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COLLECTIVE BARGAINING AND COMPULSORY ARBITRATION:  
PRESCRIPTIONS FOR THE BLUE FLU

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ABSTRACT

This paper reveals that municipal police departments are much less likely to strike in states that have collective bargaining laws than in states with no police bargaining law or when police bargaining has been outlawed. Unlike previous research which has used the state as the unit of observation, this study examines the municipal level decision to strike for a pooled cross-section of 2998 municipal police departments.

Pooled cross-section estimates of this study reveal two important relationships. First, municipalities in states that provide for collective bargaining in any form experience significantly fewer police strikes than do municipalities in environments where there is no law or where police bargaining is specifically outlawed. Second, among states with duty-to-bargain rights for police, those with compulsory arbitration provisions experience significantly fewer strikes. Fixed-effect estimates that consider strike probabilities of the same cities under different statutes qualifies the first finding. Municipalities that experienced a change from a "no law" environment to a bargaining law environment are less likely to experience strikes while in the "no law" environment than are municipalities which have always been in no law environments. However, fixed-effect estimates confirm the finding that a compulsory arbitration provision significantly reduces strike propensities. Interviews with representatives from cities that experienced a police strike suggest that state agencies responsible for the administration of arbitration mechanisms could help avoid strikes by avoiding lengthy delays in the arbitration process after the expiration of contracts.

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Strikes by public safety employees in the government sector interrupt vital services and may pose serious threats to life and property. To avoid such interruptions, state policy makers have enacted a wide range of regulations for the process that determines compensation and working conditions. Strikes by government employees with few exceptions are illegal in all states and for all occupational categories. Such regulations do not eliminate public sector strikes, as strikes by government employees have become more numerous over recent years reaching a historical high in 1978 with 481 strikes involving 193,700 workers.<sup>1/</sup> Other state laws provide for different degrees of collective bargaining rights for public employees and for various dispute resolution mechanisms sometimes including compulsory binding arbitration.

How would one expect these various statutory provisions for bargaining rights and impasse procedures to affect the incidence of strikes by public employees? The institutional forces embodied in state bargaining laws could influence work stoppages in several ways. Environments with collective bargaining may experience more industrial conflict than those without statutory bargaining frameworks, with some control for the level of union activity in other sectors of the environment, since collective bargaining formally institutes a process which may increase the interaction of labor and management representatives and thereby increase the opportunity for bringing up issues that are difficult to resolve.

Conversely, despite increasing formal communication, a process which increases the voice and participation of employees may tend to lessen conflict by presenting opportunities for parties to work out issues with a broader perspective on possible solutions. Furthermore, by providing for unit certification procedures, collective bargaining statutes could reduce the likelihood of recognition strikes.

The impact of these bargaining statutes and dispute resolution

mechanisms has been studied extensively in recent years. Many of the studies have focused on the impact of these statutory provisions on compensation of public employees with particular emphasis on the effect of compulsory arbitration.<sup>2/</sup> Yet, specifically for the case of arbitration statutes, the intended effect of the statute is the avoidance of work stoppages by providing for a binding resolution to impasses in negotiations short of a strike. No study has presented a comprehensive test of the impact of bargaining statutes and of other factors on public sector strikes. This study seeks to fill this void by calculating estimates of the impact of different bargaining laws across the fifty states on strike activity for the occupation of police.

The effects of collective bargaining laws on strikes have been analyzed in rather misleading ways. For example, the Public Service Research Council (PSRC) found that states that passed collective bargaining laws experience an increase in the number of public employee strikes per year after the passage of a collective bargaining law. This study does not adequately address the existence of a similar time trend in strike activity for states not passing bargaining laws, differences in the number of municipalities across states, or differences in strike activity or bargaining provisions for different occupational groups; still these shortcomings do not prevent the PSRC from concluding "that compulsory collective bargaining in the public sector causes an increase in strikes...and that a method other than compulsory collective bargaining for conducting employee-employer relations in the public sector would be in the public interest."<sup>3/</sup>

Academic research has also been constrained by data availability, the most widely cited study to date (Burton and Krider, 1975)<sup>4/</sup> limited to aggregate state level data for the years prior to 1971. Since then other simple descriptive comparisons of strike frequencies across states (Wheeler, 1975; Kochan, 1979)<sup>5/</sup> have been conducted. Yet much more sophis-

ticated research designs are needed to provide a definitive assessment of the effects of alternative laws on strikes,<sup>6/</sup> Specifically, to achieve such an assessment, the design should account for several important considerations. The design should allow for a comparison of strike ratios across environments where no law exists, and where different types of impasse procedures exist. To the extent possible, this comparison should go beyond a simple dichotomy such as law/no law or factfinding/arbitration. The design should also consider differences in the number of municipalities and thereby obtain a better control over the number of departments that could go on strike. The design should control for other municipal and state characteristics that could affect strike frequencies, while also considering the specific details of bargaining statutes for different occupational groups. Finally, one would want the design to maintain valuable time series features and examine the same municipalities in different statutory environments.

This study is able to examine the impact of different bargaining statutes on the municipal-level decision to strike by integrating all occurrences of work stoppages for the years 1972, 1973, 1976, 1977 and 1978 by municipal police departments into a pooled cross-section time series data set of 2,998 municipalities. Furthermore, by examining thirteen states which legislated changes in the police bargaining statutes in the 1972 to 1978 period, this study elaborates on pooled cross-section estimates by examining the same municipalities but in different statutory environments. To provide a better understanding of the causes of police work stoppages these two sets of empirical estimates are enhanced by interviews with union and municipal officials from the cities experiencing work stoppages.

Pooled cross-section estimates of this study reveal two important relationships. First, municipalities in states that provide for collective

bargaining in any form experience significantly fewer police strikes than do municipalities in environments where there is no law or where police bargaining is specifically outlawed. Second, among states with duty-to-bargain rights for police, those with compulsory arbitration provisions experience significantly fewer strikes. Fixed-effect estimates that consider strike probabilities of the same cities under different statutes qualifies the first finding. Municipalities that experienced a change from a "no law" environment to a bargaining law environment are less likely to experience strikes while in the "no law" environment than are municipalities which have always been in no law environments. Moreover, though based on a limited number of observations, municipalities that switch from a "no law" environment to a duty-to-bargain environment without arbitration are more likely to experience police strikes in the duty-to-bargain environment. However, fixed-effect estimates confirm the finding that, among cities that switched from duty-to-bargain environments without binding arbitration to environments with arbitration, the arbitration provision significantly reduces the strike propensities of those cities. Interview responses suggest that state agencies responsible for the administration of arbitration mechanisms should try to avoid delays in the arbitration process after the expiration of contracts in order to make this mechanism as effective as possible in avoiding work stoppages.

The remainder of this paper is divided into five sections. Section II describes the municipal police data set. Section III presents the models used to estimate the hypothesized relationships. Section IV presents the two sets of empirical estimates of determinants of strike frequency: pooled cross-section estimates and "fixed effect" estimates that examine the same cities in different environments. Section V presents evidence gathered about the cities experiencing strikes through interviews with labor and management representatives from those cities. The final section summarizes

the major findings.

## II. Data and Hypotheses

### 1. Strike Activity

Work stoppages among the municipal police departments in the national sample of cities are measured according to information collected by the Industrial Relations Unit of the U.S. Bureau of Labor Statistics (BLS). BLS Form 3006 contains data on work stoppages by occupational category and level of government. Information for all municipal level work stoppages involving police were collected for the years 1972, 1973, 1976, 1977 and 1978. These work stoppages are analyzed along three dimensions--frequency, duration, and breadth. The frequency of strike activity is determined by the existence of a work stoppage for a given municipality. The duration of the stoppage is taken directly from BLS Form 3006, while breadth is measured by man-days idle which is calculated as the product of duration and the number of uniformed police officers. The nature of the data collection process by the BLS virtually assures that no police work stoppage in any city in the national data set is overlooked.<sup>7/</sup>

### 2. State Bargaining Laws

The major objective of this study is to gauge the impact that different legal environments for collective bargaining have on the frequency of police work stoppages. The variables constructed to measure the legal environment take into account two principal dimensions of these laws--bargaining rights and impasse procedures. These bargaining environments have been defined by networks of state statutes, court opinions, and opinions by state attorney generals.<sup>8/</sup>

The most important right granted employees in a state bargaining environment is the duty-to-bargain. Under such a bargaining right not only is the public employer required to bargain with representatives of

employee organizations, but employees also have the right to form and join an organization. Other state bargaining frameworks which permit bargaining establish the legality of collective bargaining for covered employees. However, under such frameworks employers are not obligated to participate in the bargaining process. Other state frameworks establish the right of employees or employee organizations to "meet and confer" with employers or the right "to present proposals". In some state jurisdictions collective bargaining for certain groups of public employees is explicitly illegal, while in others the law is completely silent on the subject of public employee bargaining rights.

In states where the duty of employers to bargain with employee organizations has been established, impasse procedures for resolving bargaining conflicts have generally been instituted. Dispute resolution mechanisms in this sector generally are perceived in a hierarchical fashion: mediation, factfinding, voluntary binding arbitration, and compulsory arbitration. These third party processes have a tendency of becoming more formal and judicial in nature the closer they come to compulsory arbitration in the hierarchical setting.

In this study, four general classifications of collective bargaining laws for police are considered. States with no specific provisions for police collective bargaining or states in which bargaining is explicitly illegal form one category. In a second group are states which grant police labor organizations the right to present their views to employers regarding terms of employment, but which fall short of a duty-to-bargain provision. In the third category are states which have a duty-to-bargain provision but no compulsory arbitration mechanism for resolving disputes. Finally, the fourth group is composed of states which not only have duty-to-bargain laws, but which provide for compulsory arbitration as well. A complete list of the states and the categorization

of their police bargaining laws according to this classification scheme is given in the Appendix.

In formulating these four categories, a degree of subjectivity was used in selecting the criteria to differentiate among the state bargaining laws. States having no explicit law governing police collective bargaining are grouped with states in which bargaining is illegal. This is partly done in consideration of the historical development of state bargaining laws. Until 1958, no state had a municipal employee bargaining law and sentiment toward public employee bargaining, particularly for public safety employees, was hostile. Viewed this way, the absence of a specific state law is more correctly classified with laws specifically outlawing police bargaining. A critical distinction between duty-to-bargain and bargaining permitted provisions is that in the former class the choice to bargain lies with the employee organization, while with the latter the choice lies with the public employer. Compulsory binding arbitration within a duty-to-bargain environment makes another significant change in the bargaining process. In such an environment, police labor organizations need not rely on the final consent of the public employer to determine the terms and conditions of their employment, but rather a neutral third party has power to arbitrate these contract terms.

Since this study involves all fifty states, some potentially important differences among bargaining statutes within a given category may be overlooked. Maine's police bargaining statute, for example, does provide for a degree of binding arbitration; however, the arbitrator's decision is only "advisory in regard to salaries and wages".<sup>9/</sup> Because of the nature of salaries and wages as a preeminent subject of bargaining, the Maine statute was classified with duty-to-bargain states having no compulsory arbitration. Yet, the Maine statute does possess an important feature which differentiates it from the other states within that category. Like-

wise, among arbitration statutes, differences between types of arbitration are overlooked; issue-by-issue arbitration, last-best offer arbitration, and last-best offer package arbitration are all distinct versions of legislated arbitration but are included within the same category.

Moreover, differences in state laws regarding strike penalties are not captured. The New York statute penalizes striking public employees two-for-one in pay (the loss of a day's pay for not working and an additional penalty of a day's pay for striking illegally<sup>10/</sup>), while Wisconsin fines an employee ten dollars per day.<sup>11/</sup> Both these states are included in the same arbitration category. Penalties levied against police employee organizations involved in a strike also vary within groups. Still, police strikes (either by statutory language or court opinion) with rare exception are treated as illegal. Despite these caveats, looking at all fifty states does have a significant advantage in that the entire spectrum of attitudes toward police bargaining are considered by examining categories of reasonably similar forms of state legislation.

### 3. Other Bargaining Status Variables

The collective bargaining status of a municipality with its police department is measured by a dummy variable for the presence of a written labor contract. This variable is derived from responses to two questions ("does your city have a written labor contract covering wages, hours, and conditions of employment for police personnel?" and "what year was the first written labor contract signed?") in a survey of municipal personnel officials by Freeman, Ichniowski, and Lauer.<sup>12/</sup> It is assumed that cities responding affirmatively to the first question have continually been party to a police contract since the date given in response to the second question.<sup>13/</sup> The absence of a contract could be an additional factor that could cause a strike, as municipal police officers seek the "recognition" embodied in a written agreement. This contract variable will examine the possibility

that these "recognition" strikes are relatively more frequent.

Also used in the analysis is a variable introduced in the Freeman, Ichniowski, and Lauer study which measures the police bargaining environment in a state; that is the percentage of all municipalities in a state with police labor agreements. This variable is derived from 985 responses obtained from the above contract status survey.<sup>14/</sup> As hypothesized in other research on bargaining statutes,<sup>15/</sup> a high level of this percent contract variable may be the result of the passage of a bargaining statute. Because of this possibility of collinearity and a causal relationship between the law and percentage contract variable, strike frequency equations will be estimated with and without the latter variable, since part of an effect attributable to percent contract may, in fact, be indirectly attributable to the passage of a bargaining law.

#### 4. State-level Control Variables

Since the bargaining environment is defined along state boundaries, it is important to control for other state-wide characteristics having an effect on strike outcomes so that the state bargaining law does not substitute for the effects of other factors. The collective bargaining variable (percentage of cities in a state with a written labor agreement with their police departments) is one such variable. Other state characteristics used as controls include four geographic region dummy variables (Northeast, North Central, South, and West),<sup>16/</sup> the percentage of a state's nonagricultural work force who are public employees, and the percentage of the nonagricultural work force that is unionized. The latter two controls will indicate how favorable is the climate toward unions initiating work stoppages. Also, high levels of unionism should correspond to higher area wages and may increase the expectations about a reasonable wage increase. This would again lead one to expect a positive relation between these state unionism variables and strike activity. Specifically, one wants a

negative coefficient on a bargaining law variable in a strike activity equation to represent more a dampening effect of the law on work stoppages and less the effect of other state influences which tend to discourage public sector union activity or militancy.

#### 5. Municipal Level Control Variables

Other explanatory variables included in this analysis are three government type dummies (Council-Manager, Mayor-Council, and Commission), a central city dummy, population, per capita income, total municipal revenue, number of departmental employees, and maximum step salary for patrolmen.<sup>17/</sup>

Negative coefficients are expected for the salary variable since relatively low salaries should produce greater dissatisfaction among police officers. In work stoppage equations, the effects of other municipal level characteristics are less obvious in specifications which include a police salary level control. For a given level of police compensation, one might reasonably expect ability-to-pay measures to signal to police employees unrealized potential wage gains; therefore, positive relationships should be expected. However, one might expect negative coefficients on these variables if wealthier communities can use revenues to avert strikes in ways other than by granting higher salaries, possibly through better fringes not captured with the salary variable or by providing better equipment or conditions. Central city status and population might also be expected to have positive coefficients in strike equations with salary controlled since police duties in urban or high population areas could be more hazardous. Furthermore, for a municipality of a given population, a larger department could signal a situation in which it is less likely that all employee interests will be satisfied. In this case, one would expect positive coefficients on department size. The government type controls will investigate how differences in municipal government structure affect strike activity.

### III. Econometric Models

In order to identify the correlates of strike activity among police, the most appropriate unit of observation is the bargaining unit level at which the decision to strike occurs. Although multiple bargaining units do exist within a police department, particularly distinguishing superior officers from patrolmen in large cities, this study uses the municipal police department as the unit of observation.

To identify the determinants of strike frequency a logit model of the following form is specified:

$$P(\text{STRIKE}_i) = \frac{1}{1 + e^{-\beta \vec{X}_i}}$$

where  $P(\text{STRIKE}_i)$  = the probability of a police work stoppage occurring in the  $i^{\text{th}}$  municipality

and  $\vec{X}_i$  = a vector of independent variables of the  $i^{\text{th}}$  municipality

The logit specification overcomes the difficulty that ordinary least squares regression would have for modeling the binary strike outcome, as the OLS technique produces inefficient estimates and predictions, possibly outside the 0-1 range by forcing a normal structure on the error term. Since no a priori reason exists for preferring the normal cumulative density function to that of the logistic cumulative density function, no attempt to supplement logit with probit analysis is made. Once  $\beta$  coefficients are estimated, relative strike probabilities for an average city, differing only on the bargaining law variable, will be calculated by evaluating the logistic equation using the mean level of the control variables while allowing the law dummies to vary.

Since police strikes are extremely rare events, one would like to extend the cross-section model given in the logistic equation by utilizing the additional strike information across five years. A pooled

cross-section sample using 206 cities for 1972 and 1973 and 862 cities for 1976, 1977, and 1978 is constructed. For the 1972 and 1973 portions of the sample, 1973 levels of the control variables are used. For the 1976, 1977, and 1978 portions, 1978 levels of control variables are used.<sup>18/</sup> For the salary control in 1972, 1976, and 1977, the salary level will be a salary post-dating certain cities' strikes, so that the salary coefficient in the strike equation may be distorted by potential impacts of strikes on salaries in these years. For the bargaining statutes, variables will reflect the actual environment in each of the five years.

Despite the weakness of pooled cross-section analysis caused by possibly inappropriate levels of certain independent variables in some years, it does have many advantages. It provides additional data on rare events. Furthermore, municipalities in any one year's cross-section may not have equal conditional probabilities of striking in that year along the dimension of an omitted variable, a multi-year contract. If the multi-year contracts that do exist do not expire in the same proportion across the different bargaining environments in any given year and, as one would expect, the probability of a work stoppage is less during the term of a contract, coefficients on bargaining statute dummy variables in a one year cross-section equation will not accurately reflect the effect these laws have on strikes. Municipalities not bargaining in one year should be engaged in bargaining during the time considered in the pooled sample.

The issue which remains is that states with fewer written labor agreements (most commonly the no bargaining law states) may tend more often toward an annual salary determination process. To the extent that this is true, any positive coefficient on the no bargaining law variable should be taken as a signal of the salary determination process occurring more often. This should increase the number of opportunities for police to

become disgruntled over the setting of salaries. However, this influence should not be viewed as reducing the validity of any law coefficients obtained from this analysis. The degree to which the different bargaining laws move the parties away from an annual salary determination process, is measured by the degree to which this method of reducing industrial conflict in this sector is promoted by different statutory bargaining environments.

The pooled cross-section equations to be estimated make a significant methodological improvement in the study of the impact of bargaining laws on public sector strike activity by controlling for other observable state and municipal characteristics that could affect strike propensities. Still, there exist two very different explanations for any correlations between bargaining laws and strike propensities detected in the cross-section equations. One possibility is that the institutional forces embodied in the statutes affect how likely a police department is to go on strike according to the arguments presented in the previous section. Another very different possibility is that the law-strike relation reflects selectivity in the passage of bargaining statutes, with states passing bargaining laws differing from those without laws in unobserved characteristics. For example, a negative relation between strikes and environments with bargaining laws may be due to collective bargaining laws reducing the probability of strikes. Conversely, states that enact bargaining statutes may be composed of cities that had already been less likely to strike, so that the negative coefficients would indicate a grouping of states with cities that were less prone to strike prior to the enactment of a law. (Parallel arguments can be made for arbitration-strike relationships).

By taking advantage of the longitudinal component of the law variables, a fixed effect model disentangles the two possible causes for a law-strike

relationship. Fixed-effect logit equations cannot be estimated since the maximum likelihood procedure will not converge for equations using categories for which there is no variance along the dimension of the dependent variable. Furthermore, the precise nature of the data set precludes estimating a fixed effect model using an OLS "differencing from the mean" procedure. As described above, when municipalities are pooled across the years 1976, 1977, and 1978, the 1978 levels for the control variables (other than for the bargaining environment variables) are used. Since 1973 levels of the control variables are available for a much smaller number of cities, there will be no variation in the level of most independent variables for a majority of the municipalities in the sample. Because the "differencing from the mean" procedure is not well-suited to this data set, it is not possible to control completely for "omitted city-specific" effects.

However, by extending the four bargaining statute dummy variables to reflect information on changes in bargaining statutes, one can specifically control for those omitted city-characteristics that are correlated with the passage of bargaining laws by comparing the likelihood of striking for the same municipalities but in different statutory environments. Over the seven year period examined in this study, states experienced the following changes: no law to bargaining permitted, to duty-to-bargain and to arbitration. Of particular interest will be comparisons of the "no law" coefficients for states that never experienced a change in law status to "no law" coefficients for states that did experience a law change. Also, special attention will be given to the comparisons between the before and after environments of states that switched to arbitration mechanisms during the period considered in this study. While the inclusion of this more detailed set of law variables in an OLS regression does not control for omitted city-specific effects as rigorously as a "differencing from the mean" procedure, it will allow for the examination of selectivity bias in the passage of

bargaining laws. Moreover, by including a set of law variables that describes all possible law changes, the law variables are less constrained than those used in a "differencing" procedure.<sup>19/</sup>

#### IV. Empirical Results

Table 1 presents a tabulation of strikes occurring under the different statutory bargaining environments. For any of the five years, strikes were most frequent in the "no law" category. Furthermore, across all years, more strikes occurred in the no law category (60.5% of all strikes) than all other categories combined. Only 5 strikes, or 6.2% of all strikes took place in environments that provided for compulsory interest arbitration mechanisms. However, with the number of cities in the pooled cross-section sample as a divisor, the strike propensity among duty-to-bargain cities is of the same magnitude as it is among no law cities. These proportions for arbitration and bargaining permitted cities indicate much smaller strike propensities.

To investigate this pattern in more detail and to discover to what extent certain bargaining statutes can be thought to reduce the likelihood of police strikes, more rigorous statistical tests are applied to the data. Table 2 presents logit coefficients obtained from estimating pooled cross-section equations in which a number of other characteristics which could affect strike probabilities are controlled. The results are clear-cut, never affected by the addition of the possibly confounding percent contract variable. Across both the earlier years (1972 and 1973) and the later years (1976, 1977, and 1978), the point estimates indicate that cities in environments which provide for some form of collective bargaining experience fewer strikes than cities in environments outlawing bargaining or with no provision for bargaining. The consistent ranking indicates that among these three types of bargaining environments, cities in states with arbitration are least likely to experience strikes, and

TABLE 1: POLICE STRIKES IN DIFFERENT BARGAINING ENVIRONMENTS  
FOR 1972, 1973, 1976, 1977, and 1978

(Strikes as a proportion of city-years given in parentheses)\*

Type of Police Bargaining Statute	Year					Total
	1972	1973	1976	1977	1978	
1. arbitration	2 (.061)	0 (0)	1 (.004)	2 (.007)	0 (0)	5 (.006)
2. duty-to-bargain	4 (.182)	1 (.043)	4 (.030)	8 (.078)	3 (.031)	20 (.053)
3. bargaining per- mitted	1 (.016)	1 (.016)	2 (.010)	2 (.010)	1 (.005)	7 (.009)
4. no bargaining law or bargaining illegal	11 (.126)	3 (.035)	10 (.036)	8 (.029)	17 (.063)	49 (.049)
Total	18 (.087)	5 (.024)	17 (.020)	20 (.023)	21 (.024)	81 (.027)

\*City-years in the data set include 206 municipalities in 1972 and 1973, and 862 municipalities in 1976, 1977, and 1978.

TABLE 2: LOGIT COEFFICIENTS AND STANDARD ERRORS FROM POOLED CROSS-SECTION ESTIMATES OF THE LIKELIHOOD OF POLICE STRIKES WITH DIFFERENT BARGAINING STATUTES

	1972 - 1978		1972 - 1973		1976 - 1978	
	(1)	(2)	(3)	(4)	(5)	(6)
Observations	2998	2998	412	412	2586	2586
Mean of independent variable	.027	.027	.056	.056	.022	.022
1. Law variable						
a. arbitration	-2.60*** (.59)	-2.73*** (.61)	-2.16** (1.28)	-2.41* (1.58)	-3.31*** (.81)	-3.48*** (.81)
b. duty to bargain	-.24 (.37)	-.31 (.39)	-.58 (1.31)	-.70 (1.35)	-.59* (.41)	-.70* (.43)
c. bargaining permitted	-1.42*** (.47)	-1.45*** (.47)	-1.32* (1.11)	-1.38* (1.13)	-1.79*** (.57)	-1.78*** (.57)
2. Percent Contract	--	.48 (.70)	--	.62 (2.34)	--	.75 (.81)

Other control variables include two government type dummies, three region dummies and up to four year dummies; population (and control for population less than 10,000); per capita income; total municipal revenue; department size; central city dummy; percentage of nonagricultural work force that is unionized; percentage of nonagricultural work force that is in public employment; private maximum salary; and a dummy for the presence of written labor contract.

\*\*\* - Significant at .01 level  
 \*\* - Significant at .05 level  
 \* - Significant at .10 level

"duty-to-bargain" cities most likely of the three. While the logit coefficients on the "duty-to-bargain" law variables are not significant at conventional levels for the earlier years, all other law coefficients are significant. To interpret these coefficients in terms of strike probabilities the column 2 equation is estimated at the mean for all characteristics while allowing the law dummies to vary. This estimation yields strike probabilities of .0025, .0271, and .0089 for arbitration, duty-to-bargain and bargaining permitted cities respectively. The estimate for no law cities is .0367 or from two to fifteen times greater than the strike probabilities for cities in states in the other law categories. <sup>20/</sup>

To understand the degree to which the negative relationship between the passage of bargaining laws and strike activity is causal in nature, the more detailed set of law variables that describe all changes in statutory environments is introduced into the analysis. To what degree is the increase in the absolute value of the negative law coefficients from the 1972-1973 sample to the 1976-1978 sample in Table 1 the result of the movement of less strike prone cities from no law environments into the various bargaining law environments? The law dummies listed in the left hand column of Table 3 differentiate cities that were always in no law environments from cities in no law environments but that moved into another statutory environment. With 862 cities in the later year cross-sections, and twelve cities from the earlier year cross-sections that are not contained among the 862 cities, there are 874 distinct municipalities in this data set. Column 1 of Table 3 gives the number of these cities in each category, while columns 2 and 3 give the number of strikes and strikes as a proportion of the 2998 city-year observations in each category. From these proportions, several interesting observations can be made. Cities in "no law" environments through 1978 (line 1) account for all no law strikes. Despite only 53 city year observations in no law states

TABLE 3: REGRESSION COEFFICIENTS AND STANDARD ERRORS FOR DETAILED LAW VARIABLES IN STRIKE FREQUENCY EQUATIONS

Description of Statutory Environment	Number of Cities	Number of strikes (as % of city-years)	OLS Regression for					OLS Regression for Complete pooled cross-section sample <sup>a</sup>		
			(1)	(2) prior to law change	(3) after law change	(4) prior to law change	(5) after law change	(6) prior to law change	(7) after law change	(8) prior to law change
Observations	874	81				1239	978	2998		
Mean of Independent Variable	--	.027				.045	.012	.027		
1. NO LAW throughout	277	.49 (.052)	--	--	--	.00	--	.00	--	--
2. NO LAW to BARGAINING PERMITTED	6	0 (.000)	0 (.000)	0 (.000)	-.03 (.05)	omitted -.09 (.09)	--	omitted -.07 (.06)	--	-.03 (.04)
3. NO LAW to DUTY TO BARGAIN	65	0 (.000)	9 (.049)	0 (.000)	-.03 (.02)	-.09*** (.04)	--	-.07*** (.03)	--	-.02* (.01)
4. NO LAW to ARBITRATION	16	0 (.000)	0 (.000)	0 (.000)	-.12*** (.06)	-.12 (.09)	-.08 (.06)	-.10 (.06)	--	-.08*** (.03)
5. BARGAINING PERMITTED throughout	202	--	0 (.000)	0 (.000)	--	--	--	--	--	-.05*** (.01)
6. BARGAINING PERMITTED to ARBITRATION	21	0 (.000)	0 (.000)	0 (.000)	--	--	-.04 (.04)	-.12** (.06)	--	-.06*** (.03)
7. DUTY TO BARGAIN throughout	32	--	4 (.037)	4 (.005)	--	--	--	--	--	-.05*** (.02)
8. DUTY TO BARGAIN to ARBITRATION	89	7 (.084)	1 (.005)	4 (.007)	--	--	.06*** (.02)	-.01 (.02)	--	-.08** (.02)
9. ARBITRATION throughout	166	--	4 (.007)	4 (.007)	--	--	--	--	--	-.10*** (.02)

a) Other control variables include those listed in note to Table 2 plus percent contract.

\*\*\* - Significant at .01 level

\*\* - Significant at .05 level

\* - Significant at .10 level

that eventually changed to a bargaining statute of some form (column 2 of lines 2, 3, and 4), one observes no strikes in these cells. An examination of these same cities in their later statutory environments (column 3 of lines 2, 3, and 4) indicate that these same cities did not experience strikes if the change was to bargaining permitted or arbitration environments. However, the cities described in line 3 exhibit greater strike propensities following a change to a duty-to-bargain environment. Columns 4 and 5 present coefficients from an OLS regression for the sample of cities for which the initial year bargaining environment was no law. OLS point estimates in column 4 indicate that no law cities that eventually moved into bargaining law environments were less strike prone than other no law cities. In fact, the movement from no law to duty-to-bargain produces a significant increase in strike activity for the same cities. These results, while based on a limited number of observations in certain cells, indicate that part of the negative relationship observed in bargaining law coefficients in Table 1, is the result of a "selectivity" scenario in which states with cities less prone to strikes under no law environments pass bargaining legislation.

When one makes the same sort of comparisons between duty-to-bargain environments with and without compulsory arbitration one arrives at a very different conclusion. The proportions in Column 3 indicate strike propensities for cities in duty-to-bargain environments for all seven years of .037 (line 7) and .007 for cities in environments with arbitration (line 9). Tracking the set of cities which moved from duty-to-bargain environments without arbitration to environments with arbitration, one observes an even greater reduction in strike probabilities (.084 in column 2 of line 8 and .005 in column 3 of line 8). The coefficients from the OLS regression presented in columns 6 and 7 for the sample of city-years for which the statutory environment was arbitration by 1978 corroborate these results. Arbitration coefficients, whether it is arbitration throughout the seven

year period or arbitration preceded by another environment, are all virtually identical in magnitude. For this set of coefficients, the only coefficient significantly different from zero is a significantly positive coefficient for cities with duty-to-bargain preceding arbitration (line 8, column 6). Results for the entire 2,998 sample using the complete set of detailed law variables are given in columns 8 and 9 and consistently reflect the above patterns.

While these results lend support to the argument that part of the negative relationship for bargaining law coefficients discerned in the cross-section estimates of Table 2 is due to states with less strike prone cities passing bargaining laws, the impact of a compulsory arbitration mechanism is unambiguous. Even among those cities moving from duty-to-bargain environments without arbitration to environments with arbitration, one observes a significant decrease in strike frequency consistent with the earlier cross-section estimates. Compulsory arbitration provides an effective dispute resolution mechanism to avoid strikes. However, a duty-to-bargain provision without arbitration is in some ways a "half-way house" which grants employees the right to bargain but is not necessarily coupled with an effective means to settle contract disputes. The evidence suggests that strike activity in states with a duty-to-bargain provision could be reduced significantly providing for a compulsory arbitration mechanism.

#### V. Police Strikes - Evidence from Interviews

The sample of 81 strikes were also investigated separately. However, quantitative estimates from equations which modeled strike duration as a function of the array of independent variables yielded no significant partial correlations. This is consistent with the previous literature of strike activity which indicates that factors which explain one dimension of strike activity well (frequency), do not generally provide much explanatory power for other dimensions of strike activity (duration in days

21/  
or man-days). Part of the explanation for this is due to very little variation in strike duration. Across all statutory environments, police strikes are short. For the 81 strikes, the average duration was 6.1 days with 51% 3 days or less and 84% 9 days or less.

Still, important questions remain about these 81 strikes. Most obvious is that despite the consistency of the finding that compulsory arbitration mechanisms reduce strike frequency, five municipal police departments in such environments did strike during the period considered in this study. What differentiates these departments, and what problems in the bargaining relationship could the arbitration provision not overcome? To answer such questions interviews with labor and management representatives from the five cities with arbitration strikes were conducted. Also interviewed were representatives from the municipalities which experienced police strikes in any state in 1978. Compensation and working conditions were of course issues in the majority of these strikes; however, the interviews were intended to find out why the various processes available for arriving at the terms and conditions of employment could not avert work stoppages.

For the five police work stoppages in states with compulsory interest arbitration mechanisms, the most common reason cited by labor and management representatives was the occurrence of lengthy delays in the dispute resolution process and frustration over the length of time that had elapsed since the expiration of the last contract. These delay related causes were cited for four of the five strikes. In one instance, a lack of continuity in the city's representative helped to cause the delay with the city having seven city managers in the seven year period prior to the strike. Furthermore, this department, operating without a successor contract, felt the management was in no hurry to reach a settlement because of a state law mandating a 3% cap on interest on back pay awards. In another instance,

TABLE 4: INTERVIEW RESPONSES FROM REPRESENTATIVES FROM MUNICIPALITIES EXPERIENCING POLICE STRIKES

Strikes by Law type	Number Contacted	Number of Responses	Years Represented	Reasons Cited for Strike	Number times Cited*
Arbitration (5)	5	5	1972, 1976, 1977	frustration over delays of arbitration process and length of time elapsed after expiration of last contract	4
				lack of clarity in communications between or within negotiating parties	3
				attempt to gain recognition in a written contract	2
Duty to Bargain (20)	3	3	1978	police strike as show of support for firefighter strike	1
				police seeking voluntary local option interest arbitration provision in current contract	1
				protest over demotion of chief	1
Bargaining Permitted (7)	1	0	1978	-----	--
No law (49)	17	14	1978	No alternative but to strike in the absence of any form of impasse procedure	5
				attempt to gain union recognition in a written contract	5
				extreme and persisting political tensions between police department and city administration	2

\* Only reasons cited two or more times are included in the table for arbitration and no law categories. For the three strikes in duty-to-bargain states, the principal reason for each strike is listed.

the strike occurred over the first contract ever negotiated under the state's arbitration law, and the parties' lack of familiarity with the system helped to cause delays. Related to the problems of delays in the arbitration system are unclear communications between negotiators or within a negotiating team. In one case, a police union representative said the department's attorney had misinformed union officials of the city's most recent contract offer and this helped to induce the strike. In the situation involving the turnover of seven city managers, communications channels were further confused by the practice of union officials of lobbying the members of the city council to transmit their bargaining proposals to the city manager. In two cases, additional problems were caused by the attempt to gain the recognition of a written agreement for the first time. In one of these cases, a department for a small town of about 3,000 people, negotiators were unfamiliar with the workings of the arbitration mechanism. In the other case, involving a city of approximately 39,000 people, management refused to recognize the employees' right to make use of the arbitration procedure, relying instead on the long-standing practice of deciding the terms of employment unilaterally in meetings between the mayor and police chief.

For the other three statutory environments, representatives from the 21 municipalities and their police departments which experienced strikes in 1978 were contacted. Representatives of four of these 21 municipalities would not respond to telephone or written inquiries, including three from "no law" environments and the only 1978 strike in a "bargaining permitted" environment.

For the three duty-to-bargain strikes, one was a sympathy strike for a firefighters work stoppage; another was a protest of the demotion of a police chief who then joined the rank-and-file police on the picket line. The final strike was specifically aimed at obtaining a contract

provision providing for a local option to arbitrate future contract disputes. While the department was successful in securing this contract language, the local option to arbitrate contract disputes has not since been employed.

For the fourteen "no law" strikes, six involved municipalities without written contracts and eight with contracts. In five of the six municipalities in which some form of bargaining had led in the past to written agreements, police representatives cited the absence of any procedures to address impasses as forcing the strike to bring additional pressure to bear on the dispute. In five of the eight strikes in a no contract situation, representatives cited the desire of the police to obtain a written agreement as a major issue leading to the strike. The final reason listed in this category in Table 4 is political tension. While this factor is evident to some degree in many strikes, in two cases, it was given particular emphasis. In one case, the newly appointed police chief, a former marine corps general, was attempting to change department procedures drastically, and a protest strike eventually occurred. In another case, the police department's persistent difficulties with the mayor, who had a penchant for tearing up tentative agreements in front of the employees' representative, was a major cause of the strike.

## VI. Conclusion

The statutory bargaining environments under which police bargaining is conducted have a significant impact on the likelihood that a municipal police department will be engaged in work stoppages. Simple cross-section estimates indicated that fewer police strikes occur in states with some statutory provision for bargaining. Fixed-effect estimates revealed that this relationship is due to greater strike propensities for municipalities in states that never enacted a bargaining law. Cities in "no law" environments that did enact a bargaining statute were less likely to experience strikes than cities in no law environments that never enacted any such pro-

visions. Furthermore, cities that did switch from no law environments to duty-to-bargain environments without arbitration were significantly more likely to experience strikes after the enactment of the duty-to-bargain law.

By providing for a compulsory binding arbitration mechanism to resolve disputes over negotiations of police contracts, states reduce the likelihood that their municipalities will experience police work stoppages. This result is especially clear in comparisons between duty-to-bargain environments with and without arbitration provisions, and is borne out in both the simple cross-section estimates and the estimates that control for selectivity bias. Based on the less rigorous tests in other research, one could reasonably extend this finding to the occupation of firefighters. Milwaukee and New York City, which is subject to a special city-level law and not the state law, provide the only examples where firefighters have engaged in a work stoppage where a compulsory arbitration law existed.<sup>22/</sup>

Questions remain to be answered. Precisely how costly are these interruptions in essential services caused by work stoppages by public safety employees in terms of the threat to lives and property? What costs are incurred in providing the administrative mechanisms for dispute resolution mechanisms? What are the incremental costs in providing an arbitration mechanism where the dispute resolution procedures such as mediation and fact finding already exist? It is clear that the sentiment of voters and tax payers toward public employees is changing significantly. In a 1980 public referendum, Massachusetts voters followed the lead set by taxpayers in California in passing a "tax-cap" provision. Moreover, the language of Massachusetts' Proposition 2 1/2 included a rider for the repeal of its compulsory arbitration mechanism for police and firefighters--the first such change back from an environment with arbitration. It is clear that legislative debate will be influenced by a wide variety of economic and political factors. Still, legislators should have the benefit of the best evidence possible concerning the impacts that public employee bargaining

legislation are likely to have on outcomes as important as the interruption of essential protective services. Finally, while some evidence exists on the impact of the bargaining provisions on other outcomes such as compensation,<sup>23/</sup> it would be instructive to calculate the impact of such statutory provisions on salaries, fringe benefits and work stoppages for the same data set. In this way, one can see whether a dampening effect of arbitration on strikes is obtained at the price of higher levels of compensation, or whether such a statutory provision is preferable over a range of industrial relations outcomes. Research with this data set is in progress to obtain such estimates.

APPENDIX: STATE COLLECTIVE BARGAINING LAWS FOR POLICE

State	Year of Initial Bargaining Law	Extent of Bargaining Right in 1978	Year Arbitration Law Enacted (where applicable)
Alabama	--	none	
Alaska	1972	duty to bargain	
Arizona	1974	bargaining permitted	
Arkansas	--	none	
California	1961	bargaining permitted	
Colorado	--	none	
Connecticut	1965	arbitration	1975
Delaware	1970	duty to bargain	
Florida	1974	duty to bargain	
Georgia	--	none	
Hawaii	1970	duty to bargain	
Idaho	--	none	
Illinois	1961	bargaining permitted	
Indiana	1975	duty to bargain	
Iowa	1974	arbitration	1974
Kansas	1972	bargaining permitted	
Kentucky	--	none	
Louisiana	1966	bargaining permitted	
Maine	1969	duty to bargain	
Maryland	1977	duty to bargain	
Massachusetts	1965	arbitration	1973
Michigan	1969	arbitration	1969
Minnesota	1971	arbitration	1971
Mississippi	--	none	
Missouri	--	none	
Montana	1973	duty to bargain	
Nebraska	1967	arbitration	1967
Nevada	1967	arbitration	1969
New Hampshire	1975	duty to bargain	
New Jersey	1968	arbitration	1977
New Mexico	1966	bargaining permitted	
New York	1967	arbitration	1974
North Carolina	--	none	
North Dakota	1961	bargaining permitted	
Ohio	--	none	
Oklahoma	1971	duty to bargain	
Oregon	1971	arbitration	1978
Pennsylvania	1968	arbitration	1968
Rhode Island	1968	arbitration	1968
South Carolina	--	none	
South Dakota	1969	duty to bargain	
Tennessee	--	none	
Texas	--	none	
Utah	--	none	
Vermont	1973	duty to bargain	
Virginia	--	none	
Washington	1967	arbitration	1967
West Virginia	--	none	
Wisconsin	1971	arbitration	1971
Wyoming	--	none	

Footnotes

- 1/ Bureau of Labor Statistics, Analysis of Work Stoppages, 1978, Bulletin 2066, (Washington, DC: U.S. G.P.O., June 1980) Tables 18 and 19, pp. 38-43.
- 2/ For a recent cross-state analysis of the impact of arbitration on salaries, see Craig Olson, "The Impact of Arbitration on the Wages of Firefighters," Industrial Relations, Vol. 19, No. 13 (Fall 1980), pp. 325ff. For a general review of studies of the impact of public sector unionism on compensation, see David Methe and James Perry, "The Impact of Collective Bargaining on Local Government Services: A Review of Research," Public Administration Review, (July/August, 1980) no. 4, pp. 361-365.
- 3/ Public Service Research Council, "Public Sector Bargaining and Strikes," in Bureau of National Affairs, Government Employee Relations Reporter, no. 676, (September 27, 1976) pp. F-1 to F-7.
- 4/ James F. Burton and Charles E. Krider, "The Incidence of Strikes in Public Employment," in Labor in the Public and Non-Profit Sectors, Daniel S. Hamermesh, ed. (Princeton, NJ: Princeton University Press, 1975), pp. 161-170.
- 5/ See Hoyt H. Wheeler, "An Analysis of Firefighter Strikes," 26 Labor Law Journal, 17, (January, 1975); Thomas Kochan, "Dynamics of Dispute Resolution," in Public Sector Bargaining, Benjamin Aaron, Joseph Grodin, James Stern, eds. (Washington, DC: B.N.A., 1979).
- 6/ Research in progress by Craig A. Olson, James L. Stern, Joyce Najita, and June Weisberger on public sector strikes in different occupations overcomes these problems to some extent. Their empirical tests focus on six states all with duty-to-bargain provisions, so that results apply only to a limited sample of states.
- 7/ Eugene Becker, Director of the Industrial Relations Unit of the Bureau of Labor Statistics, described the process of collecting work stoppage

data. This unit of the BLS tracks strikes through the Federal Mediation and Conciliation Service, the Census of Governments, and a network of newspaper clipping services. Because of the public attention given to police strikes, Eugene Becker was confident that police strikes in any municipality with a population over 10,000 are captured by a data collection process which surveyed newspapers extensively. Since the only municipalities with populations less than 10,000 are ones which submitted strike forms to the BLS, it is unlikely that the strike variable is mismeasured for the municipalities in this data set. Among the 81 municipalities experiencing police strikes, 16 had populations of less than the 1978 10,000 person lower limit that existed in the I.C.M.A. Municipal Yearbook data. Still, simply entering these cities into the equations will contaminate the results with an obvious effect of biasing the coefficient of the population variable downward. In the econometric models to follow, this problem is accounted for in two ways. First, a dummy variable is added to the strike equation which assumes a value of one for cities with less than 10,000 people. Second, cities of less than 10,000 are dropped from the sample. In the latter case, results can be viewed strictly as those for a sample truncated at populations of 10,000 so that results only can be extended to the smallest communities with caution. For OLS estimates for 1972 to 1978 pooled cross-section equations (equivalent to the equation in column 2 of Table 2) all coefficients are virtually unchanged from the truncated sample to the full sample with the less than 10,000 population dummy. While the population dummy attains the significant positive coefficient one would expect given this sampling procedure, the coefficients on the population variable are  $-.023$  in the truncated sample and  $-.024$  in the full sample both with standard errors of  $.009$ .

1978," (Washington, DC: G.P.O., 1979); Bureau of National Affairs, "Summary of State Labor Laws," (Washington, DC: B.N.A., 1978); Allen Gammage and Stanley Sacks, Police Unions (Springfield, Illinois: Charles C. Thomas, 1973).

9/ U.S. Department of Labor, "Public Sector Labor Relations, 1978," pp. 22-24.

10/ U.S. Department of Labor, "Public Sector Labor Relations, 1978," p. 39.

11/ Wisconsin Statutes Ch. 111, Sec. 111.70, sub 4, ch. 2 (1959; last amendment effective June 7, 1978).

12/ Richard B. Freeman, Casey Ichniowski, and Harrison Lauer, "Collective Bargaining, City Interdependencies, and Public Sector Compensation: The Case of Police," National Bureau of Economic Research (October, 1980), unpublished.

13/ For cities which experienced work stoppages but which did not respond to the survey, contract status is taken from item 5 of BLS Form 3006.

14/ Freeman, Ichniowski, and Lauer, "Collective Bargaining: The Case of Police".

15/ Thomas Kochan, "Correlates of State Public Employee Bargaining Laws," Industrial Relations, Vol. 12, No. 3, (October 1973), pp. 322ff. Burton and Krider, "The Incidence of Strikes in Public Employment," p. 155.

16/ The Northeast states are: Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, Vermont, New Jersey, New York, and Pennsylvania. North Central encompasses Illinois, Indiana, Michigan, Ohio, Wisconsin, Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, and South Dakota. The South region is composed of Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, West Virginia, Alabama, Kentucky, Tennessee, Mississippi, Arkansas, Louisiana, Oklahoma, and Texas.

And the West is Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, Wyoming, Alaska, California, Hawaii, Oregon, and Washington.

17/ Government type and metropolitan status data are from International City Managers Association, "Master Code" Data Tape (Washington, DC: I.C.M.A., 1978). International City Management Association, Municipal Yearbook, 1978, (Washington, DC: I.C.M.A., 1978) provides data on per capita income and per capita city revenue and department size. U.S. Bureau of the Census, City and County Data Book, 1972 provides data on per capita income and municipal revenue for an earlier period.

1978 compensation data are from International City Management Association, "1978 Police Salary" Data Tape, Washington, DC, 1978. 1973 compensation and department size data are from International Management Association, "Personnel Policies in Municipal Police Departments," Washington, DC, 1973. Cities experiencing police work stoppages for which no salary information was available from these sources were surveyed by telephone for this data.

18/ In this data set, certain municipal level characteristics were missing on several observations. At least one municipal control variable was missing for 47 of the 862 observations in the large sample of cities. In a seven-step process these missing variables were estimated by an ordinary least squares estimating equation using available variables that would a priori seem reasonable predictors for missing variables.

The following list indicates the variables for which observations were missing and the variables used to estimate missing values:

Actual uniformed police in 1978 = f(actual uniformed police in 1973),  
Actual uniformed police in 1973 = f(actual uniformed police in 1978),  
1970 Population = f(1975 population),  
1975 Population = f(1970 population),  
Actual uniformed police in 1978 = f(population in 1975),

Actual uniformed police in 1973 = f(population in 1970),  
Total city revenue in 1978 = f(state, 1975 population),  
Total city revenue in 1973 = f(total city revenue in 1978),  
Per capita income in 1978 = f(state, 1975 population, 1978 total revenues),  
SMSA dummy = f(1978 laws, government type, 1975 population, percent unionized in state, percent public workers in state, contract status in 1978, actual uniformed police in 1978, 1978 total city revenue, 1978 per capita income).

19/ A "differencing from the mean" procedure would produce four law change variables: a no change group; and three other groups for those changing into bargaining permitted, into duty-to-bargain, and into arbitration. In the differencing procedure, a city that remained arbitration throughout would obtain the same value for the change-in-law variables as a city that remained no law throughout (i.e., all change-in-law variables will be zero). The more detailed set of law variables can differentiate between such groups. Also, with the more detailed set of law variables, one can obtain a direct estimate of the difference between a city that changed from a duty-to-bargain to an arbitration environment; whereas, with the differencing procedure, the strike propensity of such a city would be constrained to be the difference between the "change to duty-to-bargain" and "change to arbitration" coefficients.

20/ Calculating the differences in strike probabilities for an "average city" but in different legal environments is suspect since characteristics of cities vary significantly across states in different bargaining environments; that is, the "average city" in the "no law" category is different than the "average city" in the "arbitration" category. To compute how much of the difference in strike propensities is attributable to differences in the levels of intrinsically strike-inducing characteristics across en-

vironments, separate strike equations are estimated for cities in no law environments and for cities in environments with some form of bargaining law. When one evaluates the "bargaining law" strike equation using the mean level of characteristics from cities in "no law" states, one estimates a hypothetical strike probability of .010. While the two groups' strike equations indicate that strikes in "no law" cities are over four times as likely (predicted strike frequencies of .025 for "no law" cities and .006 for "bargaining law" cities), only 21% of the 1.9 percentage point difference can be attributed to differences in the characteristics of the cities between the two different kinds of statutory environments.

Separate strike equations were also estimated for the sample of cities with and without written contracts. From information contained in the BLS strike form, 48 strikes for recognition are identified and all such strikes do in fact occur in the no contract sample. The number of recognition strikes and the percentage of cities without contracts by law category are: arbitration - 2 (of 5) and 4.3%; duty-to-bargain - 9 (of 20) and 33.5%; bargaining permitted - 5 (of 7) and 33.5%; no law - 32 (of 49) and 79.1%. Bargaining statute logit coefficients have the same rank order in both samples, but tend to be smaller in magnitude for the recognition strikes in the no contract sample. For the law variables, the coefficients from the no contract and contract samples respectively are: arbitration -- -2.78 and -3.51; duty-to-bargain -- -.16 and -.42; and bargaining permitted -- -.87 and -2.17.

21/ Robert N. Stern, "Quantitative Strike Analysis," Industrial Relations, Vol. 17, No. 1, (February 1978), p. 38.

22/ Thomas Kochan, "Dynamics of Dispute Resolution," in Public Sector Bargaining, Benjamin Aaron, Joseph Grodin, James Stern, eds., (Washington, DC: B.N.A., 1979).

23/ Craig Olson, "The Impact of Arbitration of the Wages of Firefighters,"  
pp. 325ff.