model 2-AIC. (*f*) The addition of the variables *RTW* and *RTW*LNP1945* to Model 10-SIC produces the same variable specification as Model 8-AIC.

Table 4
Details of the Moore (1980) and Farber (1984) Studies

Study	*Categories*	*Specific Variables*
Moore (1980) *Year = 1970* $R^2 = .5566$ *Effect = -3.71%*	Location	Region of Country (8 dummies), Distance Between Residence and SMSA (4 dummies).
	Occupation	2-Digit Census Occupation (6 dummies-including Farm).
	Industry	2-Digit Census Industry (12 dummies-including Agriculture).
	Personal Characteristics	Male dummy, Marital Status (4 dummies), Nonwhite dummy.
	Human Capital	Education (5 dummies), Vocational Training dummy, Veteran dummy, Number of Children, Experience (4 variables), Unemployment Rate.
	Union/RTW	Union dummy, RTW dummy.
Farber (1984) *Year = 1977* $R^2 = .440$ *Effect = -6.90%* [a]	Location	South dummy.
	Occupation	Clerical, Service, and Professional/Technical dummies.
	Industry	Manufacturing dummy.
	Personal Characteristics	Female dummy, Married dummy, Female*Married dummy.
	Human Capital	Education, Age, Experience.
	Union/RTW	Union dummy, RTW dummy, Union*RTW dummy.

Note: (*a*) Calculated from reported coefficients. This estimate is the combined effect accounting for the *Union*RTW* interaction term.