Right to Work Laws Do Matter*

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I. Introduction

Recent economic literature has ascribed a benign influence to right to work (RTW) laws. The apparent consensus is that RTW laws do not reduce union membership, because organized labor must have already been weak at the time right to work laws were enacted. This time series observation (i.e., union membership is not significantly lower after RTW laws have been enacted than what it was before) has been misapplied to argue that right to work laws are not associated with economic differences between right to work states and union shop states. A closer look at this nonsequitur is in order.

This paper contends that right to work laws do matter. The consensus to the contrary rests on a shakey econometric foundation. Proponents of the proposition that RTW laws are harmless have never subjected that hypothesis to empirical scrutiny. Instead, they have tested weak models of the "right to work laws do matter" hypothesis. By rebutting a characterature of a proposition they set out to refute, these analysts make their own hypothesis the null hypothesis. It is not hard to support a precept which is "proved" merely by showing that the probability that it is false is less than 95 percent. Such procedures generate impressive polemics; they do not further the interests of positive economics.

II. History of the Controversy

Opponents of right to work laws argue that union shop contracts are an important source of union strength in collective bargaining with powerful employers. Union shop contracts require that, several months after being hired, all nonsupervisory production workers must join the union which represents them in collective bargaining. Since section 9a of the National Labor Relations Act requires that one union represent all workers in a bargaining unit, mandatory union membership provisions in collective bargaining contracts are necessary to prevent workers from enjoying union benefits unless they share the costs of winning

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those benefits. If workers can avoid paying union dues which support strike funds and can continue to work during strike actions, employers will have increased leverage in wage negotiations as lack of labor solidarity weakens the resolve of union members.

Not surprisingly, arguments in favor of RTW laws are virtually the same as arguments against. Unions raise wage rates above what employers would voluntarily pay [18; 4], presumably meaning the competitively determined wage rate. Forcing workers to join unions can give unions de facto closed shop control over employment [3, 73]. Laws which weaken labor unions sustain lower wage rates, a result prefered by employers. However, in order for right to work laws to be enacted, there must be considerable support for them from disinterested citizens: voters who are neither union members nor employers of union (or would-be union) labor.

In the early 1970s, Neil and Catherine Palomba [17] sought to determine why disinterested citizens would favor state interference in the collective bargaining process. They hypothesized that RTW laws are enacted because voters believe that prohibiting union shop contracts would attract industry to their state. The Palombas showed that RTW states tend to rank lower in economic development than union shop states, once the impact of union strength on the passage of RTW laws is taken into account.

Ironically, Moore, Newman and Thomas [16] attacked the Palombas, arguing that RTW laws are passed: "to make unions more insecure—to slow down or halt the rate at which unions are organizing and to destroy existing unions."[1] We presume that employers do not like unions; states pass right to work laws to harm unions, hoping to attract industry to their state. Contrary to the strident tone of their paper, Moore, Newman and Thomas in no way contradict the findings of the Palombas.

A twist in the analysis of RTW laws occurred in the paper by Lumsden and Petersen [11]. They showed that the proportion of workers who belonged to unions in RTW states was not significantly different in 1953—after most RTW laws had been passed—than it had been in 1939—before the enabling legislation of the 1947 Taft-Hartley amendments to the National Labor Relations Act. Lumsden and Petersen inferred that the lower proportion of workers in RTW states "really reflect the tastes and preferences of the population, rather than a substantive impact of the laws themselves" [11, 1248]. The same year, Moore and Newman [15] argued that once simultaneous determination of union membership and the likelihood of the passage of RTW laws was taken into account, RTW laws did not have a statistically significant impact on the proportion of workers who belong to unions. Similar arguments, supported by mixed logit analysis, have been presented by Warren and Straus [21] and Wessels [22]. The consensus that RTW laws do not have an economic impact rests on the dubious translation of the time series results of Lumsden and Petersen into the argument that right to work laws do not identify a cross-sectional difference between otherwise similar states.

One must admire the cleverness of the techniques used in recent right to work literature. In order for right to work laws to matter, proponents of the null hypothesis that RTW laws do not matter require evidence that union membership falls precipitously after those laws have been enacted, even though union membership in those states must have been lower than in comparable states which did not prohibit union shop contracts. Not only must right to work laws "slow down or halt the rate at which unions are organizing," they

1. Moore, Newman and Thomas [16], quoting Meyers [14].

must also "destroy existing unions," if they are to matter. By analogy, one might argue that minimum wage laws do not matter because they do not eliminate poverty. Most economists condemn minimum wage laws because of their observable side effects (e.g., teenage unemployment), rather than dismissing them as innocuous.

Given the weight of evidence interpreted to mean that right to work laws are benign, another interpretation is in order. In the absence of right to work laws or widespread anti-union sentiments, organized labor will employ union shop contracts to counteract employer monopsony or collusive oligopsony [5]. The proportion of the labor force belonging to unions will tend toward an equilibrium determined by the industrial mix of the state (i.e., the mix of competitive and oligopsonistic labor markets) and the relative wages offered in union and nonunion industries. Once unionism begins to encroach on competitive labor markets, raising the earnings of union workers will reduce employment in unionized industries, causing the proportion of workers in unionized jobs to stabilize.

The remainder of this paper will reevaluate the evidence on the economic effects of right to work laws. The next section presents simple F-tests showing that right to work states have a lower proportion of workers in unions and lower average earnings in manufacturing than union shop states do. This circumstantial evidence will be enhanced by evaluating departures from union membership and average earnings predicted by the industrial composition of each state. F-tests will also show weak support for the contention that RTW laws encourage employment growth and reduce unemployment rates.

The fourth section presents two stage least squares regressions relating four endogenous variables—the proportion of workers in unions, the unemployment rate, the real average hourly wage rate in manufacturing, and real value-added per production hour in manufacturing—to a right to work and three regional dummy variables, measures of the states industrial composition, and various proxies for anti-union "tastes and preferences." We will encounter strong evidence linking right to work laws to lower average earnings for manufacturing workers through their impact on union membership.

III. Some Circumstantial Evidence

Table I presents a breakdown of states by right to work status and region, which provides the framework for the F-tests which will be used in this section. Note the stability of right to work status. Between 1964 and 1978, only one union shop state, Louisiana, enacted a right to work law. Also, only one right to work state, Indiana, repealed its law. In fact, the Indiana law which was repealed did permit agency shop contracts—contracts which require nonunion workers to pay union dues. It is actually unprecedented for a state law prohibiting agency shops to be repealed. It is possible that anti-union preferences by the citizens of right to work states are very stable. It is also plausible that right to work laws perpetuate the feebleness of organized labor which was necessary for RTW laws to be enacted.

Table II presents circumstantial evidence that right to work and union shop states differ in economically meaningful ways. A significantly larger proportion of workers in union shop states belonged to unions during the period 1964 to 1978 than in right to work states. Since F-tests do not control for influences other than the categories used in the breakdown, exhibit 1 does not prove that lower union membership in RTW states is caused by the prohibition of union shop contracts.

Table I. Breakdown of States by Region and Right to Work Status: 1978†

	Northeast	South	Midwest	West	Total
Union Shop	11	3	7†	9	30
Right to Work	0	11†	5	4	20
Total	11	14	12	13	50

Northeast	South	Midwest	West
Connecticut	Alabama*	Illinois	Alaska
Delaware	Arkansas*	Indiana†	Arizona*
Maine	Florida*	Iowa*	California
Maryland	Georgia*	Kansas*	Colorado
Massachusetts	Kentucky	Michigan	Hawaii
New Hampshire	Louisiana*†	Minnesota	Idaho
New Jersey	Mississippi*	Missouri	Montana
New York	North Carolina*	Nebraska*	Nevada*
Pennsylvania	Oklahoma	North Dakota*	New Mexico
Rhode Island	South Carolina*	Ohio	Oregon
Vermont	Tennessee*	South Dakota*	Utah*
	Texas*	Wisconsin	Washington
	Virginia*		Wyoming*
	West Virginia		

* State with right to work law in effect in 1978

† Indiana, which repealed its right to work law in 1965, is treated as a RTW state in 1964, and as a union shop state beginning in 1966. Louisiana, which passed its right to work law in 1976, is treated as a RTW state in 1978, and as a union shop state in previous years.

While the evidence remains circumstantial, the data in exhibit 2 enhances the argument that right to work laws are associated with lower union membership than would otherwise be the case. Relative union membership is defined as the proportion of workers belonging to unions divided by the predicted proportion of workers in unions based on a state's industrial composition. To obtain the predicted union membership, the proportion of a state's labor force in each of twenty manufacturing industries[2] and nine nonmanufacturing industries[3] was multiplied by the national average union membership in that industry. After summing the results to obtain the predicted union membership for each state, relative union membership was calculated as the ratio of the actual proportion of workers in unions to that state's predicted union membership. Exhibit 2 shows that union shop states consistently met or exceeded their expected union membership. By contrast, the relative union membership for RTW states was consistently less than 100 percent, implying that RTW laws reduce union membership below what it would have been, based on national union membership patterns.

2. The twenty manufacturing industries are: food and kindred products; tobacco; textiles; apparel; lumber and wood products; furniture and fixtures; paper mill products; printing and publishing; chemicals; petroleum and coal; rubber and plastics; leather; stone, clay and glass; primary metal products; fabricated metal products; nonelectrical machinery; electrical and electronic equipment; instruments; and miscellaneous manufacturing industries.

3. The nonmanufacturing industries are: mining; contract construction; transportation and other public utilities; wholesale and retail trade; finance, insurance and real estate; service; and government.

Table II.

Exhibit 1	Northeast	South	Midwest	West	All Regions
Union Shop	25.27	25.12	33.42	26.12	27.32
Right to Work		14.52	16.53	19.81	16.16
F-statistic		40.20	374.1	13.64	207.2
(significance)		(.001)	(.001)	(.001)	(.001)
Actual Percentage of Workers in Unions: 1964-1978					
Exhibit 2	Northeast	South	Midwest	West	All Regions
Union Shop	109.0	99.9	137.9	118.4	117.1
Right to Work		61.2	75.9	95.2	72.8
F-statistic		29.8	383.3	6.26	143.6
(significance)		(.001)	(.001)	(.013)	(.001)
Relative Union Membership (Ratio of Actual Union Membership to Predicted Union Membership)					
Exhibit 3	Northeast	South	Midwest	West	All Regions
Union Shop	3.81	3.88	4.52	4.23	4.10
Right to Work		3.25	3.82	4.08	3.58
F-statistic		6.25	5.61	.250	14.24
(significance)		(.014)	(.020)		(.001)
Actual Average Hourly Wage Rate in Manufacturing					
Exhibit 4	Northeast	South	Midwest	West	All Regions
Union Shop	98.87	95.08	102.42	108.32	101.24
Right to Work		86.11	94.80	97.09	90.18
F-statistic		39.84	6.18	6.11	46.65
(significance)		(.001)	(.015)	(.015)	(.001)
Relative Average Hourly Wage Rate (Actual Wage Rate Divided by Predicted Wage Rate)					
Exhibit 5	Northeast	South	Midwest	West	All Regions
Union Shop	13.23	15.06	14.94	15.49	14.50
Right to Work		11.27	14.46	16.74	13.26
F-statistic		10.17	.166	.649	3.64
(significance)		(.001)			(.06)
Value Added per Production Hour					

Exhibit 3 suggests one economic side effect of right to work laws: lower labor earnings. Except for Western states, wage rates in RTW states were significantly lower than wage rates in union shop states. To control for the possibility that lower wage rates in RTW states are due to a greater proportion of low wage industries in those states, exhibits 4 and 5 are presented. Exhibit 4 breaks down the relative average wage rate, which was derived by techniques similar to those used in exhibit 2. The proportion of workers in each

two-digit manufacturing industry was multiplied by the national average wage rate in that industry each year. The actual average hourly wage rate was then divided by the predicted wage to obtain the relative wage rate. Note that the discrepancy between relative wage rates has more significance than the discrepancy between actual wage rates between right to work and union shop states. Further, the difference in relative wage rates is statistically significant in the West. Manufacturing jobs pay less, on the average, in right to work states than in union shop states; this pattern is not explained by differences in the industrial composition of states which forbid and allow union shop contracts.

Exhibit 5 contrasts value added per production hour in manufacturing for regions and between union shop and RTW states. Except in the South, wherein workers in RTW states have lower average products than in union shop states, there is little support for the proposition that lower earnings in right to work states are due to lower productivity.

The possibility that reduced earnings in right to work states are counterbalanced by enhanced employment security is explored in table III. As the Palombas suggested, states ranking low in economic development might enact right to work laws in the hope of attracting jobs to their states. Even if weakened unionism reduced average wages in right to work states,[4] workers in those states might prefer employment growth and security enough to offset their lower earnings.

According to exhibit 6, total employment grew more rapidly, on the average, in right to work states than in union shop states,[5] between 1964 and 1978. This result is statistically significant at the 10 percent level in each region, and is significant at the 5 percent level for the country as a whole. Exhibit 7 shows that manufacturing employment grew more rapidly in right to work states than in union shop states, although this result is statistically significant only in the West (at the 10 percent level), and for the country as a whole (at the 5 percent level). Finally, exhibit 8 displays consistently lower unemployment rates in right to work states relative to union shop states—a result which is significant in all regions except the West.

The circumstantial evidence in Table II and Table III hints at flaws in the "right to work laws do not matter" consensus of recent literature. Since lower union membership in right to work states is not explained away by industrial composition of those states, two alternative explanations are possible. One is that unexplained and unobserved differences in tastes and preferences result in variations in union membership, implying that RTW laws themselves are but proxies for those idiosyncracies. The other explanation is that right to work laws, enacted when union membership was less than predicted by a state's job mix, prevent union membership from reaching otherwise predicted levels. In the next section, these competing hypotheses will be scrutinized more closely.

IV. Regression Analysis

We now build upon the circumstantial evidence that right to work laws perpetuate low union membership and cause lower earnings in RTW states by considering the following four equation system:

4. See, however, the dissent by Hirsch [8].

5. Of the twenty states with right to work laws in force in 1978, all but six were located in the "sunbelt," (here defined as the Southern region, plus Arizona, California, Hawaii, Nevada, New Mexico and Utah in the West). The

Table III.

Exhibit 6	Northeast	South	Midwest	West	All Regions
Union Shop	1.03	3.25	2.48	4.34	2.60
Right to Work		3.94	3.09	5.39	4.01
F-statistic		3.59	3.16	3.45	4.72
(significance)		(.061)	(.080)	(.066)	(.030)
Average Annual Percent Change in Total Employment: 1964-78					

Exhibit 7	Northeast	South	Midwest	West	All Regions
Union Shop	0.47	2.67	1.14	2.82	1.54
Right to Work		3.03	3.44	4.73	3.48
F-statistic		1.11	.420	2.87	4.16
(significance)				(.091)	(.042)
Average Annual Percent Change in Manufacturing Employment: 1964-78					

Exhibit 8	Northeast	South	Midwest	West	All Regions
Union Shop	5.44	5.58	4.75	6.40	5.59
Right to Work		4.73	3.51	6.34	4.74
F-statistic		9.17	24.15	.006	10.27
(significance)		(.003)	(.001)		(.002)
Average Unemployment Rate: 1964-78					

$$PU_{it} = \Theta(JMIX, RTW, South, RAHW, UE, \text{time}) \quad (1)$$

$$UE_{it} = \Psi(JMIX, RTW, REGION, RAHW, PU, \text{time}) \quad (2)$$

$$RAHW_{it} = \Pi(MAN, REGION, RVAPH, PU, \text{time}) \quad (3)$$

$$RVAPH_{it} = \Omega(MAN, REGION, PU, UE, \text{time}). \quad (4)$$

The first equation provides the direct test of whether right to work states have lower union membership than union shop states, *ceteris paribus*. Since we dissent from the argument that RTW laws must reduce union membership below what it was before those laws were enacted in order for RTW laws to have economic relevance, the RTW dummy is treated as an exogenous variable. This dummy can be interpreted either as a proxy for the prohibition of union shop contracts or as a proxy for the anti-union tastes and preferences which allowed those laws to be enacted. Later these interpretations will be more closely contrasted.

The proportion of nonagricultural workers in unions in state i and year t, PU_{it}, also depends on the state's job mix ($JMIX$), a dummy variable for Southern states to pick up the alleged anti-union sentiments of this region [15, 437], and two endogenous variables: UE_{it}, the unemployment rate, and $RAHW_{it}$, the real average hourly wage rate for production workers in manufacturing (deflated by the implicit price deflator for consumer goods). If workers join unions to increase their earnings [13], then the proportion of workers in

hypothesis that "sunbelt" and "right to work" are independent categories of states can be rejected on the basis of a chi-square statistic equal to 9.5, which is significant at the .002 level with one degree of freedom.

unions should be an increasing function of the real average hourly wage rate, unless unions raise wage rates for members solely by reducing the earnings or employment opportunities of nonunion workers, in which case we would expect to find $\partial PU/\partial RAHW \leqq 0$. If collective bargaining sometimes counteracts monopsony or wage discrimination [7, 339], or provides compensating wage differentials for union workers [20], we expect workers to migrate from nonunion jobs to union jobs in the wake of collective bargaining wage gains. Particularly where union shop contracts are enforceable, this migration would show up as a simultaneous increase in the proportion of workers in unions and the real average wage rate: $\partial PU/\partial RAHW > 0$ and $\partial RAHW/\partial PU > 0$.

Even if the results of equation (1) showed a lower proportion of workers in unions in right to work states, whether or not that result is of economic consequence depends upon how unions influence employment, earnings and productivity. Equation (2) tests whether right to work laws reduce unemployment rates. In addition to the RTW dummy, the unemployment rate will depend upon the state's region and the national rate of unemployment ($USUE$), used here to control for business cycles of exogenous cause. According to the competitive model of unionism, organized labor reduces employment in unionized industries to sustain wage rates above competitive levels [17; 9]. If this scenario is true, we should find $\partial UE/\partial PU > 0$. Since workers migrate from low wage states to high wage states, UE_{it} is predicted to increase with the real average wage rate, $RAHW$.

The third equation concerns the impact of unionism on average earnings in manufacturing. Even if RTW laws sustain low union membership, the economic impact of those laws is different, depending on whether unions redistribute wage income among workers (predicting $\partial RAHW/\partial PU = 0$), or if unions increase average earnings by counteracting employer monopsony (predicting $\partial RAHW/\partial PU > 0$). In order to show a meaningful economic side effect of right to work laws, we require that real average hourly wages increase with the proportion of workers in unions, *ceteris paribus*.

Since Pettengill [18, 645] has argued that higher union wages merely cause employers to hire more productive workers, it is important to control for variations in productivity in the average wage equation. Output per worker is represented by the endogenous variable, real value added per production hour, $RVAPH$, which is adjusted by the implicit price deflator for total GNP. Average wages will also depend upon the proportion of workers employed in durable and nondurable manufacturing industries (MAN). Since a positive association between $RAHW$ and PU is predicted, there are multiple null hypotheses for the average wage equation: $\partial RAHW/\partial PU \leqq 0$ could mean that unions are ineffective at raising manufacturig earnings [2], that unions reduce the earnings of nonunion manufacturing workers when raising the earnings of union workers [19], or that the union-earnings relation is captured by the impact of unions on labor productivity [18; 10, 442].

If collective bargaining affects the hiring practices of employers, then output per worker, here measured as $RVAPH_{it}$, becomes a dependent variable. Equation (4), introduced to avoid simultaneous equations bias in equation (3), does not have a viable null hypothesis, however. A positive association between $RVAPH_{it}$ and PU might indicate that unionized firms *replace* less productive workers with more productive ones in the wake of a union-induced labor surplus in erstwhile competitive labor markets. A positive influence of PU on $RVAPH_{it}$ would also occur if monopsony employers hire workers whose reservation wages were greater than the preunion wage (due to their superior human capital endowments) *in addition to* workers hired under preunion market conditions.

To isolate the union-productivity effect, other variables are included in equation (4): the region and mix of jobs between durable and nondurable manufacturing, and the percent of manufacturing workers who are production workers, *PCPW*. The latter variable is a proxy for labor intensity: the greater the capital to labor ratio, the more supervisory and white collar workers there will be relative to production workers; the higher the proportion of production workers, the lower will be value added per production hour. Since production labor is usually a variable input while capital costs are fixed in the short run, we expect that increasing unemployment will increase output per worker. However, because real value added per worker includes a price effect and an output effect, a significant result will not appear if downward price pressure accompanies unemployment.

Table IV presents the results of the second stage regressions using mixed time series and cross section data for fifty states in the even years from 1964 through 1978.[6] Two stage least squares regressions were used because all equations are over-identified, with fifteen exogenous variables and four endogenous variables.

The negative coefficient on the right to work dummy in equation (1) is statistically significant at the .01 level. The coefficient on the instrumental variables for the real average hourly wage rate (generated by the first stage regression) is positive and significant at the .01 level. The trend toward decreasing union membership through time is captured by the negative coefficient on Year (1964 = 0; 1978 = 14). The coefficient on the Southern dummy variable is negative, but virtually zero; apparently the anti-union bias in the South is expressed through the preponderance of right to work states in that region.

The proportion of workers employed in industries in which monopsony is prevalent (transportation and public utilities, durable manufacturing, service) generally had positive coefficients. Coefficients for structurally competitive industries (construction; sales; and finance, insurance and real estate) were associated with negative coefficients. If unions raise wages by restricting employment, we expect union membership to be inversely related to employment in competitive labor markets. However, since collective bargaining need not reduce employment in monopsonistic industries, we expect union strength to increase as employment in monopsonistic labor markets increases. And while monopsony is often associated with government employment, the negative coefficient on the proportion of workers on government payrolls probably reflects legal impediments to public employee collective bargaining.

The results of equation (2) appear to contradict the belief that right to work laws lower unemployment rates. The coefficient on the RTW dummy is positive and significant at the .01 level, allowing us to reject the hypothesis that RTW laws pay off with greater employment security. As expected, unemployment rates fall with real value added per worker, because employers are reluctant to lay off highly productive employees. However, unemployment increases with the real wage rate, presumably due to worker migration from low wage to high wage states. Although the instrumental variable for union membership had a positive t-statistic, the coefficient itself could not be calculated due to low tolerance in the step-wise regression. Somewhat surprising are the negative coefficients on the dummy variable for the Midwest and on the proportion of workers in durable manufacturing, since cyclical unemployment has long been associated with variations in durable manufacturing employment in the Great Lake states. A plausible explanation is

6. The cross section results for even years from 1964 through 1978 mirrored those obtained in the pooled regressions; the cross section results were pooled to simplify Table IV.

Table IV. Second Stage Regressions, 1964-1978

	(1) PU_{it}	(2) UE_{it}	(3) $RAHW_{it}$	(3a) $RAHW_{it}$	(4) $RVAPH_{it}$
Intercept	37.22*	−2.70	1.90*	2.10*	19.21*
	(2.86)	(−.571)	(10.52)	(10.30)	(10.21)
Northeast			−.349*	−.374*	−1.94*
			(−4.99)	(−5.30)	(−4.06)
South	−.020	.471	−.314*	−.315*	−.784**
	(−.017)	(1.01)	(−5.88)	(−5.92)	(−2.04)
Midwest		−2.21*			−.322
		(−5.92)			(−.799)
Right to Work	−4.88*	1.16*		−.269*	
	(−5.95)	(3.05)		(−5.65)	
Year	−.344**		.042*	.039*	.328*
	(−1.96)		(6.27)	(5.69)	(10.60)
XRAHW	.524*	5.80*			
	(2.50)	(3.37)			
XUE	−.139				.073
	(−.491)				(.817)
XRVAPH		−.879*	.072*	.074*	
		(−2.56)	(4.97)	(5.15)	
XPU		***	.030*		.125*
		(1.49)	(7.19)		(5.32)
YPU				.022*	
				(4.07)	
PCPW					−.122*
					(−4.93)
Durable Manufacturing	.178*	−.086*	.004	.006*	−.080*
	(2.44)	(−3.18)	(1.38)	(1.84)	(−4.18)
Nondurable Manufacturing	−.184**	−.024	−.004	−.004	−.076*
	(−2.07)	(−.614)	(−.857)	(−.985)	(−3.03)
Mining	−.226	−.077			
	(−1.28)	(−1.08)			
Construction	−1.59*	.149			
	(−6.05)	(.764)			
Transportation & Public Utilities	2.14*	.513*			
	(4.47)	(3.59)			
Sales	−.750*	−.043			
	(−4.14)	(−.522)			

Table IV. (continued)

	(1) PU_{it}	(2) UE_{it}	(3) $RAHW_{it}$	(3a) $RAHW_{it}$	(4) $RVAPH_{it}$
Finance, Insurance & Real Estate	−1.07*	−.044			
	(−6.31)	(−.887)			
PCPW					−.122*
					(−4.93)
Government	−.332*	−.149*			
	(−3.51)	(−3.69)			
Service	.151	−.026			
	(1.62)	(−.574)			
USUE		.832*			
		(6.13)			
R^2	.7100	.4353	.7052	.7089	.5681

(t-statistics in parentheses)
* significant at .01 level
** significant at .05 level (one tail test)
*** coefficient could not be calculated

that cyclical effects are completely captured by the national unemployment rate, *USUE*, meaning that states which retain durable manufacturing jobs tend to have less than their expected unemployment rates, *ceteris paribus*.

Equation (3), showing the determinants of the real average hourly wage rate in manufacturing, $RAHW_{it}$, gives the indirect support to the premise of this paper: right to work laws do matter because they reduce labor income by compromising union bargaining power. Because right to work laws work through their impact on union membership, the RTW dummy does not appear in structural equation (4). To provide a closer link, the instrumental variable for union membership, *XPU*, was modified to reflect all causes of union membership except RTW laws, yielding the instrumental variable *YPU*.[7] Purging union membership of the effect of RTW laws assigns the common effect of right to work laws and union membership to the RTW dummy. Equation (4a) shows that right to work laws appear to reduce average real earnings by about 27 cents, *mutatis mutandis*.[8] When

7. *YPU* was calculated as: $YPU = XPU - (-6.247 \times RTW)$, where −6.247 is the coefficient on the RTW dummy in the first stage multiple regression on PU_{it}.

8. An alternative link between right to work laws and the real wage effects of collective bargaining involve a right to work slope dummy. Using the same data as in Table IV, the following equation was fit:

$$RAHW_{it} = \underset{(13.60)}{2.00} - \underset{(-5.97)}{.406}\,\text{Northeast} - \underset{(-6.43)}{.332}\,\text{South} + \underset{(6.97)}{.042}\,\text{Year}$$

$$+ \underset{(6.22)}{.070}\,XRVAPH + \underset{(8.95)}{.030}\,XPU - \underset{(-2.18)}{.006}\,RTWPU, \quad R^2 = .7072$$

RTWPU is an interaction term obtained by multiplying the RTW dummy and *XPU*. T-statistics, shown in parentheses, indicate that the negative coefficient on *RTWPU* is significant at the .05 level, while all other coefficients, having their expected signs, are significant at the .01 level. Not only are wages lower because fewer workers in RTW states belong to unions, but the impact of an increase in union membersip on the real wage rate is smaller in right to work states than in

the original instrumental variable, *XPU*, is included along with the RTW dummy, the coefficient on the latter decreases by 50 percent, and its t-statistic changes to 5 percent. Since lower earnings associated with RTW laws appear to be due to their impact on union membership, these laws cannot be dismissed simply as inferior goods [22].

The other endogenous variable, *XRVAPH*, has a positive coefficient which is significant at the .01 level in equations (3) and (3a): the positive coefficient of *XPU* cannot be dismissed as an indirect effect of union membership on labor productivity. The negative coefficients on the dummy variables for the Northeast and the South indicate that, after variations in union membership and productivity have been accounted for, labor earnings in these regions are lower than in the Midwest and the West, with the lowest earnings being in the Northeast. Indeed, lower earnings in the Northeast relative to the South (*mutatis mutandis*) suggest a possible link between right to work laws and the interregional relocation of manufacturing.

The results of equation (4) are generally as expected. The dummy variables for the Northeast and the South have negative coefficients which are statistically significant. Real output per worker-hour falls with the proportion of production workers, and with the proportion of workers in both durable and nondurable manufacturing jobs—a result which is consistent with diminishing marginal productivity of labor. While positive, the coefficient on *XUE* is not significant. Average output increases at the rate of 33 cents per year, in contrast to the annual wage increase (not explained by other variables) of about 4 cents: indeed, average product consistently exceeds marginal product. As predicted, the coefficient on *PU* is positive and statistically significant: an increase in union membership raises the average output per worker.

Table V introduces several proxies for "tastes and preferences" into the regression analysis of right to work laws. Unions have been accused of raising wages through job discrimination [3, 62-74], although the distinction has been made between the discrimination incentives of craft unions and the nondiscriminating behavior of industrial unions [1]. Hence, states with a large proportion of female or black workers may more readily pass right to work laws to avoid discrimination. If so, measures of the percentage of labor force female (*PCF*) or the percentage of labor force black (*PCB*) would capture the union membership effect of RTW laws.

Since workers with the least education may require the greatest protection from monopsony exploitation [12, 568], we expect the proportion of workers in unions to be a declining function of the proportion of the labor force with a high school education (*PCHSG*). We are interested in whether *PCF*, *PCB* or *PCHSG* pick up the explanatory power of the RTW dummy. If not, the likelihood that the RTW dummy is merely a proxy for tastes and preferences of citizens in RTW states becomes more dubious.

Another taste proxy, percent of population urban, *PCURB*, was tried and dropped, since neither that variable nor *PCB* was ever significant in any of the equations. In fact, inclusion of all four taste proxies in the union membership equation caused all variables except the right to work dummy and the percent of workers in construction to become insignificant. Another reason for dropping *PCB* from the final results was because this

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union shop states, *ceteris paribus*. While $\partial RAHW/\partial PU \simeq .03$ in union shop states, $\partial RAHW/\partial PU \simeq .024$; this result is consistent with weakened bargaining power of organized labor due to free riding abetted by the prohibition of union shop contracts.

Table V. Second Stage Regressions, 1970-1976

	(1) PU_{it}	(2) UE_{it}	(3) $RAHW_{it}$	(3a) $RAHW_{it}$	(4) $RVAPH_{it}$
Intercept	−3.82	19.71**	2.30*	2.39*	21.50*
	(−.233)	(2.27)	(3.80)	(3.65)	(3.83)
Northeast			−.147	−.165	−1.15
			(−1.36)	(−1.39)	(−1.20)
South	−2.80	−1.82	−.138	−.137	.380
	(−1.26)	(−1.28)	(−1.20)	(−1.01)	(.333)
Midwest		−3.59*			−.309
		(−3.44)			(−.329)
Right to Work	−5.11*	2.63		−.322*	
	(−3.17)	(1.43)		(−3.31)	
Year	−.064	***	.020	.019	.396*
	(−.165)	(−.917)	(.886)	(.846)	(2.38)
Percent of Labor-force Female	.628**	−.424**	−.030**	−.030**	−.193**
	(2.08)	(−2.25)	(−1.82)	(−1.79)	(−1.71)
High School Graduates	−.415*	.143**	.016**	.016**	.100**
	(−2.67)	(1.73)	(2.06)	(2.00)	(1.90)
XRAHW	8.68*	***			
	(3.05)	(−.323)			
XRVAPH		−.077	.050*	.051*	
		(−.241)	(2.90)	(2.91)	
XUE					−.088
					(−.693)
XPU		.183	.038*		.148*
		(.893)	(5.64)		(2.83)
YPU				.035*	
				(3.52)	
PCPW					−.156*
					(−3.65)
Durable Manufacturing	.067	−.087			−.050
	(.542)	(−.967)			(−1.37)
Nondurable Manufacturing					−.055
					(−.773)
Construction	−1.56*	−.026			
	(−3.70)	(−.061)			
Transportation & Public Utilities	2.28*	.094			
	(2.72)	(.153)			

Table V. (continued)

	(1) PU_{it}	(2) UE_{it}	(3) $RAHW_{it}$	(3a) $RAHW_{it}$	(4) $RVAPH_{it}$
XUE					−.088
					(−.693)
Finance, Insurance & Real Estate	.889	−.285			
	(1.25)	(−.781)			
Sales	−.773*				
	(−2.52)				
Service	.358**	−.218**			
	(2.17)	(−1.75)			
Government	.104	−.479*			
	(.647)	(−5.23)			
USUE		1.11**			
		(2.01)			
R^2	.6765	.4645	.6762	.6767	*.5792*

(t-statistics in parentheses)
* significant at .01 level
** significant at .05 level
*** coefficient could not be calculated

variable was available for only 38 states in 1976, while all four taste proxies were available only for 1970 and 1976. The results in Table V were picked because this set of variables consistently gave the highest adjusted R^2 and the greatest number of significant variables.

With these caveats in mind, it is nevertheless impressive that including the taste proxies *PCF* and *PCHSG* does not undermine the significance of the RTW dummy in either equation (1) or equation (3a). In both equations, the right to work dummy has a negative coefficient which is significant at the .01 level. These results verify our hypotheses that right to work laws stifle union membership thereby, reducing earnings for manufacturing workers, *mutatis mutandis*. While the coefficient on the RTW dummy is positive in equation (2), there appears to be a 10 percent chance that RTW laws are actually associated with lower unemployment rates.

The coefficients on *XRAHW* in equation (1) supports the hypothesis that union membership increases as real average hourly wage rates rise, a result consistent with the migration of workers from nonunion to union jobs. The positive coeficient on the instrumental variable for union membership (*XPU* and *YPU*) in equations (3) and (3a) also imply that unions increase average manufacturing earnings, *ceteris paribus*.

The coefficients on *PCHSG* are uniformly consistent with human capital theory. Both average wages (*RAHW*) and average output per worker (*RVAPH*) increase with the proportion of workers who are high school graduates. A higher reservation wage for high school graduates could also explain the positive coefficient on *PCHSG* in equation (2). As expected, the proportion of workers in unions decrease as the proportion of high school graduates increases, *ceteris paribus*.

The results for *PCF* seem to give rise to conflicting interpretations. The negative coefficients on *PCF* in equations (3), (3a) and (4) are consistent with the premise that women workers are crowded into low productivity, low wage industries [6, 583-585]. Contrary to the notion of greater job turnover for female employees, the coefficient on *PCF* in equation (2) is negative and significant. Further, the positive coefficient on *PCF* in the union membership equation contradicts the argument that unions, in general, discriminate against female job seekers.

Otherwise, the results in Table V parallel those in Table IV. Incidental differences involve reduced significance of the dummies for the Northeast and the South, perhaps due to the higher proportion of female workers in the Northeast and the lower incidence of high school graduation in the South. By similar logic, *PCHSG* may capture the explanatory power of the proportion of workers in durable and nondurable manufacturing. As in Table IV, the instrumental variable for union membership is positive but insignificant; *XRAHW* and *XRVAPH* both become insignificant in the unemployment rate equation.

V. Conclusion

This study has departed from other investigations of right to work laws in recent economic literature by clearly distinguishing between the valid inference that right to work laws do not precipitously reduce union membership after those laws are passed and the nonsequitur that RTW laws do not cause weak unionism in RTW states. The methods used in this paper place the burden of proof on the hypothesis being tested ("right to work laws do matter"), using the consensus judgment ("right to work laws do not matter") as a viable null hypothesis. The results displayed here justify rejecting the null hypothesis.

If the equilibrium rate of union membership in a state is based on the mixture of monopsony and competitive labor markets and the relative pay in union and nonunion jobs, then right to work laws can be seen as distortions of the equilibrium process. While RTW laws do not destroy existing unions, they do slow down or halt the rate at which unions are organizing. Treating the RTW dummy as an endogenous variable is a red herring: a means of shifting attention from the meaningful economic issue. Do right to work laws generate observable economic side effects? The answer is yes.

Lower union membership in right to work states means that unions are weaker in collective bargaining, thereby lowering average earnings in both competitive and noncompetitive sectors of manufacturing. In union shop states, unions are better able to raise wages for union workers, creating temporary union-nonunion wage differentials which are closed as workers migrate from nonunion jobs to union jobs. In right to work states, unchecked monopsony and collusive oligopsony power reduce earnings and employment in concentrated industries, thereby crowding manufacturing workers into structurally competitive labor markets, lowering earnings there as well.

Unfortunately, the concern in recent literature with the trivial issue of whether right to work laws cause precipitous decreases in union membership has detracted from the more interesting economic questions. First, it does appear that right to work laws do not "work" in the sense that they do not accomplish the ultimate goals of their proponents. They do not reduce union membership below the low rates necessary for such laws to pass; although they do slow down or halt the spread of unionism, they do not destroy existing unions.

Second, while the Palombas [18] may have been correct about the motives of disinterested citizens who vote in favor of RTW laws to attract industry, right to work laws actually appear to increase unemployment rates rather than decrease them, *ceteris paribus*. However, there is a wage differential between Northeastern and Southern states, plus a preponderance of RTW states in the sunbelt which warrants further investigation of the economic development effects of RTW laws. Third, and most importantly, there is strong evidence of a negative wage effect of RTW laws. This paper has shown a significant link between labor earnings in RTW states and lower union membership in those states, *mutatis mutandis*. Indeed, right to work laws *do* matter.

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