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Longitudinal Analysis of Strike Activity

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This article presents an empirical study of strike activity in a panel of contract negotiations for some 250 firm-and-union pairs. Evidence is presented on two sources of variation in dispute rates: changes in the characteristics of the collective bargaining agreement that affect subsequent strike outcomes and the effects of lagged strikes on the incidence and duration of subsequent disputes. Strike probabilities are significantly affected by the duration and expiration month of the previous agreement. Dispute rates are also increased by the occurrence of a short strike during the previous negotiations and reduced by the occurrence of a long strike.

I. Introduction

The growing availability of data on collective bargaining settlements in the union sector has generated widespread interest in the empirical analysis of strikes.[1] In contrast to earlier studies based on the aggregate number of

I am grateful to Wayne Vroman for making available his contract data and to Sheena McConnell and Joe Tracy for access to their data on strikes. Thanks to John Abowd, Rebecca Blank, and George Jakubson for comments on earlier drafts.

[1] Recent studies include papers by Gramm (1986, 1987), Gunderson, Kervin, and Reid (1986), McConnell (1986), Schnell and Gramm (1987), Tracy (1986, 1987),

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strikes, recent studies have shifted attention to the microlevel determinants of strike outcomes.[2] This article focuses on the time-series variation in bargaining-pair-specific strike outcomes. Evidence is presented from a panel of collective bargaining agreements on two sources of variation: changes in the characteristics of the collective bargaining agreement that affect subsequent strike outcomes and the effects of lagged strike outcomes on the incidence and duration of subsequent disputes.

The first part of this article addresses the question of whether bargaining parties can vary the probability of future work stoppages by their choice of contract characteristics. I concentrate on three aspects of the preceding contract: the expiration month of the contract; the duration of the contract; and the provision of a limited reopening clause, which restricts subsequent negotiations to a small number of issues (usually wages). The empirical analysis shows that strike probabilities are higher following a long contract and lower in limited reopening situations. Strike probabilities are also higher following expirations in summer and fall, relative to winter and spring.

The second part of the article addresses the question of whether strike probabilities and durations are affected by preceding strike outcomes. Contrary to the findings of previous researchers, there is no evidence of state dependence in strike incidence.[3] The absence of simple state dependence, however, is the product of two competing effects: an increase in strike probabilities following a strike of less than 14 days' duration and a decrease in strike probabilities following a longer work stoppage. In contrast to the effects of lagged strike outcomes on subsequent strike probabilities, the effects on subsequent durations are small and imprecisely measured.

II. Data Description

The data in this study represent strike outcomes for contract negotiations of 253 bargaining pairs during the period from 1955 to 1979. Contract terms and strike information were originally collected from the Bureau of Labor Statistics' monthly publication, *Current Wage Developments* (*CWD*), by Wayne Vroman.[4] Vroman's data set contains information on some 300 collective bargaining situations covering 1,000 or more workers. For pur-

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and S. Vroman (1986). Earlier studies include Farber (1978), Mauro (1982), and Swidinsky and Vanderkamp (1982). Much of the recent empirical and theoretical literature on strikes is summarized by Kennan (1986).

[2] Among the aggregate studies are papers by Ashenfelter and Johnson (1969), Pencavel (1970), Kaufman (1982), and Abbott (1984).

[3] Mauro (1982) and Schnell and Gramm (1987) both report evidence that strike probabilities are reduced following a strike in the previous negotiation.

[4] I am grateful to Wayne Vroman for making these data available. A further description of the sample is presented in W. Vroman (1982).

poses of this article however, I have focused on bargaining pairs with complete information on at least seven consecutive agreements. The resulting sample contains 2,543 contracts, or an average of 10 contracts per bargaining pair.

Table 1 presents a cross-tabulation of the data by industry. With the exception of contracts for seven bargaining pairs in transportation, communications, and public utilities, the contracts are drawn from the manufacturing sector.[5] The coverage within two-digit manufacturing industries is irregular and reflects Vroman's original interest in "pattern" bargaining and wage determination. The data set contains single-employer contracts covering multiple establishments (such as the Autoworkers' agreements with General Motors, Ford, and Chrysler), multiple-employer agreements (such as the Distillery Workers' agreements with the Winery Employers Association of California), and single-establishment agreements (such as the Machinist's agreements with Morse Chain). The average number of workers covered by each contract is 13,000.

The contract sample is "unbalanced" in the sense that there are different numbers of contracts for each bargaining pair. Contracts in the textile industry, for example, tend to be short. As a result, there is an average of 14 contracts per bargaining pair from this industry over the 24-year sample period. Most contracts in the transportation equipment industry, in contrast, run for 3 years. As a result, there are approximately eight contracts for each bargaining pair in the sample from this industry.

In addition to fixed-duration noncontingent contracts, the sample contains a variety of alternative contract forms, including fixed-duration contracts with contingent cost-of-living wage-adjustment formulas (approximately 25% of the sample), contracts with scheduled wage-reopening provisions (approximately 7.5% of the sample), and contracts with contingent wage-reopening provisions (approximately 1% of the sample).[6] Since wage-reopenings place the parties at risk of a strike, I have defined each reopening as a new contract. The impact of reopener clauses on strike probabilities is analyzed in the next section.

Even in the absence of explicit reopening provisions, most long-term

[5] The nonmanufacturing bargaining pairs are: Class I Railroads and the Railroad Engineers; Class I Railroads and the Brotherhood of Railroad and Airline Clerks; Trucking Employers Incorporated and the Teamsters (National Master Freight Agreement); New York Shipping Association and the Longshoremen; Pacific Maritime Association and the Longshoremen; United Airlines and the Machinists; and ATT (Longlines Division) and the Communication Workers.

[6] A reopening clause states that the parties will open an ongoing agreement for purposes of renegotiating a limited number of contract issues (often only wages): see Bureau of National Affairs (1986, sec. 36.2). Most reopener clauses in the sample specify a fixed date for reopening. A small number specify reopening contingent on a specific event (e.g., discontinuation of wage and price controls; wage adjustments in contracts at other firms; inflation rates above a certain maximum).

Table 1
Strike Characteristics by Industry

Industry	No. of Pairs	All Available Contracts: No. of Contracts	All Available Contracts: Strike Probability (%)	All Available Contracts: Strike Duration (Days)	Last 6 Contracts for Each Pair: Strike Probability (%): Overall	1959–64	1965–69	1970–74	1975–79	Last 6 Contracts for Each Pair: Strike Duration (Days)
1. Food and beverages	20	205	8.3	31.0	10.8	.0	17.2	12.0	6.9	34.2
2. Tobacco	3	24	12.5	27.0	16.7	.0	75.0	.0	.0	27.0
3. Textile mills	13	185	4.9	20.2	7.7	.0	11.1	15.8	2.6	13.8
4. Apparel	13	118	1.7	7.0	2.6	.0	3.9	4.6	.0	7.0
5. Lumber and wood	9	88	6.8	79.7	3.7	.0	5.6	5.9	.0	59.0
6. Furniture	5	41	14.6	15.3	20.0	.0	14.3	50.0	.0	15.3
7. Paper	19	229	6.1	62.3	7.9	.0	6.1	4.7	15.1	58.8
8. Printing	6	48	8.3	34.3	8.3	.0	15.4	.0	12.5	43.0
9. Chemicals	21	242	10.3	56.9	14.3	.0	20.0	19.6	7.5	64.4
10. Petroleum	4	56	10.7	17.3	.0	.0	.0	.0	.0	. . .
11. Rubber	7	73	27.4	56.6	40.5	16.7	35.7	58.3	40.0	59.8
12. Leather	5	45	.0	. . .	.0	.0	.0	.0	.0	. . .
13. Glass	22	215	8.8	42.7	12.1	.0	24.4	7.5	9.4	40.7
14. Primary metals	17	165	19.4	89.3	18.6	10.0	24.1	18.8	19.1	92.3
15. Fabricated metals	14	131	19.9	76.5	25.0	29.4	28.6	28.0	14.3	40.9
16. Machinery (nonelectrical)	14	124	29.8	33.1	38.1	21.4	40.0	42.9	41.2	28.3
17. Electrical machinery	24	203	16.8	44.7	20.8	9.7	22.5	23.9	25.9	48.6
18. Transportation equipment	24	200	20.5	42.3	24.3	14.3	32.5	16.3	33.3	42.1
19. Instruments	4	54	3.7	12.5	8.3	.0	20.0	8.3	.0	12.5
20. Miscellaneous manufacturing	2	27	.0	. . .	.0	.0	.0	.0	.0	. . .
21. Transportation and utilities	7	70	18.5	32.1	23.4	33.3	16.7	12.5	45.5	35.7
22. All industries	253	2,543	12.4	49.4	16.1	10.0	20.8	16.3	13.9	45.7

NOTE.—Data source is described in text. Strikes include strikes from *Current Wage Developments* and strikes added from McConnell-Tracy strike listings.

labor contracts can be reopened at any time with the mutual consent of the parties, and many contracts are automatically extended past their scheduled expiration date unless one party or the other files a formal notice of intent to terminate the contract and put the pair at risk of strike.[7] Approximately 1% of contracts in the sample were reopened earlier than 3 months before their scheduled expiration date, and another 6% ran more than 3 months past their scheduled expiration date before a new contract was reached or a strike was declared. While these facts introduce some ambiguity into the notion of a contract expiration date, I have adopted the convention of dating expirations by the earlier of the expiration date of the preceding contract and the actual renegotiation date of the next contract, as reported in *Current Wage Developments.*[8]

The third and fourth columns of table 1 contain information on the probability and mean duration of strikes by industry. For most of the contracts in the sample, the source of strike information is the contract listing in *Current Wage Developments.*[9] For contract expirations after 1970, however, strike information is also available from an exhaustive listing of major strikes assembled by Sheena McConnell and Joseph Tracy.[10] McConnell and Tracy's data include local-issue strikes associated with the ratification of multiestablishment master contracts, as well as more widespread disputes. For comparability with the *CWD* definition of contract strikes, however, I restricted attention to disputes involving at least 70% of workers covered by each contract.[11] The McConnell-Tracy strike listings

[7] See ibid., sec. 36.2. A small number of contracts in the sample from the textile industry specify an indefinite contract duration, although these contracts all reopened at regular 12- or 24-month intervals.

[8] An interesting issue for further research is the question of when and why the parties continue to operate under the terms of the old contract.

[9] The original source of the strike information in *CWD* is the contract report filed by firms at the request of the Bureau of Labor Statistics (BLS), in connection with their ongoing analysis of wage changes in contracts with 1,000 or more workers. For contracts negotiated between 1960 and 1970 I checked the *CWD* strike information against contract information reported in the *Monthly Labor Review's* monthly summary of recent developments in industrial relations. I found only three examples of strikes reported in the *Review* that were not recorded in *CWD*.

[10] McConnell and Tracy's strike listings were assembled from three sources: a weekly BLS in-house newsletter entitled "Industrial Relations Facts"; a BLS data tape listing strikes recorded from published newspaper reports; and a Bureau of National Affairs' data tape listing strikes from published media and other sources. The Bureau of National Affairs' data are only available after 1970; for this reason McConnell and Tracy began their merged strike listings in that year. A further description of their data is provided by McConnell (1986).

[11] The precise definition of a strike in *Current Wage Developments* is unclear. It is clear, however, that the BLS does not report local-issue strikes in *CWD*. The question of whether local-issue strikes should be treated differently from more general disputes is an important issue for further research.

contribute an additional 29 strikes to the 114 strikes reported in Vroman's data for contracts negotiated after January 1970. These figures suggest that some 20% of strikes associated with contract renegotiations are missing from the *CWD* listings. The added strikes are shorter than the strikes in *CWD* (27 vs. 48 days) but are more or less evenly distributed over time and across industries. Assuming that 20% of actual strikes are randomly missing from the *CWD* listing prior to 1970, the true probability of strikes in the data set is 14.1%.

For purposes of a longitudinal analysis, it is convenient to work with a fixed number of contract expirations per bargaining pair. To this end, I have extracted a balanced sample of contracts based on the six most recent negotiations for each bargaining pair. Strike probabilities and durations for this sample of 1,518 contracts are presented in the right-hand columns of table 1. The pattern of strike probabilities and durations across industries is similar between the total sample and the balanced subsample, although strike probabilities are somewhat higher in the subsample. This reflects the fact that the subsample contains relatively fewer contracts from the 1950s and early 1960s, when strike probabilities were relatively low. By the same token, underreporting of strikes is less significant in the subsample since relatively fewer of the contract expirations in the subsample occurred before 1970 (39.7% vs. 63.2%). Assuming that 20% of strikes prior to 1970 are unreported, the true probability of strikes in the balanced sample is 17.8%.

The simple averages in table 1 show considerable variation across industries in both the probability and duration of strikes. Strikes are more likely in durable than nondurable manufacturing and are most frequent in the rubber and nonelectrical machinery industries.[12] There is a weak positive rank-order correlation across industries between strike probabilities and the conditional duration of strikes (.21), although the correlation falls to zero if the apparel industry is ignored. The longest strikes are in lumber and wood products, primary metals, and fabricated metals, while the shortest strikes are in apparel, petroleum refining, and instruments.

Some additional insight into the distribution of strike lengths is provided by table 2, which gives the weekly settlement rates for 244 strikes drawn from the balanced sample of contract expirations. Over 95% of these strikes are settled in 20 weeks or less. The average weekly settlement rate during the first 20 weeks is 13.8%. With only two exceptions, the individual weekly

[12] Industry average strike probabilities from larger samples of agreements are presented by McConnell (1986) and Gramm (1987). McConnell's data include 4,592 agreements in the manufacturing sector from the period 1970–81. Gramm's data include 3,812 agreements from the period 1971–80. The correlation coefficients between the industry strike probabilities in the fifth column of table 1 and those presented by McConnell and Gramm are .91 and .73, respectively.

Table 2
Empirical Hazard Rate of Strike Settlement*

Week	No. of Ongoing Strikes	Fraction Settled in Week	t-Ratio for Test of Constant Hazard†
1	244	.127	−.50
2	213	.197	2.50
3	171	.181	1.64
4	140	.136	−.08
5	121	.165	0.87
6	101	.069	−2.00
7	94	.117	−.59
8	83	.169	.81
9	69	.159	.52
10	58	.086	−1.14
11	53	.189	1.07
12	43	.070	−1.30
13	40	.275	2.51
14	29	.069	−1.08
15	27	.111	−.41
16	24	.042	−1.37
17	23	.130	−.11
18	20	.200	.80
19	16	.125	−.15
20	14	.143	.05

* Calculated from 244 strikes from last six contracts for each bargaining pair. After 20 weeks there were 12 strikes in progress.

† t-ratio for the hypothesis that the weekly hazard rate is equal to the average hazard rate over the first 20 weeks (13.8%). The chi-squared test that all 20 hazard rates are equal is 29.58 with a marginal significance level of 5.8%.

settlement rates are within two standard errors of the overall average: the test statistic for the hypothesis of a constant settlement hazard has a probability value of about 6%. It should be noted, however, that the number of strikes after only a few weeks is relatively small. Recent studies by Kennan (1980, 1985) and Harrison and Stewart (1986), using much larger sample sizes, conclude that strike settlement rates tend to decrease with the duration of the strike. While there is no evidence of this phenomenon in table 2, it is difficult to draw firm conclusions because of the relatively small sample size.

The tabulation of strike probabilities by 5-year interval in table 1 shows considerable time-series variation in aggregate and industry-specific strike propensities. Figure 1 presents average annual strike probabilities for 1961–79 from the balanced subsample of 1,518 contracts. Actual probabilities and strike durations by year are recorded in table A1. The figure shows both the probability of strikes associated with contract expirations in each year and an adjusted probability that controls for the industry composition of contract expirations. These adjusted probabilities represent estimated year effects from a linear probability model that includes two-digit industry controls. Both series show relatively low strike probabilities in the early 1960s, followed by sharply higher strike probabilities from 1965 to 1970,

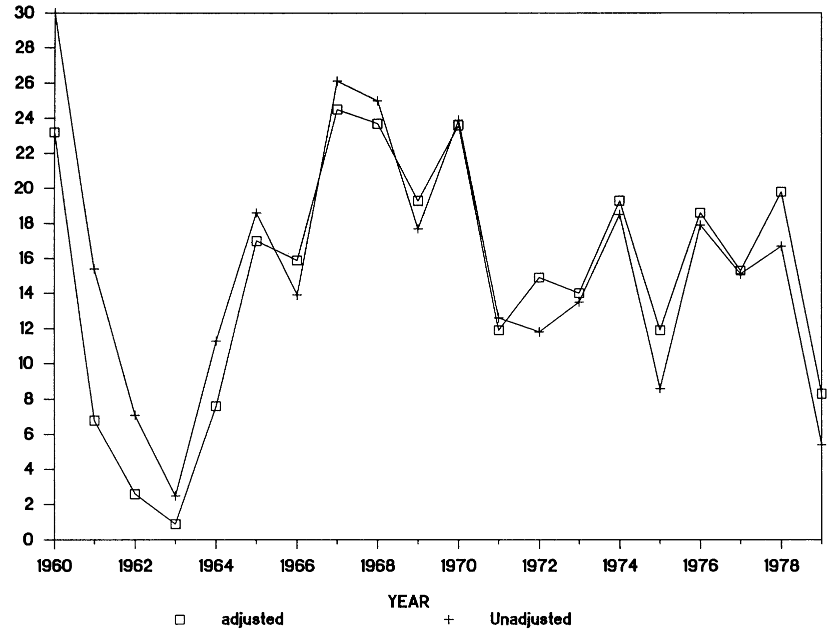

FIG. 1.—Strike probabilities by year

and lower but widely varying strike probabilities throughout the 1970s. Since the focus of this article is on the longitudinal structure of strike activity, in the empirical analysis reported below I control for time-varying aggregate strike propensities with a series of year effects. Experiments with both ordinary and fixed-effect probability models suggest that the time-varying component of strike activity is well represented by a simple four-step function, with steps at 1965, 1971, and 1975. In particular, this function captures the relatively low strike probabilities observed in the early 1960s and the relatively high strike probabilities from 1965 to 1970.[13]

III. The Effects of Contract Characteristics on Strike Incidence

In this section I analyze the effects of three characteristics of collective bargaining agreements on the probability of strikes: the seasonal timing

[13] Susan Vroman (1986) has investigated the effects of a variety of macroeconomic variables on the probability of strikes in this data set. Her results suggest that unemployment rates, past inflation rates, and wage and price controls all effect the probability of strikes. She does not, however, compare the explanatory power of her probit regressions to regressions with unrestricted year effects. The likelihood ratio test statistic of the four-step time function against an unrestricted specification of the year effects in a logistic regression that includes two-digit industry effects has a probability value of 16%.

of contract expirations; the provision for a limited contract reopening; and the length of time between successive negotiations. Estimation of the effects of contract characteristics on the probability of strikes is complicated by the nonrandom incidence of these characteristics across industries and bargaining pairs. The fact that strike probabilities are relatively low among contracts that expire in December, for example, does not imply that a bargaining pair who traditionally negotiate in June could reduce the probability of a work stoppage by scheduling their negotiations in December. To control for unobserved heterogeneity in strike propensities across bargaining pairs, I use three alternative estimation strategies. The first strategy is to model heterogeneity across industries using a series of two-digit industry effects. The second estimation strategy is a conditional logistic regression—the direct analogue of a fixed-effects estimator for logistic probability models. The third strategy is a first-differenced version of the linear probability model. In all cases, the empirical analysis accounts for random underreporting of strikes among expirations prior to 1970.

A. Seasonality of Expirations

It is widely acknowledged that there is a strong seasonal pattern in measures of aggregate strike activity.[14] Only recently, however, has it been possible to decompose this pattern of seasonality into the seasonal component of expirations and the seasonal component of strike probabilities.[15] Evidence in Gramm (1987, table 3) based on a large sample of manufacturing and nonmanufacturing contracts suggests that monthly strike probabilities vary widely. Her data analysis does not address the question of whether this seasonal variation is due to the monthly composition of contract expirations, however, or to an intrinsic seasonal effect. Indeed, the year-to-year variation in relative monthly strike probabilities in Gramm's data suggests that compositional effects may be an important component of seasonal variation in strike probabilities.[16]

In an effort to isolate intrinsic seasonal variation in strike probabilities, table 3 presents estimated month effects from several models that control for the composition of expirations. For reference, the first two columns of the table report the fraction of expirations in each month and the associated raw strike probabilities. Column 3 of table 3 reports estimated

[14] Kennan (1986, sec. 11) provides a brief summary of the evidence on seasonality, starting with the study by Yoder (1938).

[15] Aggregate measures of strike incidence or duration include intracontract strikes. Evidence reported by Flaherty (1983) shows that these strikes also have a seasonal pattern.

[16] For example, the rank-order correlation between monthly strike probabilities in 2 consecutive years in Gramm's data is typically less than .25 and is in many cases negative.

Table 3
Seasonal Effects on Strike Incidence*

	Percent of Contracts (1)	Actual Strike Probability (%) (2)	Ordinary Logit† No Industry or Year Effects (3)	Ordinary Logit† Industry and Year Effects (4)	Conditional Logit‡ with Year Effects (5)	First Differenced Linear Probability with Year Effects§ (6)
January	6.5	9.2	.0	.0	.0	.0
			(. . .)	(. . .)	(. . .)	(. . .)
February	5.5	18.1	.79	1.09	.09	.15
			(.36)	(.73)	(.63)	(.98)
March	6.9	24.8	1.24	1.65	.65	.18
			(.32)	(.50)	(.61)	(1.02)
April	7.6	8.7	−.03	−.02	−.39	−.71
			(.04)	(.14)	(.67)	(.86)
May	10.3	7.7	−.19	−.01	−.00	−.75
			(.36)	(.57)	(.67)	(.84)
June	15.6	11.8	.30	.70	.65	.57
			(.30)	(.50)	(.61)	(.71)
July	13.4	21.7	1.07	1.08	1.07	1.54
			(.28)	(.39)	(.58)	(.77)
August	5.6	22.4	1.10	1.07	2.03	1.40
			(.34)	(.37)	(.73)	(.90)
September	7.9	15.0	.61	.48	1.08	1.05
			(.34)	(.42)	(.66)	(.86)
October	11.0	22.2	1.10	.95	.76	.24
			(.29)	(.35)	(.57)	(.92)
November	6.3	26.0	1.30	1.51	1.09	.91
			(.33)	(.45)	(.59)	(.99)
December	3.5	1.9	−1.65	−1.53	−.83	−.16
			(1.02)	(.82)	(1.18)	(.84)
Exclusion Test (probability value)‖	. . .	. . .	59.52	53.62	22.80	21.97
			(.00)	(.00)	(.02)	(.02)

NOTE.—Standard errors are in parentheses.
* Based on last six contracts for each bargaining pair. Sample size is 1,518.
† Estimated month effects from logit regression with and without controls for two-digit industry and time periods.
‡ Estimated month effects from conditional logit regression with approximate correction for unreported strikes prior to 1970. See text.
§ Normalized estimated month effects from first-differenced linear probability model. Standard errors and exclusion test statistic are corrected for heteroscedasticity. The estimates represent the actual estimates multiplied by 8 and are directly comparable with the estimates obtained from the logit specifications.
‖ Likelihood ratio test for exclusion of month effects. The test statistic has 11 degrees of freedom. For the linear probability model, the test statistic is a Wald test that the month effects are all zero.

month effects from a benchmark logistic regression model with no controls for heterogeneity. In order to incorporate the assumption that 20% of strikes associated with expirations before 1970 are unreported, I assume that the probability of an observed strike is

$$p_{it} = (1 - .2D_{it})p_{it}^{*}, \tag{1}$$

where p_{it}^{*} is the predicted probability of a strike in the tth negotiation for the ith pair in the absence of any underreporting, and D_{it} is an indicator variable for expirations prior to January 1970. The probability of a dispute in the absence of underreporting is given by the usual logistic regression formula

$$\text{logit}(p_{it}^{*}) \equiv \log[p_{it}^{*}/(1 - p_{it}^{*})] = \mathbf{x}_{it}\boldsymbol{\beta}, \tag{2}$$

where $\mathbf{x}_{it}$ represents a vector of indicators for expiration month, and $\boldsymbol{\beta}$ is a vector of month effects with the normalization that the effect for January is zero.

The estimates in column 3 show significantly different strike probabilities depending on the expiration month of the preceding contract. The likelihood-ratio test associated with the hypothesis that strike probabilities are constant across months is reported in the last row of the table and is highly significant.

The fourth column of table 3 introduces two-digit industry controls and a four-step function of time as additional covariates of strike activity.[17] Comparing the estimates in the third and fourth columns of the table, there are only small differences between the estimated month effects. These results suggest that the monthly pattern of strike probabilities is not simply an artifact of the industry distribution of contract expirations. In fact, the monthly pattern of strike probabilities is very similar with and without industry controls.

The fifth column of table 3 contains estimated month effects from a conditional logit model of strike incidence.[18] This model permits a separate fixed effect for each bargaining pair in the data set. Specifically, the prob-

[17] The main effect of the adjustment for unreported strikes is to change the estimated year effects in the specification for p_{it}^{*}, the probability of strikes in the absence of underreporting. Comparing estimated logistic regression models that include industry effects and time-period effects with and without the correction of underreporting prior to 1970, the year effects for time periods before 1970 are .20–.25 higher in the corrected model. The industry effects are very similar between specifications.

[18] This model is described in Chamberlain (1980), who also provides references to earlier work.

ability of a strike for the *i*th bargaining pair at the *t*th negotiation is assumed to be given by

$$\text{logit}(p_{it}^{*}) = \alpha_i + \mathbf{x}_{it}\beta, \tag{3}$$

where $\mathbf{x}_{it}$ represents a vector of year and month indicators and α_i represents a fixed pair-effect. As a consequence of the functional form of a logistic probability model, the pair-effects are eliminated by considering the likelihood of the sequence of strike indicators for the *i*th bargaining pair (y_{i1}, $y_{i2}, \ldots, y_{i6}$) conditional on the total number of strikes observed for the pair in the sample. Since the number of strikes is a sufficient statistic for the pair-effect in the logistic probability model, conditioning eliminates the pair-effect from the likelihood while permitting estimation of the coefficients associated with the time-varying determinants of strike probabilities.[19]

Unfortunately, the addition of the simple underreporting model described by equation (1) to strike probability model (3) leads to a probability model for observed strike outcomes that no longer satisfies the necessary conditions for the conditional logit model. The fixed-effects specification can be combined with an *approximation* to the underreporting model, however, that leads to a workable conditional likelihood.[20] I have therefore used this approximate correction for underreporting of strikes prior to 1970 to compute the estimates in column 5 of table 3. These estimates are less precise than the conventional logit estimates in column 4 and indicate a somewhat different seasonal pattern. The conditional logit estimates, in fact, suggest that the monthly effects can be divided into just two "seasons": a low strike probability season from December to May and a high strike probability season from June to November. The likelihood ratio test for the hypothesis that strike probabilities are constant from December to May and from June to November has a marginal significance level of .38. The estimated month effect for June–November in this simple 2-season model is .96, with a standard error of .28. Assuming an average strike

[19] Note that bargaining pairs who never strike, or who strike in every negotiation, do not contribute to the conditional likelihood.

[20] Specifically, I consider the approximate formula for the probability of an observed strike:

$$p_{it} \cong \frac{e^{\alpha_i + \mathbf{x}_{it}\beta - \gamma D_{it}}}{1 + e^{\alpha_i + \mathbf{x}_{it}\beta - \gamma D_{it}}},$$

where $\gamma = .256$ is chosen to approximate the behavior of the true model at the sample mean strike probability. This model is clearly amenable to a conditional likelihood formulation. The approximation is also relatively precise, at least for values of the individual effects that give rise to predicted strike probabilities between 2% and 30%.

probability of 15%, this implies a 12-percentage point difference in strike probabilities between expirations in summer and fall, on one hand, and winter and spring, on the other.

The conditional logit estimation scheme relies very heavily on the functional form of the logistic distribution function. As a check on the results in column 5, consider an alternative linear-probability specification

$$p_{it}^* = \alpha_i + \mathbf{x}_{it}\beta, \tag{4}$$

where, as before, α_i represents a permanent pair-effect and $\mathbf{x}_{it}$ represents a vector of year and month indicators. Although this specification suffers from the objection that the predicted probabilities can lie outside the unit interval, it is particularly convenient for handling fixed effects since (ignoring underreporting) it implies the linear regression equation

$$\Delta y_{it} = \Delta \mathbf{x}_{it}\beta + \xi_{it},$$

where $\Delta y_{it} = y_{it} - y_{it-1}$ represents the change in strike outcomes between the $(t-1)$st and tth negotiation, $\Delta \mathbf{x}_{it} = \mathbf{x}_{it} - \mathbf{x}_{it-1}$ represents the vector of differences in the covariates, and ξ_{it} can be interpreted as a residual. This first-differenced linear probability model can also be combined very easily with the underreporting model of equation (1).[21] The results of this combined model are presented in column 6 of table 2. To make the estimates comparable with the estimated coefficients of the logistic regression models, the month effects and their standard errors have been multiplied by a factor of $[\bar{p}(1-\bar{p})]^{-1}$, where $\bar{p}$ represents the sample average strike probability. Assuming $\bar{p} = .145$, the normalizing factor in column 6 is 8.0.

The estimated month effects from the linear probability model are generally similar to the estimates from the conditional logit model, although the point estimates for March, May, August, and December differ somewhat

[21] The combined linear-probability and underreporting model implies that the probability of an observed strike is

$$p_{it} = (1 - .2D_{it})(\alpha_i + \mathbf{x}_{it}\beta),$$

where D_{it} is an indicator for expirations prior to 1970. A suitable regression equation for observed strike outcomes is

$$\frac{y_{it}}{1 - .2D_{it}} - \frac{y_{it-1}}{1 - .2D_{it-1}} = \Delta \mathbf{x}_{it}\beta + \xi_{it},$$

where ξ_{it} is a residual with expected value equal to 0. Note that ξ_{it} is conditionally heteroscedastic. The standard errors for the differenced linear probability model are therefore estimated by the White (1980) procedure.

between the two specifications. A test that the month effects from the linear probability specification are constant from December to May and from June to November has a marginal significance level of .37 (virtually identical to the significance level of the test on the conditional logit coefficients). Assuming a 2-season model, the linear probability specification implies a 15-percentage point increase in the probability of strikes between June and November relative to expirations in December–May, with a standard error of .05.

Both the conditional logit and first-differenced linear probability specifications therefore indicate that strike probabilities are significantly higher in summer and fall relative to expirations between December and May. Since both specifications control for bargaining-pair specific heterogeneity, these results suggest that contract negotiators can vary the likelihood of subsequent work stoppages by varying the expiration dates of their contracts. There is, however no evidence that the duration of strikes varies by the expiration date of the previous contract.[22] These findings raise an interesting puzzle: why do negotiators schedule expirations in high strike probability months? The interpretation of strikes as unproductive accidents (Reder and Neumann 1980; Siebert and Addison 1981) implies that bargainers should schedule negotiations when the expected costs due to work stoppages are lowest. Assuming that marginal strike costs do not vary by season, the results in table 3 clearly reject this interpretation. More detailed evidence on the seasonal variation in strike costs is obviously required for a definitive test.[23]

B. Reopeners and Contract Duration

Table 4 presents estimates of the effects of two additional contract characteristics on strike probabilities: the length of time since the last negotiation and the provision for a limited contract reopening. As is the case for the expiration month, both of these characteristics are determined in the preceding contract negotiation. Time since the last negotiation is simply the duration of the preceding contract. A limited reopening is a provision of

[22] The mean and median duration for strikes associated with expirations between December and May are 45.3 and 28, respectively. The mean and median duration for strikes associated with expirations between June and November are 45.4 and 28, respectively.

[23] In an effort to check if seasonality in strike probabilities varies significantly by region, I fit separate first-differenced linear probability models of strike incidence to 61 bargaining pairs in southern and western states and 99 pairs in northern states (the remaining bargaining situations are multistate). Neither subsample rejected a 2-season model based on December–May and June–November. The estimated increase in the probability of strikes for expiration in June–November was 13 percentage points for the northern pairs, and 23 percentage points for the southern-western pairs, with standard errors of 8% and 9%, respectively.

Table 4
Effects of Reopener Status and Previous Contract Duration on Strike Incidence

	Ordinary Logit			
	No Industry or Year Effects	Industry and Year Effects	Conditional Logit with Year Effects	First-differenced Linear Probability with Year Effects
1. Reopener	−1.68	−1.70	−1.43	−.69
	(.73)	(.92)	(.76)	(.28)
2. Previous contract duration (months)	.052	.043	.043	.039
	(.009)	(.010)	(.013)	(.012)

NOTE.—Standard errors are in parentheses. Models with year effects include a four-step time function. See notes to table 3.

the previous contract that restricts negotiations at a future date to a specific set of contract issues, with the understanding that other aspects of the collective agreement are to remain unchanged.[24] Evidence that either of these characteristics affect strike probabilities again suggests that the bargaining parties can control the likelihood of subsequent disputes and raises the question of how the parties choose among the menu of contract alternatives.

The estimates in table 4 are obtained from models that exclude seasonal effects, although as a practical matter the estimates are not much different when seasonal effects are included since the distribution of reopening provisions and contract lengths is more or less independent of expiration month. The first row of the table presents the estimated coefficient of a dummy variable for a limited reopening.[25] The second row presents the estimated coefficient of the duration (in months) of the previous contract. In cases where new contract or a strike was reached prior to the expiration date of the preceding contract, the duration of the previous contract is defined as the period of time between its effective date and the date of the next contract or strike.

The first column of the table contains the estimated effects of these two variables with no heterogeneity controls. The estimates show that strike

[24] According to Bureau of National Affairs (1986, sec. 36.2), most reopening provisions are limited to wages, fringe benefits, and cost-of-living wage adjustments. The courts have ruled that a reopening agreement limited to "wages" does not preclude negotiation over "compensation" more broadly defined. See Meltzer (1977, pp. 712–18).

[25] I have counted as "reopenings" only those negotiations that are identified as reopening in the preceding contract and whose reopening date is specified in the preceding contract. By this definition, the data set contains 91 reopeners (6% of contract negotiations), mostly in textile, apparel, and chemical industries.

probabilities are significantly lower in reopening situations and significantly higher following the expiration of a longer contract.[26] The second column introduces two-digit industry controls and a step-function of time into the logistic regression. The estimated effect of a limited reopening is unchanged, while the effect of previous contract duration is reduced slightly. Finally, the two right-hand columns of table 4 present alternative fixed-effects estimators of the impact of reopening provisions and contract length on strike probabilities. The estimated contract length effects are similar to the estimates with industry-level controls, while the estimated effects of reopening provisions are smaller, particularly in the first-differenced linear probability specification. The point estimates imply that strike probabilities are 9%–11% lower in reopening situations and about 6% higher for negotiations following a 3-year, as compared to a 2-year, contract.[27]

These results suggest two conclusions. First, the commitment to limited negotiations implied by a reopener clause actually reduces the probability of disputes. Second, increases in contract length, while reducing the number of opportunities for disputes, lead to a higher probability of disputes in each negotiation. In fact, the estimates in table 4 suggest that the expected number of disputes per period of time is approximately constant, whether bargaining occurs at 1-, 2-, or 3-year intervals.[28]

The finding that expected strike losses per year are independent of contract length yields some support for the "accident" interpretation of strikes. On the margin, there is apparently no advantage to shortening or lengthening contract duration in order to avoid costly work stoppages. The findings that strike probabilities increase with contract duration and decrease in reopener situations are also roughly consistent with the notion that strike probabilities increase with the degree of uncertainty associated with

[26] The probabilities of strikes among reopenings is 2%. Strike probabilities by the duration of the preceding contract are as follows: less than 18 months—7.11%; 18–29 months—9.93%; 30–41 months—20.91%; and 42 months and longer—29.73%.

[27] I have also estimated the effect of previous contract length by grouping contracts into 1-year (less than 18 months), 2-year (18–29 months), 3-year (30–41 months), and longer (over 42 months) contracts. Relative to a 1-year contract, the estimated effects and associated standard errors from a differenced linear probability model are 2-year contract: −.01 (.03); 3-year contract: .06 (.03); 4-year or longer contract: .26 (.09).

[28] For example, the average probability of strikes among 2-year (i.e., 19–32 month) contracts is approximately 10%. The expected number of disputes per year on a 2-year bargaining cycle is therefore .05. The estimates in table 4 suggest that the probabilities of strikes in 1- and 3-year bargaining cycles are 4% and 16%, respectively. These probabilities imply .04 and .053 expected disputes per year, bargaining annually and triennially. Mean strike duration following a 1-year contract is slightly shorter than mean strike duration following 2- or 3-year contracts: 32.5 vs. 45.6 and 46.8 days, respectively.

the bargainers' information about each other. Complex multiple-issue negotiations, on the one hand, require detailed information on the parties' trade-offs between alternative forms of compensation. Single-issue negotiations over wages, on the other hand, are closer to a zero-sum bargaining game. This characterization suggests that disputes arising out of imperfect information are less likely to occur in typical reopening situations. By the same token, it may be reasonable to assume that the parties' information about each other decays with the length of time since their most recent contract negotiations. The positive relation between contract length and strike probabilities is therefore consistent with increasing uncertainty associated with less frequent negotiations.

IV. The Longitudinal Structure of Strike Activity

A. Models of Strike Incidence

This section presents and analyzes the pattern of strike incidence over time within bargaining pairs. Using information on six contract negotiations for each bargaining pair, I first test the hypothesis that strike probabilities are related to previous strike incidence. Contrary to the findings of Mauro (1982) and Schnell and Gramm (1987), there is no evidence of either positive or negative state dependence in strike incidence, controlling for heterogeneity in underlying strike propensities. The absence of state dependence in strike incidence, however, masks an important dependence of strike probabilities on the duration of lagged strike outcomes. Specifically, I find that strike probabilities are significantly increased by a relatively short strike in the preceding negotiation and significantly reduced by a relatively long strike in the preceding negotiation.

Some simple evidence on the extent of state dependence in strike incidence is presented in table 5. This table presents a frequency distribution of the alternative strike histories represented in the sample of six negotiations for each of the 253 bargaining pairs, together with the predicted frequencies generated by two simple models of strike incidence. The first column of the table describes the relevant strike history: the strike history "000000," for example, represents the occurrence of no strikes in six negotiations. The next column gives the number of bargaining pairs with each history. For simplicity, I have not displayed the actual distributions of bargaining pairs among histories with three or four strikes in six negotiations. Since strikes are relatively rare events, the number of pairs in the individual cells with more than three strikes is typically one or zero. There are no pairs with five strikes in six negotiations, and only one pair (the Autoworkers and Allis Chalmers) with six strikes.

The third and fourth columns of table 5 present the predicted numbers of bargaining pairs with each strike history (or group of histories) from two alternative models: an ordinary logistic regression model of strike

Table 5
Actual and Predicted Strike Histories: Last Six Contracts for Each Pair

Strike History	Actual No. of Pairs	Predicted No. (absolute t-statistics)*: Ordinary Logit with Industry Effects	Predicted No. (absolute t-statistics)*: Conditional Logit
No strikes:			
1. 000000	118	105.8 (3.07)	118 (. . .)
One strike:			
2. 100000	12	10.7 (.43)	8.1 (1.49)
3. 010000	8	13.3 (1.56)	9.9 (.66)
4. 001000	17	17.3 (.09)	15.6 (.42)
5. 000100	9	13.9 (1.41)	10.8 (.59)
6. 000010	9	12.0 (.95)	10.20 (.41)
7. 000001	11	13.4 (.81)	11.49 (.41)
8. Total of 6 cases	66	80.7 (2.20)	66.0 (. . .)
Two strikes:			
9. 110000	1	2.17 (.82)	1.6 (.49)
10. 011000	5	3.6 (.80)	3.1 (1.11)
11. 001100	3	4.2 (.61)	4.3 (.66)
12. 000110	2	2.9 (.53)	2.6 (.39)
13. 000011	8	2.7 (3.34)	2.4 (3.69)
14. 101000	2	2.8 (.48)	2.2 (.11)
15. 010100	1	2.8 (1.08)	2.3 (.88)
16. 001010	2	3.5 (.84)	3.4 (.81)
17. 000101	2	3.2 (.71)	3.0 (.60)
18. 100100	3	2.2 (.52)	1.7 (1.04)
19. 010010	1	2.5 (.95)	2.0 (.75)
20. 001001	4	4.0 (.02)	3.9 (.03)
21. 100010	0	2.0 (1.43)	1.5 (1.23)
22. 010001	2	2.8 (.47)	2.3 (.21)
23. 100001	2	2.2 (.15)	1.6 (.28)
24. Total of 15 cases	38	43.4 (1.06)	38 (. . .)
Three strikes:			
25. Total of 20 cases	24	17.1 (2.06)	24 (. . .)
Four strikes:			
26. Total of 15 cases	6	5.0 (.49)	6 (. . .)
Five strikes:			
27. Total of 6 cases	0	.9 (1.05)	0 (. . .)
Six strikes:			
28. 111111	1	.1 (3.21)	1 (. . .)
29. Goodness of fit for table uncorrected for parameter estimation (probability value)†		74.47 (.15)	49.35 (.75)

NOTE.—See Section IV A of text for explanation of strike histories. Absolute t-statistics are in parentheses.
* Predicted number represents the expected number of pairs with a given history, conditional on the estimated parameters. The number in parentheses represents the absolute t-statistic associated with the test that the predicted and actual number of pairs are equal. t-Statistics are not corrected for parameter estimation.
† Goodness-of-fit statistic for the overall table (64 elements) treating the estimated parameters as known constants.

incidence with industry and year effects and a conditional logit model with year effects.[29] Both of these models are estimated under the assumption of no state dependence in strike incidence. A comparison of the predicted frequencies under these simple models to the actual frequencies of the alternative strike histories therefore provides a simple test for the presence of state dependence.

A comparison of the actual cell frequencies to the predicted frequencies generated by the ordinary logit model with year and industry effects suggests that the model does a relatively good job of predicting the various alternatives. The absolute t-statistics for the difference between the predicted and actual cell frequencies are presented in parentheses beside each predicted cell frequency, and an overall goodness-of-fit statistic is presented in the last row of the table.[30] Apart from the cells with no strikes and six strikes, and the "000011" cell, the individual t-statistics are relatively small. There is very little evidence that the model systematically over- or underpredicts cells associated with significant state-dependence effects. For example, negative state dependence (as reported by Mauro [1982] and Schnell and Gramm [1987]) would show up in this table as significant overprediction of cells with two consecutive strikes. The results in rows 9–13, by comparison, show a tendency to underpredict these cells, at least among histories with two strikes in six negotiations.

A similar conclusion emerges from an examination of the goodness of fit of the conditional logit model. By construction, the conditional logit model fits the number of observations with each strike total exactly. Looking at the individual strike histories, the only significant outlier under the conditional logit specification is the "000011" history; otherwise, the goodness of fit to the individual cells and the overall table are acceptable by conventional standards.[31] Again, the fit of the model does not indicate any significant state dependence in strike incidence.

A close inspection of the individual strike histories, however, suggests that the absence of state dependence in strike incidence is the product of two offsetting effects: a tendency for increased strike probabilities following

[29] The models incorporate the corrections for underreporting of strikes prior to 1970 discussed in the preceding section.

[30] The t-statistics and the overall goodness-of-fit statistic are not corrected for the estimation of the industry and year effects in the logit model. A suitable correction is suggested by Heckman (1984) (see also D. Moore [1977]). The impact of this correction on the individual t-statistics in column 3 of table 5 is trivial. The overall goodness-of-fit statistic, however, increases to 83.55 (with a probability value of .04) when this correction is applied. The derivation of the corrected and uncorrected goodness-of-fit statistics is described in an earlier version of this article (Card 1986).

[31] The eight bargaining pairs with the "000011" strike history are apparently unrelated (i.e., are drawn from different industries and different time periods).

Table 6
Probability of Strikes Conditional on Preceding Strike Outcome

	Previous Strike Outcome					
	No Strike	1–7 Day Strike	8–14 Day Strike	15–28 Day Strike	29–42 Day Strike	43 Day or Longer Strike
1. Probability of strike	13.9	40.0	47.5	24.4	18.5	19.0
2. Mean strike duration	47.3	41.2	37.0	47.5	33.4	39.8
3. Number of contracts	1,291	35	40	41	27	84
(% of sample)	(85.05)	(2.31)	(2.64)	(2.70)	(1.78)	(5.53)

NOTE.—Sample cross-tabulation of strike outcomes by previous strike outcome.

relatively short disputes and a tendency for reduced strike probabilities following relatively long disputes. Some simple evidence is presented in table 6, which describes the probability and duration of strikes conditional on the length of the preceding strike. The first row of the table reveals a sharp difference between the effects of shorter strikes and longer strikes on the probability of subsequent disputes. In contrast to this finding, a simple model of state dependence implies that the probability of a subsequent dispute depends only on the occurrence of a strike and not on its length. Despite the variation in strike probabilities by lagged strike length, the mean strike durations in the second row of table 6 show no significant variation by lagged strike length.

In order to investigate the apparent dependence of strike probabilities on lagged strike outcomes, a statistical model is required that permits both unobserved heterogeneity in strike propensities and potential state dependence in consecutive strike outcomes. A convenient model is a random-effects logit specification

$$\mathrm{logit}(p_{it}^{*}) = \alpha_i + \mathbf{x}_{it}\beta + \sum_k \delta_k y_{it-1}^{k}, \tag{5}$$

where p_{it}^{*} represents the probability of a strike at the tth negotiation for the ith bargaining pair in the absence of underreporting, $\mathbf{x}_{it}$ represents a vector of time effects, y_{it-1}^{k} is an indicator for a strike in the kth duration class in the preceding negotiation, δ_k represents a vector of state dependence effects, and α_i represents a randomly distributed individual effect. A simple model for the distribution of the random effects is a point-mass distribution with a small number of mass points.[32] The mass points and associated probabilities, together with the parameters $\{\beta, \delta_1, \delta_2, \ldots\}$ can be estimated

[32] A similar model was proposed by Card and Sullivan (1987) as a description of individual employment probabilities.

jointly by conventional maximum likelihood techniques, treating the presample strike outcomes as fixed.[33]

Estimation results for this random-effects specification are reported in table 7.[34] For reference, the first column of the table presents the results of the model with no allowance for lagged strike effects. The goodness-of-fit statistic reported in the last row of the table indicates that the random-effects model with a simple two-point distribution of effects generates a relatively good fit to the table of strike incidence histories. Relative to a conventional logit model with two-digit industry effects, for example, the model has 14 fewer parameters but generates a slightly better fit to the table of strike histories. The addition of an extra mass point to the distribution of random effects yields an insignificant improvement in the log-likelihood (from −637.2 to −636.3) and only a slight improvement in the goodness of fit to the table of strike histories.

The second column of the table adds a single parameter for the change in the log-odds of a strike following a strike in the previous negotiation. This specification corresponds to a conventional model of state dependence. As the results in table 5 suggest, there is no strong evidence of state dependence. The point estimate of the lagged strike effect is actually positive, consistent with the finding in table 5 that consecutive strike outcomes are slightly underpredicted by models with no allowance for state dependence.

The models in columns 3–6 allow for differential effects of lagged strikes on future strike probabilities, depending on the duration of the earlier dispute. As suggested by table 6, a simple two-outcome model that distinguishes between 1–14-day strikes, on one hand, and 15-day or longer strikes, on the other, is adequate to describe the data. Whereas strikes of less than two weeks duration (some 30% of all strikes) significantly increase the

[33] Ignoring underreporting, the probability that the ith pair has a sequence of strike indicators $(y_1, y_2, \ldots, y_6)$, conditional on the strike outcome in the presample negotiation, is

$$\sum_{k=1}^{J} \{\prod_t p_{it}^*(\alpha_k)^{y_t}[1 - p_{it}^*(\alpha_k)]^{1-y_t}\}\phi_k,$$

where $p_{it}^*(\alpha)$ is the probability of a dispute conditional on the individual effect, $\alpha_1, \alpha_2, \ldots, \alpha_J$ are the mass points of the distribution function of α_i, and ϕ_k are the associated probability weights. In principle, conditioning the estimation on the distribution of the presample strike outcomes introduces a bias in the estimation of the parameters—see Heckman (1981). In other work using strike outcomes for Canadian contracts (Card 1987), however, I have experimented with alternative methods of handling the initial conditions and found relatively small differences between them.

[34] The likelihood of observed strike outcomes is corrected for 20% random underreporting of strikes prior to 1970. No correction is made for the effect of underreporting on the distribution of the lagged strike outcome indicators.

Table 7
Estimates of the Effect of Preceding Strike Activity on Strike Probabilities: Random Effects Logit Model*

		Lagged Strike Effect				
Duration of Previous Strike	No Lagged Strike Effect (1)	One Duration Class (2)	Two Duration Classes (3)	Three Duration Classes (4)	Three Duration Classes (5)	Two Duration Classes† (6)
1. 0–7 day strike	.0	.13	1.02	.84	1.02	.94
	(. . .)	(.23)	(.33)	(.47)	(.33)	(.33)
2. 8–14 day strike	.0	.13	1.02	1.18	1.02	.94
	(. . .)	(. . .)	(. . .)	(.46)	(. . .)	(. . .)
3. 15–60 day strike	.0	.13	−.40	−.41	−.47	−.82
	(. . .)	(. . .)	(.28)	(.29)	(.33)	(.39)
4. 61 day or longer strike	.0	.13	−.40	−.41	−.28	.82
	(. . .)	(. . .)	(. . .)	(. . .)	(.42)	(. . .)
5. Log likelihood	−637.21	−637.06	−629.79	−629.65	−629.71	−598.26
6. Goodness of fit for strike outcome table (probability value)‡	60.07	62.77	59.76	58.31	59.28	56.93
	(.58)	(.48)	(.59)	(.64)	(.61)	(.69)

NOTE.—Standard errors are in parentheses.
* All models include a four-step time function and a two-point distribution of bargaining-pair effects.
† Model includes four grouped industry effects.
‡ Goodness of fit to 64-element table of strike incidence outcomes. The statistic does not account for estimation of the parameters in the model.

probability of a subsequent dispute, longer strikes actually reduce the probability of a subsequent dispute. As a check that these results are not biased by imperfect heterogeneity controls, the model in column 6 of table 7 introduces a set of industry effects. In order to reduce the number of parameters in the model, two-digit industries were grouped according to their estimated effects in a conventional logit model of strike incidence.[35] The results with the grouped industry variables further strengthen the conclusion that lagged strike outcomes affect current strike probabilities, with shorter strikes increasing the likelihood of future disputes and longer strikes reducing the likelihood of future disputes.

B. Models of Strike Incidence and Duration

In view of the impact of strike duration on subsequent strike outcomes, this section presents a more detailed analysis of the duration and incidence of strikes over time within bargaining pairs. The analysis is based on a model that describes strike outcomes in terms of only three possibilities: no strike; a strike for less than 2 weeks; and a strike for longer than 2 weeks. While this simple three-outcome model of strike activity is obviously incomplete, the results of the last section suggest that the distinction between short and long strikes is very useful in longitudinal models of strike incidence. The three-outcome case is therefore a natural starting point for studying the joint determination of strike incidence and duration over time.

In order to describe the distribution of bargaining outcomes between no strikes, short strikes, and long strikes, consider a pair of indicator variables y_{it} and z_{it}, where $y_{it} = 1$ if a strike occurs in the tth negotiation for the ith bargaining pair, and 0 otherwise, and $z_{it} = 1$ if the strike lasts for longer than two weeks, and 0 otherwise. Let p_{it}^* and q_{it}^* represent the probability of a strike and the conditional probability of a long strike, respectively, ignoring underreporting of strikes. Suppose that

$$\text{logit}(p_{it}^*) = \alpha_i + \mathbf{x}_{it}\beta + \delta_1(y_{it-1} - z_{it-1}) + \delta_2 z_{it-1}, \tag{6a}$$

$$\text{logit}(q_{it}^*) = \gamma_i + \mathbf{x}_{it}\theta + \rho_1(y_{it-1} - z_{it-1}) + \rho_2 z_{it-1}, \tag{6b}$$

where α_i and γ_i are individual effects, $\mathbf{x}_{it}$ is a vector of control variates (for example, year and/or industry effects), and β and θ are conformable vectors of parameters. Finally, assume that y_{it} and z_{it} are independent, conditional

[35] The four groups are a low strike-probability group (apparel, lumber and wood), a moderate strike-probability group (primary metals, fabricated metals, electrical machinery, transportation equipment), a high strike-probability group (rubber, nonelectrical machinery), and a base group (all other industries).

on α_i, γ_i, x_{it}, y_{it-1}, and z_{it-1}.[36] The parameters γ_1 and γ_2 measure the effect of a short or long strike in negotiation $t - 1$ on the probability of a strike in negotiation t.[37] The parameters ρ_1 and ρ_2 measure similar effects on the probability of a strike continuing for more than two weeks.

Given values for α_i and γ_i, and presample strike outcomes, equations (6a) and (6b) can be used to calculate the likelihood of an observed sequence of strike outcomes over time. The two equations describe a first-order Markov model of transitions between three alternative states: no strikes; strikes of less than 14 days; and strikes of more than 14 days. Following the approach in the previous section, I treat the joint distribution of α_i and γ_i as a discrete distribution with a small number (two or three) of mass points. I also condition the estimation and inference on the observed strike outcomes for each bargaining pair in the immediate presample period.

Table 8 presents estimation results for the trinomial outcome model of strikes implied by equations (6a) and (6b). The first column of the table presents estimates based on a two mass-point bivariate distribution of individual effects. Apart from individual effects, the only covariates of strike activity are a step-function of time. For simplicity, the time effects in equations (6a) (the incidence equation) and (6b) (the conditional duration equation) are restricted to be proportional: $\theta = \xi\beta$. The estimated proportionality constant ξ is reported in the fifth row of the table.

The estimated effects of previous work stoppages on the probability of a strike are presented in the first two rows of the table. The point estimates are very similar to the estimates in table 7, with about the same level of precision. The estimated effects of short and long strikes in the preceding contract on the conditional probability of a long strike in the current negotiation are reported in the third and fourth rows of table 8. The point estimates suggest that a long strike is more likely if there was a long strike in the previous contract and less likely if the previous contract was settled peacefully, although the estimates are relatively imprecise.

The second column of the table presents estimation results for a three mass-point model of the distribution of individual effects. Overall, the results are very similar to the two mass-point specification, and the likelihood of the sample is not significantly improved.

The hypothesis that lagged strikes do not affect the probability of short or long strikes is addressed in the third column of the table. Comparing the likelihood and parameter estimates to those in the second column, there is very little evidence against the hypothesis. While lagged strike

[36] The assumption of independence between y_{it} and z_{it} is not particularly restrictive if the distribution of the individual effects α_i and γ_i is flexible.

[37] In the event of a short strike in $(t - 1)$, $y_{it-1} = 1$ and $z_{it-1} = 0$, so logit(p_{it}^*) is increased by δ_1. In the event of a long strike in $t - 1$, y_{it-1} and $z_{it-1} = 1$, so logit(p_{it}^*) is increased by δ_2.

Table 8
Trinomial Outcome Model of Strike Probability and Duration

	Random Effects Logit Model*			
	2 Mass Points (1)	3 Mass Points (2)	3 Mass Points (3)	2 Mass Points and 4 Industry Effects†
Effect of previous strike on strike probability:				
1. 0–14-day strike	1.08	1.04	1.11	1.01
	(.36)	(.34)	(.35)	(.35)
2. 15-day or longer strike	−.44	−.51	−.49	−.69
	(.29)	(.29)	(.29)	(.30)
Effect of previous strike on probability of long strike:				
3. 0–14-day strike	.13	.17	.00	.00
	(.46)	(.48)	(. . .)	(. . .)
4. 15-day or longer strike	.44	.37	.00	.00
	(.58)	(.57)	(. . .)	(. . .)
5. Relative time-effect on probability of long strike	1.49	1.53	1.60	1.45
	(.58)	(.57)	(.61)	(.53)
6. Log likelihood	−771.34	−771.03	−771.34	−749.92
7. Goodness of fit for strike outcome table (probability value)‡	56.17	55.68	55.21	55.85
	(.72)	(.73)	(.75)	(.73)

NOTE.—Standard errors are in parentheses.
* All models include a four-step time function normalized to have a unit effect on the probability of a strike. The coefficient in row 5 gives the relative effect of this time function on the probability of a strike longer than 14 days.
† Model includes four grouped industry effects in probability of strike and probability of long strike equations. The industry effects are restricted to enter proportionately in the two equations. The estimated relative effect of the industry variables in the duration equation is .17 (with a standard error of .18).
‡ Goodness of fit to 64-element table of strike incidence outcomes. The statistic does not account for estimation of the parameters in the model.

activity apparently influences the probability of subsequent strikes, the probability of continuing a strike beyond 2 weeks is not much affected by previous strike outcomes. Some caution is nevertheless required in interpreting these results since the number of strikes in the data set is small and strike duration is apparently a noisy phenomenon.[38] A larger data set is probably required to fully analyze the determinants of strike duration, and particularly the effects of lagged duration on current strike durations.[39]

[38] For example, the R^2 in a regression equation for log strike duration that includes two-digit industry effects and time variables is about 20%. The industry effects in such an equation are jointly significant at only about the 10% level.
[39] The number of consecutive strikes in the data set is relatively low: 58 (distributed among 42 bargaining pairs).

The fourth column of the table presents estimates for a two mass-point specification that includes grouped industry effects as well as time covariates in equations (6a) and (6b). Here I have restricted the time effects and industry effects to be proportional in the two equations, but I have allowed different factors of proportionality for the two types of effects. While the time effects are significant in the duration equation, the industry effects, which play a strong role in the strike probability equation, have very little effect on duration.[40] As in table 7, the addition of grouped industry effects increases the absolute value of the estimated effect of a longer strike in the preceding negotiation. Overall, the estimates for the incidence equation imply that strike probabilities increase 10–12 percentage points following a relatively short strike and decrease 6–9 percentage points following a longer work stoppage. The estimates for the duration equation, while relatively imprecise, indicate that previous strike outcomes do not affect the relative likelihood of short or long disputes.[41]

These findings are consistent with a very simple dynamic model in which the probability of strikes depends positively on a state variable whose value is unaffected by short strikes but is significantly reduced by the occurrence of a long dispute.[42] In such a model, the occurrence of a short strike signals a high level of the state variable and an increased probability of further strikes. The occurrence of a long strike, in contrast, reduces the probability of further strikes by reducing the level of the state variable. There is a variety of interpretations of the state variable in such a model: as an index of union members' wealth, for example; or, alternatively, as a general index of worker discontent. In the absence of more complete information, however, it is impossible to distinguish between these various interpretations.

The pattern of state dependence in strike probabilities also bears an

[40] This is consistent with results from a simple cross-sectional logit model of short and long strike durations. In such a model, none of the industry effects is individually significant, and the probability value of the test for the hypothesis that the industry effects are jointly equal to zero is .12.

[41] Some additional evidence on the weak relation between lagged strike outcomes and strike durations is provided by the pattern of average strike durations by the number of strikes in six negotiations. These average durations are 52 days for one strike; 40 days for two strikes; 45 days for three strikes; 56 days for four strikes; and 16 days for six strikes.

[42] Suppose, e.g., that the occurrence of a strike is governed by a latent variable $y_t = \gamma w_t + u_t$, where w_t represents the state variable and u_t represents an error term. Suppose further that w_t follows a first-order process $w_t = w_{t-1} - c_{t-1} + v_t$, where c_t measures the effect of a dispute on the state variable and v_t is another error term. Finally, suppose that costs are measured by strike length and that strike duration is a random variable equal to 1 with probability q and k $(0 < k < 1)$ with probability $1 - q$. Under suitable assumptions, this model generates a steady-state transition matrix in which lagged short strikes increase the probability of future strikes and lagged long strikes reduce the probability of future strikes.

interesting relation to the literature on judging winners and losers of strikes.[43] Studies from a number of different countries and time periods have concluded that unions are more likely to win short strikes than long ones.[44] If the impression of union victory is related to the union's willingness to engage in subsequent work stoppages, then this pattern of wins and losses by strike duration is consistent with the pattern of state dependence observed in tables 7 and 8.

V. Conclusions

This article has presented evidence on two aspects of strike activity associated with the renegotiation of union contracts: the effects of endogenously determined contract characteristics on the probability of disputes and the effects of lagged strike outcomes on future strike incidence and duration. Although the evidence is based on a relatively small sample of bargaining pairs drawn mainly from the manufacturing sector, the findings suggest a number of conclusions and avenues for further research.

First, strike probabilities are significantly affected by contract characteristics determined in earlier negotiations. Strikes are more likely following a longer contract than a shorter one and are less likely in limited reopening situations. Strike probabilities are also affected by the expiration month of the preceding contract, with expirations in summer and fall leading to an increased likelihood of disputes. The effects of contract duration and reopening provisions are potentially consistent with the hypothesis that strike probabilities increase with increases in the bargainers' uncertainty about each other. Uncertainty may be expected to increase with the length of time between negotiations and decrease when bargaining is restricted to a smaller number of issues. The effect of the monthly timing of negotiations is less easily explained. Judging by the duration of disputes, marginal strike costs are not much different in summer and fall than in winter and spring. A simple model of negotiator behavior based on the hypothesis that the parties try to minimize expected losses due to work stoppages cannot readily explain the predominance of scheduled expirations in high strike probability months.

Second, strike probabilities are significantly affected by preceding strike outcomes. Relative to strike probabilities after a peaceful settlement of the most recent contract negotiation, strike probabilities are 10 percentage points higher if the contract was settled after 1–14 day strike and 5–7 percentage points lower if the contract was settled after a longer work

[43] This literature is reviewed by Kennan (1986, pp. 1113–14).

[44] For example, H. Moore (1911, p. 119) presents contingency tables of the union success rate against strike duration for disputes in Germany (from 1899 to 1905) and France (from 1890 to 1905). Both tables show declining union success rates with the length of the strike.

stoppage. The effects of previous strike outcomes on subsequent strike durations are less precisely estimated and are not significantly different between negotiations that ended peacefully and those that ended in shorter or longer strikes.

This pattern of state dependence is not consistent with the simple model of learning proposed by Schnell and Gramm (1987).[45] Rather, it suggests that any dynamic model of strike propensities must carefully distinguish between the effects of relatively short strikes, on one hand, and longer strikes, on the other. It also suggests that one should distinguish between disputes of various lengths in investigating the effects of strikes on other aspects of the collective bargaining agreement, including wage outcomes.

Appendix

Table A1
Strike Characteristics by Year: Sample of Six Contracts for Each Pair

		Strike Probability (%)			
Year	No. of Contracts	Unadjusted	Adjusted for Industry Composition*	Average Strike Duration Days	Strikes Longer than 2 Weeks (%)
1960 and earlier	10	30.0	23.2	52.7	100.0
1961	26	15.4	6.8	11.0	25.0
1962	42	7.1	2.6	21.0	33.3
1963	40	2.5	.9	35.0	100.0
1964	71	11.3	7.6	20.1	50.0
1965	86	18.6	17.0	20.5	43.8
1966	65	13.9	15.9	23.0	55.5
1967	92	26.1	24.5	79.3	75.0
1968	92	25.0	23.7	47.0	86.9
1969	79	17.7	19.3	38.4	78.7
1970	92	23.9	23.6	64.1	86.4
1971	111	12.6	11.9	38.9	71.4
1972	68	11.8	14.9	23.6	25.0
1973	104	13.5	14.0	26.6	57.1
1974	135	18.5	19.3	31.7	64.0
1975	81	8.6	11.9	68.4	57.1
1976	95	17.9	18.6	52.7	76.5
1977	126	15.1	15.3	63.2	100.0
1978	66	16.7	19.8	50.0	72.0
1979	37	5.4	8.3	57.5	50.0

* Estimated year effects from linear probability model that also includes two-digit industry effects.

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[45] I have also found a similar pattern of state dependence among strike outcomes for a sample of over 2,200 collective bargaining agreements from Canada (Card 1987).

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