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Abstract
The authors use a unique longitudinal data set from Ontario, covering the years 1984–92, to estimate the determinants of strike incidence and duration. Unlike most empirical analyses of strikes, the data set for this study contains both small and large bargaining units. The authors find strong evidence that the likelihood of a future strike was lower among bargaining units that had struck before than among those that had not (the "teetotaler" effect); the longer a strike lasted, the greater was the probability of settling (positive duration dependence); and smaller bargaining units were less likely to strike than were larger bargaining units, but had longer strikes when they did strike.

Keywords
strike incidence, strike duration, Ontario, teetotaler effect, positive duration dependence, bargaining unit size and strikes

Cover Page Footnote
The authors thank George Jakubson of Cornell for his valuable contribution to an earlier version of this paper.

STRIKE INCIDENCE AND STRIKE DURATION: SOME NEW EVIDENCE FROM ONTARIO

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The authors use a unique longitudinal data set from Ontario, covering the years 1984–92, to estimate the determinants of strike incidence and duration. Unlike most empirical analyses of strikes, the data set for this study contains both small and large bargaining units. The authors find strong evidence that the likelihood of a future strike was lower among bargaining units that had struck before than among those that had not (the "teetotaler" effect); the longer a strike lasted, the greater was the probability of settling (positive duration dependence); and smaller bargaining units were less likely to strike than were larger bargaining units, but had longer strikes when they did strike.

During the past several decades, a large literature studying the incidence and duration of strikes has evolved. The bulk of this literature has been concerned with estimating and testing economic models of strikes. However, behavioral models of industrial conflict that incorporate sociological, psychological, and political considerations surrounding negotiations have also appeared (for example, Godard 1992). Estimation of these models requires information about the bargaining units that is not available in most micro data studies of strike incidence or duration.

Most of the existing literature for Canada and the United States is primarily focused on the incidence and duration of strikes in large bargaining units—in the United States, usually units with 1,000 or more workers; in Canada, usually units with 500 or more workers, but sometimes those with 200 or more workers. There is mounting evidence, however, that these large unit samples are unrepresentative of all strikes (Skeels, McGrath, and Arshanapalli 1988; Harrison and Stewart 1993). Not surprisingly, when small units are included in an empirical analysis, the estimates from strike incidence and duration models often show diverging patterns. For example, studies that include small strikes reveal some evidence of opposite incidence and duration effects for such factors as the business cycle and bargaining unit size (Gunderson and Melino 1990; Harrison and Stewart 1993).

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The data, code books, and computer programs used for this paper are available from the authors upon request. Contact the first author at Centre for Industrial Relations, University of Toronto, 121 St. George Street, Toronto, Ontario, Canada M5S 1A1; Campolie@chass.utoronto.ca. Stata 8.0 was used to produce the empirical results.

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More specifically, strike incidence is shown to be procyclical but duration has been found to be either counter-cyclical or not related to the business cycle (Kennan 1985; Harrison and Stewart 1989). This suggests that in order for a sample of strikes to be representative, small bargaining units must be included.

In this paper, we take advantage of a unique micro data set of settlements in all bargaining units in Ontario over the period 1984–92, which we combined with information on work stoppages, to examine the correlates of both the incidence and duration of strikes. One of the principal advantages of this data set is that it contains a diverse mix of bargaining units of different sizes, which means that we are not primarily focused on large bargaining units. The explanatory variables in our analysis are similar to those that are frequently used in microeconomic studies. However, our data set also contains some bargaining unit characteristics that were not available to other studies. Thus, we can control for some of the factors that are stressed in behavioral models of strike activity. Finally, some of our empirical strategies for the analysis of strike incidence and duration also take advantage of the longitudinal nature of these data, which permits us to make statements about the degree of state dependence and duration dependence in our strike outcomes.

Literature Review

The three most common models of strikes used in microeconomic studies are those based on adjusting expectations, joint costs, and asymmetric information (Card 1990). The adjusting expectations model of Ashenfelter and Johnson (1969) sees both strike frequency and strike duration as functions of the discounted present value of profits and the wage expectations of the union's membership. This model is a political model of union behavior in which union leaders may operate to protect their leadership position at the expense of the rank and file. Ashenfelter and Johnson's model also focuses on union members' lack of information. The joint cost hypothesis states that the probability of a strike will be lower, and its expected duration shorter, the greater are the joint costs of striking to the firm and its employees (see the summary in Card 1990). The asymmetric information models of strikes are commonly based on the union's lack of information about the firm's ability to pay. In particular, these models usually assume that the firm's profitability is unobservable to union members, who can use strikes as a mechanism to extract higher wages from more profitable employers.

By restricting the focus to the economic well-being of the players, economic models ignore other behavioral factors (Godard 1992). Models within the behavioral tradition, in contrast, view strikes as sometimes involving the mobilization of workers by appeals to their conceptions of fairness and legitimacy. For example, issues such as job security and working conditions may substantially affect both the incidence and length of strikes. Within the behavioral tradition, strikes may also be attributed to problems of suspicion and hostility arising primarily out of managerial policies and practices.

The political-economy tradition proceeds from the assumption that workers have grievances but engage in strike actions only when they have the resources to make such action worthwhile or when they lack direct access to political power (Godard 1992). Since workers are unable to alter their subordinate position vis-à-vis their employer by collective bargaining alone, a strike serves as a primary means by which they can compel change.

In a collective voice theory of strikes that integrates the economic, behavioral, and political economy interpretations, Godard (1992) generated several hypotheses. These strike hypotheses link increases in both the incidence and duration of strikes to worker discontent and solidarity, management strategies of cost containment and accommodation, perceived strike effectiveness, the militancy of negotiators, and uncertainty and imperfect information. Although these factors are hypothesized to affect strike frequency and duration in the same manner and direction, the explanation in the case of duration is not always clear. For example, the same "accommodative" management strategy that may reduce the probability of a strike may also increase the length of a strike if the ultimate purpose of the strategy is to weaken or even replace the union.

The empirical evidence suggests a number of key findings. First, the incidence and duration of strikes both vary markedly across industries. Second, some economic factors, such as higher unemployment rates and real wage gains during the previous agreement, reduce the probability of a strike occurring. However, the empirical evidence suggests that the relationship between economic factors and strike activity is weaker in Canada than in the United States or the United Kingdom (Gunderson, Hyatt, and Ponak 2001). Third, the duration of strikes tends to vary with the business cycle, but the direction of the effect is not clear; some studies have found a positive relationship and others a negative one (Card 1990). Relationships between seasonality of contract expiration and the length of the previous contract and strike activity have also been found in the empirical literature (Card 1990).

Data

Two data sets have been merged in this study. The first contains 31,076 settlements in Ontario between 1984 and 1992 from the Ontario Ministry of Labour. These data are for all bargaining units, ranging in size from 1 employee to over 36,000 employees. In fact, 76% of these settlements were in small bargaining units (those with under 200 members) covering 27.3% of employees. Data are also taken from the (Federal) Labour Canada Work Stoppage data set, which includes information on length of strike, strike issue, and strike outcome. It was not possible to mesh these two sets of data perfectly, but 1,398 of 1,445 (96.7%) strikes in Ontario from 1984 to 1992 were successfully merged. One strike was dropped due to missing data; the 47 strikes that could not be found were also dropped; and missing data on a number of the other explanatory variables required us to exclude another 34 observations from our sample. Once these unusable observations are dropped, merging the two data sets yields 1,363 strikes, of which 1,055 (77.4%) are in small units (under 200 employees). Because all of these strikes were settled during the period 1984–92, they all have a finite strike length in days, that is, none of the strikes are censored.

Most of the existing strike literature has focused on fairly large bargaining units. One of the benefits of our data is that we have information on bargaining units of different sizes. To control for the effects of bargaining unit size on both the incidence and duration of strikes, we create bargaining unit size dummy variables for the following intervals: 1–20 (under 21), 21–49, 50–99, 100–149, 150–199, 200–299, 300–499, and the omitted category of 500 or more employees.

We include the lost time injury rate for the firm as a measure of risk. The joint cost model suggests that risk-averse workers who would sort themselves into jobs with lower accident rates would also be averse to the risk associated with striking (Gunderson, Kervin, and Reid 1986). From the perspective of the joint cost model, their risk aversion is a component of the joint costs of using the strike as opposed to other mechanisms for solving differences. We use the inverse of the unemployment rate for prime age men to control for tightness in the labor market as well as business cycle effects. We also include the change in the number of employees between the previous contract and the current contract (change in employment) to provide a firm-specific measure of changing circumstances, which may also capture some business cycle effects. The joint cost hypothesis predicts a relationship between labor disputes and the state of firm- or industry-specific demand. We also include real wage changes in our empirical models as an additional control for business cycle effects. These variables are controls for economic factors, which have been found to influence both the incidence and duration of strikes in the empirical literature. Finally, we account for some special legislation that applies to some bargaining units (teachers and construction workers) with dummy variables.

Previous empirical analyses of longitudinal strike data have also suggested some other factors that could influence both the propensity of a bargaining unit to strike and the duration of the strike. Card (1988) found two empirical regularities of particular interest in his U.S. contract data. First, he found that the longer the previous agreement, the greater the probability that the bargaining unit would strike. Second, he found strong evidence of seasonal effects, as defined by the month of contract expiration. To account for these factors, we include two dummy variables (for 2-year contracts and contracts of more than 2 years, with 1-year contracts as the excluded reference group) to control for the length of the previous agreement, and a full set of month dummies. We also include dummy variables for year effects, which control for unobserved factors that may have a year-specific component, as well as industry dummies, which capture unobserved factors that might be industry-specific.

In addition to these variables, which are fairly standard in the economic literature, we also take account of some of the information on bargaining unit characteristics that is available in our data to control for a few of the factors that have been hypothesized in behavioral models of strike activity. These variables are intended to be our proxies for these effects.

Godard's behavioral model of strikes (Godard 1992) suggests that solidarity in a workplace can affect the bargaining unit's propensity to strike as well as the duration of the strike. We include a number of variables to control for these effects. First, using the recognition clause in the collective agreement, the Ontario Ministry of Labour classifies bargaining units according to their full- or part-time status. We use this information to create dummies for part-time only, mixed full- and part-time, a category that was identified as part-time not excluded, and the omitted reference category of full-time employees. We expect that mixed units of full- and part-time employees will have less solidarity than units that are comprised exclusively of full-time or part-time workers because they may have heterogeneous interests. Occupational dummies are created for white-collar, technical, professional, sales, and (the reference category) production workers. We expect that worker solidarity will be greater in the more craft/professional categories, that is, technical, sales, and professional. On a related note, some unions may be able to draw support from other locals of the union. As suggested by models of industrial conflict from the political economy literature, unions with more resources are more likely to strike. We control for this effect using the number of members of that union in Ontario (members in Ontario).

The bargaining unit size dummies described earlier may also proxy the effects of solidarity because workers in small units may have a greater moral commitment to work than do workers in larger units. There is some evidence that workers in small plants exhibit a lower economic commitment but a greater moral dedication to their work than do workers in large plants (Ingham 1970). Another factor that could make solidarity easier to achieve in smaller bargaining units is the greater homogeneity of those units. Relatedly, Rose (1992) found small units might have greater group cohesion, which can lead to longer strikes.

Like many other studies of strikes, ours includes controls for union type in our analyses of incidence and duration: woodworkers (IWA); Ontario Public Service Employees (OPSEU); Canadian Union of Public Employees (CUPE); Steelworkers (USWA); Canadian Autoworkers (CAW); Communication, Energy, and Paper Workers (CEP); Teamsters; United Food and Commercial Workers (UFCW); independent unions; Retail and Wholesale; Service Employees (SEIU); and Christian Labour Association (CLAC). The union dummies control for varying circumstances faced by different unions. For example, as noted by Gunderson, Kervin, and Reid (1986), for some unions strikes may be a more viable mechanism to obtain information, set patterns, and elicit truth telling than are other means (for example, joint committees or continuous bargaining). In addition, the union dummies might capture the total economic resources that a bargaining unit could call upon to support a strike.

Our hazard models include some additional control variables. Among these are controls for strike issue, bargaining structure, and company scope. The strike issue dummies, aimed at broadly differentiating strikes by cause, were non-monetary (job security, working conditions, union security, grievance/contract), mixed monetary and non-monetary (wage and non-wage, wage and unspecified other), unreported, and the omitted category of monetary only, which included any strike issue that was clearly monetary in nature (wages, piece rates, incentive pay rates, job classes, pay retroactivity, wage differentials, cost-of-living, overtime rates, wage adjustments, pensions). An interviewer from the Labour Program at Human Resources Development Canada, which collects the work stoppage data, collected the strike issue information by contacting the parties involved in the negotiations. The strikes/conflict literature suggests that strikes over issues of principle (for example, union recognition) may be longer than strikes involving monetary reasons because it may be harder to find middle ground on issues of principle.

We also control, in our hazard models, for bargaining structure by using a series of dummy variables for single-unit strikes, multi-unit strikes, multi-plant strikes, strikes that are both multi-unit and multi-plant, and (the reference category) multi-firm strikes. The extent to which bargaining units in several plants or several different units are able to bargain independently and simultaneously can affect the resources available to a union during a strike as well as the firm's production, and can thus influence strike duration. Also taken into account is the status of the agreement at the time of the strike, on the basis of which we created four categories: strikes during the term of the agreement (term), first agreement strikes (first agreement), and strikes for which the contract status is unknown, with strikes during renegotiations as our excluded reference group.[1]

Finally, Marginson (1984) and Rose (1994) found that company size (as distinct from plant or bargaining unit size) had a statistically significant impact on strike duration. The scope and diversity of a firm's operation will directly affect the ability of the firm to survive a strike. To control for these factors, we include a dummy variable to capture company scope, identifying whether the bargaining unit is a plant or division of a larger corporate entity. In most cases this information is readily obtainable from the Ontario Ministry of Labour employer record; other bargaining units are well-known corporate conglomerates, and a few others are coded based on judgment.

We provide means and standard deviations for these variables in Table 1.[2]

Methods

Strike Incidence

We estimate the incidence of strikes with the logit model

$$\text{(1)} \qquad \textsc{Strike}_{it} = \mathrm{F}(x_{it}'\beta + u_{it}),$$

where Strike is a dummy variable that equals 1 if a strike occurs and 0 otherwise, and x is a vector of variables that capture bargaining unit characteristics, economic factors, and legislative factors.

[1]All strikes during the agreement term are illegal under the Ontario Labour Relations Act.

[2]We also examined the correlation matrix for these variables to ensure that we did not have many redundant proxy variables among our explanatory variables. Most of the correlations for our explanatory variables were smaller than about 0.12 in absolute value. The only exceptions to this were some of the union dummy variables, which have large correlations with the industry dummies (for example, logging and IWA, USWA and metal working), and the dummy variable part-time not-excluded, which was correlated with the part-time only and mixed part-time and full-time dummies. The correlation matrix is available from the authors on request.

Table 1. Full Sample Variable Means and Standard Deviations.
[Omitted reference category in square brackets]

Variable Name	*Mean*	*Std. Dev.*
Strike Incidence (proportion)	.044	.205
Strike Duration (in days, conditional on having a strike)	48.9	62.2
Union Members in Ontario ('000s)	48.974	39.657
Bargaining Unit Size		
Under 21	.209	.407
21–49	.205	.404
50–99	.174	.379
100–149	.102	.303
150–199	.065	.247
200–299	.077	.267
300–499	.060	.232
[500+]	.108	.311
Change in Employment	2.38	12.6
Inverse of Unemployment Rate (prime age men)	.191	.067
Unemployment Rate (prime age men)	5.988	2.266
Increases in Weekly Earnings (wage(t)/wage(t–1))	1.048	.034
Type of Workers		
Part-Time Only	.029	.167
Mixed Pt/Ft	.098	.297
P/T Not Excluded	.632	.483
[Full-Time]	.239	.421
Occupation		
White Collar	.097	.296
Professional	.048	.214
Technical	.003	.053
Sales	.033	.178
[Production]	.819	.385
Union		
Ontario Public Service Employees	.027	.163
Canadian Union of Public Employees	.072	.258
Industrial Woodworkers and Allied Workers of Canada	.027	.161
United Steelworkers of America	.147	.354
Canadian Auto Workers	.128	.334
Communications, Energy and Paper Workers Union of Canada	.006	.076
Teamsters	.075	.264
United Food and Commercial Workers	.058	.234
Independent	.009	.096
Retail, Wholesale and Department Store Union	.044	.206
Service Employees International Union	.018	.133
Christian Labor Association	.001	.027
[Other]	.389	.488
Industry		
Logging	.006	.080
Mining	.017	.127
Food	.043	.203
Textiles	.016	.125
Wood	.014	.116
Paper	.017	.130
Printing	.004	.060
Metals	.028	.165
Appliances	.009	.092
Chemical	.013	.113

Variable Name	*Mean*	*Std. Dev.*
Industry (cont'd)		
Transportation	.004	.073
Wholesale	.003	.053
Hotel	.022	.147
Public	.198	.399
[Other]	.607	.488
Special Law		
Teacher	.026	.158
Construction	.008	.092
[Ontario Labour Relations Act]	.966	.182
Year		
1985	.144	.352
1986	.119	.324
1987	.152	.359
1988	.119	.324
1989	.111	.314
1990	.126	.332
1991	.089	.285
1992	.067	.251
[1984]	.073	.260
Season		
February	.048	.220
March	.119	.323
April	.106	.308
May	.056	.230
June	.083	.275
July	.038	.191
August	.107	.309
September	.058	.233
October	.053	.224
November	.050	.217
December	.232	.422
[January]	.051	.220
Lost Time Injury Rate	6.376	3.722
Prior Contract Length		
Two Years	.502	.500
More Than Two Years	.257	.437
[One Year]	.241	.428
Strike Issue[a]		
Job Security	.036	.186
Working Conditions	.038	.191
Union Security	.033	.178
Grievance/Contract	.017	.127
Wage and Non-Wage	.101	.301
Wage and Unspecified	.127	.334
Unreported	.125	.330
[Monetary]	.524	.500
Structure[a]		
Single Unit	.722	.448
Multi-Unit	.125	.331
Multi-Plant	.055	.228
Multi-Unit & Multi-Plant	.019	.135
[Multi-Firm]	.079	.271
Agreement Status[a]		
First Agreement	.142	.350
During Term	.014	.116
Not Reported	.003	.152
[Renegotiation]	.841	.380
Company Scope[a]	.683	.465

[a]These variables are only available for the subsample of strikes, and are only included in the strike duration analysis. The means and standard deviations presented in the table for these variables are based on the subsample of strikes.

The panel nature of the data also allows us to examine whether there is state dependence in the incidence of strikes. In particular, does prior strike occurrence reduce the likelihood of future strikes? In order to avoid problems with initial conditions, we estimate panel data fixed effects linear probability models, using the method employed by Gramm and Schnell (1987). Negative estimates on the lagged values of strike outcomes are evidence in favor of a "teetotaler" effect, whereby previous strikes predict fewer strikes in the future.

Strike Duration

We examine the determinants of strike duration using hazard models. These models estimate the probability of exiting a strike episode in period t conditional on being on strike for $t–1$ periods. In general, hazard models are particularly useful when there are time-varying covariates or censored strike episodes. Our data contain neither of these, which suggests that a log duration regression may be appropriate. However, although we do not have time-varying covariates or censored strike durations, we do have repeated strikes in some of the bargaining units in our sample (about 20%). By estimating a hazard model, we can take advantage of this additional information and improve our estimates, since they will reflect all possible information on the bargaining unit's strike history during our study period.

We specify our hazard model using a proportional hazards specification, which specifies the hazard rate at time t as

$$h(t) = \exp(x'_i\beta)\, h_0(t), \tag{2}$$

where x_i is a vector of explanatory variables, β is a vector of parameters for the controls for individual characteristics, and $h_0(t)$ is the baseline hazard.

We use two approaches to obtain estimates of equation (2). First, we employ a Cox (1972) proportional hazard model, which treats the baseline hazard function $h_0(t)$ as a nuisance parameter. This approach conditions the baseline hazard out of the model. The Cox approach also treats the duration data as continuous. An alternative approach, which is becoming more common in economic literature (for example, Baker and Rea 1998; Han and Hausman 1990), is to treat the duration data as discrete and use a flexible specification of the baseline hazard, which is sometimes also referred to as the piece-wise constant baseline hazard, in equation (2). The flexible specification of the baseline hazard with discrete data, which can be viewed as generating time-specific effects (Ham and Rea 1987), has the benefit of placing no restrictions on the shape of the baseline hazard, unlike some other specifications (for example, Weibull and Log-Logistic specifications). With discrete data the flexible baseline hazard can be specified as a series of time- or interval-specific dummy variables.[3]

Following a practice commonly employed in models with multiple spells (see, for example, Ham and Rea 1987 and Campolieti 2001), we initially included an unobserved heterogeneity distribution in our duration model. That distribution controls for the effects of unobserved variables or, alternatively, the misspecification of the functional form of the model. Ignoring this unobserved heterogeneity in the specification of the model can lead to biases in the other model parameters. We incorporated the unobserved heterogeneity distribution in our hazard model by including an individual-specific random effect, which we assumed had a Gamma distribution with mean 1 and variance σ^2. However, our estimates indicated that the unobserved heterogeneity term was not statistically significant in any of the models we estimated. Likelihood ratio tests confirmed that the

[3]We specify the flexible baseline hazard as a set of individual dummy variables for days 2–50. We group durations beyond 50 days as follows: 51–55 days, 56–60 days, 61–65 days, 66–70 days, 71–75 days, 76–80 days, 81–85 days, 86–90 days, 91–95 days, 96–100 days, 101–125 days, 126–150 days, 151–200 days, and more than 200 days. Dummy variables for each of these intervals were included with our explanatory variables when we estimated the discrete time hazard model.

models without unobserved heterogeneity would be preferred to the models with it. In addition, the parameter estimates from the model that included the unobserved heterogeneity distribution were virtually identical to those from the model that excluded it. These results suggest that unobserved heterogeneity is not likely to be a concern in these data. We consequently did not include the unobserved heterogeneity distribution in any of our hazard model specifications.

Results

Strike Incidence Results

A descriptive analysis of strike incidence is presented in Table 2. As shown in panel A, the number of strikes, and the proportion of bargaining units experiencing a strike, exhibit an alternating pattern: a year in which strike activity was high tended to be followed by a drop in strike activity in the following year. However, this pattern appears to have broken down after 1990. The number of strikes fell in both 1991 and 1992, which was also the period during which Canada experienced an economic slowdown. The peak year for strike activity in our sample was 1987.

There also appear to have been strong seasonal effects (see panel B, Table 2). There were very few strikes during the coldest months of the year (December, January, and February) in Ontario. But there was an increase in strike activity in March and April. May and June appear to have been the start of a strike season, which ran into the middle of autumn. The number of strikes was relatively constant during this period. Strikes began to decline after October, with a large drop off from November to December as the winter months of low strike activity began.

We also examined the distribution of bargaining unit size over time (not presented, but available upon request). There was a decline in the number of bargaining units in our sample. Most of the decrease was concentrated in the smaller bargaining units (21–49, 50–99, and 100–149 members). The largest bargaining units (more than 500) and smallest (less than 20) did not experience dramatic declines, although their shares did vary across the study period.

Table 2. Strike Incidence by Year and by Month.

Year/Month	*Number of Bargaining Units with No Strikes*	*Number of Bargaining Units with Strikes*	*Proportion of Bargaining Units with Strikes*
A. By Year			
1984	3,543	98	0.027
1985	3,653	198	0.051
1986	3,168	160	0.048
1987	3,235	207	0.060
1988	3,258	165	0.048
1989	3,286	149	0.043
1990	3,244	172	0.050
1991	3,239	121	0.036
1992	3,052	93	0.030
B. By Month			
January	2,369	80	0.033
February	2,261	62	0.027
March	2,570	97	0.036
April	2,656	117	0.042
May	2,872	155	0.051
June	3,372	169	0.048
July	2,318	117	0.048
August	1,951	125	0.060
September	2,332	125	0.051
October	2,270	121	0.051
November	2,528	118	0.044
December	2,179	77	0.034

We find no clear trends in the distribution of strike durations in our data (not presented, but available upon request). For example, there were as many short strikes (less than 7 days) in 1992 as in 1984. Similarly, while the number of very long strikes (more than 100 days) was highest in the 1985–88 period, no obvious long-term trend in the incidence of long-duration strikes emerges from our data.

Logit Incidence Estimates

Logit estimates of strike incidence are shown in Table 3. We present estimates of

Table 3. Odds Ratios from Logit Estimates of Strike Incidence.

	With Bargaining Unit Size Dummies	*With Number of Members*	
	(1)	*(2)*	*(3)*
Independent Variable	*All Observations*	*All Observations*	*Excludes Bargaining Units with <500 Members*
Unit Size [more than 500 members]			
Under 21	0.1800*** (14.66)	—	—
21–49	0.2988*** (10.44)	—	—
50–99	0.3392*** (9.17)	—	—
100–149	0.4284*** (6.52)	—	—
150–199	0.5379*** (4.24)	—	—
200–299	0.5976*** (3.69)	—	—
300–499	0.5767*** (3.71)	—	—
Number of Members	—	1.0032*** (6.06)	1.0004 (0.69)
Change in Employment	0.9991 (0.57)	1.0022 (1.46)	0.9983 (0.81)
Inverse of Unemployment Rate	0.7992** (2.33)	0.8053** (2.27)	0.4259** (2.44)
Increase in Weekly Earnings	0.9359 (0.08)	0.6870 (0.45)	0.0352 (0.97)
Union Members in Ontario ('000)	1.0000 (1.12)	1.0000 (1.22)	1.0000 (0.47)
Special Law [Ontario Labour Relations Act]			
Teacher	0.3369*** (3.88)	0.5010*** (2.51)	0.1897** (2.16)
Construction	0.4424*** (2.68)	0.6304 (1.51)	0.5600 (1.38)
Lost Time Injury Rate	1.0305*** (3.16)	1.0378*** (4.02)	1.0661 (1.57)

Continued

our preferred specification, which includes a full set of control variables for bargaining unit characteristics, industry dummies, and economic variables, as well as year and month dummies. Most of the estimates were not sensitive to the exclusion of the year and month dummies. However, in the text that follows, we comment on those differences that did occur.

Most of the previous literature has studied larger bargaining units (500 or more members), so we also estimated our model using only bargaining units that had 500 or more members. However, restricting the sample to these units means that we cannot use the bargaining unit dummies, so we used the number of members to control for bargaining unit size. We also estimated the

Table 3. Continued.

Independent Variable	*With Bargaining Unit Size Dummies* (1) *All Observations*	*With Number of Members* (2) *All Observations*	(3) *Excludes Bargaining Units with <500 Members*
Prior Contract Length [one year]			
Two Years	1.3718*** (3.93)	1.5033*** (5.14)	2.2881** (2.33)
More Than Two Years	1.6228*** (5.26)	1.9025*** (7.14)	2.2963** (2.07)
Missing/Unknown	0.9411 (0.62)	0.8975 (0.56)	2.3095 (0.99)
Type of Workers [Full-Time]			
Part-Time Only	1.2626 (1.27)	1.2952 (1.43)	1.0138 (0.01)
Mixed Part-Time and Full-Time	0.7759** (2.26)	0.8146* (1.84)	1.7853 (1.20)
Part-Time Not Excluded	0.9282 (1.06)	0.9583 (0.61)	1.5210 (1.10)
Chi-Squared Tests			
All Coefficients Equal Zero	968.35 {<0.0001}	739.95 {<0.0001}	149.32 {<0.0001}
Bargaining Unit Size Coefficients All Equal	223.12 {<0.0001}	—	—
Type of Worker Coefficients All Equal	6.92 {0.0745}	6.28 {0.0988}	1.12 {0.2278}
Occupation Coefficients All Equal	8.22 {0.0838}	6.52 {0.1653}	2.13 {0.4587}
Number of Observations	31,066	31,066	1,399
Value of Log-Likelihood Function	–5,217.48	–5,328.33	–404.04

Notes: Absolute value of t-statistics in parentheses; excluded reference group in square brackets; p-values for chi-squared tests in curly brackets. All specifications include dummies controlling for month and year effects as well as the occupation, industry, and union affiliation of the bargaining unit.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

model on the sample containing all the bargaining units using the number of members as a control to better compare the estimates across subsamples.

Rather than present coefficient estimates in Table 3, we present odds ratios. An odds ratio is the exponentiated value of the coefficient estimate, that is, $OR_j = \exp(\hat{\beta}_j)$ for variable j. Odds ratios greater than 1.0 indicate that the variable is associated with an increase in the odds of a strike occurring—(strike probability) / (1 – strike probability)—and correspond to positive coefficient estimates (Menard 1995). Conversely, odds ratios less than 1.0 indicate that the variable is associated with a decrease in the odds of a strike occurring and correspond to negative coefficient estimates. For example, if the odds ratio for a dummy variable is 1.05, then that variable is associated with a 5% (that is, $100 * (\exp(\hat{\beta})-1)$) increase, relative to the excluded reference group, in the odds of a strike occurring. On the other hand, if the odds ratio for a dummy variable is 0.95, then the variable is associated with a 5% decrease in the odds of a strike occurring, relative to the excluded reference group.

The odds ratios for the dummy variables controlling for bargaining unit size in column (1), which does not place any restrictions on the sample, indicate that the odds of a strike for small bargaining units were statistically significantly smaller than for bargaining units with more than 500 members. In particular, the odds ratios for bargaining units with 20 or fewer members suggest that the odds of a strike for these bargaining units were about 82% smaller than for bargaining units with more than 500 members. Similarly, the odds of a strike for bargaining units with 20–49 members were about 70% smaller than for bargaining units with more than 500 members. In general, there appears to be a declining pattern in the odds ratios for the bargaining unit size dummies, with the smaller bargaining units having smaller odds of going on strike than the larger units. All of the estimates on the size dummies are statistically significant at the 1% level and are insensitive to the inclusion of the year or month dummy variables. We also tested the hypothesis that all the coefficient estimates on the bargaining unit variables are equal. The chi-squared test statistics (see the bottom of Table 3) indicate that this restriction could be rejected, which suggests that there is statistically significant variation in strike incidence by bargaining unit size.

The change in employment variable, our control for the change in the firm's circumstances, is not statistically significant in any of the logit models we estimated. Our control for business cycle effects, the inverse of the unemployment rate, was sensitive to the presence of year and month dummy variables. When the logit model excludes the month dummies, the results suggest that strike incidence was procyclical. However, when the month dummies are added to the model, the results suggest that strike incidence was counter-cyclical. Excluding the month dummies, we find that a one standard deviation increase in the inverse of the unemployment rate would be associated with a 22.7% increase in the odds of a strike occurring. When we include the month dummies in the specification (see the estimate in column 1), we find that a one standard deviation increase in the inverse of the unemployment rate would be associated with a 20.1% decrease in the odds of a strike. The sensitivity of the estimates on the inverse of the unemployment rate to the presence of the month dummies may reflect that in the specification without the month dummies the inverse of the unemployment rate is capturing seasonal factors as well as business cycle effects. These estimates suggest that changes in the tightness of the labor market had a fairly large effect on the probability of a strike occurring. However, our estimates for the inverse of the unemployment rate would be consistent with the predictions of the joint cost theory of strikes only if we exclude the month dummies from the empirical specification.

The control for the increase in weekly earnings did not have a statistically significant effect on the probability of a strike occurring. Similarly, we did not obtain a statistically significant estimate on the control for the number of union members in Ontario.

The estimates for special laws are statistically significant and suggest that the odds of a strike occurring for bargaining units of teachers and construction workers were, respectively, 66.3% and 55.8% smaller than for bargaining units covered by the Ontario Labour Relations Act. We also found that increases in workers' compensation claim rates were associated with an increase in the odds of a strike: a one-unit increase in the lost-time injury rate was associated with a 3.1–3.3% increase in the odds of a strike.

The controls for the duration of the previous contract are also statistically significant. The odds ratios for a strike occurring for bargaining units that had 2-year contracts were 37.2% larger than for units that had 12-month agreements (see column 1). The effects are even larger for units that had contracts lasting longer than 2 years, with the odds of a strike being 62.3% larger for these bargaining units. These results are consistent with previous empirical evidence on the relationship between strike incidence and previous contract length (Card 1988).

We find few statistically significant results for our two controls for bargaining unit characteristics—occupation (results not shown in the tables, but are available on request) and type of workers. The estimate on the dummy variable for mixed part-time and full-time bargaining units is significant at the 5% level in the model in column (1), which includes bargaining units of all sizes. The estimate on the professional occupation dummy variable is significant only at the 10% level. These variables are some of our proxies for the effects of solidarity and worker cohesion, and the results suggest that those two factors were not critical determinants of strike incidence. One explanation for this finding is that if these factors are known to both sides, they may be internalized in the bargaining process. Alternatively, the size dummies may capture most of the hypothesized cohesion or solidarity effects, so these variables might be poor proxies for these factors and have no predictive power.

Although we do not present the estimates on the union dummies, some of the union dummies are associated with statistically significant increases in the odds of a strike, relative to the reference group of "other" unions. The Woodworkers, Canadian Autoworkers, and Retail and Wholesale Workers union dummies are significant at the 5% or 10% level. On the other hand, CUPE and independent unions are associated with a statistically significant decline in the incidence of strikes.

We present the logit estimates for the sample excluding the bargaining units with fewer than 500 members in column (3) of Table 3. We compare these results to the estimates from the sample with no restrictions in column (2). Recall that these specifications differ from those in the first column of Table 3 because they use the number of members rather than the bargaining unit size dummy variables.

To make the effect of the bargaining unit on the odds ratio easier to interpret, we followed the convention that is typically used in empirical work and scaled the control for the number of members so as to interpret a one-unit increase in the number of members as an increase of 20 members. When we include small bargaining units in the sample (see column 2), we find that an increase of 20 members in a bargaining unit raises the odds of a strike occurring by 0.3%, a statistically significant effect. This is consistent with the estimate in column (1), which suggests that smaller bargaining units had smaller odds of a strike occurring. However, we do not find a statistically significant relationship between the number of members in a bargaining unit and the probability of a strike occurring when we exclude bargaining units with fewer than 500 members from the sample (see Table 3, column 3).

The inverse of the unemployment rate is associated with a much larger decline in the odds of a strike occurring (a 56.3% decrease) when we restrict the sample to large bargaining units than when we include the smaller bargaining units (a 19.5% decrease) in column (2). However, when we exclude the month dummies in the sample of large bargaining units (not shown in the table), the inverse of the unemployment rate is no longer statistically significant (odds ratio of 0.94, with a t-statistic of 0.33). The estimates on the dummy variables for special laws for the big bargaining units indicate that teachers and construction workers had odds ratios that were, respectively, 81% and 44% smaller than that for bargaining units covered by the Ontario Labour Relations Act. The effects of these variables on strike incidence are larger than those in column (2), which include bargaining units with fewer than 500 members. In addition, we find that the effects of previous contract length on strike incidence for units that had 2-year contracts are much larger when we restrict the sample to bargaining units with 500 or more members. In particular, the estimate for the sample that excludes the small bargaining units (column 3) suggests the odds of a strike for bargaining units that had previous contracts of 2 years would be 128.8% greater than for bargaining units whose previous contract was only one year in duration. However, when we include small bargaining units (see column 2), the estimate on the variable for previous contracts of 2 years would be associated with an increase of 50.3% in the odds of a strike. The odds ratio for the dummy variable for previous contracts of more than two years in column (2) is smaller (90.3%) than the estimate in column (3) (129.6%), which excludes the small bargaining units from the sample.

State Dependence Estimates

We estimate a set of fixed effects linear probability models using the methods proposed by Schnell and Gramm (1987) to determine whether there was any state dependence in strike incidence. This method restricts the sample to bargaining units that had four or more settlements during our study period. We find that each of the lagged strike variables (see Table 4) has negative and statistically significant estimates. The estimates are fairly consistent across the subsamples we examined. In particular, they suggest that the probability of a strike in the present term is about 0.45 (0.25 + 0.20) lower if the bargaining unit went on strike during each of its two prior contract terms. Moreover, if we restrict our sample to the larger bargaining units, the estimates suggest that if the bargaining unit went on strike during each of the previous contract terms, the strike probability was 0.43 (0.25 + 0.18) lower. The effect of the previous bargaining outcome (first lag) slightly exceeds the effect of the outcome before that (second lag) in all of our estimates. These estimates are also robust with respect to different sets of explanatory variables.

Our estimates, like those in Schnell and Gramm (1987) and Mauro (1982), suggest strong negative state dependence. In other words, we find evidence of a teetotaler effect. Further evidence in favor of the teetotaler effect can be inferred from the frequency distribution of the number of strikes for the bargaining units that had strikes. We had strikes for 1,130 of the bargaining units in our sample during the study period. Only 200 of these bargaining units had two strikes. Furthermore, very few bargaining units (33) had three strikes. This distribution of subsequent strikes is consistent with our estimates on the lagged strike outcome variables—that is, it is consistent with a strong teetotaler effect.

Strike Duration

We present the parameter estimates for Cox proportional hazard models in Table 5 and the estimates for the proportional hazard models with a flexible baseline hazard in Table 6. Our specification of these duration models includes the variables in the strike incidence specification, together with a number of additional variables (strike issue, bargaining structure, contract status, and company scope). This information is available only for those collective agreements associated with a strike. As with the incidence analysis, we estimated our duration models on a sample of bargaining units of all sizes, as well as on the subsample of bargaining units with 500 or more members. We also estimated the duration models on further subsamples that excluded strikes occurring in the course of negotiating a first collective agreement.

Since the coefficient estimates from a hazard model are difficult to interpret, we present hazard rate ratios, which are the exponentiated coefficient estimates from the hazard model ($HR_j = \exp(\hat{\beta}_j)$).[4] Like

[4]The hazard rate at time t for a covariate x is $h(t) = h_0(t)\exp(x\beta)$. Suppose x is a dummy variable (if $x = 1$, then $x(1) = 1$, and if $x = 0$, then $x(0) = 0$). Then the hazard rate ratio can be written as

$$HR(t,x(1),x(0)) = \frac{h_0(t)\exp(x(1)\beta)}{h_0(t)\exp(x(0)\beta)} = \frac{\exp(x(1)\beta)}{\exp(x(0)\beta)}$$

$$= \exp(\beta(x(1)-x(0))) = \exp(\beta(1-0)) = \exp(\beta).$$

If the covariate we are interested in is continuous, then we can interpret the hazard rate ratio as the effect of a one-unit change in x on the hazard

Table 4. Estimates of Degree of State Dependence from Fixed Effect Linear Probability Model.

	With Bargaining Unit Size Dummies	With Number of Members	
	All Observations	All Observations	Excludes Bargaining Units with <500 Members
Strike (–1)	–0.2491*** (32.96)	–0.2553*** (33.89)	–0.2487*** (6.95)
Strike (–2)	–0.1921*** (25.76)	–0.2053*** (27.71)	–0.1788*** (4.57)
Sample Size (number of bargaining units)	4,263	4,263	301

Notes: Absolute values of t-statistics in parentheses. The estimates on the other variables are not presented, but the three columns in this table correspond to those in Table 3.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

the odds ratios, hazard rate ratios greater than 1 correspond to positive coefficient estimates, while those less than 1 correspond to negative coefficient estimates. As applied to strike duration, the hazard rate can be interpreted as a conditional exit probability—for example, the probability the strike ends on the 10^{th} day given that it has lasted 9 days. We refer to the conditional exit probability as the settlement hazard or conditional settlement probability. Finally, we interpret the hazard rate ratios much as we do the odds ratios. For example, if a variable has a hazard rate ratio of 1.05, then a one-unit change in that variable is associated with a 5% increase in the hazard. Note that since there is an increase in the settlement hazard, we can equivalently say that the variable is associated with shorter strikes or a decline in the expected duration of a strike.

The estimates on the dummy variables for the size of the bargaining unit are statistically significant. These estimates indicate that the smaller bargaining units were slower to settle strikes than were bargaining units with 500 or more members. The effect on the hazard rate was also largest for the smallest bargaining units (those with 20 or fewer members): the settlement hazard for these small units was 46% smaller than that for a unit with 500 or more members, including all the observations in our sample (column 1, Table 5). This also implies that the small bargaining units had longer strikes than the large bargaining units. Excluding the first agreement strikes from the sample (see column 2) produces estimates that are quite similar to those in column (1). We can reject the hypothesis that all these coefficient estimates are equal (see bottom of Table 5) in favor of the alternative hypothesis that strike duration did vary by bargaining unit size.

These findings, along with the estimates from the logit strike incidence analysis, indicate that smaller bargaining units were less likely to strike, but had longer strikes when they did strike. Our estimates are consistent with a few earlier studies' finding that some factors have opposite effects on strike incidence and duration. They also point to the problems in the use of an econometric model (for example, Tobit) that restricts a variable to have the same effects on both incidence and duration. Our finding of longer strikes in smaller

$$HR(t,x_1,x_0) = \frac{h_0(t)\exp(x_1\beta)}{h_0(t)\exp(x_0\beta)} = \frac{\exp(x_1\beta)}{\exp(x_0\beta)}$$

$$= \exp(\beta(x_1-x_0)) = \exp(\beta),$$

if the difference between x_1-x_0 equals 1. We will then have the exponentiated coefficient estimate.

Table 5. Hazard Rate Ratios for Cox Proportional Hazards Model.

	With Bargaining Unit Size Dummies		*With Number of Members*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Variable Name	*All Observations*	*Excludes First Agreement Strikes*	*All Observations*	*Excludes First Agreement Strikes*	*Excludes First Agreements and Bargaining Units with <500 Members*
Unit Size [more than 500 members]					
Under 21	0.5400*** (4.84)	0.5319*** (4.76)	—	—	—
21–49	0.5975*** (4.03)	0.5904*** (3.95)	—	—	—
50–99	0.5961*** (4.34)	0.5721*** (4.48)	—	—	—
100–149	0.5735*** (4.24)	0.5842*** (3.99)	—	—	—
150–199	0.6561*** (2.97)	0.6351*** (2.99)	—	—	—
200–299	0.5910*** (3.72)	0.5692*** (3.88)	—	—	—
300–499	0.6638*** (2.74)	0.6702*** (2.66)	—	—	—
Number of Members	—	—	1.0016*** (3.00)	1.0016*** (2.87)	0.9988 (1.40)
Change in Employment	1.0012 (0.86)	1.0011 (0.81)	1.0012 (1.01)	1.0012 (0.94)	1.0007 (0.17)
Inverse of Unemployment Rate	1.1569 (1.42)	1.0422 (0.37)	1.1402 (1.28)	1.0319 (0.28)	2.5096 (1.12)
Increase in Weekly Earnings	0.5233 (0.78)	0.2529 (1.53)	0.4775 (0.89)	0.2548 (1.53)	6.20e–08** (2.18)
Union Members in Ontario ('000)	0.9999 (1.11)	0.9999 (1.61)	0.9999 (1.39)	0.9999** (1.96)	1.0000*** (2.76)
Company Scope	0.8291** (2.54)	0.8235** (2.37)	0.8648** (2.01)	0.8652* (1.81)	2.1824 (0.54)
Special Law [Ontario Labour Relations Act]					
Teacher	1.1011 (0.44)	1.0785 (0.30)	1.3144 (1.33)	1.3389 (1.26)	0.0096** (2.15)
Construction	1.1639 (0.37)	0.9199 (0.17)	1.3307 (0.74)	1.0518 (0.11)	2.3761 (1.19)
Lost Time Injury Rate	1.0114 (1.22)	1.0084 (0.84)	1.0149 (1.56)	1.0119 (1.17)	0.9745 (0.23)
Prior Contract Length [one year]					
Two Years	1.0673 (0.75)	1.0365 (0.37)	1.0622 (0.69)	1.0309 (0.31)	0.5265 (0.88)
More Than Two Years	0.8895 (1.23)	0.8364* (1.71)	0.8914 (1.22)	0.8376* (1.71)	0.5205 (0.71)
Type of Workers [full-time]					
Part-Time Only	0.9987 (0.01)	1.0103 (0.04)	1.0305 (0.19)	1.0581 (0.24)	3.8287 (0.47)

Continued

Table 5. Continued.

	With Bargaining Unit Size Dummies		*With Number of Members*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Variable Name	*All Observations*	*Excludes First Agreement Strikes*	*All Observations*	*Excludes First Agreement Strikes*	*Excludes First Agreements and Bargaining Units with <500 Members*
Mixed Part-Time and Full-Time	0.8695 (1.32)	0.8779 (1.07)	0.8893 (1.11)	0.8993 (0.88)	1.3691 (0.47)
Part-Time Not Excluded	0.9379 (0.92)	0.9809 (0.26)	0.9434 (0.84)	0.9904 (0.13)	1.6384 (0.62)
Strike Issue [monetary only]					
Non-Monetary	0.8508 (1.60)	0.8425 (1.47)	0.8675 (1.41)	0.8635 (1.26)	1.3791 (0.72)
Mixed	1.0889 (1.22)	1.0596 (0.78)	1.0804 (1.10)	1.0389 (0.51)	0.8676 (0.34)
Unreported	1.7035*** (4.81)	1.6969*** (4.47)	1.6416*** (4.39)	1.6263*** (4.03)	—
Bargaining Structure [multi-firm]					
Single Unit	0.9047 (0.81)	0.8222 (1.52)	0.8913 (0.89)	0.8090 (1.57)	0.6986 (0.80)
Multi-Unit	0.8497 (1.16)	0.7154** (2.22)	0.8281 (1.28)	0.7021** (2.25)	0.4522 (1.41)
Multi-Plant	0.9311 (0.40)	0.9937 (0.03)	0.9240 (0.44)	1.0011 (0.01)	0.4199 (1.11)
Multi-Unit & Multi-Plant	0.7568 (0.96)	0.9857 (0.03)	0.7213 (1.11)	0.8777 (0.28)	29.8506 (1.53)
Agreement Status [renewal]					
First Agreement	1.0257 (0.27)	—	0.9851 (0.16)	—	—
During Term	4.3479*** (3.75)	4.1018*** (3.52)	4.2351*** (3.58)	4.0763*** (3.40)	57.9122*** (4.75)
Not Reported	2.5817* (1.71)	2.5562* (1.76)	2.3634 (1.53)	2.3337 (1.58)	—
Chi–Squared Tests					
All Coefficients = 0	328.48 {<0.0001}	804.68 {<0.0001}	288.21 {<0.0001}	779.96 {<0.0001}	324.02 {<0.0001}
Bargaining Unit Size Coefficients All Equal	19.26 {0.0074}	18.88 {0.0085}	—	—	—
Type of Worker Coefficients All Equal	13.06 {0.0045}	12.62 {0.0055}	13.32 {0.0040}	12.9 {0.0049}	0.68 {0.8779}
Occupation Coefficients All Equal	8.92 {0.0631}	5.08 {0.2992}	9.18 {0.0568}	7.56 {0.1091}	0.10 {0.9990}
Number of Bargaining Units	1,130	950	1,130	950	99
Number of Strikes	1,363	1,167	1,363	1,167	140
Value of Log-Likelihood Function	–8,379.11	–6,995.59	–8,386.85	–7,003.16	–487.01

Notes: Absolute values of t-statistics in parentheses; excluded reference group in square brackets; p-values for chi-squared tests in curly brackets. All specifications include dummies controlling for month and year effects as well as the occupation, industry, and union affiliation of the bargaining unit.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

bargaining units is consistent with the notion of greater moral commitment to work by employees in small plants or units (Ingham 1970). We also interpret this finding as supporting the industrial relations view (for example, Rose 1972) that workers in smaller units exhibit greater solidarity or cohesion, which would make them more likely to experience longer strikes than workers in larger bargaining units. Similarly, smaller firms may be found in more competitive industries and may have less pricing power than larger firms. Consequently, they might be less likely to settle than larger firms once there is a strike, since settling might adversely affect their financial position. These considerations about firm behavior and the smaller bargaining units' greater solidarity might explain why we see longer strikes in smaller firms and why the bargaining process cannot internalize information about solidarity and group cohesion.

The change in employment and the inverse of the unemployment rate are not statistically significant in any of the subsamples we examine. These estimates indicate that business cycle effects did not influence the duration of strikes. Also found not to be statistically significant were the increase in weekly wages; special laws, the lost-time injury rate, and the duration of the previous contract; and the control for the number of union members in Ontario (which did not have a statistically significant effect on the settlement hazard in either of the samples examined in columns 1 and 2 in Table 5).

Few of the other controls for bargaining unit characteristics achieve statistical significance. None of the controls for the type of workers in the bargaining unit are statistically significant.[5] Similarly, of the estimates for the occupation type of the bargaining unit, only the controls for technical and sales occupations are statistically significant.

The estimates for the union dummies (which are not presented, but are available upon request) for the sample used in column (1) of Table 5 indicate that the settlement hazards for the Teamsters are 32.6% larger than those for the unions that constitute the "other unions" reference category, although this effect is only significant at the 10% level when we exclude the first agreement strikes. The IWA, CEP, and CLAC unions are associated with significantly longer strikes in the subsamples we examined. In particular, the estimates suggest that the settlement hazards for bargaining units that are members of the IWA, the CEP, and the CLAC are 53.2%, 68.8%, and 72.6% smaller, respectively, than the settlement hazards for the excluded category of "other unions." None of the other union dummies are statistically significant. Only the estimates on the IWA and CLAC dummy variables were statistically significant at the 5% level when we dropped the first agreement strikes from the sample.

Our controls for strike issue (non-monetary and mixed, the latter including monetary and non-monetary reasons) did not have a statistically significant effect on the settlement hazard in any of the specifications we estimated. Only the estimates on the dummy variable for issues that were not reported by the parties are statistically significant at the 1% level in both columns (1) and (2) of Table 5.

None of our estimates for bargaining unit structure are statistically significant in column (1), which includes all the observations in our sample. However, if we exclude the first agreement strikes from the sample (column 2), we find that the settlement hazards for multi-unit bargaining structures are 28.5% smaller than those for multi-firm structures. The dummy variable for first agreement strikes (column 1) does not have a statistically significant effect on the settlement hazard. On the other hand, the control for strikes during the term of the agreement is always statistically signifi-

[5]We can reject the null hypothesis that all of the types of worker coefficient estimates are the same for four of the specifications we estimated (see the bottom of Table 5). On the other hand, we can reject the null hypothesis that all the occupation coefficient estimates are the same only at the 10% level and only in a few cases.

cant and suggests a very large increase in the conditional probability of settling (relative to renewals), which indicates that this variable is associated with a decrease in expected duration.

The control for company scope is associated with a statistically significant increase in the expected duration of a strike. This suggests that bargaining units that are divisions of companies with international operations have settlement hazards that are about 18% smaller than companies without international operations and, consequently, have longer strikes.

We also compared our Cox proportional hazard estimates for the whole sample and the sample excluding first agreement strikes with the sample excluding the large bargaining units (see columns 3–5 in Table 5). As in the analysis of strike incidence, we find some differences in the estimates across subsamples. However, one result of particular interest is the estimate on the variable controlling for the number of members in the bargaining unit. The estimates in columns (3) and (4) indicate that an extra 20 members in a bargaining unit increased the settlement hazard by 0.2%. This is consistent with the estimates on the size dummies in columns (1) and (2), which suggest that smaller bargaining units were less likely to settle quickly. However, we did not find a statistically significant relationship between the numbers of members in the bargaining unit and the settlement hazard for the bargaining units with 500 or more members.

The estimates of the discrete time proportional hazard model with the flexible baseline hazard are presented in Table 6. Most of the estimates are fairly similar to the estimates in Table 5 for the continuous time Cox proportional hazard model, which does not specify the baseline hazard. The dummy variables for bargaining unit size still suggest that smaller bargaining units, being less likely than larger bargaining units to conditionally settle strikes, had longer strikes. Since the estimates in Table 6 are from a hazard model that specified the baseline hazard, we can infer whether the settlement hazard exhibits positive duration dependence (that is, the longer the strike, the greater the probability of settling the strike).[6] We rejected the null hypothesis of no duration dependence (p-values less than 0.0001) in all of the subsamples we examined (see Table 6). Previous work using Weibull hazard models (for example, Schnell and Ondrich 1993; Tracy 1986) has found evidence of positive strike duration dependence. The estimates from our flexible baseline hazard specification are consistent with that finding. However, unlike previous estimates from Weibull specifications of hazard models, the positive duration dependence we identify would not be monotone. This finding of positive duration dependence in strike data is consistent with asymmetric/private information models of strike behavior.

Conclusion

We have estimated models of strike duration and incidence using a unique panel data set of collective bargaining outcomes from Ontario, Canada. We find that small bargaining units were less likely to strike than were larger units, but had longer strikes when they did strike. In particular, the odds of a strike were at least 82% smaller in bargaining units with fewer than 21 members than in those with 500 or more members, but when strikes did occur, settlement hazards for the small units were about 45% below those for the larger ones.

Our estimates are consistent with earlier studies that have found that some factors have opposite effects on strike incidence and duration. Also supported by our findings are some propositions in behavioral models of industrial conflict suggesting that solidarity and group cohesion are strong determinants of strike duration. The analysis yielded at least two other interesting

[6]The estimates of the baseline hazard for the Cox proportional hazard model can also be retrieved using the coefficient estimates. However, it is more difficult to interpret whether there is positive duration dependence with those estimates.

Table 6. Hazard Rate Ratios for Proportional Hazards Model with Flexible Baseline Hazard.

	With Bargaining Unit Size Dummies		*With Number of Members*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Variable Name	*All Observations*	*Excludes First Agreement Strikes*	*All Observations*	*Excludes First Agreement Strikes*	*Excludes First Agreements and Bargaining Units with <500 Members*
Unit Size [more than 500 members]					
Under 21	0.5398*** (4.58)	0.5154*** (4.72)	—	—	—
21–49	0.5628*** (4.32)	0.5335*** (4.53)	—	—	—
50–99	0.5911*** (4.18)	0.5627*** (4.45)	—	—	—
100–149	0.5337*** (4.46)	0.5384*** (4.35)	—	—	—
150–199	0.6717*** (2.68)	0.6448*** (2.77)	—	—	—
200–299	0.5699*** (3.87)	0.5397*** (4.13)	—	—	—
300–499	0.6826** (2.55)	0.6819** (2.55)	—	—	—
Number of Members	—	—	1.0017*** (3.07)	1.0017*** (2.96)	0.9986 (1.32)
Change in Employment	0.9960*** (2.88)	0.9962 (1.37)	0.9968** (2.40)	0.9972 (1.09)	1.0007 (0.38)
Inverse of Unemployment Rate	1.1181 (1.04)	1.0116 (0.10)	1.0967 (0.87)	0.9919 (0.07)	3.2701 (.1.27)
Increase in Weekly Earnings	0.8638 (0.17)	0.4617 (0.84)	0.7324 (0.37)	0.4124 (0.97)	0.0003 (0.92)
Union Members in Ontario (‘000)	0.9999 (1.28)	0.9999* (1.73)	0.9999 (1.44)	0.9999* (1.95)	1.0000** (2.14)
Company Scope	0.8110*** (2.70)	0.8167** (2.36)	0.8429** (2.26)	0.8592* (1.83)	4.6144 (0.79)
Special Law [Ontario Labour Relations Act]					
Teacher	1.2704 (1.07)	1.2679 (0.93)	1.5104** (1.98)	1.5778* (1.93)	0.0043** (2.01)
Construction	1.5094 (0.96)	1.1134 (0.21)	1.7102 (1.35)	1.2712 (0.52)	7.9079** (2.30)
Lost Time Injury Rate	1.0037 (0.37)	0.9987 (0.12)	1.0073 (0.74)	1.0031 (0.29)	0.9703 (0.32)
Prior Contract Length [one year]					
Two Years	1.0433 (0.47)	1.0322 (0.31)	1.0379 (0.41)	1.0250 (0.24)	0.4066 (1.28)
More Than Two Years	0.9018 (1.04)	0.8621 (1.34)	0.9023 (1.03)	0.8594 (1.37)	0.7187 (0.37)
Type of Workers [full-time]					
Part-Time Only	1.0411 (0.24)	1.1020 (0.39)	1.0822 (0.48)	1.1663 (0.63)	0.3722 (0.34)
Mixed Part-Time & Full-Time	0.8958 (1.01)	0.9146 0.72	0.9036 0.93)	0.9224 (0.65)	2.2695 (0.90)

Continued

Table 6. Continued.

	With Bargaining Unit Size Dummies		*With Number of Members*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Variable Name	*All Observations*	*Excludes First Agreement Strikes*	*All Observations*	*Excludes First Agreement Strikes*	*Excludes First Agreements and Bargaining Units with <500 Members*
Part-Time Not Excluded	0.9510	0.9977	0.9556	1.0057	2.4304
	(0.69)	(0.03)	(0.63)	(0.07)	(1.02)
Strike Issue [monetary only]					
Non–Monetary	0.8901	0.8584	0.9025	0.8789	1.0983
	(1.14)	(1.31)	(1.00)	(1.09)	(0.17)
Mixed	1.1068	1.0989	1.0862	1.0661	1.6169
	(1.39)	(1.22)	(1.12)	(0.82)	(1.01)
Unreported	1.6749***	1.7271***	1.6107***	1.6471***	—
	(4.57)	(4.43)	(4.15)	(3.99)	
Bargaining Structure [multi-firm]					
Single Unit	1.0870	1.0055	1.0614	0.9748	1.3007
	(0.66)	(0.04)	(0.46)	(0.19)	(0.63)
Multi-Unit	0.9673	0.8224	0.9424	0.7999	0.5873
	(0.23)	(1.24)	(0.40)	(1.39)	(0.75)
Multi-Plant	1.1306	1.1944	1.1207	1.2004	0.7045
	(0.66)	(0.87)	(0.61)	(0.89)	(0.40)
Multi-Unit & Multi-Plant	1.0652	1.7594	1.0080	1.5973	12.3541
	(0.21)	(1.28)	(0.03)	(1.03)	(1.44)
Agreement Status [renewal]					
First Agreement	1.0228	—	0.9792	—	—
	(0.22)		(0.21)		
During Term	1.7278	1.9324	1.6987	1.8578	10.4719**
	(1.19)	(1.43)	(1.09)	(1.25)	(2.37)
Not Reported	2.6070	2.7148	2.4225	2.4884	—
	(1.49)	(1.58)	(1.38)	(1.46)	
Chi–Squared Tests					
All Coefficients = 0	439.38	353.86	423.00	350.40	250.54
	{<0.0001}	{<0.0001}	{<0.0001}	{<0.0001}	{<0.0001}
Bargaining Unit Size Coefficients All Equal	16.44	14.00	—	—	—
	{0.0214}	{0.0512}			
Type of Worker Coefficients All Equal	11.28	10.22	8.14	1.04	8.04
	{0.0103}	{0.0168}	{0.0432}	{0.7916}	{0.0452}
Occupation Coefficients All Equal	15.56	4.84	12.1	5.78	9.54
	{0.0037}	{0.3041}	{0.0167}	{0.2162}	{0.0489}
No Duration Dependence	202.30	148.08	198.60	158.68	155.76
	{<0.0001}	{<0.0001}	{<0.0001}	{<0.0001}	{<0.0001}
Number of Bargaining Units	1,130	950	1,130	950	99
Number of Strikes	1,363	1,167	1,363	1,167	140
Value of Log-Likelihood Function	–5,952.30	–5,031.64	–5,960.49	–5,040.37	–411.76

Notes: Absolute values of t-statistics in parentheses; excluded reference group in square brackets; p-values for chi-squared tests in curly brackets. All specifications include dummies controlling for month and year effects as well as the occupation, industry, and union affiliation of the bargaining unit.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

findings, as well. First, we find strong evidence of a teetotaler effect, or "state dependence," in strike outcomes: experience with strikes made bargaining units less likely to strike in the future. Second, some of our hazard models suggest that the longer a strike lasted, the greater was the probability of settling. This finding is consistent with asymmetric/private information models of strikes.

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