THE IMPACT OF RIGHT-TO-WORK LAWS ON UNION ORGANIZING

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ABSTRACT

In contrast to previous studies which have examined the impact of Right-to-Work (RTW) laws on the level or stock of union membership, this paper examines their impact on the most updated flow into membership and the organizing of workers through certified elections. Since detailed annual data are available by state, we are able to estimate an accelerator model of the flow into unionism, and adjust for possible omitted variable and simultaneity bias. The results show dramatic falls in organizing immediately after the passage of a RTW law, with more moderate declines in later years, just as an accelerator model could predict. Overall, the results are consistent with a 5-10 percent reduction in unionism as a result of the passage of RTW laws.

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Perhaps no controversy in recent American labor relations has catalyzed such confrontation or stirred such strong emotions as the battle over "right-to-work" (RTW) laws. The 1947 Taft-Hartley amendments to the Wagner Act granted states the power to pass laws which outlaw the union shop -- a contract provision which requires new employees to join and pay dues to the union.[1] Table 1 displays the states where laws have been passed and the date of passage. A few states passed laws prior to 1947 but their legal status was in jeopardy until Taft-Hartley.

The philosophical debate turns on the classic confrontation between freedom and free riders. Unions certified by the National Labor Relations Board (NLRB) as the bargaining agent for a group of workers known as the bargaining unit must represent all the employees in the unit in negotiations and grievance handling, whether or not they join the union. Depending on one's perspective then, union security clauses either ensure that workers pay their fair share of representation costs or force them to join an organization which they do not necessarily favor.

The controversy over these laws is a puzzle to many labor economists because most recent studies have found that RTW laws have little real impact on the level of union membership in a state.[2] Most authors conclude, to paraphrase the words of Fredrick Meyers in 1959,[3] that the laws are symbol not substance.

The important academic question involves the quantitative impact of RTW laws on unions. Most previous studies have examined the impact of these laws on the level (or stock) of union membership in a state. In contrast we explore the impact of the laws on union organizing -- a flow into organizing. By focusing on the flows rather than stocks, we

TABLE 1

States With Right-to-Work Laws

STATE	YEAR OF ADOPTION
Alabama	1953
Arizona	1946
Arkansas	1944
Florida	1944
Georgia	1947
Indiana (repealed in 1965)	1957
Iowa	1947
Kansas	1958
Louisiana	1976
Mississippi	1954
Nebraska	1946
Nevada	1951
North Carolina	1947
North Dakota	1947
South Carolina	1954
South Dakota	1946
Tennessee	1947
Texas	1947
Utah	1955
Virginia	1947
Wyoming	1963

can simultaneously examine two questions. We can of course collect
evidence of the impact of RTW laws on organizing itself. And we can gain
considerable insight into the impact of the laws on the level of unionism
since adjustments to the stock must come in the form of flows. Moreover
our emphasis on organizing provides us with an opportunity to overcome
some of the methodological problems which have confronted previous authors.

It is our finding that the number of workers in newly organized
bargaining units is substantially reduced in the first decade after
passage of a right-to-work law, particularly in the first five years.
The reduction arises largely from a shrinking in the number of certifi-
cation elections held and from a reduction in the average number of workers
in each new unit. In later years the effects are relatively small.
Our results suggest that a decade after passage, the reduced organizing
will have lead to a 5% reduction in the number of union members, and our
results are consistent with a prediction that the 5% reduction is permanent.
Nonetheless our data does not allow definitive statements about the long
term effects the laws have on the level of unionism in a state partially
because we do not observe flows out of unionism and partially because
of uncertainty about the exact size of the long term impact on new organizing.

We begin with a discussion of stock models of union membership and
a discussion of some of the problems which have confronted previous authors.
We discuss an alternative approach which focuses on flows. We describe
our model and its specification. We present results for all fifty states
and then for seven states where data are available to allow controls
for fixed effects and for a rough test for simultaneity/exogeneity.
Next we decompose our dependent variable into several components to

examine the mechanisms whereby unionism is reduced. We conclude with a discussion of the results and their implications.

Stock Models of Unionism

We take as our starting point the standard "reduced form" model of the stock of union membership which is common in the literature on the level of union membership and which has been used commonly in the right-to-work research. The proportion of workers who are union members in a state is thought to be a function of a variety of exogenous variables including demographic characteristics of the labor force, the tastes and preferences of both workers and employers, economic conditions, and the presence or absence of a right-to-work law. For simplicity of exposition we represent this function as linear. Obviously this function could be modeled as a logistic or any other non-linear fashion.

(1) $$U_{it}/LF_{it} = X_{it}B + \delta\, RTW_{it} + e_{it}$$

where

U_{it} = Number of union workers in state i at time t

LF_{it} = Size of the labor force

X_{it} = Vector of exogenous variables

RTW_{it} = Dummy variable equal to one if the state has a right-to-work law

e_{it} = Error term

This equation can be easily estimated in cross section even if data are available for only a single year. When such equations are estimated the

RTW coefficient is typically moderately sized (around -.10) and significant. There are several obvious problems with such a specification, most notably omitted variable bias and simultaneity.

Omitted Variable Bias

The first problem is that measured variables included in the specification may not fully capture differences across states in attitudes toward unions. Since a populous with strong anti-union attitudes is likely to resist union organizing attempts and is also more likely to support the passage of a RTW law, the RTW coefficient may spuriously capture the impact of omitted attitudes, tastes, and preferences. The law itself might have no impact, but states with such laws tend to be hostile toward unions and thus we see a non-zero coefficient.

The obvious solution to this problem is to exploit time-series cross sectional data and allow for fixed state effects.[4] In a two period regression this amounts to a before/after type experiment. Unfortunately, comparable data on the stock of union membership before and after the passage of RTW laws are not available in most states. Membership data published by the Bureau of Labor Statistics (BLS) become available only after 1964, nearly twenty years after the passage of many of the laws. Prior to that the only state data available are those created by Troy for 1939 and 1953. Comparisons between the BLS and Troy data are difficult because Troy gathered his data from the financial records of unions whereas the BLS data are based on union self-reporting of their membership. The BLS figures are likely to overstate union membership relative to Troy, particularly in RTW states where many workers are covered under collective

bargaining contracts negotiated by unions but do not pay union dues.
Indeed, Troy specifically cautions against comparison of his data and
BLS information in assessing the impact of RTW laws.[5]

Even ignoring these comparison problems, it seems unreasonable to
assume that omitted state differences in attitudes were roughly constant
between 1936 and 1964. Small impacts caused by the law might easily be
lost in the changing attitudes of the populous.

Simultaneity

Other authors have wrestled with issues of causality and simultaneity.
The fear is that in states where unions command a larger share of the
work force, their political power is greater and thus they can influence
whether or not a right-to-work law is passed. Although this is arguably
similar to the omitted variable bias problem, it is conceptually distinct.

The solution proposed by several authors is to estimate cross sectional,
simultaneous equations models where the level of union membership and the
probability of the state having a RTW law is estimated simultaneously.
The problem with these models is that identification is achieved either
through the exclusion of one or several variables or by exploiting functional
form assumptions. Both methods have troubling implicatons. For example,
Moore and Newman[6] include the percentage of the work force which is non-
agricultural and the population density in their equation which predicts
the presence or absence of a RTW law, but exclude these in their second
equation which explains the level of union membership. This exclusion
does not have obvious economic appeal. By contrast Warren and Strauss[7]
estimate a mixed logit model which requires no exclusions where identifica-
tion can be made by exploiting non-linearities in the system. Identification

through functional form is always troubling. And in this case the estimation technique constrains the coefficient on the RTW variable to be exactly equal to the coefficient on union membership in the RTW equation times the variance of the error term in the union equation, a restriction which also lacks economic appeal.

A Flows Approach

We propose an alternative to the stock approach which exploits the fact that data are available annually on the number of workers organized under union certification elections certified by the National Labor Relations Board (NLRB). Because our data are consistent over time, and because flows can be a very sensitive indicator of a changed environment, this approach allows us to conduct a more powerful test of the impact of RTW laws than might be achieved with stock models. We are able to control for omitted variable bias and to test for simultaneity bias.

Potential bargaining units are established by the NLRB in particular plants or for particular crafts. A union is certified by the NLRB as the sole bargaining agent for a unit either by winning a certification election or, much less commonly, through voluntary recognition of a union by the employer. Under the election process, when at least 30% of the workers in a potential bargaining unit sign organization cards asking to be represented by a union, the NLRB will supervise a certification election. If the union captures a majority of the votes cast, the union is certified as the sole bargaining agent for all the workers in the unit regardless of whether or not they join the union. Data on the number of elections are available yearly by state after 1946, and on the number of workers in newly certified bargaining units after 1950.

Once established, these units remian unionized until the firm moves,
goes out of business, is sold to an employer who does not recognize the
union, or until a union decertification election is held. In a state
with a constant labor force, unions can grow either by having new units
certified, or by growing along with firms that have already been organized.
Unions decline when they are decertified and when the firms or trades
to which they are linked shrink. If the labor force is growing, unions
will decline relatively if they are unsuccessful in organizing the new
workplaces and the workers in them. Even in steady state equilibrium
then there will be a need for new organizing since declines caused by
old firms shrinking or closing must be offset by organizing.

When conditions change, and the equilibrium level of union membership
is higher or lower than the existing level, the stock must adjust somehow.
In many ways, the adjustment process is similar to that in a putty-clay
model of capital. Once established, bargaining units tend to remain in
place and decay or depreciate slowly because the units fortunes are largely
tied to the fortunes of the firm. Each year a few union plants go out
of business. The stock would be reduced if organizing did not compensate.
Just as in such models of capital, organizing (or new investment in union
capital stock) will be sensitive to changes in the union climate. And
changes which lead to a permanent reduction in the equilibrium level
of unionism will lead to dramatic falls in new organizing until the
stock has adjusted.

We envision a partial adjustment accelerator model whereby:

(2) $\quad ORG_{it}/LF_{it} = \theta(U^{*}_{it}/LF_{it} - U_{it-1}/LF_{it-1}) - \delta U_{it-1}/LF_{it-1}$

where

ORG_{it} = Newly organized workers

θ = Rate of adjustment

U^{*}_{it} = Equilibrium level of unionization [similar to equation (1)]

LF_{it} = Labor force

U_{it-1} = Actual level of unionization

δ = "Decay" rate of existing units

Suppose for example that passage of a RTW law actually caused the equilibrium level of union membership as a percent of the labor force to fall by 3 percentage points, say from 33% to 30%. Suppose the decay rate was 5%, and that the size of the labor force is unchanging. Then prior to passage of the RTW law, 1.65% of the workforce would have to be organized each year to maintain a steady state. After the law is passed, if the rate of adjustment was .4, we would expect to see new organizing fall to .45% in the year immediately following passage. (See Figure 1). After the initial fall, organizing would increase slowly and would ultimately stabilize at a new level of 1.5%. Thus in the first five years after passage, organizing would be depressed by nearly 40% on average. In the next five organizing would be reduced about 20%. And ultimately it ought to be 10% lower overall than previously. Such large changes in organizing ought to be easy to observe and measure.

By contrast the stock of union workers would decline by just
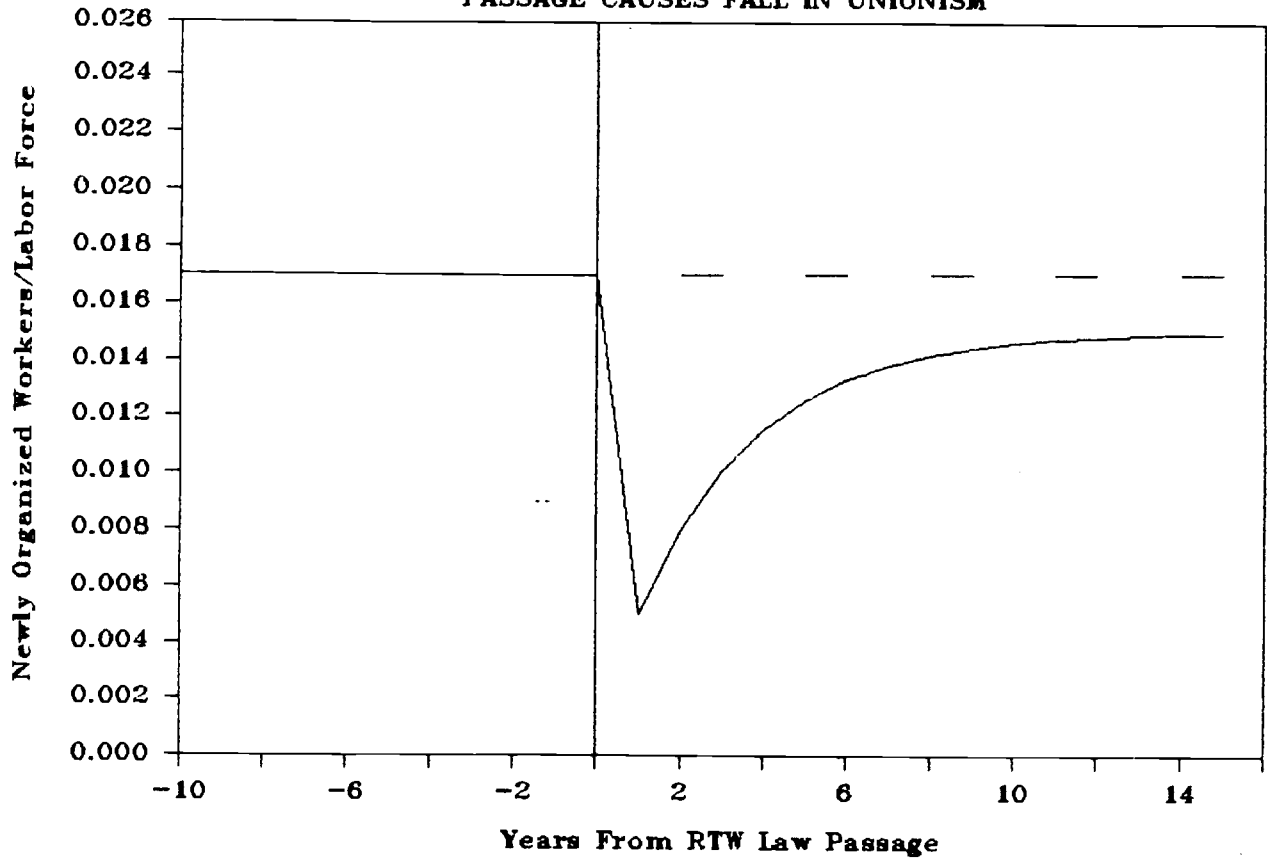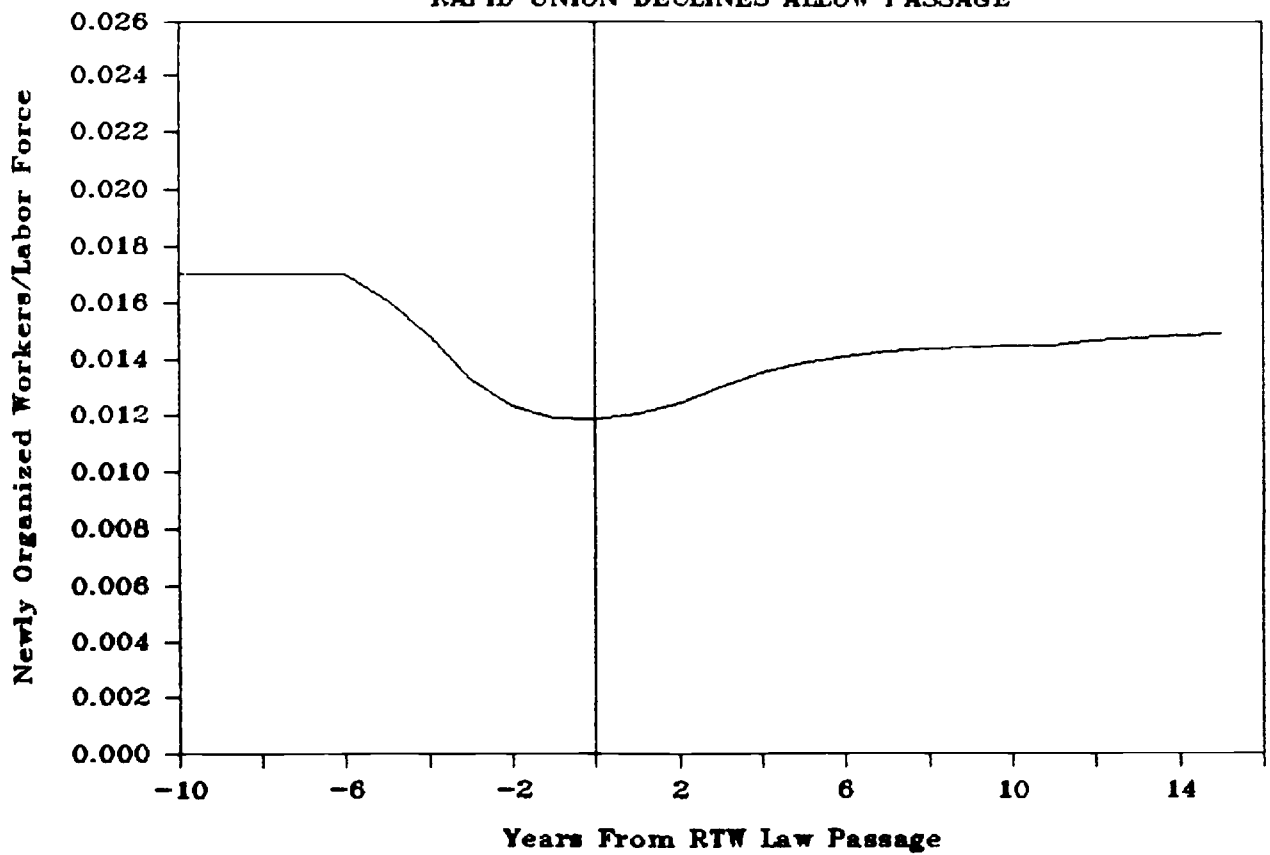
FIGURE 1

PASSAGE CAUSES FALL IN UNIONISM

FIGURE 2

RAPID UNION DECLINES ALLOW PASSAGE

.6 percentage points or 2% in the first years and the decline would reach 10% only asymtotically. These smaller changes will be much harder to detect. A primary advantage of the organizing data then is the much greater sensitivity of flows to a changed environment. In addition, the fact that annual data are available starting in 1951 means that in seven states we can perform strong tests for omitted variable bias and simultaneity. We can easily allow for fixed effects over a relatively short time series to control for omitted. variable bias. And we can exploit the basic ideas behind Granger/Sims causality tests to explore the seriousness of simultaneity problems in the specification.

We can estimate a model of new organizing and allow for fixed state effects. This controls for any time invariant state to state differences in tastes which might account for the passage of RTW laws in some states, but not in others. Data are available on organizing both before and after passage of RTW laws in the seven states which passed such laws after 1955. For these states, we can estimate a fixed effects model to examine the importance of omitted taste variables in the results of the 50 state equation.

It is possible, though, that tastes had changed over time in these seven states or that unions lost power. These changes could explain why a RTW law was finally passed. Thus, we might expect to see less organizing in the period following passage of a RTW law because the mere passage of the law indicates that anti-union sentiments were rising. Here again, the law need have no real impact; it might merely reflect omitted variables which influence union organizing. Since the omitted variables are changing over time, fixed effects models will not adequately capture them.

The basic idea behind Granger/Sims type causality tests is that events cannot have an impact before they have occurred. If RTW laws have a real impact on unionization, then their impact ought to be felt only after the passage of the laws. By contrast, if passage of a RTW law indicates the culmination of growing anti-union sentiments, organizing declines ought to be as significant in the years just prior to passage as just after passage. Assuming these new anti-union sentiments level off and a new lower equilibrium level of union membership exists, the alternative pattern is illustrated in Figure 2. So the important test is whether or not organizing was diminished in the period just prior to passage as well as the period just afterward.

With annual data, then, we can impose rather strict tests of both omitted variable bias and simultaneity. Fixed effects can be used to test for omitted variable bias. A leading indicator of impending RTW passage can be used to test for simultaneity. If we do in fact observe the pattern displayed in Figure 1, rather than some other pattern, it is powerful evidence that RTW laws do in fact have real impact.

In fact since stock and flow models always have a correspondence, these same methods could be applied to accurate annual data on the stock of membership. The advantages to using an organizing model rather than one based on stocks is that annual data are available and this flow ought to be very sensitive to changes in the union environment. A 50% change in a measured variable is much easier to capture than a 5% change.

A Model of Union Organizing

We can begin with a simplistic model derived from equations (1) and (2). If we assume instantaneous adjustment we can derive:

(3) $ORG_{it}/LF_{it} = \Delta X_{it}B + \delta \Delta RTW + (\Delta LF_{it}/LF_{it} + \delta)X_{it}B + u_{it}$

Although this instantaneous adjustment model is unrealistic, it does point out that new organizaton is influenced by changes in exogenous variables (including RTW), by the need to organize new workers when the labor force is growing, and by the need to replace members lost to the various flows out of unionism. A model that allows for non-instantaneous adjustment would look similar but it would include many lagged values of X and RTW and LF.

We experimented with a variety of specifications involving a considerable number of lagged independent variables, non-linear functional forms, and multiple interactions. Ultimately we found a straightforward log-linear specification, which included both levels of the explanatory variables (the Xs) and the change in these variables in the previous five years as the independent variables, to be the most stable and intelligible. For obvious reasons, we concentrated particular attention on our RTW variable.

We ran state cross-section annual time-series regressions for the period from 1951 to 1977. Our primary dependent variable is the natural log of ORG/LF -- the number of employees in bargaining units where unions won an NLRB election divided by the non-agricultural labor force in the state. As independent variables we have included variables which measure demographic and industrial structure, local tastes and preferences, economic conditions, and the governmental/legal environment.

Demographic characteristics of workers are emphasized as important determinants of the levels of union membership by virtually every student of union membership.

The most commonly cited factors are sex and race. Women typically display

weaker labor force attachment than men. If their expected tenure is

shorter than that of their male counterparts, the present value of long

term gains to unionism is reduced so that the up-front organizational

costs are likely to be a greater deterrent to organizing a union.[8]

Blacks, on the other hand, may be more inclined toward unions as a defensive

reaction to employer discrimination.[9] In addition, urban residents are

generally considered more likely to organize.[10] Apparently, small towns

suffer diseconomies of scale and/or different attitudes toward unionism.

In our models, %FEMALE and %BLACKS and %URBAN represent the proportion of

the non-agricultural labor force which is female, black, living in urbanized

areas, respectively.

Structural differences in the nature of the product or factor markets,

the nature of technology, and the "strategic position" of workers is the

cornerstone of the work of Dunlop, Shuster, and many others who analyze

union growth.[11] These variables have obvious impact on the short and

long-run elasticity of demand for labor plus important implications for the

dynamics of relations between labor and management. The proportion of workers

in each one digit industry and occupation was therefore included in many of

our regressions. We also tested a more crude structural variable -- %BCW --

the percentage of the state's non-agricultural work force which is in

blue-collar occupations.

None of the demographic and structural variables are available

annually in the early period of our sample. Therefore, we collected the

information from the decennial Census of Population and used straight

line extrapolation to fill in the missing data. This methodology is

unlikely to create a severe errors in variables problem since these
measures change only slowly over time.

Local tastes and preferences have generally been cited to account
for the large differences in the level of unionization across states.
Studies of right-to-work laws have emphasized the importance of local
attitudes in stimulating RTW laws and in holding down the level of unionism.
This is the heart of the simultaneity issue and requires particular attention
in any RTW study even though our methodology allows for a correction of these
problems. Ashenfelter and Pencavel used the percentage of a state's
Congressional delegation which was Democratic as a measure of attitudes
towards unions.[12] We chose to use a similar but more sophisticated proxy.
The AFL-CIO publishes a so-called COPE rating for each Congressman based
on his/her voting record. Using this information, we derived our
own COPE rating for each state over five year periods based on the voting
behavior of a state's Congressional delegation on key labor issues over
that five year time. The variable was detrended so that the variable measures
preferences in the state relative to the national average over any five
year period. Changes in organization caused by movements in the national
average will be picked up on our time dummies as described below.

Freeman and Medoff have pointed out that an important feature of
local tastes and preferences which has been ignored in most empirical
work, is local employer oppostion.[13] In our work we include a variable,
TOUGH, which represents the number of unfair labor practice charges
filed against employers divided by the number of elections in each state. We also
include a five year average change in both TOUGH and COPE in our models.

Finally, in our fifty state regression, region dummy variables are also included to attempt to capture any differences in attitudes prevalent in a particular region. The unique environment of the South may negatively influence union organizing. This region may be difficult to organize because of the nature, composition, and location of industry, the surplus of low-income agricultural workers, the influence of racial prejudice, the prevailing ideology, and the strong anti-union attitude existing among Southern workers, employers, and the community at large. We include four region dummies to control for any geographic variation. In later seven state regressions we allow for individual state effects eliminating any stationary differences across states.

Commons and Davis both pointed to economic conditions and cyclical swings in employment and prices to explain union growth.[14] Davis postulated that grievances built up in downturns and could be acted upon in the upswing. Both Davis and Ashenfelter-Pencavel suggested that sharp price rises contributed to union growth. This latter finding is present in several other works including Dunlop, Adams, and Krisklov.[15] The usual explanation for this finding is that real wages fail to keep pace with inflation and workers organize as a "defensive reaction." Price changes may also be capturing business conditions or they may be co-linear with the periods of "fundamental unrest," as emphasized by Dunlop.

Individual year dummies were included to capture effects of national business cycle swings. It is difficult to capture the business cycle by state because unemployment rate data are not available until late in the period. Our principal proxy is the percentage change in the manufacturing

weekly wage. Presumably this variable captures relative tightness of the
labor market in an area. We used two versions of this variable. The
five year average rate of growth in wages, TRENDW, is an attempt to
capture something of the long-run tightness of the market. %CHGW is
the change in wage relative to this long-run trend. This latter variable
is introduced to capture the cycle within the state.

No price indexes are available by state until late in our sample
period, thus we cannot test the hypothesis that rapid price increases
spur union organization. The impact of differences over time in the
national rate of inflation are captured by the individual year dummy
variables.

In states where there is long term employment growth, unions will
have to engage in more organizing if they are to keep pace. Thus we
created a long term growth rate of employment, EMRATE. To minimize
the cyclical component of any measure of employment growth we created
five year moving average annualized growth rates.

A partial adjustment accelerator model predicts a sharp fall in
organizing immediately after a RTW law is passed and smaller declines
from the pre-law level in later years. We have separated the RTW
variable into a series of mutually exclusive dummy variables designed to
capture the impact in five year intervals following passage of the law.
RTW1-5YR was 1 if and only if a RTW law had been passed in the state
in the past five years. RTW6-10YR was unity if a law was passed between
6 and 10 years previously and so on.

We began by running regressions for all fifty states without additional controls for fixed effects or a leading RTW indicator to test for simultaneity. We then explored the potential problems that omitted variables or simultaneity could be causing in these fifty state results by running a separate set of regressions on the group of seven states where data are available both before and after passage of a RTW law.

Fifty State Empirical Results

The results presented on Table 3 are consistent with the model we have advanced. The demographic variables performed as expected. In states with fewer women or more blacks in the labor force there is more organizing. A five percentage point change in these variables will lead to a ten and three percent increase in organizing respectively. Similarly, states with five percentage points more urban workers have four percent more organization.

When structural variables representing the percentage of the work force in one digit occupations or industries are included in the equation, their coefficients are erratic and their effect on other variables minor. The simple variable %BCW, representing the proportion of the work force in blue collar occupations, performed better and was therefore included instead of the more detailed measures.

The taste and preference variables captured a sizable proportion of the explained variations in organizing. TOUGH performed exceptionally well. States with one standard deviation higher level of TOUGH show organization rates almost 10% lower. Morever, changes over time are also quite important. States where employers are becoming increasingly

TABLE 2

Variables Used in Empirical Work

ORG/LF	employees in units choosing representation/non-agricultural labor force in thousands
ELECT	NLRB elections/non-agricultural labor force in thousands
PERC	union victories/elections
WINSIZE	employees in units choosing representation/union victories
EMPGROWTH	average annual rate of growth of the non-agricultural work force over five years
%BLACK	percent of the labor force that is black
%FEMALE	percent of the labor force that is female
%URBAN	percent of the population living in urban areas
%BCW	percent of the labor force that is blue-collar
TOUGH	number of employer unfair labor practices per election
COPE	rating of the congressional delegation's votes on labor issues
NORTH	dummy variable for Northern states
SOUTH	dummy variable for Southern states
CENTRAL	dummy variable for Central states
TRENDW	average annual growth in the manufacturing weekly wages over five years
CHGW	difference between the proportionate change in wages in a given year and TRENDW.
PRERTW	dummy variable which is 1 in the 5 year period prior to passage of a RTW law
RTWO-5YR	dummy variable which is 1 only in the first 5 year period after passage of a RTW law
RTWt-5YR	dummay variable which is 1 only in the period t to 5 years after a RTW law was passed
_XXXX	five year change in variable XXXX

TABLE 3

Fifty State Regression Results on LN(MEMB)

	MEAN (Standard Deviation)	COEFFICIENT (Standard Error)
Ln ORG/LF	1.33 (0.79)	-
%BLACK	9.89 (13.03)	.007 (.002)
%FEMALE	33.29 (4.84)	-.027 (.011)
%URBAN	62.83 (15.30)	.008 (.002)
%BCW	48.09 (7.13)	.023 (.003)
TOUGH	1.42 (0.86)	-.106 (.035)
ΔTOUGH	0.21 (0.86)	-.098 (.030)
COPE	-0.56 (29.18)	.001 (.001)
ΔCOPE	-0.23 (24.72)	.000 (.001)
NORTH	.185 (.388)	.049 (.070)
SOUTH	.326 (.469)	.148 (.070)
CENTRAL	.246 (.431)	.150 (.054)
TRENDW	.049 (.032)	-.509 (.933)
%CHGW	.011 (.066)	1.68 (6.77)

TABLE 3 (cont.)

Fifty State Regression Results on LN(MEMB)

	MEAN (Standard Deviation)	COEFFICIENT (Standard Error)
RTW1-5YR	.046 (.209)	-.531 (.093)
RTW6-10YR	.070 (.255)	-.262 (.078)
RTW11-15YR	.070 (.253)	.040 (.080)
RTW16-20YR	.065 (.247)	-.026 (.084)
RTW21tYR	.113 (.316)	-.075 (.071)
EMPGROWTH	.021 (.068)	1.75 (0.39)
N	1364	1364
SEE	-	.638
R^2	-	.367

All regressions include Δ%FEMALE, Δ%BLACK, Δ%BCW, Δ%URBAN, an intercept and individual year dummies

hostile to unionism, according to this measure, show reduced organization. COPE, on the other hand, performed poorly. Neither levels or changes seemed to affect organizing. TOUGH appears to dominate COPE. If TOUGH is removed from the equation, COPE's performance improves considerably.

The region dummies show a surprising pattern. When controls, we used for demographics, structure, tastes and preferences, and the presence of a RTW law were included, SOUTH no longer has a negative coefficient. Indeed the coefficient was higher than those for North or West. Without these controls the variable was strongly negative. The lower rate of organizing in the South therefore can apparently be "explained" by the factors included in the equation.

The short run cyclical variable %CHGW, the difference between the annual wage change and the long run trend, had the expected positive sign and was significant. Wage growth which is 6% higher than average, yields a 10% increase in organizing. The long run trend in wages had no influence here. We experimented with a variety of functional forms using lagged values of %CHGW; none yielded satisfactory results.

As expected states with greater employment growth had higher levels of organizing. A 10% growth rate in employment caused organizing to grow by 17%.[16] The yearly time dummies are not reported here, but they show a strong negative trend. Relative to 1950, the 1977 dummy shows that organization has fallen by 40%, other things equal.

Finally we turn to examine the impact of RTW laws based on the fifty state regression.

The Effects of RTW

The coefficients on the RTW variables are quite consistent with the hypothesis that the passage of a RTW law diminishes the equilibrium level of unionism and that a partial adjustment model characterizes the resulting changes in organizing. In the first five years following passage of a RTW law in a state, organizing is reduced by 50%. In the next five years, it is reduced by roughly 25%. In later years it appears to be reduced somewhat, though we do not have sufficient data to conclude that the reduction is statistically significant.

Over our sample period, employees in new bargaining units average 2% of all union workers -- i.e. the flow is roughly 2% per year. If organizing falls by 50% for five years and 25% during the next five, then after 10 years, membership will be roughly 7% lower than it would have been otherwise. If 7% was the true long run reduction, then once the new equilibrium is achieved organizing should be permanently depressed by 7%. In our data, the standard errors on the dummy variables capturing the impact of the laws after 10 years are typically around 8%. Thus the modest 7% long term reduction could not show up as statistically significant. The actual coefficients for the later years average closer to 3 or 4%, though the coefficient designed to capture the period after 20 years is almost exactly what we would predict if RTW laws ultimately caused a 7% decline in unionism (and organizing).

Thus, these results seem to be consistent with an interpretation that RTW laws ultimately diminish membership by 5 to 10% -- or union workers as a fraction of the total work force falls by between one and three percentage points in most states. (This impact would be in addition to the loss of membership that might occur if members of existing bargaining

units choose not to be union members when union shop rules are eliminated.)
It is possible, however, that organizing is depressed only in the short
run. Then the stock would also be diminished only in the short-run. We
think it is more plausible that the stock is permanently reduced by 5 to 10%,
but other interpretations of the long-term impact are possible.

If there is a permanent reduction in the stock, it is easy to
see that with the relatively small sample sizes available for the stock
equations estimated by previous authors, a statistically significant
impact amounting to 5% might not be detected. After all, the standard
errors in our equations which have considerably more observations make
measurements that precise impossible.

It is possible, however, that the results reported in these fifty
state regressions are spurious, and that either omitted variable bias
or simultaneity contaminates the results. We turn then to an examination
of these issues by exploring the impact of RTW laws in the seven states
where sufficient data on organizing is available both before and after
passage of the law to allow fixed effects and simultaneity tests.

Seven State Empirical Results

It seems unlikely that time invariant differences across states
could account for the results we have observed. Generally we would not
expect that fixed state differences could generate the pattern of a
declining impact of RTW laws as time since passage increases. Nonetheless
our first step was to determine the impact of fixed effects on a regression
based only on these seven states. Those results appear on Table 4.

TABLE 4

Seven State Results on RTW Coefficients

	FIXED EFFECTS	SIMULTANEITY TEST
PRGRTW	-	.255 (.198)
RTW1-10YR	-.321 (.121)	-.318 (.125)
RTW11+	.054 (.177)	.012 (.245)
SEE	1.03	.642

Note: All other variables included in Table 3 are included here except YEAR variables which have been collapsed to cover five year intervals.

Because of the smaller number of observations, we needed to make some minor changes in our specification. We collapsed our RTW variables to cover a longer interval. We allowed five year time dummies rather than the individual year dummies we used in the fifty state regression. As an additional test, in these regressions we also controlled for first order serial correlation and for heterskedasticity using the Parks method.

The fixed effect results are remarkably similar to those found in the fifty state regressions. In the first 10 years after passage, organizing is diminished 32% -- a figure very similar to the 39% average for the first decade obtained by averaging the first two five year coefficients in the fifty state regression. After a decade, the impact is insignificant, as in the fifty state case. And once again a large standard error leaves open the possibility that organizing remains depressed by 5 or 10% in the long run.

Thus, time invariant state differences do not seem to account for the results we find. A second concern is that declining union strength may allow passage of a RTW law. Thus reduced organizing and passage of the law are both indications of recently reduced union power. In this case, a leading indicator of impending passage of a RTW law provides an important test. If declining union strength leads to reduced organizing and to the passage of the law, then the period prior to its passage ought to be one where organizing is depressed reflecting the declines in union power. If the RTW law itself causes the declines, then there ought to be no reduction in organizing in the period prior to passage.

We included a leading indicator, labeled PRE-RTW, as a exogeneity/ causality indicator in our seven state regression. This dummy variable

took on the value of one only in the period five years before the passage of a RTW law. It thus captures any changes in organizing just prior to passage of a RTW law.

The results shown on Table 4 are quite striking. In the period before the passage of RTW laws, organizing is not depressed. Indeed, in these seven states, organizing was actually somewhat above average, though the coefficient on PRE-RTW is not significant. The decline in organizing seems to follow the passage of a RTW law, not precede it. Indeed it is even possible that RTW laws are passed when unions appear to be becoming stronger (as evidenced by increased organizing) in order to diminish their strength.

A look at two states where RTW laws were recently passed or repealed offers some final evidence. In the three years prior to passage of Louisana'a RTW law in 1976, an average of 4,024 workers were organized each year; in the three years after, the figure averaged 2,380.

Indiana offers an ideal natural experiment because it is the only state where a law was first passed and later repealed. Figure 3 reveals a final bit of evidence that the effects of the law are real. Displayed is the number of workers in newly certified bargaining units per 1,000 non-agricultural workers. Two year moving averages were used to reduce the variability of year-to-year fluctuations. There is a clear downward trend in organizing in the state over the period before the RTW law was passed. The trend was interrupted by a sudden drop in organizing in the period when the law was in effect. It promptly resumed after the law was repealed.

FIGURE 3

Organizing in Indiana
Two Year Moving Averages

Newly Cert. Workers/Labor Force(1000s)

RTW Passed

RTW Repealed

Year
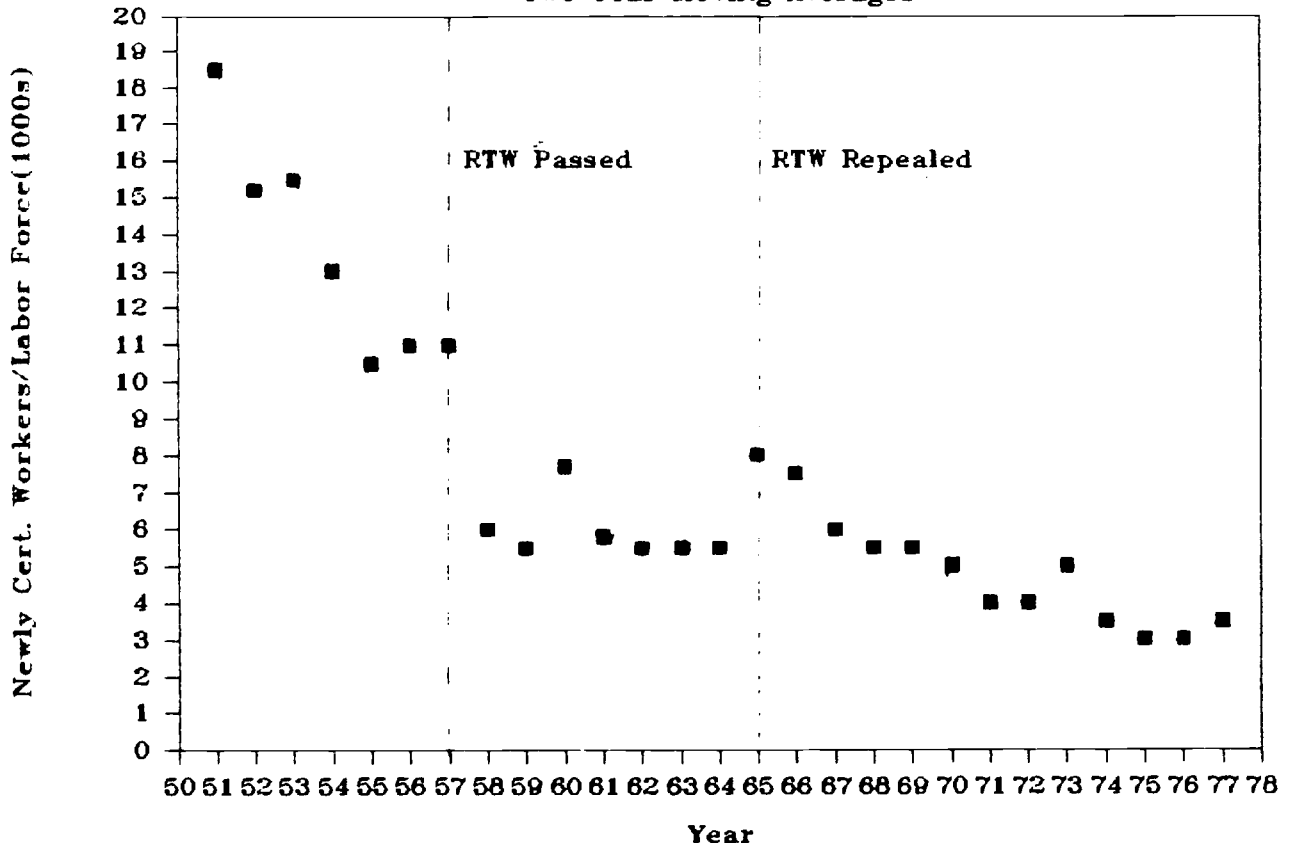
Thus it appears that the results presented earlier are not the product of omitted variable bias or simultaneity. They strongly suggest that organizing is dramatically depressed for roughly a decade after passage of a right-to-work law and quite possibly is slightly reduced permanently. The stock of union memberhsip falls between 5 and 10% initially and this reduction may be permanent.

The obvious question remains as to how or why RTW laws have the impact they do. We cannot infer the exact reasons why laws diminish organizing so sharply in the short run. We can, however, decompose the decline and shed some light on the mechanism.

Decomposition of RTW Laws Impact

It is possible to decompose MEMB into several components and consider the impact of RTW laws on each of these. The decomposition can be performed as follows:

$$ORG/LF = \#ELECTIONS/LF \times \#WON/ELECTIONS \times ORG/\#WON$$

$$ORG/LF = ELECT \times PERC \times WINSIZE$$

The number of new members (per 1,000 non-agricultural workers) obtained through elections is the product of the number of elections times the percent of elections won by the union times the average size of the bargaining unit where elections are won. We ran separate regressions on all fifty states for each of these variables. The RTW coefficients from the fifty state regression are provided in Table 5.

Right-to-work laws seem to have their most important impact in reducing the number of elections held. Over half of the decline in

TABLE 5

Fifty State Regressions - Decomposition
of Components in Ln ORG/LF

	Ln(ORG/LF)	ln(ELECT)	ln(PERC)	ln(WINSIZE)
RTW5YR	-.531 (.093)	-.288 (.045)	-.054 (.025)	-.189 (.079)
RTW10YR	-.263 (.078)	-.130 (.038)	.015 (.021)	-.148 (.067)
RTW15YR	.040 (.080)	.055 (.039)	.004 (.022)	-.019 (.068)
RTW20YR	-.026 (.084)	-.024 (.041)	.018 (.023)	-.020 (.072)
RTW25+YR	-.075 (.071)	-.135 (.034)	.040 (.019)	.019 (.061)
SEE	.638	.308	.173	.543
R^2	.367	.444	.371	.410

Note: All regressions included all the variables listed in Table 3.

new membership in the ten years after passage can be attributed to a
decline in elections. The remainder of the effect is accounted for
largely by a reduction in the average size of units won. Over the
first decade after passage WINSIZE is reduced by roughly 16%. Interest-
ingly, the laws have only a small effect on the proportion of elections
won.

If employees are organized up to some threshold where expected benefits
equal expected costs, an increase in costs or reduction in benefits
will surely cause a decrease in elections. If larger bargaining units
are more difficult to organize, presumably a change in the organizational
environment could result in the medium and smaller units being organized
first. In general, we would not expect much change in the winning per-
centage. Presumably, unions organize plants up to some point where they
have a certain probability of winning. Organizers almost never call
elections until they have 65 percent of the workers to sign cards in
support of the unions. If this is the stopping rule, then the winning
percentage is unlikely to vary substantially.

Thus organizing is clearly reduced in the short-run with fewer
elections and smaller winning unit sizes. The ultimate question is
why? The most obvious explanation is simply that passage of a RTW
law makes union membership less attractive. Without the ability to
enforce payment of dues or to fine those who cross the picket line,
unions may prove less powerful. Their strike threats are diminished
both by reduced financial resources and by less certain participation.
As a result after a right-to-work law is passed, membership is sharply

reduced as plants formerly at the margin are now far removed from it.

There is a second interpretation which arose in discussions with those on both sides of the controversy. A major part of the law's impact may be through the pyschological/symbolic effect passage of a RTW law may have on workers. Successful organization requires that a few workers inside a plant take a highly visible and activist role. The costs to these activists can be enormous, ranging from harrassment to loss of their jobs. Even those who are not activists must take the highly visible step of signing an authorization card. And in considering whether or not to vote for a union, workers often fear they will lose their jobs or suffer other costs if their company is hostile. Thus the perceived strength of the union may be critical to the willingness of activists and others to become involved in an organizing drive. A highly visible defeat such as the passage of a RTW law (or the crushing of PATCO) may severely damage the union's credibility and appeal to workers. There is at least some evidence that the psychological impact may be important. In Missouri, for example, after a RTW law was defeated, new organizing jumped dramatically.

Conclusion

Our results show a strong short-run reduction in union organizing following the passage of a RTW law. Ommitted variable bias and simul- taneity problems do not skew our findings. Organizing is reduced by nearly 50% in the first five years after passage of a RTW law and by half that amount in the subsequent five. Overall our results suggest that membership in unions is reduced between 5 and 10% after passage. The findings are consistent with a permanent reduction of this magnitude

in the stock though it is also possible that the stock gradually recovers over many decades. It appears that right-to-work laws are not merely symbols. They have real significant effects.

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Footnotes

1. The Taft-Hartley ammendments also outlawed the "closed shop" which required that workers be union members before they are hired, although the legal status of such arrangements in construction was ambiguous until the passage of the Landrum-Griffith Act in 195?.

2. Lumbsden, Keith and Craig Peterson, "The Effect of Right-to-Work Laws on Unionization in the United States," Journal of Political Economy, (December, 1975): 1237-1248.

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3. Meyers, Frederic, Right-to-Work in Practice, New York: Fund for the Republic, (1959): 21-40.

4. This is essentially the method used by Lumsden and Peterson.

5. Troy, Leo, "Trade Union Membership 1897-1962," Occasional Paper No. 92, New York: National Bureau of Economic Research, (1965): 18.

6. Moore and Newman, loc. cit.

7. Warren and Strauss, loc. cit.

8. See for example:

   Brotslaw, Irving, "Attitudes of Retail Workers Toward Union Organization," Labor Law Journal, vol. 18, no. 3, (1967): 167.

   Dunlop, John T., "The Development of Labor Organizations: A Theoretical Framework," In Lester, R.A. and Joseph Shister, eds., Insights into Labor Issues, New York: MacMillan, (1948): 163-193.

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   For the opposite result--that women are just as likely to desire unions as are men, see:

   Farber, Henry and Daniel Saks, "Why Workers Want Unions: The Role of Relative Wages and Job Characteristics," Journal of Political Economy, vol. 88, no. 2, (1980): 349-369.

Footnotes (continued)

9. Kochan, Thomas A., "How American Workers View Labor Unions," Monthly Labor Review, vol. 102, no. 4, (1979): 23-31.

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    Moore and Newman, loc. cit.

11. Dunlop, loc. cit.

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15. Ashenfelter and Pencavel, loc. cit.

    Dunlop, The Development of Labor Organization, op. cit.

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16.   This result does not imply that new organization is more common
       among new workers. On the contrary, it suggests a very low rate of
       organizing among new workers. With no employment growth, roughly
       2 percent of the work force are organized each year. With 10 percent
       annual growth in employment, unions should organize a third of the
       new workers--or 3 percent of the labor force--in addition to the pre-
       vious 2 percent, just to maintain their relative share of the work
       force. Instead organizing rises only to 2.5 percent. This result
       may in part reflect simultaneity. Firms are moving to the areas
       where organizing is difficult.