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Joseph S. Tracy

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By JOSEPH S. TRACY*

This paper outlines the construction of a new panel data set of U.S. contract negotiations and strike activity. This is the first to contain data on both strikes and contract expirations. Key findings are that 15 percent of scheduled negotiations end as strikes, but strike probabilities are higher in June and lower in December; the variability, and not the level, of firm profitability affects strike activity; personal characteristics of the union workforce affect strike activity; and strikes are more likely when the local labor market is tight, but less likely when the industry labor market is tight.

To gain an understanding of why strikes occur and what factors lead to a settlement, it is important to examine data pertaining to the level at which negotiations take place, that is, the individual bargaining unit. Despite this, there has not been to date a study of the incidence and duration of strikes in the United States on a comprehensive micro data set of contract negotiations. This reflects the difficult problems involved in collecting this type of data. The purpose of this paper is to take a step in this direction by illustrating how these problems can be overcome and to present some results for the five-year period from 1973 to 1977.

One of the principal findings is the way in which a firm's profitability influences the bargaining process. The firm's level of profitability has no impact on the likelihood of a strike. However, profit volatility increases both the incidence and duration of strikes. Important scale effects were found in the data. Large firms have both lower strike probabilities and shorter strike durations. Personal characteristics of the union workforce in the industry were also important determinants of strike activity. Strike incidence is higher the more educated workers are, the younger they are, and the higher is the percentage of white workers. Finally, labor market conditions significantly affect the course of the negotiations. Strikes are less likely when industry labor markets are tight, and more likely when local labor markets are tight.

The outline of the paper is as follows. A complete description of the construction of the negotiation data is presented in Section I. Particular attention is given to discussing solutions to the problems encountered in using the Bureau of Labor Statistics (BLS) strike data. The section concludes with a discussion of the motivation for and construction of the variables to be included in the analysis. The econometric methods used to examine the data are outlined in Section II, followed by a presentation of the empirical results.

I. Construction of the Data and Variables

The bulk of the empirical work on U.S. strike activity has used aggregate time-series data. These studies typically estimate some variant of Orley Ashenfelter and George Johnson's model (1969). One of the principal difficulties with using aggregate data is controlling for the underlying number of negotiations taking place. Micro data on individual contract negotiations solve this problem in a natural way. However, a characteristic of many of the existing micro studies is that their samples include only a small number of

*Department of Economics, Yale University, New Haven, CT 06520. I thank Sherwin Rosen, Edward Lazear, and Robert Topel for their extensive comments, and also thank Gary Becker, George Neumann, John Abowd, and David Card for helpful discussions. Any errors are my responsibility.

firms (see Henry Farber, 1978; Drew Fudenberg, David Levine, and Paul Rudd, 1983; and Martin Mauro, 1982). For example, Farber's data followed ten firms while Mauro's followed fourteen. A strong point of these data sets is the long time period covered by the data. What is needed, though, is a panel data set which follows a broad spectrum of firms and unions.

A second difficulty encountered when trying to analyze U.S. strike activity is the accuracy of the strike information itself. In most studies, this information is gathered from public sources such as newspaper reports. Two potential problems exist. First, it is possible that a strike could go unreported. This may lead the researcher to miscode a negotiation as a nonstrike. Second, information on the actual number of workers involved, the duration of the strike, etc., may be subject to reporting error. This again introduces measurement error into the analysis. A concerted attempt has been made in this study to minimize both of these problems. The techniques used will be outlined below.

The focus of this study will be on strikes that occur during renegotiations of contract terms. This excludes, for example, organizational strikes and sympathy strikes from the analysis. The omitted categories comprise about 40 percent of all major strikes.[1] This bargaining process can be initiated in one of two ways. The first is a scheduled negotiation. This can occur either at the expiration of the current contract, or at an agreed-upon reopening of the contract. The second manner is an unscheduled negotiation due to an unanticipated reopening of the contract. This latter type of negotiation occurs infrequently and usually in response to a dramatic development that requires immediate attention.

This study deals exclusively with scheduled negotiations. This decision was made because of the difficulty in obtaining reliable information on unscheduled reopenings. Information on scheduled negotiations is available from the BLS. This information is based on a file of union contracts which the BLS maintained. For recent years, they had fairly extensive coverage of major contract, that is, contracts covering 1,000 or more workers. The expiration and reopening dates for all of these contracts were published annually in the bulletin *Wage Calendar*. Information on smaller bargaining units is available only in unpublished form and is not as comprehensive in coverage. Consequently, only major contracts are included in the sample. While major contracts account for less than 50 percent of all contracts, they cover roughly 90 percent of all union workers. The sample was further restricted to manufacturing contracts.

For each contract, the BLS lists the year and month it expires, the name of the firm(s) and the union(s) comprising the bargaining pair, the number of workers covered by the contract, the 2-digit SIC classification for the major product line affected, and the state or region involved. In addition, a contract identification number is assigned which allows you to follow that bargaining pair through each successive contract negotiation. The day of the expiration, the contract length, and the 4-digit SIC classification were found in unpublished listings provided by the BLS. The contract identification numbers made it possible to merge this additional information in with the published expiration data.

The BLS also collected extensive data on U.S. strike activity. This information was summarized annually in their bulletin *Work Stoppages*. The collection process began with an unpublished weekly summary of strikes in progress, *Industrial Relations Facts* (*IRF*), which the BLS gathered from public sources. Each company listed as being involved in a strike was contacted and a request was made for verification of the information given in the public source. To increase their response rate and to insure the accuracy of the information provided, the BLS pledged confidentiality over the use of this strike data. As a result, the BLS has only released this data with the names of the firm and the union removed from each record.

As a consequence of this confidentiality issue, the only published data from the BLS strike file that identifies the names of the

[1] These estimates are based on work stoppage data provided by George Neumann.

parties to the negotiations are for contracts with 10,000 or more workers. This is the fundamental roadblock confronting anyone attempting to construct a data set at the bargaining unit level of observation. Restricting the analysis to contracts involving 10,000 or more workers would significantly reduce the sample size and coverage. Including all contracts of 1,000 or more workers would seem to preclude using the most accurate information available. Relying on the *IRF* data would probably be adequate for studies of strike incidence among major contract negotiations, since it is unlikely that many major strikes would go unreported. However, not having access to the BLS work stoppage data could be a more serious problem for studies of strike durations.

The best solution to this problem seems to be to try and circumvent the difficulties raised by the confidentiality issue. If the names of the firm(s) and the union(s) could be recovered and reinserted onto the BLS strike data, then the sample could include all major contracts and still use the cleaned-up BLS strike information. Two sources of information are available for this identification effort. The primary source is the strike listings of the *IRF*. A secondary source was the BLS publication *Current Wage Developments* (*CWD*). The purpose of *CWD* is to report on the major changes in the contract provisions following a negotiation. However, *CWD* indicates for some contracts that the settlement was preceded by a strike of some specified duration. Presumably, this information is based again on secondary sources and not the actual BLS strike data.

I received from the BLS a set of the *IRF* covering the period from 1973 to current. For each strike listed, I followed the strike through each weekly issue from its start up to the settlement. This provided a single observation for each strike which contained all of the information that is also reported on the BLS strike tape, with the exception of a contract status and major issue variable. The strike listing generated from the issues of the *IRF* was merged with a similar listing complied from issues of the *CWD*. I then matched the strikes from the BLS work stoppage tape to the strikes from this combined public listing using the overlap in information. Care had to be taken since the information in the public listing was subject to reporting error. Using this procedure, I was able to recover the names of the firms involved in over two-thirds of the strikes during the period from 1973 to 1977.

Having reinserted as many names as possible, I then selected a subsample of strikes relevant to my analysis. These selections were necessary to make the strike sample conform with the negotiation sample. Strikes were kept if they took place at an expiration or reopening of a contract, and if they involved 1,000 or more workers. Separating out the strikes by type is possible since the work stoppage data includes a contract status variable that indicates whether contract terms were under negotiation at the time of the strike. A strike satisfying the above criteria was kept even if no match had been found in the public strike listing. The reason for this is that, even without the names of the firm and the union, it is sometimes possible to match a strike with its corresponding expiration using common information on detailed SIC classification, region, number of workers, and dates.

For the period from 1973 to 1977, the total sample contains 2,100 contract negotiations for which detailed strike information is available. The sample consists of 1,130 distinct bargaining pairs, 392 firms, and 75 unions. A total of 120 3-digit industry classifications and 45 states are represented in the data.[2] Tables 1 and 2 show the distribution of these contracts and strikes by year and by month. They illustrate the uneven distribution of negotiations across both years and months within a year. This underscores the point made earlier concerning the importance of being able to control for the amount of negotiating activity.

[2]Several contract negotiations had to be dropped from the estimation due to missing information on variables used in the analysis. The sample used in the estimation contains 1,319 contract expirations and reopenings involving 358 firms and 61 unions. Currently, the sample is being expanded to include all major manufacturing and nonmanufacturing negotiations from 1970 to the present.

TABLE 1—DISTRIBUTION OF CONTRACT EXPIRATIONS AND STRIKES BY YEAR

Year	Number of Contract Expirations	Number of Strikes	Strike Frequency
1973	214	40	18.69
1974	327	65	19.88
1975	187	19	10.16
1976	248	35	14.11
1977	343	39	11.37
Total	1,319	198	15.01

TABLE 2—DISTRIBUTION OF CONTRACT EXPIRATIONS AND STRIKES BY MONTH

Month	Number of Contract Expirations	Number of Strikes	Strike Frequency
January	99	14	14.14
February	65	11	16.92
March	102	10	9.80
April	122	22	18.03
May	148	20	13.51
June	150	34	22.67
July	107	12	11.21
August	177	25	14.12
September	129	20	15.50
October	117	18	15.38
November	49	10	20.41
December	54	2	3.70
Total	1,319	198	15.01

In the remainder of this section I explain why I selected the variables included in the analysis. I will also provide details concerning the construction of these variables. The first set of variables takes advantage of the fact that we know the firm involved in the bargaining. Two sources of firm-specific data were used. The CRSP data provides security price information and the COMPUSTAT data provides accounting information. Both data bases use the same firm identification number called a CUSIP number.

The only difficulty encountered in adding these CUSIP numbers to the data set was handling mergers and takeovers. When these occur, firm-level information may no longer be available for one or possibly both firms involved. However, the BLS will typically continue to list the old firm names in the expiration data. In these cases, the appropriate new firm name was obtained from a directory of firms and the CUSIP number for that firm was added to the data.

The first aspect the firm attempts to control for is its recent profit performance. Several micro studies have included measures of the firm's profitability. However, the level of profitability may not be the only relevant feature. Harold Grubert (1968) argued that instability in firm profits might lead to increased strike activity for two reasons. The first is that this volatility might make the union leadership less certain about the firm's willingness to make concessions during the bargaining. Secondly, Grubert argued that to the extent that management has better information about the firm's future profitability, "... it may be necessary for the union to threaten a strike or to begin one in order to force the company to reveal the level of profits it really expects" (p. 23). Grubert did not explicitly model how a strike might allow the union to infer the firm's private information. However, recent game-theoretic models of bargaining have been based exactly on such an idea (see Peter Cramton, 1982; Fudenberg and Jean Triole, 1981; Beth Hayes, 1984; Joel Sobel and Ichiro Takahashi, 1983; and my 1984 dissertation).

Grubert measured profit instability as the sum of the absolute deviations of annual profits from trend over the past five years scaled by the firm's employment. He found that this measure of volatility had a positive effect both on the profitability and the conditional duration of a strike with the later effect significant at the 0.025 level (p. 40). For the level of the firm's performance, I chose to use the rate of return on the firm's stock for the year preceding the contract expiration. The volatility measure used is the standard deviation of the firm's daily stock return. This is calculated on a year of daily trading data ending six months prior to the contract expiration.[3]

[3] The reason for not including trading data right up to the contract expiration is that speculation over the upcoming contract negotiations will begin to occur as this expiration date approaches. This speculation will induce variability into the stock returns that need not reflect any uncertainty about the firm's future demand conditions. Instead, this variability may simply reflect uncertainty about the division of future profits between the union and the stockholders (see John Abowd, 1985).

The next two firm-specific variables attempt to control for the firm's ability to self-insure against the event of a strike. C. Lawrence Christenson (1953), in discussing the effects of the coal strikes, mentions two basic ways for firms to "offset" the interruption in the flow of union labor services. The first method is to use intertemporal substitution in production; that is, the firm builds "buffer" inventories prior to the start of the negotiations. The second method is for the firm to attempt to continue production at a reduced rate during the course of the strike.

Estimating a firm's buffer stock of inventory prior to the expiration of its contract is a difficult task. The COMPUSTAT data provides inventory data at the firm level. While COMPUSTAT asks for inventory by stage-of-process, most firms only report total inventory levels. Consequently, in order to prevent the sample size from being significantly reduced, raw materials, work-in-progress, and final goods must be assumed to provide identical contributions toward the firm's insurance efforts.

The other limitations of the inventory data are that only year-end figures are given and all product lines are aggregated together. Ideally, we would like to observe the firm's inventory position close to the expiration date and we would like to focus just on the product lines that may be potentially affected by a strike. The proxy used for the firm's buffer stock is the percentage change in its inventory-to-sales ratio for the year preceding the contract expiration. The inventory figures were scaled by the firm's sales in order to account for normal inventory growth due to sales growth.

The second method available to the firm to offset the costs of a strike is to attempt to maintain production during the strike. The ability of the firm to continue production may in part be determined by how capital intensive the production technology is in that industry. Firms in highly capital-intensive industries may be able to train their managers to continue operations at a reduced rate. An example of this was the nationwide telephone strike. Due to the high degree of automation in the telephone industry, many types of services continued throughout the strike. The firm's capital-labor ratio was calculated using the previous year's net plant and equipment and total firm employment.

The final firm-specific variable included in the analysis is a measure of firm size. Significant scale effects have been found in studies of wage determination. This indicates that the structure of internal labor markets within a firm may differ in important ways with the size of the firm. These differences may also affect the bargaining process. Firm size could be measured with either sales, capital stock, or employment. Since the latter two were used to form the capital-labor ratio, sales were chosen as the size measure.

The next set of variables attempts to control for the personal characteristics of the union workforce. Ideally, we would like to have information on the union workers actually in the bargaining unit. Since this information is not available, I constructed measures for the union workers in the same industry as the bargaining unit. Individuals who were working full time and who were covered by a union contract were selected from May *Current Population Survey* tapes from 1973 to 1977. These individuals were pooled and then sorted by 2-digit industry classifications. Industry averages for the age, education, percent male, and percent white were then calculated.

Two variables were added to the data to control for differences in industry structure. First, it is possible that the presence or absence of monopoly rents in an industry may significantly affect the bargaining process. A measure of potential monopoly rents that has been used extensively in the past (as well as debated over) is the concentration ratio. Specifically, this ratio is the percent of the total sales in a 4-digit industry classification that is accounted for by the four largest firms. The second variable is the percent of the industry employment that is unionized. The motivation for this variable is that higher unionization rates may place the union in a stronger relative bargaining position. Econometric studies of union wage effects often find a positive and significant effect for this variable (see H. Gregg Lewis, 1983). Estimates of unionization rates at a 3-digit industry level were taken from the work of Richard Freeman and James Medoff (1979).

Labor market conditions at the time of the negotiations may also be important determinants of strike activity. Ashenfelter and Johnson argue that "... during periods of low unemployment there will be decreased opposition among the rank and file to a militant course of action since there will be part-time job opportunities available for potential workers" (p. 40). The national unemployment rate has been the measure used in many previous studies. Almost unanimously, the finding among studies is that this unemployment rate has a negative and significant effect on the amount of strike activity.

The potential exists in this data set to more fully characterize the labor market conditions. For each negotiation, we know both the industry and the region affected. It would be interesting, then, to separately control for the industry and the regional labor market conditions. The only state unemployment rates going back to the early 1970's are constructed from state Unemployment Insurance claims. While attempts have been made to remove any inconsistencies due to the differences in state Unemployment Insurance laws, I decided not to use this data. Instead, both the industry and the local labor market conditions will be measured in terms of residuals from trend employment.

The industry trend regressions were estimated using quarterly 3-digit employment data for the period 1970–81. The local regressions were based on state or regional employment for the same time period.[4] The specification estimated was

$$(1)\quad \ln E_{it} = \beta_{i0} + \beta_{il}t + \sum_{j=1}^{3} \delta_{ij}Q_j + U_{it}$$

$$U_{it} = \Phi(L)U_{it-1} + \epsilon_{it}$$

where $\ln E_{it}$ = log quarterly employment in industry or region i at time t; Q_j = dummy variable for the jth quarter; $\Phi(L)$ = distributed lag polynomial; and ϵ_{it} = white noise.

The order of Φ was chosen so that the ϵ process showed no serious indications of departure from white noise. In most cases, a first- or second-order polynomial was sufficient. A potential feedback problem exists if the actual residuals are used in the analysis. The BLS gathers the monthly employment figures by counting the number of workers on payrolls as of the second week of the month. Workers on strike at this time are not added into the figures. This introduces a negative correlation between the actual level of strike activity and the estimated residual.

The autoregressive structure of the estimated residuals provides a method for avoiding this feedback problem. The current residual can be decomposed into a predicted and an unpredicted component. The predicted component is calculated using the $\Phi(L)$ polynomial and past employment residuals. Consequently, the predicted component of the current residual should be free of any significant correlation with the actual extent of strike activity in that quarter.

The employment growth rates from the trends regressions will be used to control the long-term trends in the industry and locality. Several other variables that need no explanation will also be tested. The next section explains the econometric methods used in the analysis and presents the empirical results.

II. Empirical Specification and Results

Two alternative estimation strategies exist for testing a variable's impact on the probability and duration of a strike. The first approach is to jointly estimate these effects using a Tobit model. The alternative is to estimate separate models for the probability and the conditional duration. The Tobit model builds in the assumption that if a variable increases the likelihood of a strike it also increases the conditional duration. While this may be a reasonable restriction, the second estimation strategy allows the data to indicate this rather than assuming it be be true. For this reason, the second approach will be used here.

[4]When a contract involves two or more states from different regions, the BLS assigns an interstate code. If these individual states could be identified, then the residual used is a population weighted average of the state residuals.

I assume that the probability of a strike occurring during the contract negotiations between union i and firm j at time t is given by the logistic function

$$(2)\qquad Pr_{ijt}=1/\left(1+\mathrm{Exp}(-X_{ijt}\beta^{s})\right).$$

The implied marginal effect of a variable on the probability of a strike is given by the function

$$(3)\qquad \frac{\partial Pr}{\partial X_j}=\beta_j^s\frac{\mathrm{Exp}(-X\beta^s)}{[1+\mathrm{Exp}(-X\beta^s)]^2}.$$

The transition from a strike to a settlement is modeled by a hazard function, $\lambda(t;X)$. The choice of the hazard function uniquely determines the probability distribution function for the conditional strike durations. The probability of observing a strike of duration t^* days conditional on its occurrence is

$$(4)\qquad f(t^*;X)=\lambda(t^*;X)\mathrm{Exp}\left[-\int_0^{t}\lambda(t;X)\,dt\right].$$

The manner in which time and the exogenous variables affect the hazard rate must be specified. A hazard function exhibits "duration dependence" if $\partial\lambda(t;X)/\partial t\neq 0$. In particular, when $\partial\lambda(t;X)/\partial t>0$, positive duration dependence exists. In this case, the longer a strike continues, the more likely it is that the strike will be settled in the next interval of time. If no duration dependence exists, then the conditional strike durations follow an exponential distribution.

I use a form for the hazard function that allows for any monotonic duration effect:

$$(5)\qquad \lambda(t;X)=\lambda\gamma(\lambda\gamma)^{\gamma-1}h(X),$$

so that

$$(6)\qquad \partial\lambda(t;X)/\partial t=\lambda\gamma(\gamma-1)(\lambda t)^{\gamma-2}h(X)\gtreqless 0 \text{ as } \gtreqless 1.$$

This allows the data to select the type of duration effect through the estimated value of *gamma*. The parameter γ is called the "baseline" hazard.

The remaining choice is the form for the function $h(X)$. A widely used functional form is the exponential: $h(X)=\mathrm{Exp}(X\beta^d)$. This choice for $h(X)$ gives the "proportional" hazard model. Let X_t denote the value of the exogenous variables at the outset of the negotiations. Assuming that these variables are held constant throughout the strike, then the probability that a strike starting at time t continues for t^* days is

$$(7)\qquad f(t^*;X_t)=\lambda(t^*;X_t)\mathrm{Exp}[(-\lambda(t^*;X_t)t^*)/\gamma].$$

The assumption that the exogenous variables remain constant throughout a strike, though, is unreasonable given that over half of the strikes in the sample continue beyond the quarter in which the contract expired. The longest strike lasted for a total of seven quarters. Consequently, the hazard function will incorporate variations in the industry and the local employment residuals as a strike enters a new quarter.

Partition the vector of exogenous variables, X_t, into a subvector that remains constant during a strike, X_{1t}, and a subvector that can vary from quarter to quarter, X_{2t}. Let t_k denote the number of days from the outset of the strike to the end of the kth quarter if the strike continues beyond that quarter; otherwise, t_k is the total duration of the strike. The probability of a strike starting at time t, lasting t^* days, and involving k quarters is

$$(8)\qquad f(t^*;X_t)=\lambda\gamma(\lambda t^*)^{\gamma-1}\mathrm{Exp}[X_{1t}\beta_1^d]\mathrm{Exp}[X_{2t_k}\beta_2^d]$$
$$\times\mathrm{Exp}\left[-\lambda^{\gamma}\mathrm{Exp}\left(X_{1t_1}\beta_1^d\right)\left\{t_1^{\gamma}\mathrm{Exp}\left(X_{2t_1}\beta_2^d\right)+\sum_{j=2}^{k}(t_j^{\gamma}-t_{j-1}^{\gamma})\mathrm{Exp}\left(X_{2t_j}\beta_2^d\right)\right\}\right].$$

The implied marginal effect of a variable on the conditional strike duration is

$$(9)\ \partial E(D|S)/\partial X_j = \frac{-\beta_j^d}{\lambda\gamma}\frac{\Gamma(1+1/\gamma)}{\left[\mathrm{Exp}(X\beta^d)\right]^{1/\gamma}}$$

Prior to estimation, the data were standardized by subtracting out the variable means and dividing by their standard deviations. These means and standard deviations are presented in Table 3. Tables 4 and 5 give the estimated coefficients from the logistic and hazard models as well as the implied marginal effects. The hazard marginal effects are measured in calendar days.[5]

Specification (2) differs from specification (1) in each table in that it includes fixed effects for eight major unions. Several variables have been constructed to capture differences among firms involved in the bargaining. However, no similar variables capture heterogeneity among unions. An example of such a union-specific variable that has been used in previous studies is a measure of potential strike benefits. Farber used the union's national strike fund balance per member while Grubert used the monthly contribution per member to the national fund. Farber found no significant effect for his proxy, while Grubert reported that his proxy had a positive and significant effect of the probability of a strike and a negative and significant effect on the duration of a strike.

Given the difficulty in obtaining estimates of these strike benefits and the inconclusive findings to date, the approach taken here is to not attempt to specify the source of the union heterogeneity. To the extent that these differences remain roughly constant both across bargaining pairs and through time, then their influence can be captured by a simple fixed effect. The choice of which unions to include a fixed effect for was dictated by the need for a sufficient number of contract negotiations and strikes involving that particular union. Consequently, a fixed effect was estimated for a union if at least five strikes involved that union. Eight unions comprising 51 percent of the negotiation sample and 74 percent of the strike sample satisfied this selection rule. Table A1 gives these fixed-effect estimates. (A table showing the distribution of each union's negotiations across major industry classifications can be obtained from the author.)

Turn now to the results given in Tables 4 and 5. Consider first the impact of the firm-specific variables. The firm's performance as measured by the rate of return on its stock has no effect of the likelihood of a strike. Conditional on a strike occurring, a one-standard-deviation increase in the firm's rate of return results in slightly over a five-day reduction in the expected duration. However, this effect is not very precisely measured and

TABLE 3—UNCONDITIONAL SAMPLE MEANS AND STANDARD DEVIATIONS

Variable	Mean	Standard Deviation
Rate of Return on Stock	7.2583	38.0756
Volatility of Stock Returns	0.0204	0.0077
Net Sales	3,598.2704	6,665.6834
Change in Inventory/Sales	−2.2368	16.1865
Capital-Labor	23.0301	30.8244
Average Age	39.6963	1.1031
Average Education	11.9631	0.4174
Percent White	87.0558	3.7908
Percent Male	80.3392	14.1186
Concentration Ratio	45.9378	21.0837
Union Coverage Rate	42.6122	12.4427
Industry Predicted Employment Residual	0.0816	5.0055
Local Predicted Employment Residual	−0.6690	4.0798
Industry Employment Growth Rate	0.1284	0.4494
Local Employment Growth Rate	2.1749	1.1450
Conditional Duration	50.0000	64.9289

[5]Table 4 also reports "pseudo" R^2 statistics for each specification. This R^2 is calculated as follows:

$$R^2 \equiv \left(1-(L_\beta/L_\Omega)^{2/N}\right)\Big/\left(1-(L_\Omega)^{2/N}\right),$$

where L_β = maximized value of the unrestricted likelihood function, L_Ω = maximized value of the likelihood function restricted to an intercept term, and N = sample size. This measure was proposed by John Cragg and Russell Uhler (1970).

TABLE 4—LOGISTIC MODEL

Variable	(1) Logistic Coefficient	(1) Marginal Effect	(2) Logistic Coefficient	(2) Marginal Effect
Intercept	−2.02396		−2.45879	
	(−20.96)		(−15.92)	
Rate of Return on Stock	0.02819	0.00291	0.00897	0.00088
	(0.34)	(0.34)	(0.10)	(0.10)
Volatility of Stock Returns	0.24225	0.02497	0.20536	0.02010
	(2.71)	(2.70)	(2.22)	(2.22)
Net Sales	−0.40221	−0.04146	−0.32898	−0.03220
	(−3.03)	(−3.08)	(−2.45)	(−2.47)
Change in Inventory/Sales	−0.06434	−0.00663	−0.02306	−0.00226
	(−0.77)	(−0.77)	(−0.26)	(−0.26)
Capital-Labor	0.23032	0.02374	0.19737	0.01932
	(1.72)	(1.72)	(1.40)	(1.41)
Average Age	−0.62140	−0.06313	−0.41975	−0.04108
	(−5.18)	(−5.41)	(−3.03)	(−3.11)
Average Education	0.37123	0.03827	0.26119	0.02556
	(2.71)	(2.77)	(1.72)	(1.75)
Percent Male	−0.09032	−0.00931	−0.08125	−0.00795
	(−0.78)	(−0.78)	(−0.58)	(−0.58)
Percent White	0.36811	0.03795	0.44929	0.04398
	(3.04)	(3.09)	(3.17)	(3.27)
Concentration Ratio	0.26354	0.02717	0.22710	0.02223
	(2.65)	(2.66)	(2.14)	(2.14)
Union Coverage Rate	0.20853	0.02150	0.21608	0.02115
	(1.60)	(1.62)	(1.38)	(1.40)
Industry Predicted Employment Residual	−0.20480	−0.02111	−0.22903	−0.02242
	(−2.31)	(−2.32)	(−2.43)	(−2.44)
Local Predicted Employment Residual	0.47323	0.04878	0.51824	0.05072
	(4.57)	(4.65)	(4.69)	(4.80)
Industry Employment Growth Rate	0.10218	0.01053	0.14674	0.01436
	(1.07)	(1.07)	(1.42)	(1.42)
Local Employment Growth Rate	0.17486	0.01802	0.20055	0.01963
	(2.02)	(2.02)	(2.23)	(2.24)
Log Likelihood	−499.737		−476.316	
Pseudo R^2	0.15		0.20	
$N = 1{,}319$				

Note: Specification (1) contains no union fixed effects and specification (2) contains fixed effects for eight unions.

disappears when the union fixed effects are introduced. On the other hand, greater volatility of the firm's stock returns increases both the probability and conditional duration of a strike. A one-standard-deviation increase in the measure of volatility results in over a $2\frac{1}{2}$ percent increase in the strike probability and nearly a seven-day increase in the expected duration. Controlling for the union fixed effects reduces both marginal effects, but the incidence effect remains significant.

The size of the firm as measured by its previous year's sales has a large and significant effect on both the likelihood and duration of a strike. A one-standard-deviation increase in sales reduces the probability of a strike by 4 percent and the expected duration by over two weeks. These effects remain significant even when the union fixed effects are included. The measure for the firm's buffer inventory has no effect on either measure of strike activity. Finally, there is some indica-

TABLE 5—PROPORTIONAL HAZARD MODEL

Variable	Hazard Coefficient (1)	Conditional Marginal Effect (1)	Hazard Coefficient (2)	Conditional Marginal Effect (2)
Rate of Return on Stock	0.11020	−5.17477	0.05862	−2.54764
	(1.55)	(−1.52)	(0.78)	(−0.78)
Volatility of Stock Returns	−0.14723	6.91371	−0.14620	6.35402
	(−1.75)	(1.77)	(−1.61)	(1.65)
Net Sales	0.31198	−14.65060	0.36441	−15.83820
	(2.41)	(−2.51)	(2.76)	(−2.76)
Change in Inventory/Sales	0.03048	−1.43129	0.05473	−2.37867
	(0.37)	(−0.37)	(0.63)	(−0.62)
Capital-Labor	−0.21169	9.94070	−0.22475	9.76797
	(−1.63)	(1.66)	(−1.64)	(1.68)
Average Age	0.03843	1.80470	0.09489	−4.12425
	(0.33)	(−0.34)	(0.66)	(−0.67)
Average Education	0.05622	−2.63997	0.17423	−7.57257
	(0.43)	(−0.43)	(1.16)	(−1.14)
Percent Male	0.04595	−2.15769	0.02800	−1.21711
	(0.40)	(−0.40)	(0.20)	(−0.20)
Percent White	0.26586	−12.48470	0.15704	−6.82541
	(1.93)	(−1.89)	(1.04)	(−1.04)
Concentration Ratio	0.07479	−3.51197	0.00045	−0.01975
	(0.77)	(−0.76)	(0.00)	(−0.00)
Union Coverage Rate	0.22786	−10.70020	0.17704	−7.69461
	(1.45)	(−1.42)	(0.92)	(−0.92)
Industry Predicted Employment Residual	0.01724	−0.80967	−0.01225	0.53267
	(0.19)	(−0.19)	(−0.14)	(0.14)
Local Predicted Employment Residual	0.27544	−12.93450	0.30545	−13.27560
	(2.72)	(−2.54)	(2.86)	(−2.60)
Industry Employment Growth Rate	0.12794	−6.00782	0.15694	−6.82111
	(1.23)	(−1.21)	(1.46)	(−1.44)
Local Employment Growth Rate	0.07071	−3.32044	0.17670	−7.67962
	(0.84)	(−0.83)	(1.96)	(−1.88)
Lambda	0.01923		0.02039	
	(10.78)		(6.87)	
Gamma	1.07641		1.13320	
	(18.14)		(17.88)	
Log Likelihood	−952.652		−944.129	
$N = 198$				

Note: Specification (1) contains no union fixed effects and specification (2) contains fixed effects for eight unions.

tion that highly capital-intensive industries have higher strike probabilities and longer expected durations.

The personal characteristics of the union workforce seem to be important determinants of strike activity. Increasing the average age of the union workers by 1.1 years is associated with a dramatic 6 percent decline in the strike probability. While controlling for the union fixed effects lowers this estimate to 4 percent, it is still highly significant. In addition, increasing the average education level by 0.42 years is associated with nearly a 4 percent increase in the incidence of strikes. While both age and education play important parts in the logistic function, neither seems to affect the expected conditional duration of a strike. The percent of the union workforce that is male had no significant effect on the amount of strike activity. On the other hand, increasing the percent that is white leads to more frequent but shorter strikes. Including the union fixed effects increases the marginal effect of the racial composition variable on the strike probability, but cuts the duration marginal effect and its significance level by almost half.

Industry structure as measured in this study does not seem to be a major factor in determining strike activity. The degree of concentration in the industry does have a positive and significant effect on strike probabilities, yet it has no effect on expected durations. Increases in the union coverage rate tends to result in more frequent but shorter strikes. However, neither of these effects is measured with great precision.

An interesting result comes out of looking at both the industry and local labor market conditions. Above-average predicted employment in the industry significantly reduces the likelihood of a strike, while similar conditions in the locality significantly increase this likelihood. The marginal effect for the local labor market conditions is also twice the magnitude of the industry effect. Both marginal effects are increased in size and significance when we control for the union fixed effects. While each variable plays an important role in determining strike incidence, only the local conditions also affect the expected duration of a strike. A one-standard-deviation increase in the local predicted employment residual is associated with a thirteen-day reduction in the expected conditional duration. In addition, higher employment growth rates in the locality are also associated with higher strike frequencies.

The estimate for *gamma* in specification (1) does not significantly differ from one. This would tend to indicate that no duration dependence exists in the data. However, if unobserved heterogeneity among the bargaining pairs is present, then it is easy to show that this estimate for *gamma* will be biased downwards. One possible source of this heterogeneity could be differences among unions. Notice that including the union fixed effects does increase the estimate of *gamma* significantly above one. This implies that the conditional settlement probability does increase with the length of the strike.[6]

Several other variables were also tested that are not reported in Tables 4 and 5. The sample contains both contract reopenings as well as renegotiations. It is possible that the likelihood of a strike differs significantly between these two types of bargaining situations. A dummy variable for a contract reopening was added to the basic specification. The logistic coefficient was -1.03416 with a standard error of 0.74682. The point estimate indicates a lower strike probability for reopeners of around 10 percent, but this effect is not measured with much precision. In addition, it may be possible that bargaining is affected by how long it has been since the last contract negotiation. The logistic coefficient for contract length measured in months was 0.02119 with a standard error of 0.10201. There is no evidence, then, that contracts with longer durations are more or less difficult to renegotiate.

Grubert hypothesized that strikes would be less likely when either a small fraction or a large fraction of the firm's employment was involved. His basic argument is as follows: "If the labor share is extremely high or extremely low, strikes are unlikely because the party with the larger share will be willing to make acceptable concessions to the other party. That is, there will be a big loser who will readily give in to the other side" (p. 16). It is possible to do a simple test for this inverted U-shaped effect since both the number of workers covered by this contract and total firm employment are available. The finding was that increasing the employment share has a negative and diminishing impact on the probability of a strike throughout the range of shares in the data. While no evidence for Grubert's hypothesis was found, this measure does not account for factors such as possible spillover effects.

Recall that Table 2 listed the sample strike frequencies by month. These ranged from a high of 22.67 in June to a low of 3.70 in

[6]John Kennan (1986), and Alan Harrison and Mark Stewart (1985), estimate strike duration models which allow for more general duration dependence effects. Kennan uses U.S. data and finds that the hazard first decreases and then increases. Harrison and Steward use Canadian data and find that the hazard slowly increases throughout the first 99 days.

TABLE 6—TEST FOR SEASONALITY OF STRIKES

Month	Difference in Strike Frequency	Logistic Coefficient	Difference in Strike Probability
January	−1.24	0.42371	4.58
		(1.00)	(0.97)
February	1.54	0.40068	4.30
		(0.89)	(0.84)
March	−5.88	−0.20232	−1.71
		(−0.46)	(−0.46)
April	2.65	0.28564	2.93
		(0.76)	(0.76)
May	−1.87	0.08912	0.84
		(0.24)	(0.24)
June	7.29	0.61550	7.16
		(1.78)	(1.83)
July	−4.17	−0.42174	−3.26
		(−1.00)	(−1.00)
August	−1.26	0.37929	4.03
		(1.02)	(1.02)
September	0.12	−0.11271	−0.99
		(−0.30)	(−0.30)
October	–	–	–
November	5.03	0.14236	1.38
		(0.31)	(0.30)
December	−11.68	−1.41208	−7.50
		(−1.81)	(−2.36)

December. This variability does not in itself indicate that there is seasonality to strike probabilities. There may be monthly variation in other factors in the model that would account for this result. To test for this seasonality, monthly dummy variables were added to the logistic function. October was selected to be the omitted month. Table 6 gives the differences in monthly strike frequencies from October, the logistic coefficient, and the implied differences in monthly strike probabilities from October. Only June and December have strike probabilities that differ significantly from October once other factors are controlled for.

As an aid in seeing how well the variables tested in this study explain interindustry variation in strike activity, Table 7 gives the industry strike frequencies and average strike durations. Strike probabilities and expected durations were then calculated for each negotiation in the sample. The model without the union fixed effects was used in these calculations. These were then averaged by industry and included as well in Table 7. For industries with a large number of observations, the two sets of figures are in fairly close agreement.

III. Summary of the Findings

Firm-specific factors are key determinants of strike activity. It is not the rate of return on a firm's stock, but rather its volatility that affects the bargaining process. This suggests that the asymmetric information theories of strikes should be carefully examined. A connection may exist between this measured variability and the benefit to the union from trying to infer from the firm information about future demand conditions. Large firms were found to have significantly less strike activity. This result should also be examined in future work on bargaining models. Finally, capital-intensive technologies tend to increase the frequency and duration of strikes. If higher capital-labor ratios indicate a greater ability for the firm to maintain production during a strike, then this finding is consistent with the view that strikes will be used more when their joint costs are smaller (see Melvin Reder and George Neumann, 1980).

While the personal characteristics of the union workforce have not been included in previous strike studies, they do have important effects on the bargaining process. Older and less-educated workers tend to be less involved in strike activity. Greater proportions of nonwhites in the workforce also reduces the use of strikes. Clearly, these factors should be incorporated into models of bargaining.

Studies of union and nonunion wages have found that the union wage differential increases with the rate of unionization in that industry. These wage gains, though, are not accompanied by any significant difference in the number of strikes. More concentrated industries do experience a higher incidence of strikes but no difference in expected durations. These findings suggest that industry structure is not a primary determinant of strike activity.

The contrasting effects of the industry and the local labor market conditions may also be consistent with the joint cost view of strikes. Above-average industry conditions

TABLE 7—AVERAGE AND EXPECTED VALUES FOR MEASURES OF STRIKE ACTIVITY BY INDUSTRY

Industry	N	Strike Frequency	Expected Strike Probability	Average Conditional Duration	Expected Conditional Duration
Food	94	0.05319	0.05952	94.40	60.22
Tobacco	14	0.07143	0.02291	42.00	218.06
Textile	23	0.00000	0.02269	–	–
Apparel	13	0.00000	0.01180	–	–
Lumber	20	0.10000	0.09654	98.00	86.41
Furniture	14	0.07143	0.09983	47.00	94.20
Paper	112	0.07143	0.14094	29.00	45.84
Printing	10	0.10000	0.13640	101.00	57.81
Chemicals	121	0.06612	0.10754	79.38	69.71
Petroleum	47	0.08511	0.03339	47.25	53.24
Rubber	41	0.46341	0.28316	52.84	61.99
Leather	18	0.11111	0.10289	85.00	74.11
Stone, Clay and Glass	55	0.05454	0.08504	37.33	36.58
Prim. Metal	157	0.08917	0.06812	37.50	39.60
Fab. Metal	50	0.20000	0.16051	53.20	44.81
Mach. except Elec.	152	0.28289	0.27766	39.65	39.00
Elec. Eq.	161	0.19876	0.21541	55.38	48.35
Trans. Eq.	182	0.20330	0.20176	52.62	54.91
Prof. Instr.	25	0.28000	0.20688	28.43	51.22
Misc.	10	0.10000	0.07585	20.00	97.03

may indicate that it is expensive for production to be halted due to a strike. On the other hand, above-average local conditions may indicate that union workers have good opportunities outside the firm during a strike. In any event, the findings point out the importance of controlling separately for each type of labor market effect.

Finally, the significance of some of the union fixed effects suggest that it is important to attempt to characterize specific ways in which unions may differ. The distribution of union contracts by industry illustrate that some of the union effects must be interpreted with caution. For example, both the United Rubber Workers and the Oil, Chemical, and Atomic Workers unions are both heavily concentrated in a single industry and comprise a large fraction of contracts in that industry. Consequently, it is impossible to say whether this is a union or an industry fixed effect. On the other hand, the Marine and Shipbuilding Workers, while being entirely concentrated in the transportation equipment industry, comprise only 6 percent of the total contracts in that industry. The union fixed effect in this case is clearly not simply an industry effect in disguise.

TABLE A1—UNION FIXED EFFECTS

Union	Logistic Coefficient	Hazard Coefficient
Electrical Workers (IBEW)	0.12194	−0.73828
	(0.27)	(−1.75)
Machinists	0.45918	−0.49669
	(1.45)	(−1.68)
Marine and Shipbuilding Workers	2.67857	−0.49938
	(3.98)	(−1.05)
Rubber Workers	1.83145	0.30769
	(4.19)	(0.78)
Steelworkers	0.29866	0.06455
	(1.04)	(0.23)
Electrical Workers (IEU)	0.91477	0.40752
	(2.08)	(0.99)
Oil, Chemical, and Atomic Workers	1.22526	−0.73493
	(2.52)	(−1.48)
Auto Workers	1.01437	0.23874
	(3.70)	(0.94)

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