NONUNION WAGE RATES AND THE THREAT OF UNIONIZATION

HENRY S. FARBER*

Using CPS data for 1977–2002, the author investigates the extent to which the threat of union organization increases nonunion wages and reduces the union/ nonunion wage differential. The results are mixed. Estimates employing the predicted probability of union membership as a measure of the union threat show no important link between the union threat and either nonunion wages or the union wage gap. Estimates focusing on two states' introduction of right-to-work laws, which arguably affect the threat of union organization independently of changes in labor demand, show that in one state the law was associated with a statistically significant drop in nonunion wages. Finally, an analysis of wage data for three industries that underwent deregulation—another natural experiment in which labor demand changes are unlikely to have been a complicating factor—yields stronger evidence of threat effects on nonunion wages than do either of the other two analyses.

The decline of labor unions in the United States over the last quarter of the twentieth century has important implications for the wages of workers, particularly for those who are less skilled.[1] In this study, I use data from the Current Population Survey (CPS) from 1973 through 2002 to investigate the extent to which the wages of nonunion workers are affected by the threat of unionization and, by extension, how the decline in union membership over this period has affected the wages of workers.

Lewis (1963) identified two effects of unionization on the wages of nonunion workers. The first is a "spillover effect" that results from a reduction of union employment due to the increase in the union wage. The workers who are no longer employed

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The data and programs used to generate the results presented in the paper are available from the author at Industrial Relations Section, Firestone Library, Princeton University, Princeton, NJ 08544-2098. E-mail: farber@princeton.edu.

[1]Based on data from the Current Population Survey from 1973–2002, the fraction of the private sector work force in the United States who were union members fell from approximately 25% in 1973 to approximately 9% in 2002. See Farber and Western (2001, 2002).

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in the union sector move to the nonunion sector, resulting in an increase in labor supply in that sector that reduces the equilibrium wage. The second is the "threat effect" that results from a desire by nonunion employers to avoid unionization by providing higher wages to their workers.[2]

Rosen (1969) and Ashenfelter, Johnson, and Pencavel (1972) presented models of wage-setting by nonunion employers that account explicitly for the role of higher wages in deterring union organization. In the same spirit, Lazear (1983) developed a model of wage determination by nonunion employers in which these employers bear costs to avoid organization, but these costs are not directly in the form of higher wages. Rosen (1969) found that wages of both union and nonunion workers are positively related to the fraction unionized in their industry (industry union density), which is consistent with a threat effect. Freeman and Medoff (1981) found that while union wages are positively related to industry union density, nonunion wages are not systematically related to industry union density. Lewis's (1986) survey suggested that nonunion wages are positively related to industry union density but that this relationship is not as strong as that found for union workers.

A general problem with the empirical literature on the threat effect is that it is not clear how to measure the threat itself. The existing literature uses industry union density to measure the threat, but there are at least two potentially important problems with this. First, the threat effect relies on the *marginal* effect of the nonunion wage on the probability of unionization for nonunion workers. It is not at all clear that this marginal effect is positively related to industry union density. Second, there are potentially serious omitted variable problems in using this measure. In the cross-section, it may be that unions are more successful in organizing workers in high-wage industries. To the extent that these omitted factors are fixed over time, use of repeated cross-section data with industry fixed effects could eliminate this problem, but there are reasons to suspect the existence of time-varying factors that affect both industry union density and wages. For example, it might be that increased international competition at the industry level reduces demand for less-skilled workers and makes it harder to organize workers.

While I do not have a complete solution to these problems, I present several alternatives. I start by outlining the standard model of wage determination by a nonunion employer when faced with the threat of union organization. I use this model to shed some light on how the threat of unionization, however measured, might affect the wage rate and the union wage gap. The model suggests that the nonunion wage will be directly related and the union wage gap inversely related to the threat of union organization. Next, I use repeated cross-section data from the CPS from 1977–2002 to develop a richer measure of the threat as the predicted probability of union membership as a function of individual demographics, year, industry, and state of residence. I use this measure to estimate earnings functions that use several sources of variation in the likelihood of union membership to identify the threat effect and its change over time in a manner that reduces the likelihood of omitted variable bias. I estimate several specifications that rely on (1) within-industry and within-state variation, (2) within-state variation alone, and (3) within-industry variation alone. Finally, I investigate two cases in which there was arguably a reduction in the likelihood of union organization that was not correlated with a reduction in the demand for non-

[2]Of course, the first-order effect of unions on wages is on the wages of the union members themselves. Estimates of the union-nonunion wage differential using micro-economic data generally range from 10% to 25%. Lewis (1963, 1986) presented a comprehensive analysis and reviews of estimates of the union-nonunion wage differential. As union membership has declined, fewer workers have received this wage advantage. Card (2001) found that the decline of unions between the mid-1970s and the early 1990s, with fewer workers receiving the wage advantage, can account for approximately 20% of the increase in male wage inequality over that period.

union labor. These were the wage changes surrounding the introduction of right-to-work (RTW) laws in two states during the period studied (Idaho in 1985 and Oklahoma in 2001) and wage changes surrounding deregulation of key industries in the late 1970s and early 1980s (airlines in 1978, trucking in 1979, and telephones in 1984).

1. Nonunion Wage Determination and the Threat Effect

Following the work of Rosen (1969) and Ashenfelter, Johnson, and Pencavel (1972), I assume that nonunion employers set the wage to minimize the expected wage (the probability-weighted average of the union and nonunion wage rates) they will pay. The tradeoff in lowering the wage is that doing so will reduce wage costs if the firm remains nonunion but the likelihood of successful union organization will be higher. Let $P(\Delta, \theta)$ represent the probability of union organization, where $\Delta = (W_u - W_n)/W_n$ is the union wage gap, W_n is the nonunion wage, W_u is the union wage, and θ is a parameter that indexes the likelihood of union organization holding the wage gap fixed. This parameter is meant to capture general worker, firm, and societal attitudes toward labor unions as well as legal and other impediments to union organization (for example, right-to-work laws). Clearly, a higher union wage advantage increases the probability of successful organization, and I define θ so that higher values of this parameter imply a higher probability of successful organization. Thus, both first derivatives of the probability function, P_1 and P_2, are positive. The expected wage can be written as

$$(2.1) \qquad \begin{aligned} E(W) &= W_n + P \cdot [W_u - W_n] \\ &= W_n(1 + P \cdot \Delta), \end{aligned}$$

where I have suppressed the probability function arguments. The nonunion employer can select W_n to minimize $E(W)$, and the first order condition for this minimum is

$$(2.2) \qquad 0 = [1 - P] - P_1 \cdot \Delta(1 - \Delta).$$

The first term represents the marginal cost of raising the nonunion wage in the form of higher wages if the firm remains nonunion, and the second term represents the marginal benefit in the form of a lower probability of union organization. Note that W_u and W_n enter the first order condition only through Δ so that the nonunion employer effectively chooses the union wage gap as a function of θ. This implies that the nonunion wage is unit-elastic with respect to changes in the union wage.

The threat effect comes from the fact that P_1 is positive, so that raising the nonunion wage reduces the union wage gap and reduces the likelihood of union organization. The existing literature measures this threat by the existing fraction of the work force, usually at the industry level, that is unionized. Implicitly, this is an assumption that the nonunion wage varies directly with θ. In fact, the comparative statics of the model are unclear on this point, as the result relies on particular assumptions regarding second derivatives. The marginal effect of θ on W_n is

$$(2.3) \qquad \frac{\partial W_n}{\partial \theta} = W_n \frac{P_2 + P_{12}\Delta(1 + \Delta)}{(P_1 + P_{11}\Delta)(1 + \Delta)^2},$$

where P_{11} and P_{12} represent second derivatives of the probability function. It is convenient to work with the natural logarithm of the wage so that

$$(2.4) \qquad \frac{\partial \ln W_n}{\partial \theta} = \frac{P_2 + P_{12}\Delta(1 + \Delta)}{(P_1 + P_{11}\Delta)(1 + \Delta)^2}.$$

The denominator of this fraction is positive by the second order condition for a minimum, but the sign of the numerator is ambiguous. A sufficient condition for the numerator to be positive, so the $\partial \ln W_n / \partial \theta$ is positive, is that P_{12} is positive. Intuitively, this condition is that the marginal effect of increasing the nonunion wage on the probability of union organization increases in absolute value (becomes more negative) as θ increases.

While the sign of P_{12} and the sign of the derivative in equation (2.4) are ultimately empirical questions, it is reasonable to assume that the sign of the derivative is positive. First, since P_2 is positive, P_{12} can take on negative values while the derivative remains positive. In other words, the condition is sufficient but not necessary. Second, most probabilities of unionization are likely relatively small, as the fraction of the private sector work force that is unionized has declined (and was never very large). As θ shrinks and P approaches zero, the marginal effect of an increase in the nonunion wage on P must shrink in absolute value so that P_{12} will be positive. Thus, I proceed assuming that the threat effect increases in θ and, hence, in the probability of unionization.[3]

It may also be the case that an increase in the factors that make union organization more likely would also result in a higher union wage rate ($d\ln W_u/d\theta > 0$). The total derivative of $\ln W_n$ with respect to θ in this case is

$$\text{(2.5)} \qquad \frac{d\ln W_n}{d\theta} = \frac{P_2 + P_{12}\Delta(1+\Delta)}{(P_1 + P_{11}\Delta)(1+\Delta)^2} + \frac{d\ln W_n}{d\ln W_u} \cdot \frac{d\ln W_u}{d\theta}.$$

As noted above, the first-order condition in equation (2.2) depends on the union and nonunion wage rates only through the wage gap. Thus, the nonunion wage is unit-elastic with respect to changes in the union wage, and equation (2.5) can be written as

$$\text{(2.6)} \qquad \frac{d\ln W_n}{d\theta} = \frac{P_2 + P_{12}\Delta(1+\Delta)}{(P_1 + P_{11}\Delta)(1+\Delta)^2} + \frac{d\ln W_u}{d\theta}.$$

This relationship has important empirical implications for observed union and nonunion wages. It implies that the factors affecting the likelihood of union organization will have a larger proportional effect on the nonunion wage than on the union wage and that the union wage gap will vary inversely with the likelihood of union organization.

3. Econometric Framework

The key to the empirical analysis is finding variation in the threat (θ) that is plausibly unrelated to other factors that could have an independent effect on the wage. This is no easy task, and I use several approaches. The standard approach has been to use industry union density in a cross-section as a measure of the threat. However, this assumes that inter-industry differences in union density that are correlated with wages are uncorrelated with other factors that influence wages. To the extent that unions organize workers in high-wage industries, estimates of the threat effect derived from variation in industry union density will overstate the true threat effect. A related approach is to use repeated cross-sections to control for time-invariant industry effects in the earnings functions and rely on within-industry variation over time in union density and earnings to measure the threat effect. This goes at least part-way toward solving the omitted variable problem outlined above, but there may be time-varying industry-specific factors that both reduce wages and make union organization less likely (for example, increased international trade) so that these estimates too would overstate the threat effect.

An unexploited source of variation in the likelihood of union organization is geographic. Twenty-two states have Right-to-Work (RTW) laws that make it more difficult to organize and maintain union organization.[4] The threat of union organiza-

[3]It is the case that the observed union density results at least in part from the optimization process described here. However, it is reasonable to suppose that as θ increases and nonunion wages increase concomitantly to offset the increase in θ, the offset will only be partial and equilibrium union density will increase.

[4]The states with RTW laws are Alabama, Arizona, Arkansas, Florida, Georgia, Idaho, Iowa, Kansas, Louisiana, Mississippi, Nebraska, Nevada, North Carolina,

tion is likely to be lower in these states. Additionally, two of these states passed RTW laws during the sample period (Idaho in 1985 and Oklahoma in 2001). Strictly cross-sectional variation in the union density by state is subject to the same criticism as the use of cross-sectional variation in industry union density. There may be unmeasured factors that are correlated both with wage levels and union density at the state level. The lower union density associated with RTW laws may reflect, at least in part, lower worker demand for union representation rather than a causal effect of the RTW laws.[5] The use of repeated cross-sections with time-invariant state effects in part ameliorates this problem. This relies on within-state variation over time in the likelihood of union organization and in the wage rates. It is less likely than in the within-industry analysis that there are time-varying state-specific factors that both reduce wages and make union organization less likely. Moreover, the existence of the two states that changed the legal environment for union organization by introducing RTW laws provides an additional source of variation in union organization that is arguably exogenous to the wage-setting process.

Finally, several industries were deregulated in the early part of the sample period. These include trucking, airlines, and telecommunications. The rate and route regulation in these industries was an important factor making union organization attractive, and all of these industries had powerful unions. Deregulation is an arguably exogenous shock that is not likely to be correlated with the wages of nonunion workers other than through a diminution of the threat effect. A focused examination of the time-series behavior of nonunion wage rates in these industries relative to other industries could provide useful evidence on the importance of the threat effect.

3.1. Measurement of the Threat

As I noted earlier, the standard measure of the threat of union organization is industry union density. This measure is included as an additional regressor in a standard earnings function estimated over a sample of nonunion workers. My plan to use geographic and time-series variation as well as inter-industry variation in the threat requires a different approach. I generate a measure of the threat of union organization as the predicted probability of unionization from a set of probit models estimated over repeated cross-sections of workers from the CPS. The basic specification for year t uses a latent variable defined as

$$Y_{ijkt} = X_{it}\alpha_t + \gamma_{jt} + \omega_{kt} + \mu_{it}, \tag{3.1}$$

where i indexes individuals, j indexes industry, and k indexes state. The X vector includes standard demographic measures including education, age, sex, race, and marital status. The model is estimated separately for each year and includes fixed effects by year for both industry and state. The quantity μ is a random component with a standard normal distribution. Individual i is a union member if and only if $Y_{ijkt} > 0$, so that this specification implies a standard probit model for the probability of unionization. The estimates of this model are used to compute a predicted probability of unionization for each sample member in a given year, and this predicted probability is used as the measure of the threat. This is

$$\hat{P}_{ijkt} = \Phi[X_{it}\hat{\alpha}_t + \hat{\gamma}_{jt} + \hat{\omega}_{kt}], \tag{3.2}$$

where $\Phi[\cdot]$ is the standard normal cumulative density function.[6]

North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, and Wyoming.

[5]See Farber (1984) for an analysis of this issue and a short survey of the literature on RTW laws and union density.

[6]For industry-state-year cells in which all workers are nonunion (or, rarely, union members), the predicted probability is set to zero (or one).

3.2. Measurement and Identification of the Threat Effect Using Variation in the Likelihood of Union Membership

With the measure of the threat as defined in equation (3.2), the threat effect can be estimated using an earnings function estimated over nonunion wages defined by

$$(3.3)\quad \ln W_{ijkt} = X_{ijkt}\beta_t + \tau_t \hat{P}_{ijkt} + \psi_j + \mu_k + \varepsilon_{ijkt},$$

where τ_t is the marginal effect of the threat measure on the nonunion wage and β_t is the marginal effect of worker characteristics on the nonunion wage. Both are allowed to vary over time, and year fixed effects are implicitly included. The earnings function model also includes fixed industry and state effects. The threat measure ($\hat{P}$) is a nonlinear function of the worker characteristics that are included in the union membership probit. Thus, identification of the threat effect is based on the exclusion from the earnings function of the interactions of industry and state with year along with the nonlinearity of the probit function.

This approach provides the opportunity to explore alternative sources of variation of the threat effect. First, I can estimate a model that includes time-varying industry effects, yielding an earnings function of the form

$$(3.4)\quad \ln W_{ijkt} = X_{ijkt}\beta_t + \tau_t \hat{P}_{ijkt} + \psi_{jt} + \mu_k + \varepsilon_{ijkt}.$$

This model addresses the concern that unmeasured within-industry changes may be driving both changes in the threat measure and changes in earnings. The identification of the threat effect in this case relies only on within-state variation and the probit nonlinearity. Analogously, I can estimate a model that includes time-varying industry effects, yielding an earnings function of the form

$$(3.5)\quad \ln W_{ijkt} = X_{ijkt}\beta_t + \tau_t \hat{P}_{ijkt} + \psi_j + \mu_{kt} + \varepsilon_{ijkt}.$$

This model addresses the concern that unmeasured within-state changes may be driving both changes in the threat measure and changes in earnings. The identification of the threat effect in this case relies only on within-industry variation and the probit nonlinearity. Most generally, I can estimate a model that includes both time-varying industry effects and time-varying state effects. However, identification of this model relies entirely on nonlinearities in the probit function, and I do not present these estimates.[7]

Estimation of the set of models outlined in equations (3.3)–(3.5) allows me to evaluate different sources of identifying variation for the threat effect.

The theory outlined in the previous section has the clear implication that the effect of the threat measure on the log union wage is unambiguously smaller than the effect on the log nonunion wage. On this basis, I repeat the analysis outlined here for the sample of union workers. This provides potentially important information regarding the threat effect. First, it will provide verification of the implication of the threat model that the nonunion effect is larger than the union effect. Second, unmeasured factors that affect both changes in the likelihood of union membership and wages within industries (for example, increases in international trade) do not clearly have a smaller effect on union wages than on nonunion wages. Thus, a finding that the effects of my measure of the threat are comparable in size in the two sectors would be evidence against a causal interpretation of the relationship between the threat measure and nonunion wages. A finding that the nonunion effect is larger than the union effect would be evidence consistent with a causal interpretation.

3.3. Measurement and Identification of the Threat Effect Using Changes in Right-to-Work Status

Two states adopted RTW laws during the sample period (Idaho in 1985 and Oklahoma in 2001), and the adoption of these laws is arguably an exogenous change in

[7]Preliminary analysis yielded very imprecise estimates of the threat effect.

the likelihood of union organization. In order to focus on the experience in these states, I create two samples, one for each state's experience. I use a six-year period for Idaho, which adopted its RTW law in 1985, from two years prior to passage to three years after passage (1983–88). I use a five-year period for Oklahoma, from three years prior to passage to one year after passage (1998–2002). The size of the sample window differs across the states because of data availability issues.[8] It is unfortunate that Idaho and Oklahoma are relatively small states with relatively small numbers of sample observations.[9] Using each of the samples, I estimate a nonunion earnings function of the form

$$\ln W_{ijkt} = X_{ijkt}\beta + \psi_j + \mu_k + \pi RTW^S_{kt} + \varepsilon_{ijkt}, \tag{3.6}$$

where RTW^S_{kt} is an indicator variable that equals one for workers in the adopting state in the years after adoption of the RTW law and equals zero in the adopting state through the year of adoption and in all years in all other states. The model includes time-invariant industry and state fixed effects so that π represents the change in nonunion wages due to introduction of the RTW law, arguably due to the change in the threat effect. I also estimate this equation for union workers in order to estimate the effect of the introduction of the RTW law on union wages.

3.4. Measurement and Identification of the Threat Effect Using Deregulated Industries

Three highly unionized industries were deregulated early in the sample period (airlines in 1978, trucking in 1979, and telephones in 1984 with the breakup of AT&T). The regulation of these industries, which controlled entry and rates, resulted in the creation of rents that were captured, at least in part, by unions in the form of higher wages (Rose 1987). Deregulation made it difficult for unions to continue to demand high wages and allowed the entry of nonunion competitors. This arguably resulted in an exogenous change in the ability of unions in these industries to organize. In order to focus on the experience in these industries, I create three samples, one for each deregulated industry's experience. Each sample covers an eleven-year period, from five years before deregulation to five years after deregulation. For each sample, I then estimate a nonunion earnings function of the form

$$\ln W_{ijkt} = X_{ijkt}\beta + \psi_j + \mu_k + \eta_t + \pi DR^S_{jt} + \varepsilon_{ijkt}, \tag{3.7}$$

where DR^S_{jt} is an indicator variable that equals one for workers in the deregulated industry in the years after deregulation and equals zero for workers in the deregulated industry through the year of deregulation and in all years in all other industries. The model includes time-invariant industry and state fixed effects so that π represents the change in nonunion wages due to deregulation, arguably due to the change in the threat effect. I also estimate this equation for union workers in order to estimate the effect of deregulation on union wages.

4. Data from the Current Population Survey

My analysis requires individual-level data on wages and union status over as long a period as possible. To this end, I use the data from the CPS from 1973 through 2002 described earlier. However, because I will be focusing on inter-state variation in the threat of union organization, I do not use the data prior to 1977, because earlier CPSs do not identify all states individually.[10] The

[8]There are no data on union status in 1982, and, obviously, data for years after 2002 were not available at the time this analysis was performed.

[9]Given that only 2002 is available as a post-RTW year for the Oklahoma RTW analysis, I use both the early and late rotation groups in 2002 and the late rotation groups for 1998–2001 in this case. This doubles the post-RTW sample compared to the analysis using just the early rotation groups for all years.

[10]Because the trucking and airline industries were deregulated so early in the sample period, I do use the earlier data for that part of the analysis in order to

May CPSs I use for the years 1977–81 contain the relevant information for varying numbers of rotation groups. Wage information is available for all eight rotation groups in 1977 and 1978, for rotation groups 3, 4, 7, and 8 in 1979, and only for rotation groups 4 and 8 in 1980 and 1981. Information on union status is available for all rotation groups from 1977 to 1980 and only for rotation groups 4 and 8 in 1981. The CPS for the years 1983–2002 contains wage and union status information for rotation groups 4 and 8 (the outgoing rotation groups). This is the equivalent of three full CPSs each year (two rotation groups for each of twelve months).

The non-response rate with regard to wage information in the CPS has been growing over time, from about 15% in the 1980s to over 30% by 2002. The Census Bureau uses an elaborate imputation procedure ("hot-deck") to allocate wages to those workers for whom the information is unavailable. However, this procedure does not use information on union status to allocate wages, so comparisons of union and nonunion wages for workers with allocated wages show much smaller wage differentials than those based on data for workers with observed wages. For this reason, I drop observations with allocated wages from my analysis. Unfortunately, the flags indicating allocated wages in the 1994 and 1995 CPSs are inaccurate, and these years are dropped from the analysis as a result.[11] There are more than 2 million observations with valid information on wages and union status, and these serve as the core data for my analysis.

The rotation group design of the CPS implies that half of the observations in each year are potentially in the CPS 12 months later. For example, rotation group 4 in May 1977 is rotation group 8 in May 1978. In order to avoid problems of correlation of individual observations over time and to ensure that I have independent cross-sections, when carrying out the analysis of wages I use only the "early" rotation groups (1–4). The exceptions to this rule are 1977 and 1983, for which I use all rotation groups, because I am not using any data from the immediately prior year.

There are many fewer observations for the wage analysis in each year through 1981 (about 17,000 in 1977 and 1978, for which there are four early rotation groups, and falling to about 5,200 in 1980, for which there is only one early rotation group). There are many more observations from 1983 on, with 12 early rotation groups each year (rotation group 4 in each of twelve months). There are approximately 55,000 observations in each year in the mid- and late 1980s (110,000 in 1983), declining to about 41,000 by 2001 due to increased nonresponse. In order to provide adequate sample sizes in the early years, I pooled the years from 1977 to 1981. The resulting sample size in this year grouping is approximately 70,000. The end result is that the 1977–81 data are treated as a single year (call it 1979), and there are annual data for 1983–2002. The resulting analysis sample for wages contains 1,034,566 observations.

The analogous sample for union status is slightly larger and contains 1,079,134 observations. I rescaled the CPS final weights in order to equalize the effective number of rotation groups in each year or group of years. For example, the 1984–2002 CPSs each have 12 early rotation groups, while the 1983 CPS has 24 usable rotation groups and the 1977–81 CPSs total 16 usable rotation groups for wages (24 for union status). Thus, I multiply the 1983 weights by 1/2 and I multiply the 1977–81 weights by 3/4 for wages (by 1/2 for union status). This has no effect on within-year analyses, and it gives the years more balanced total weight in pooled analyses.

Table 1 contains sample summary statistics for the log real wage, union density,

have data for an adequate number of years prior to deregulation. When I control for state in those analyses, I employ a 31-state categorization for all years that uses the groups defined by the CPS for the smaller states.

[11]There are also problems with the allocation flags in the 1989–93 CPSs, but there is sufficient information to recreate reasonably accurate allocation indicators.

and other worker characteristics over the 1973–2002 sample period. Union density is defined as the fraction of the sample who are either union members or covered by a collective bargaining agreement. The average private sector union density is 14.1%, and it decreases with educational attainment, with 16.7% of high school dropouts and only 7.0% of college graduates being union members. Women are less likely than men to be union members, with 48.2% of nonunion workers and 29.2% of union workers being female.

The unadjusted union wage gap shown in Table 1 is 29.3 log points (34.0%). This varies systematically by educational category (not shown in the table), with unadjusted wage gaps of 49.6 log points for high school dropouts, 42.3 log points for those with 12 years of education, 37.0 log points for those with some college, and only 5.0 log points for college graduates.[12]

Table 1. Sample Summary Statistics—Private Sector.

Variable	*All*	*Nonunion*	*Union*
log(wage)	1.945	1.904	2.197
	(0.565)	(0.572)	(0.445)
Union Density	0.141	—	—
Age	36.205	35.692	39.320
	(11.781)	(11.758)	(11.440)
Female	0.455	0.482	0.292
Married	0.588	0.575	0.668
Married Female	0.249	0.263	0.162
Nonwhite	0.126	0.120	0.157
ED < 12 Years	0.151	0.147	0.179
ED = 12 Years	0.377	0.361	0.476
ED 13–15 Years	0.273	0.278	0.246
ED ≥ 16 Years	0.198	0.215	0.099

Notes: Weighted by CPS sampling weights rescaled by year to imply equal weighted totals across years. The numbers in parentheses are standard deviations. The overall sample size is 1,079,134. Sample sizes for the wage and union status variables are slightly smaller due to missing values.

5. Creating the Threat Measure: The Probability of Unionization

As I described in section 3.1, I estimate separate probit models for each year of the probability of union coverage or membership (unionization). These models include demographic characteristics as well as 3-digit industry and state fixed effects. I then use these estimates to compute a predicted probability of unionization (equation 3.2). While I do not present the results for every year, Table 2 contains estimates of this probit model for selected years (1977–81, 1987, 1992, 1997, and 2002). There are strong persistent empirical regularities across years. Not surprisingly, the marginal effects are generally larger in the earlier years, in which the probability of unionization is higher. In the later years, which have much lower union density, even in groups with relatively high union density, the absolute difference in the probability of unionization simply cannot be very large.

Workers with more than 12 years of education are substantially less likely to be unionized than are workers with less education. The high-school–college gap in union density is substantial in all periods, but falls over time, from 19.3 percentage points in 1977–81 to 4.2 percentage points in 2002. This reflects the general decline in union density over this period, which was particularly large for less-educated workers. The raw union density for workers with a high school education fell in half, from 0.27 to 0.13, between 1977–81 and 2002, while the union density for college graduates fell only 2 percentage points over the same period, from 0.09 to 0.07.

Older workers were more likely than other workers to be unionized, although

[12]The bilateral selection model of union status determination suggested by Abowd and Farber (1982) and Farber (1983) implies that the large wage gaps for less educated workers are overestimates of the wage effect of unions for these workers, while the small wage gaps for more educated workers are underestimates of the wage effect of unions for these workers. Card (1996, 2001) adjusted these wage gaps for unobserved heterogeneity and found that the adjusted gaps still fall by skill level, but not as sharply as the unadjusted estimates.

Table 2. Probability of Unionization—Private Sector, Selected Years

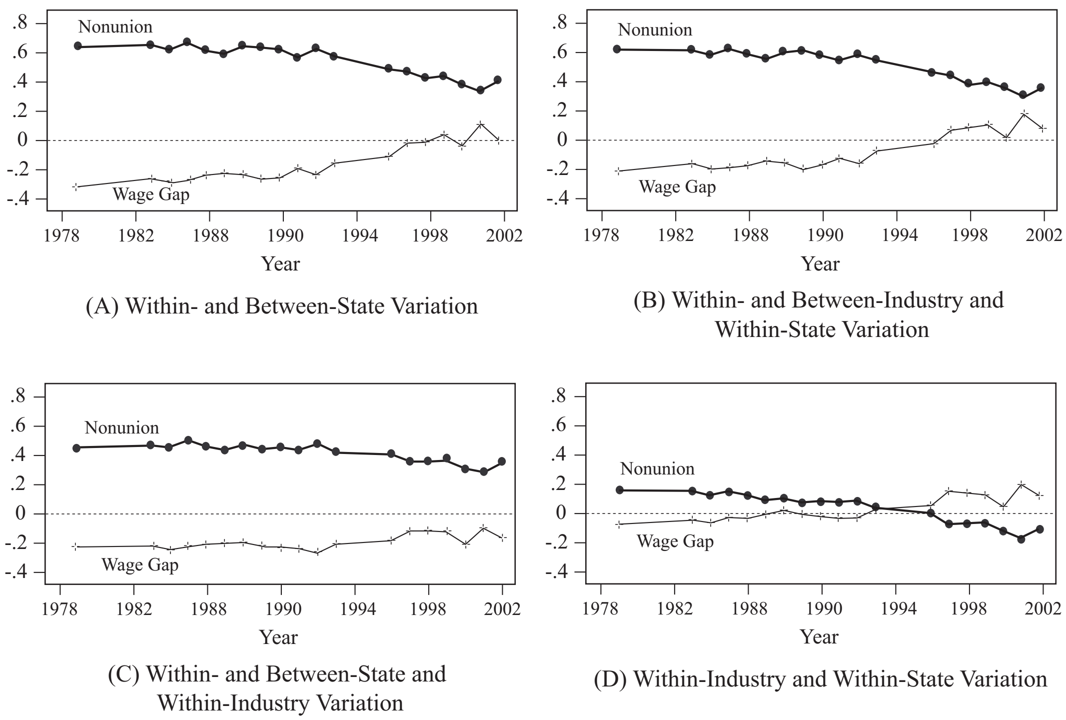
	Normalized Probit Estimates				
Variable	*1977–81*	*1987*	*1992*	*1997*	*2002*
ED < 12 Years	0.026	–0.008	–0.014	–0.013	–0.018
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
ED 13–15 Years	–0.057	–0.034	–0.024	–0.012	–0.012
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
ED ≥ 16 Years	–0.193	–0.105	–0.085	–0.060	–0.042
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Age	0.011	0.011	0.008	0.005	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	–0.000	–0.000	–0.000	–0.000	–0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	–0.081	–0.036	–0.033	–0.023	–0.017
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Married	0.024	0.013	0.015	0.008	0.006
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Married*Female	–0.036	–0.033	–0.017	–0.014	–0.018
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Nonwhite	0.086	0.053	0.058	0.033	0.027
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
Hispanic	0.027	0.017	0.013	0.012	–0.003
	(0.005)	(0.005)	(0.004)	(0.004)	(0.003)
3-Digit Industry FEs	yes	yes	yes	yes	yes
State FEs	yes	yes	yes	yes	yes
U	0.234	0.156	0.132	0.112	0.100
N	118,044	66,561	64,750	55,249	65,300
Log-L	–47,192.2	–21,758.9	–19,173.9	–15,358.4	–16,849.0

Notes: All estimates are weighted by CPS final sampling weights. The coefficients are normalized to represent the derivative of the probability of unionization with respect to a change in the explanatory variable. This is computed as $\hat{\beta}\phi(\bar{X}\hat{\beta})$, where $\hat{\beta}$ is the vector of estimated parameters of the probit model, $\bar{X}$ is the vector of means of the explanatory variables, and ϕ is the standard normal probability density function. The numbers in parentheses are standard errors. The base group consists of white non-Hispanic single men with 12 years of education.

the gap declined over time. Women were substantially less likely than men to be unionized in the early years, with the male-female gap being 8.1 percentage points for single workers and 11.9 percentage points for married workers in 1977–81. These differences fell to 1.7 percentage points for single workers and 3.5 percentage points for married workers by 2002. Nonwhites were statistically significantly more likely than whites to be unionized, with a racial gap of 8.6 percentage points in 1977–81, declining to 2.7 percentage points in 2002. The gap by ethnicity was substantial in the early years, with Hispanics 2.7% more likely than non-Hispanics to be unionized in 1977–81. However, by 2002 there was not a statistically significant relationship between Hispanic ethnicity and unionization rates.

While I do not present the results, there is substantial persistent variation across industries in union density by industry and state. For example, average union density in 1977–81 in states without RTW laws was 27.0%, but it was only 14.2% in states with RTW laws. This gap remained substantial in 2002, with union density of 12.3% in states without RTW laws and only 5.0% in states with RTW laws. Similarly, average

Figure 1. Effect of Predicted Probability of Unionization on the Nonunion Wage and the Wage Gap, by Year: Industry and State Variation (equation 3.3).



(A) Within- and Between-State Variation

(B) Within- and Between-Industry and Within-State Variation

(C) Within- and Between-State and Within-Industry Variation

(D) Within-Industry and Within-State Variation

union density in 1977–81 in manufacturing was 36.7% but only 10.2% in service industries. This gap persisted but was smaller in 2002, with union density of 15.3% in manufacturing and 5.2% in service industries.

6. Estimating the Threat Effect: Results

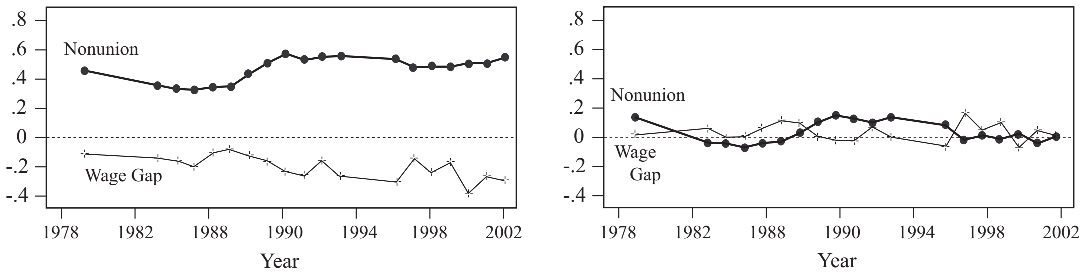
6.1. Variation in the Likelihood of Union Membership

Estimation of the threat effect using variation in the likelihood of union membership is carried out using the method described in section 3.2, equations (3.3)–(3.5). All specifications include a complete set of interactions of year fixed effects with the predicted probability of unionization. The estimated coefficients of these interactions (τ_t) are the focus of my analysis.

Figure 1 contains plots of the estimated coefficient of the predicted probability of unionization ($\hat{P}$) by year from the versions of the nonunion earnings function defined in equation (3.3). The figure also contains plots of the estimated marginal effect of $\hat{P}$ on the union wage gap. This is computed as the difference between the estimated coefficients of $\hat{P}$ by year from the union and nonunion earnings functions.

Panel A of the figure contains the estimated marginal effects of $\hat{P}$ on the nonunion wage and the union wage gap from regressions like equation (3.3) that contain neither industry nor year fixed effects. Thus, identification of the threat effect relies on both within- and between-indus-

Figure 2. Effect of Predicted Probability of Unionization on the Nonunion Wage and the Wage Gap, by Year: Variation by State (equation 3.4).



(A) Within- and Between-State Variation

(B) Within-State Variation

try and within- and between-state variation. These plots show a substantial positive relationship between the likelihood of unionization and the nonunion wage rate that declines over time. Consistent with the theoretical model, in the early years the union wage gap is negatively related to $\hat{P}$, but this relationship disappears in the later years. These estimates of the threat effect rely on both cross-sectional and time-series variation in $\hat{P}$, and, as such, are likely biased by omitted factors correlated with both union density and the wage.

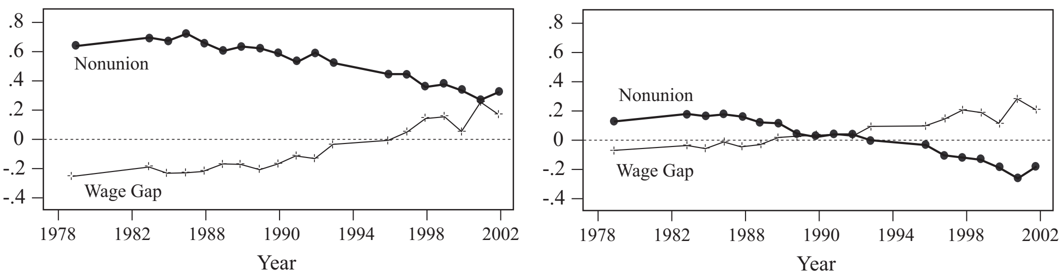
Panel B removes cross-sectional variation by state by including state fixed effects in the second step regression. The results are not very sensitive to this addition, suggesting that inter-state variation in union density is not strongly correlated with unobserved factors affecting the wage.

Panel C removes cross-sectional variation by industry by including 3-digit industry fixed effects in the second step regression. The threat effect for nonunion workers is reduced somewhat by this addition. More interestingly, the effect of $\hat{P}$ on the union wage gap remains negative throughout, suggesting that union wages are correlated with unmeasured factors that affect industry union density.

Finally, panel D removes cross-sectional variation by both industry and state by including fixed effects in both dimensions in equation (3.3). In this case, identification of the threat effect relies on variation over time within industry and within state. The effect estimated this way is much attenuated both for union workers and for the union wage gap, and even becomes negative for nonunion workers late in the sample period. By eliminating cross-sectional variation by industry and state, the panel D estimates are most plausibly interpreted as a pure threat effect. On balance, these estimates provide some evidence of a statistically significant, though diminishing, threat effect on nonunion wages through about 1994.

In order to isolate the threat effect as identified by specific sources of variation, I estimated versions of the model that include, in turn, year-specific industry effects and year-specific state effects. I start by presenting results in Figure 2 for a model that includes year-specific industry fixed effects in the first step (equation 3.4), so that identification relies on variation by state. Plotted in panel A of the figure are the estimated marginal effects of $\hat{P}$ on the nonunion wage and the union wage gap from regressions like equation (3.4) that contains year-specific industry fixed effects but no state fixed effects. These plots show a substantial positive relationship between the likelihood of unionization and the nonunion wage rate, with the effect growing

Figure 3. Effect of Predicted Probability of Unionization on the Nonunion Wage and the Wage Gap, by Year: Variation by Industry (equation 3.5).



(A) Within- and Between-Industry Variation

(B) Within-Industry Variation

slowly over time. There is also a growing negative relationship between the likelihood of unionization and the union wage gap. These estimates of the threat effect rely on both cross-sectional and time-series variation in $\hat{P}$ by state that is uncorrelated with industry in each year. However, they may still be biased by omitted state-specific factors correlated with union density and the wage. Panel B removes cross-sectional variation by state by additionally including time-invariant state fixed effects. Identification in this case relies on within-state variation in union density, and there is virtually no discernible threat effect.

I also estimated a version of the model including year-specific state effects (equation 3.5), so that identification relies on variation by industry. Plotted in panel A of Figure 3 are the estimated marginal effects of $\hat{P}$ on the nonunion wage and the union wage gap from regressions like equation (3.5) that contain year-specific state effects but no industry fixed effects. These plots show a substantial relationship between $\hat{P}$ and the nonunion wage that diminishes over time and a negative relationship between $\hat{P}$ and the union wage gap that moves toward zero over time and becomes positive after 1998. These estimates of the threat effect rely on both cross-sectional and time-series variation in $\hat{P}$ by industry that are uncorrelated with state in each year. However, they may still be biased by omitted industry-specific factors correlated with union density and the wage. Panel B removes cross-sectional variation by industry by additionally including industry fixed effects in the second step regression. Identification in this case relies on within-industry variation in union density. The estimated threat effect is slightly positive in the early years, but it falls over time and becomes negative later in the sample period.

I have presented a range of estimates of the union threat effect on the wages of nonunion workers and on the union wage gap. These estimates differ in their assumptions regarding the source of identifying variation. There is clearly persistent cross-sectional variation in both wages and union density by state and industry that could well be due to common unmeasured factors. As a result, most credible are the estimates, summarized in panel B of Figures 2 and 3, that rely on within-state and within-industry variation, respectively. The estimates relying on within-industry variation (Figure 3, panel B) are subject to some obvious omitted-variable biases. For example, it is likely that increased international trade in specific industries over time both reduced labor demand (and, consequently, wages) and made unionization less attractive. To the extent that this is the case, the within-industry variation is not an appropriate source of variation for identification of the threat effect. The estimates relying on within-state variation (Figure 2, panel B) are not so obviously contaminated by important omitted variables. As such, these are the most credible estimates of the threat effect. And the clear conclusion is that there has not been a substantial threat effect of unions on the earnings of nonunion workers. The correlation of wages of nonunion workers with union density is almost entirely due to unmeasured factors correlated with both outcomes.

6.2. Variation Stemming from Adoption of Right-to-Work Laws

A credible source of variation in the threat of union organization is the adoption of RTW laws. Union-shop and agency-shop agreements reached in collective bargaining between unions and employers make it a requirement of continued employment that workers either become dues-paying members of the union or pay a continuing fee in lieu of membership dues. These contract provisions are important facilitators of a stable union presence. RTW laws make it illegal for labor unions and employers to negotiate such agreements while requiring unions to represent, negotiate on behalf of, and provide services to even those who choose not to join the union or provide financial support. Effectively, unions are prevented from taxing workers to pay for benefits, yet they are required to provide workplace public goods. Not surprisingly, there is a substantially larger free-rider problem in states with RTW laws. For this reason, it is likely that the threat of unionization is lower after the passage of a RTW law.

Clearly, the presence or absence of RTW laws is not exogenously determined. The two states that adopted RTW laws during the sample period had relatively low union density prior to passage. Idaho, which passed its RTW law in 1985, had union density of 11.6%, compared with a union density of 16.8% in the rest of the country. Oklahoma, which passed its RTW law in 2001, had union density of 6.7%, compared with a union density of 10.4% in the rest of the country. It is likely that the weakness of unions in these states, as a result of such factors as lower underlying worker demand for union representation, an industrial structure that was focused on less unionized industries, or particularly effective employer resistance to unions, contributed to the passage of RTW laws in these states. For this reason, it is probable that the threat of unionization in these states was relatively low even before the passage of the RTW law. Thus, the introduction of an RTW law likely had a marginal effect, at best, on the likelihood of union organization. Whether this marginal effect provides sufficient variation in the threat to affect wages measurably is an empirical question.

Table 3 contains estimates of the key parameter, π, from estimation of the earnings function defined in equation (3.6). These earnings functions contain a set of demographic characteristics along with fixed effects for year, state, and 3-digit industry. The Post-RTW variable is an indicator variable that equals one in the state adopting the RTW law in the years after adoption. It is zero both in the adopting state in all years through the year of adoption and in all other states in all years. As such, its coefficient represents the change in the wage differential between the state adopting the RTW law and other states subsequent to passage of the RTW law. The earnings function is estimated separately for nonunion and union workers.

The estimates for Idaho, in columns (1) and (2), imply that the wage of nonunion workers fell by 4.2 percentage points relative to the wage of nonunion workers elsewhere after Idaho passed its RTW law in 1985. There was not a statistically significant change in the relative wage of union workers. This result is consistent with the hypothesis that the introduction of the RTW law in Idaho reduced the threat of union organization, resulting in lower earnings for nonunion workers and an increase in the union wage gap.

The adoption by Oklahoma of an RTW law in 2001 provides the second test of the threat effect using the introduction of RTW

Table 3. Effect of Adoption of RTW Laws on Earnings.

Variable (state interaction)	*(1) Idaho Nonunion*	*(2) Idaho Union*	*(3) Oklahoma Nonunion*	*(4) Oklahoma Union*
Post-RTW	–0.042	0.008	–0.017	0.060
	(0.021)	(0.046)	(0.017)	(0.062)
R-Squared	0.524	0.467	0.468	0.391
Year of RTW	1985	1985	2001	2001
Years in Sample	1983–88	1983–88	1998–02	1998–02
N	328,411	66,736	232,024	26,960

Notes: The variable Post-RTW is an indicator that equals one in the specified state in years after the passage of the RTW law in that state. All estimates are weighted by CPS final sampling weights adjusted for number of rotation groups included. All specifications include variables measuring education (4 categories), age, age-squared, race, Hispanic ethnicity, sex, marital status, the interaction of sex and marital status, state fixed effects (51 categories), year fixed effects, and 3-digit industry fixed effects. The numbers in parentheses are standard errors.

laws. While there is only one year with data subsequent to the Oklahoma adoption, columns (3) and (4) of Table 3 contain the relevant estimates. These estimates do not show any statistically significant change in the earnings of workers in Oklahoma, either union or nonunion, relative to workers in other states after Oklahoma's passage of an RTW law. This null finding could be due to the fact that I only have data for one year subsequent to Oklahoma's adoption of the RTW law.

6.3. Variation Stemming from Deregulation of Key Industries

A potentially more promising source of variation in the threat of unionization is the deregulation of industries that had been regulated in ways that greatly strengthened labor unions. Three large industries meet this criterion. The airline industry was deregulated in 1978, ending an elaborate system of rate and route regulation that helped the various craft unions in the industry maintain control over the supply of labor by preventing entry by new (that is, nonunion) firms. The trucking industry underwent similar deregulation in 1979, weakening the control of the International Brotherhood of Teamsters over interstate trucking. The breakup of AT&T, resulting in independent local operating companies and competition in the provision of long-distance telephone service, facilitated the entry of nonunion competitors and weakened the control of the Communications Workers of America.

In contrast to the marginal differences in the threat of union organization represented by within-industry and within-state changes in the probability of unionization or by the adoption of a RTW law in states that had relatively low union density prior to adoption, these regulatory changes were dramatic in weakening the unions. Deregulation represented a substantial change in the ability of unions in these industries to negotiate higher wages and benefits without fear of entry by nonunion firms and the concomitant decline in the demand for union labor. It is not an overstatement to say that unions thrived in these industries because of regulation. Thus, there may be a more substantial reduction in the threat effect due to deregulation than I found using the other sources of variation.

Table 4 contains estimates of the key parameter, π, from estimation of the earnings function defined in equation (3.7). These earnings functions contain a set of demographic characteristics along with fixed effects for year, state, and 3-digit industry. The Post-DEREG variable is an indicator variable that equals one in the deregulated industry in the years after de-

Table 4. Effect of Deregulation on Earnings.

Variable (industry interaction)	*(1) Airlines Nonunion*	*(2) Airlines Union*	*(3) Trucking Nonunion*	*(4) Trucking Union*	*(5) Telephone Nonunion*	*(6) Telephone Union*
Post-DEREG	0.008	0.067	–0.043	–0.054	–0.047	0.024
	(0.027)	(0.024)	(0.017)	(0.015)	(0.015)	(0.010)
R-Squared	0.500	0.454	0.510	0.458	0.518	0.461
Year of Deregulation	1978	1978	1979	1979	1984	1984
Years in Sample	1973–83	1973–83	1974–84	1974–84	1979–89	1979–89
N	184,824	51,373	207,885	53,396	390,610	78,850

Notes: The variable Post-DEREG is an indicator that equals one in the specified industry in years after deregulation of that industry. All estimates are weighted by CPS final sampling weights adjusted for number of rotation groups included. All specifications include variables measuring education (4 categories), age, age-squared, race, Hispanic ethnicity, sex, marital status, the interaction of sex and marital status, state fixed effects (51 categories), year fixed effects, and 3-digit industry fixed effects. The numbers in parentheses are standard errors. There are no data for 1982.

regulation. It is zero in the deregulated industry through the year of deregulation, and it is zero in all other industries in all years. As such, its coefficient represents the change in the wage differential between the deregulated industry and other industries subsequent to deregulation. The earnings function is estimated separately for nonunion and union workers.

The estimates for the airline industry, in columns (1) and (2), imply that the wage of nonunion workers underwent no statistically significant change relative to the wage of nonunion workers in other industries following deregulation in 1978. The relative wage of union workers in airlines increased by 6.7 percentage points over the same period. Nonunion wages in airlines relative to other industries did not keep pace with union wages in airlines relative to other industries. One interpretation of this finding is that as the threat diminished (θ fell), the union wage gap in airlines increased, as predicted by the theory. However, without a clear understanding of why the union relative wage in airlines increased, this interpretation must be made cautiously.

The estimates for the trucking industry, in columns (3) and (4), imply that the average wage of nonunion workers in trucking dropped by a statistically significant 4.3 percentage points relative to the wage of nonunion workers in other industries subsequent to deregulation in 1979. The average wage of union workers in trucking dropped by a similar amount, 5.4 percentage points. Thus, relative union and nonunion wages moved together after deregulation. The decline in the nonunion wage is consistent with a diminution of the threat effect, but the fact that the union wage fell by a similar amount suggests that other factors are dominating movement in both the union and nonunion wage.

The estimates for the telephone industry, in columns (5) and (6), imply that the average wage of nonunion workers in the telephone industry dropped by a statistically significant 4.7 percentage points relative to the wage of nonunion workers in other industries following the breakup of AT&T in 1984. The average relative wage of union workers in the telephone industry actually increased by 2.4 percentage points. Thus, the relative union wage gap increased by 7.1 percentage points after deregulation. This pattern is consistent with a diminution of the threat effect.

Overall, there is some evidence to support the idea that deregulation reduced the threat of union organization in each of the three industries studied.

7. Concluding Remarks

My empirical analysis finds mixed evidence regarding the importance of the threat of union organization as a factor in determining the wages of nonunion workers. The preferred estimates from the analysis using the predicted probability of unionization as the threat measure, which rely, in turn, on within-industry variation and within-state variation in the threat, imply very little relationship between either nonunion wages or the union wage gap and the threat. This suggests that the threat effect found in the existing literature, which uses inter-industry variation in union density to measure the threat, is likely biased by omitted industry-specific factors correlated with both wages and union density.

Some support for threat effects is found in the estimates that rely on the introduction of RTW laws. These estimates show a statistically significant relationship between nonunion wages and the introduction of RTW laws in Idaho. The wages of nonunion workers in Idaho relative to workers in other states fell significantly after Idaho enacted its RTW law in 1985. I find a similar, although smaller and not statistically significant, relationship in Oklahoma following passage of that state's RTW law in 2001.

More support for threat effects is found in the experience of deregulated industries. After deregulation of the trucking and telephone industries, wages of nonunion workers in those industries fell relative to the wages of nonunion workers in other industries. And while wages of nonunion workers in airlines did not fall relative to wages of other nonunion workers after deregulation, the airline nonunion wage did not keep pace with union wages in that industry, resulting in an increase in the union wage gap. Given that regulation was the key determinant of union success in these industries, perhaps it is not surprising that deregulation had an appreciable effect on wages.

It may be that where the product market is protected from competition by regulation or other means, nonunion employers find it in their interest to discourage unions by paying higher wages. While I am reluctant to generalize from the Idaho experience, it may also be that the threat of unionization is reduced marginally by the enactment of RTW laws. But there is little evidence that marginal differences in the likelihood of union membership affect the wages paid by nonunion employers or that the decline in unions over the past 25 years had a generally adverse effect on the wages paid to nonunion workers.

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