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COLLECTIVE BARGAINING LAWS AND
THREAT EFFECTS OF UNIONISM IN THE
DETERMINATION OF POLICE COMPENSATION

Richard B. Freeman

Casey Ichniowski

Harrison Lauer

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Cambridge, MA 02138
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ABSTRACT

This study examines the effect of public sector unions on compensation packages. The model of the compensation determination process incorporates distinctive institutional aspects of public sector labor relations, particularly the differences in collective bargaining laws across states. The model is estimated using data on over 800 municipal police departments. Our results indicate that the effect of public sector unions depend critically on these institutional features of the public sector. First, unionism thrives only in those states with protective legislation. Second, in states where unionism has flourished, unionism exerts a strong upward pressure on both union and nonunion compensation packages. Cross section estimates for 1978 indicate that salaries of union and nonunion departments in highly unionized states are some 30% higher than are the salaries in states with low levels of unionism. However, no significant difference between union and nonunion salaries within states is observed.

Before-after estimates of the "state-wide union effect" are more modest (9.9% to 18.1%). Finally, this "state-wide union effect" on union and nonunion departments appears to be even more pronounced on fringe benefits than it is on salaries. The net result is that in highly unionized states, a greater proportion of the larger compensation packages is paid in fringe benefits.

Richard B. Freeman
National Bureau of
Economic Research
1050 Massachusetts Ave.
Cambridge, MA 02138

Casey Ichniowski
Graduate School of
Business
Columbia Univeristy
713 Uris Hall
New York, NY 10027

Harrison Lauer
115 Yorkshire Drive
Pittsburgh, PA 15238

In spite of well-known institutional differences between public sector and private sector collective bargaining, many studies of the effect of unionization on public sector compensation have essentially followed the private sector model, estimating union effects by comparing the wages of organized and unorganized workers or cities. For the most part these studies have found rather modest union wage effects in the public sector. "The 'average' wage effect of unionism in government... is roughly on the order of five per cent... smaller than the average union wage impact in private industry."¹ "The general effects (of public sector unions) which have been measured are not huge."²

This study presents an alternative model of public sector union effects on compensation which yields larger estimates of what unions do to compensation in the public sector. The model incorporates two distinct aspects of public sector labor relations that should affect unions' ability to raise wages. First are differences in collective bargaining laws across states: the fact that some states have duty-to-bargain laws that encourage unionism, while others have legal environments that discourage or even outlaw bargaining among certain groups of employees. Second, city managers, unlike private employers, are charged with the responsibility of administering public policies. Where a state has a duty-to-bargain policy then, municipal managers will not be in a position to oppose unionism through aggressive anti-union campaigns of the type seen in the private sector. We argue that because of these factors public sector unionism proliferates in states with favorable bargaining laws, and that in these states union wages 'spill over' to nonunion cities, leading standard union wage equations to underestimate union wage effects.

We conduct our analysis using data on over 800 municipal police departments in 1978 and a smaller sample in 1965 and 1973. We find that differences

in laws regulating police collective bargaining produce two polar sets of states -- those with high levels of bargaining between municipalities and their police departments and those in which police bargaining is virtually nonexistent. Furthermore, differences in the average police salaries (all else fixed) in 'bargaining' and 'non bargaining' states are on the order of 20 to 25%. By contrast, comparisons of organized and unorganized departments within the "bargaining" and "nonbargaining" states reveal little or no difference between salaries paid in organized departments and those paid in unorganized departments. Our interpretation of these results is that in the absence of a bargaining law virtually all departments will remain nonunion with wages remaining at relatively low nonunion levels, but that in public sector environments where collective bargaining is the dominant mechanism for determining compensation, there are greater spillover effects than are usually found in the private sector.³

Direct and Threat Effects of Public Sector Bargaining

In this section we present a simple model of the inter-relation among legal regulations, public sector unionism, and direct and 'spillover' effects of unionism on compensation. The key prediction of our model is that in states where union organization is high (because of favorable laws) unionism will raise police pay in nonunion as well as in union departments. In states where unionism is weak (because of unfavorable laws), unionism will have only a modest effect on salaries in organized departments and no effect on salaries in unorganized departments.

Consider first the situation in a state with laws favorable to collective bargaining among police. A city in such a state faces the three options shown in Figure 1.

Path A: It accepts organization and raises wages through bargaining with the amount of the wage increases dependent on the union's economic or political power relative to that of the city government.

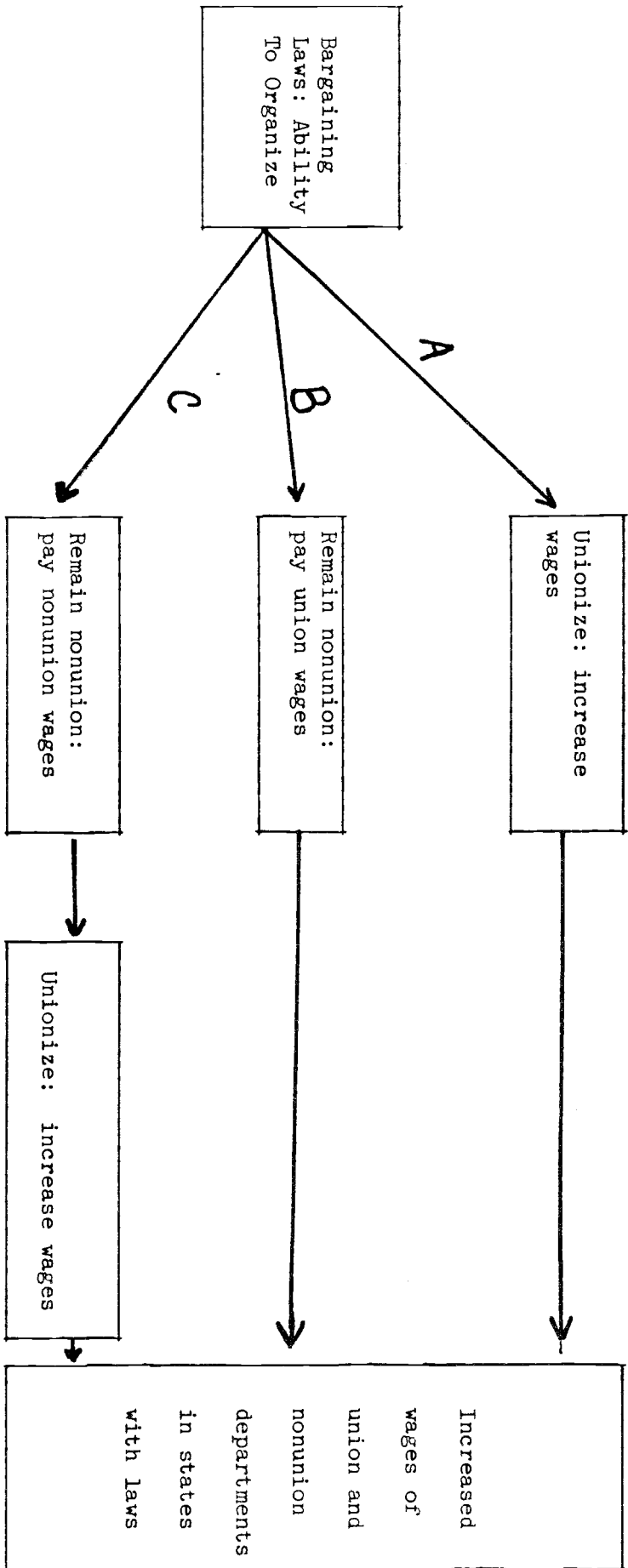
Path B: It seeks to remain non union by paying union wages to discourage organization; unlike private firms, city managers who are charged with the responsibility of administering public policies, cannot engage in expensive, often illegal campaigns against unions to remain nonunion.

Path C: It does not accept organization immediately but also does not match the increased levels in union departments. In a state with favorable laws the city runs a great risk of becoming organized over time, producing a transitory and unstable state.

Since in the long run only nonunion cities that match union wages remain nonunion, the indirect spillover effects of unionism are as large as the direct bargaining effects.

Consider next the effect of unionism in states which lack a law permitting collective bargaining. In the absence of a law, there will be no mechanism by which employees are able to become recognized formally. If workers organize a union, managers may ignore any requests or demands to bargain on the grounds that no agent has the authority to participate in the compensation determination process. In such a setting cities are unlikely to be organized. Where a union does exist, the city government will have much greater bargaining power than the union, while cities which are not organized need not copy union wages to remain nonunion. In this environment, direct effects of unionism

Figure 1: Path Model With Direct and Threat Effects of Collective Bargaining on Public Sector Wages



should be small and there should be no indirect spillover effects.

The spillover or threat model just described yields several hypotheses that will be tested in this study. First, formal collective bargaining should be concentrated in states that have bargaining statutes or policies. Second, if indirect "threat" effects of collective bargaining are equal in magnitude to the direct effect of bargaining, salaries and wages in union and nonunion departments in the states with bargaining laws will be equivalent, but at levels of compensation exceeding the levels observed in states without bargaining laws. Third, the movement of a state from little unionization to considerable unionization should greatly raise the average pay for all police in the state. Fourth, low wage nonunion municipalities in a state with a bargaining law should be highly prone to unionization in the future. Fifth, unionism should have only modest direct and no indirect effects on pay in states where collective bargaining is legally discouraged.

Data

To test these hypotheses we have gathered 1978 compensation and unionism data for over 800 municipalities with populations over 10,000. So that we could also examine changes over time, we collected data for a smaller sample of just under 200 cities with populations greater than 25,000 for the years 1965 and 1973. For each year, we have obtained patrolman's minimum and maximum salary and the average departmental salary. In 1978 we have also obtained two fringe benefit measures: per employee contributions to retirement systems, and per employee contribution to health and life insurance programs.⁴

To test how well the path model in the previous section describes the effects of public sector unions on the compensation determination process,

several variables are needed to describe the collective bargaining environment at the municipal and state levels. Previous research on the public sector indicates that a written labor contract is a better indicator of the presence of the collective bargaining process than is a union local or association.⁵ To collect contract data, we conducted a survey in which we asked the following 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?" It is assumed that cities responding affirmatively to the first question have been party to a police contract continually since the date given in response to the second question. Collective bargaining for police department i in year t is defined by the presence of a written labor contract (C_{it} ; $C=1,0$) based on this survey.

From the municipal-level variable C_{it} , we derive the state-level variable, PC_{st} : the percentage of all municipalities in state s that have written labor contracts in year t . Responses from 985 municipalities that answered the contract questions are used to calculate PC in any given year.

Also critical to the path model are state-level collective bargaining policies or laws. In this study, LAW is a dummy variable which equals one if there is a policy in the state (as defined by statute, court rulings, or state attorney general's opinions) providing that collective bargaining for police is permitted in the state.⁶

Control Variables

Since LAW and PC are defined along state boundaries, it is important to control for other state-wide characteristics that might influence police compen-

sation. Other state characteristics included in the analysis are: four geographic region dummy variables (northeast, north central, south, and west), the percentage of a state's nonagricultural workforce who are public employees in 1970, and the percentage of a states' nonagricultural workforce who are union members in 1970. In addition to state controls, we include various municipal-level characteristics: three government-type dummies (council-manager, mayor-council, and commission), a central city dummy, population, per capita income, total municipal revenue per capita. For a smaller sample of cities, information was also available on: the salary paid to craft and kindred workers as a measure of alternate salary available to police; serious crime rate; median education level in the municipality; percentage black in the population; and land area.⁷ When these additional control variables are included in analyses for 1978, the sample excludes smaller towns with populations between 10,000 and 25,000 and is reduced from just over 800 observations to under 200.

At the municipal level, ability-to-pay measures, such as income per capita and municipal revenue per capita, are expected to have a positive effect on salaries. A number of variables, such as central city, median education, percent black, land area and population, are included as controls for differences in needs or tastes (and therefore demand) for police services. The salary paid to craft-and-kindred workers should affect the supply of workers available for police work and should therefore have a positive influence on police salaries. Because independent and dependent variables come from different sources, the data do not always pertain to identical years across variables, but all independent variables are matched as closely as possible with the years of the compensation data.

The path model in Figure 1 suggests estimation of several equations.

Unionization or Contract Equation

$$C_{is} = f(LAW_s, STATE_s, CITY_i) \quad (1)$$

where C_{is} = dummy variable for contract in city i in state s

LAW_s = 0-1 variable for presence of bargaining law in state s

$STATE_s$ = vector of state controls

$CITY_i$ = vector of municipal controls

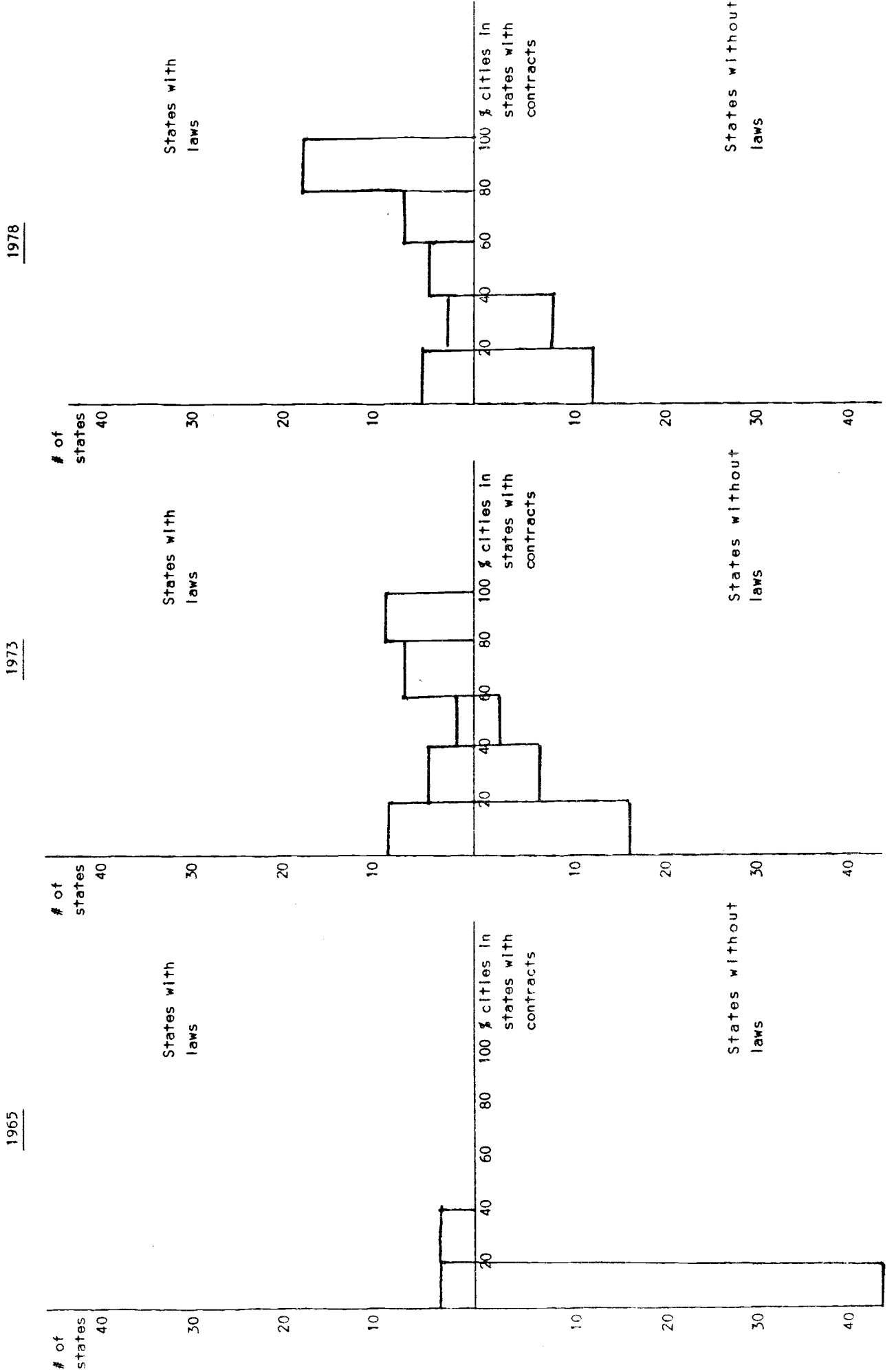
Because the dependent contract status variable (C_{is}) is binary we estimate equation (1) with a logistic function. From our model we expect that the LAW will be the major determinant of contract status of a city and, therefore, of the proportion of cities with contracts in a state. Since LAW is a state-level variable, however, there is danger that its effects could be overstated due to possible omission of unobserved state effects correlated with LAW and with unionization. To deal with this problem, we also estimate first difference equations, examining the change in contract status within a city or state over time. Such differencing removes unobserved fixed state effects:

$$\Delta C_{is} = F(\Delta LAW, \Delta STATE_s, \Delta CITY_i, STATE_s, CITY_i) \quad (2)$$

where some state and city variables change over time and others do not.

Figure 2 shows the changing pattern of contract status across states over time and the relation between contract status and state laws. It documents the sharp increase in the number of states with laws favorable to collective bargaining and a concordant increase in the levels of unionization. What stands out is the movement of the police labor market from one with essentially

Figure 2: State Laws or Policies and Percentage Contract Over Time



no unionization in 1965 to a dichotomous market with high unionization in most (though not all) states with laws and little unionization in those without laws.

Logistic equation estimates of equations 1 and 2 are given in Table 1. The coefficients on the LAW variable show that as hypothesized, passage of a collective bargaining law does, indeed, lead to collective bargaining contracts in police departments. Both the cross-section and longitudinal regressions find significant positive effects on the law (change in law) variable. More detailed analysis using panel data from 1955 to 1978 on these cities reveals that much of the growth in municipal police unionization occurs in the first few years after the passage of state bargaining laws.⁸

Compensation Equation

$$\ln W_{is} = a + b_1 C_i + b_2 PC_s + b_3 \overset{\rightarrow}{CITY}_i + b_4 \overset{\rightarrow}{STATE}_s + e_{is} \quad (3)$$

Our model also suggests that in the frequent cases where state bargaining laws have encouraged municipal unionization, wages of union and nonunion departments are both raised. Therefore, in equation (3), we expect b_2 to be positive and significant. In the absence of state legislation, the rare department that has unionized will have only limited ability to raise wages. In highly unionized states with favorable bargaining legislation, nonunion departments have wages comparable to the wages in union departments because of the importance of threat effects in these environments. With and without state legislation, union and nonunion departments will have comparable salaries, so that $b_1 \approx 0$. In this analysis, b_2 measures how much an increase in the level of state unionization will influence both union and nonunion departments and is therefore our first estimate of the threat effect. b_1 is the additional wage premium enjoyed

Table 1: Logistic Coefficient Estimates of The Effect
of Bargaining LAW on Contract Status^a

Dependent Variable observations	(1)	(2)
	CONTRACT ₇₈ 822	CONTRACT ₇₈ - CONTRACT ₆₅ 188
1. LAW ₇₈	4.131*** (.434)	--
2. LAW ₇₈ - LAW ₆₅	--	1.944*** (.378)
3. Other control variables	b	c
log-likelihood	-271.944	-94.783

a - asymptotically normal standard errors in parenthesis

b - control variables in 1978 cross-section equation included three region dummies, two government-type dummies, a central city dummy, population, income per capita, total revenue per capita, and the percentage of state employment in the public sector, and the percentage of state employment that is unionized.

c - control variables available for 1978 and 1965 that are differenced included change in population.

*** - indicates significance at the .01 level, one-tailed test;

** - indicates significance at the .05 level, one-tailed test;

* - indicates significance at the .10 level, one-tailed test

solely by union departments. The total effect of unionization on a union department is given by the sum of b_1 and b_2 . Estimates of how these different kinds of union effects depend upon the presence of state legislation will be obtained by estimating equation (3) separately for the subsamples of cities in states with and without bargaining laws.

Table 2 presents estimated coefficients on key variables for the cross-section equation (3).⁹ Entering the municipal-level contract variable (C_i) as the only union measure replicates the form of the equation most commonly estimated to gauge the effect of public sector unions on compensation. In 1978 the coefficient on C_i indicates that union departments receive 11% higher salaries than do nonunion departments. When this equation is replicated for 1973 (column 6) and for 1965 (column 8), one observes a result found in some other studies on public sector union wage effects: the simple cross-section union effect is modest but becomes larger in later time periods.¹⁰ The 1965 cross-section estimate is in fact negative (-.029); in 1973, the contract effect is .040; and by 1978 it is .107.

In column 2 and 3, one observes that the PC and LAW coefficients, entered separately, are both significant and positive. The coefficient on PC is nearly two and one-half the size of the CONTRACT coefficient in column 1. When the three union measures are entered simultaneously (column 4), only the coefficient on PC remains significant and positive. The PC coefficient from the column 4 specification suggests that both union and nonunion police departments in states that are almost entirely organized receive salaries some 30% higher than those paid to police departments in states that are generally unorganized.

Table 2: Cross-Section Estimates of the Effect of Union Measures on Natural Logarithm of Average Police Department Salary^a

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample Year	1978	1978	1978	1978	1978	1973	1973	1965	1965
Sample Observations	822	822	822	822	190	234	234	234	234
1. Contract (C _i)	.107*** (.015)	--	--	-.012 (.020)	-.004 (.041)	.040 (.031)	-.001 (.036)	-.029 (.036)	-.020 (.036)
2. Percent Contract (PC _s)	--	.249*** (.021)	--	.297*** (.034)	.229*** (.059)	--	.102* (.078)	--	-.365* (.204)
3. LAW	--	--	.120*** (.019)	-.022 (.024)	-.042 (.042)	--	.063* (.047)	--	.139*** (.031)
4. Other Municipal-Level Controls	b	b	b	b	c	c	c	c	c
5. Other State-Level Controls	d	d	d	d	d	d	d	d	d
R ²	.575	.612	.570	.613	.685	.512	.525	.640	.671

a - standard errors in parentheses.

b - municipal-level controls available for the larger samples of cities with populations over 10,000 include: natural logarithms of total revenue per capita, income per capita, and city population; and dummy variables for central city and three government types

c - municipal-level controls available for the smaller samples of cities with populations over 25,000 include those in note b plus: natural logarithms of city land area, median education, and salary of craft and kindred workers; percent black in the population; and serious crime rate.

d - state-level controls include: dummies for four geographic regions; and percent of state population unionized and the percent in public employment.

*** - indicates significance at the .10 level, one-tailed test;
 ** - indicates significance at the .05 level, one-tailed test;
 * - indicates significance at the .01 level, one-tailed test.

Our interpretation of why the LAW coefficient in column 4 is negligible is that a bargaining policy which, for whatever reason, does not produce an increase in the number of negotiated contracts for municipal police departments (as in Indiana, Kansas, Louisiana, New Mexico, North Dakota, and Vermont) does not itself raise salaries. An examination of regression diagnostics for multicollinearity for the equation in column 4 reveals, moreover, that the LAW and PC (and therefore C) variables are highly collinear making it unlikely that we would obtain well-defined estimates of both effects.¹¹

Finally, and perhaps most importantly, the column 4 specification indicates that coefficient on C is no longer significantly different from zero once PC is also entered. The simple contract (union) effect (column 1 specification) usually estimated in studies of public sector union wage effects seems to be entirely due to the greater representation of organized departments (those with a contract) in highly organized states (those with high levels of PC). These results indicate that salaries in nonunion departments are as high as those paid in union departments.

Smaller samples, columns (5) through (9), give results for the smaller sample of cities of 25,000 or more. The regression findings for 1978 are not changed appreciably when the more detailed set of municipal controls are added that limit the sample to cities and towns with populations greater than 25,000 (column 5).

Similarly, for the cities in the 1973 sample, the results are much the same. The modest positive contract coefficient (.040) in column 6 is reduced to zero once the state-level union measures are also entered in the equation

(column 7). However, in 1973 the LAW as well as the PC variable affects salaries, which may be attributed to its representing a period of transition or sorting period from an environment with very little unionization (1965) to an environment of union states and nonunion states (1978).

To obtain an estimate of the difference between average salaries in states with and without laws in 1973, one can use the column 7 coefficients to calculate:

$b_{LAW} + b_{PC} (PC_{LAW}) - b_{PC} (PC_{NO LAW})$. In 1973 the average level of PC for the 27 states with a law was .520; the average value of PC for the 23 states without a law was .122. This calculation indicates that the difference in police salaries between the average state with a law and the average state without a law would be 10.4% [i.e., $(.063) + (.102)(.520) - (.102)(.122)$]. Again, the 1973 results indicate that even as cities and states were in this period of transition, "union states" had significantly higher police salaries than "nonunion states", but that within states there was no difference between the salaries of union and nonunion departments.

For 1965, when the state-level unionization measures, LAW and PC, are entered in the equation (column 9), LAW has a significant positive coefficient, while the PC variable obtains a large negative coefficient. As Figure 2 indicated, police unionization by any state-level or municipal-level measure was not yet an accepted practice. Only 4.8% of all departments had labor contracts. Thirty five states had no police unionization (PC=0); and the maximum value of PC in 1965 was only .375. The PC coefficient, while large and negative,

translates into very small inter-state differences in average police compensation. The positive LAW coefficient is also misleading. Supplementary analysis which probe at the nature of the LAW coefficient reveals that of the four states with any kind of bargaining policies in 1965 (California, North Dakota, Rhode Island, and Wisconsin) only California has high police salaries. Taking all of these results as a whole, the cross-section estimates reveal no consistent evidence of positive effects of any of the municipal or state-level collective bargaining variables in 1965 -- a period in which police unionization was virtually nonexistent.

Splitting the sample by LAW

The cross-section estimates so far support our model's predictions that unionized states have higher average police salaries than nonunion states, but that no differences exist between the pay of union and nonunion departments within any state. Table 3 goes a step further in testing our model of spillover effects by examining contract coefficients obtained in separate equations for law and no law subsamples. For our 1978 large sample there are enough observations to split the sample into states with and without laws and still have contract and noncontract cities in both subsamples. Among no law states, the contract coefficient is modest but positive (.055). In the previous equations that included both the C and PC variables, the point estimates of the contract effect had been slightly negative. Among no law states, union departments do appear to earn a very modest positive salary differential. However, small differences in the PC variable between states in the no law sample do not significantly affect average state salaries. Among states with laws, the coef-

Table 3: Contract Effects in States Without Bargaining Laws
and States With Bargaining Laws^a

[Dependent Variable = Natural Logarithm of Average Police Department Salary]

	(1) No Law States	(2) Law States
Sample Observations	206	616
1. Contract (C_i)	.055 (.046)	-.023 (.022)
2. Percent Contract (PC_S)	.071 (.138)	.295*** (.038)
3. Other Control Variables	b	b
R^2	.523	.515

a - standard errors in parenthesis

b - other control variables are those listed in footnotes b and d in Table 2.

*** - indicates significance at the .10 level, one-tailed test;

** - indicates significance at the .05 level, one-tailed test;

* - indicates significance at the .01 level, one-tailed test.

ficients on C_1 (-.023) and PC_3 (.295) are both similar to the coefficients from the total sample reported in Table 2 (Column 4 specification).

In evaluating the coefficients from the no law sample, it should be noted that only fourteen of the 206 municipalities (6.8%) in no law environments in 1978 have contracts. In contrast, 130 of the 616 municipalities (21.1%) in states with laws in 1978 do not have contracts. The rare occurrence of a municipality having a contract in a no law state may well indicate that without the protection of a bargaining law and without some provision or mechanism for becoming organized, managers need not respond to employee demands for collective bargaining. This explanation is also supported by the fact that in nine of the fourteen cities that have contracts in no-law states, local legislatures adopted a local ordinance providing for collective bargaining the year before the negotiation of the first written contract. Interviews with managers from the other five municipalities indicated that those cities have a strong union influence in private sector employment that has made collective bargaining a widely accepted practice in the area. Weirton, West Virginia and Huntington, West Virginia are communities with strong influences from the steel workers and mine workers respectively. Pascagoula, Mississippi (the only municipality with a contract in a state in which police collective bargaining is explicitly illegal by state statute) has a number of craft unions representing workers in its dominant industry -- shipbuilding. Once these rare departments are able to obtain formal recognition and a written contract, the power of the union to raise wages would still appear to be quite limited. In these states, public managers do not respond to any demands for collective bargaining; in the rare

cases when formal bargaining does exist (usually with the protection of local ordinances), public managers do not appear to respond to any appreciable extent to demands during collective bargaining.

Longitudinal Analyses

While these initial cross-section results lend support to our model the question naturally arises: to what extent is the significant coefficient on PC evidence of a powerful union threat effect and to what extent does it reflect omitted state characteristics? Just as the unionization equations could suffer from problems of omitted variable bias, so too can equation 3. In particular, if $E(e_{1s}PC_s) \neq 0$, estimation of equation 3 will lead to an overstatement of the coefficient on PC_s . To deal with this problem we again hypothesize an omitted city-specific effect (which may be the same for all cities in a given state) in a cross-section equation for period 1.

$$\ln W_1 = \alpha_1 + \beta_1 \vec{X}_1 + \gamma_1 PC_1 + \mu_1 + u_1 \quad (4)$$

where $e_1 = \mu_1 + u_1$

\vec{X} = a vector state-level and municipal level control variable

so that $E(PC_1 e_1) \neq 0$, but that $E(PC_1 u_1) = 0$

By making use of data for a prior time period (period 0), one can difference out the effects of the omitted variables. Let the compensation equation for period 0 be represented:

$$\ln W_0 = \alpha_0 + \beta_0 \vec{X}_0 + \gamma_0 PC_0 + \frac{1}{\lambda} \mu_0 + u_0 \quad (5)$$

where $\frac{1}{\lambda}$ is a scaling factor that allows the effect of the omitted variable to be different in the two time periods. Subtracting equation (5) from equation (4), and rearranging terms, one obtains:

$$\ln W_1 = (\alpha_1 - \lambda \alpha_0) + \lambda \ln W_0 + \beta_1 (\bar{X}_1 - \bar{X}_0) + (\beta_1 - \lambda \beta_0) \bar{X}_0 + \gamma_1 (PC_1 - PC_0) + (\gamma_1 - \lambda \gamma_0) PC_0 + (u_1 - \lambda u_0) \quad (6)$$

In the equation (6) specification, the coefficient on the change in PC, γ_1 , provides a direct estimate of the union threat effect that is not biased by the possibly confounding effects of omitted variables. The equation (6) specification further allows the effects of control variables to be different in the two time periods.

When equation 6 is estimated, the results in Table 4 are obtained. Column 1 presents the results when 1965 data are used as the base year; column 2 uses 1973 data for the base year. The coefficient on the change in percent contract variable in column (1), is still positive and significant. The magnitude of the coefficient is only a little less than the magnitude of the coefficients shown in the Table 2 cross-section results. However, when the 1973 data are used for the base year in column 2, the magnitude of the coefficient on the change-in-percentage-contract variable is only .099 -- about one-half the magnitude of the column 1 coefficient, and only slightly larger than the corresponding standard error. Still, the point estimate of the coefficient is positive. The column 1 results suggest that wage growth from 1965 to 1978 was significantly stimulated by growth in state-level unionization over the period. The post 1965 unionization spurt did not occur in states that previously had high relative wages that were the result of some variable not captured by the model. However, the column 2 results suggest that the additional union growth that occurred in states after 1973 stimulated wage growth less. One possible

Table 4: Estimates of the Effect of State Unionization (PC)

Obtained From "Before-After" Methodology^a

[Dependent Variable: Natural Logarithm of 1978 Average Department Salary]

	(1)	(2)
Base Year Used	1965	1973
Observations	166	164
1. Change in Percent Contract (PC ₁ - PC ₀)	.181*** (.067)	.099 (.081)
2. Base Year Wage (W ₀)	.180*** (.090)	.201*** (.060)
3. Other Control Variables	b	b
R ²	.657	.699

a - standard errors in parenthesis

b - Other control variables include: changes in the natural logarithms of total revenue per capita, income per capita, and population; changes in contract status, and bargaining laws. The base year values of these variables are also entered as controls. Characteristics available only for one time period include: three region dummies, two government type dummies, a central city dummy, and percentage of the state population that is in public employment and the percentage of state employment unionized.

*** - indicates significance at the .10 level, one-tailed test;

** - indicates significance at the .05 level, one-tailed test;

* - indicates significance at the .01 level, one-tailed test.

reason is that the timing of unionization was selective with unions first organizing areas in which they could raise wages most. In both cases, the fact that PC variable is positively correlated with salaries or salary growth suggests that nonunion as well as unionized departments benefit from increased unionization in the state environment.

Union Threat Equation

According to the path model in Figure 1 of Section II, a city in a unionized environment can remain nonunion only by raising wages. A nonunion city that does not match union wage gains is in the unstable state depicted by path C of Figure 1. These cities run a particularly high risk of becoming organized. It is the strength of this threat that leads to the similarity between union and nonunion salaries in the "union states." This additional hypothesis of the path model suggests the following hazard function equation:

$$PR(C_{it} = 1/C_{i(t-1)} = 0) = f \left\{ \frac{W_{i(t-1)} - \bar{W}_{s(t-1)}}{\sigma_{(w)s(t-1)}} \right\} \quad (7)$$

where the expression on the right hand side of the equation is a standardized measure of the difference between the wage in nonunion city i and the average wage in state s.

To estimate equation (7) we calculated the number of standard deviations a particular department's salary is away from the mean salary in its own state (DEVSAL), then estimated a logistic equation for the probability of negotiating a first contract in 1978 (i.e., the hazard rate of unionization in 1978).¹² The independent variables include LAW, DEVSAL, and an interaction term LAW * DEVSAL. The results from this analysis are shown in Table 5. The logit coefficient on

Table 5: The Effect of Low Salaries Within a State
on the Probability of Becoming Unionized, 1978^a
[Dependent Variable: Contract]

Observations	368
1. LAW	3.273*** (1.037)
2. LAW * DEVSAL	-1.285* (.866)
3. DEVSAL	.865 (.835)
4. Other Control Variables	b
Log Likelihood	82.105

a - Assymptotically normal standard errors in parenthesis

b - Other control variables include those listed in footnotes b and d in Table 2.

*** - indicates significance at the .10 level, one-tailed test;

** - indicates significance at the .05 level, one-tailed test;

* - incicates significance at the .01 level, one-tailed test.

the law variable in line 1 reinforces the results from the unionization equation in Table 1. A municipality that does not have a contract at the start of the year is much more likely to get its first contract during the year if it is in a state with a LAW. From the logit coefficient in line 2 it appears that within states with laws, a municipality is more likely to become organized during the year if the police salaries it is paying at the beginning of the year¹³ are low relative to other police salaries in its state. From the logit coefficient on DEVSAL in line 3 of Table 3, it appears that paying relatively low salaries does not lead to any increased risk of unionization in no law sates. It would therefore appear that if a municipality in the states with bargaining laws do not continue to respond to the threat of unionization, the department is likely to begin to bargain collectively to raise salaries.

Interviews provided further evidence on the threat of unionization if salaries in nonunion departments slip below the level of union-salaries. For example, out of thirty-one municipalities from Minnesota in this sample, only Fairmont, Minnesota does not have a contract with its police department. In discussing the determination of police salaries in this town, a management representative of Fairmont said he has to match the salaries and benefits of the unionized departments, adding: "Any one of these years, if our police don't like the [compensation] package..., we're going to have collective bargaining." The threat effect also appears to operate when police unionization is at somewhat lower levels as well. A management representative from Centerville, Ohio (PC = .60 in 1978) wrote on our contract status survey: "Of all the cities with a population over 5,000 in the Dayton area, Centerville is the only one

without union representation for the police department. Management is constantly striving to keep salary levels and work procedures current so as to give no reason for the officers to seek formal representation." While this last response indicates that the threat effect in Ohio operates within regions or SMSAs (as suggested by Ehrenberg and Goldstein)¹⁴ the presence of a threat effect is certainly supported by such comments.

Fringe Benefits

One additional aspect of how public sector unionization affects the compensation determination process can be addressed with our data: the effect of the various measures of public sector unionization on fringe benefits. For 1978, we also obtained data on employer contributions to retirement and insurance programs. While detailed change-over-time analyses are not possible, the cross-section compensation equations of Section IV can be replicated using the natural logarithm of fringe benefits per employee as the dependent variable.

Table 6 presents the coefficients of the three measures of public sector bargaining, C_i , PC_S , and LAW , in fringe benefit equations. The column 1 specification replicates the basic average salary equation (Table 2, column 4), but with a slightly reduced sample. The change in sample again does not affect the basic result: states with high levels of unionization (PC_S) have relatively high salaries, but within states there is no significant difference between the salaries in contract and non contract cities. Column 2 estimates an equation for the determinants of total compensation (defined as salary plus fringe benefits). The PC coefficient in the column 2 total compensation equation is somewhat larger than the PC coefficient in the column 1 salary equation. This

Table 6: Effects of Unionization Measures on Natural Logarithm
of Fringe Benefits and Total Compensation, 1978^a
[N = 791]

(Dependent Variables In Natural Logarithms Except for Column 7 Specification)

	(1) Average Salary	(2) Total Comp.	(3) Fringe Benefits	(4) Fringe Benefits	(5) Fringe Benefits	(6) Fringe Benefits	(7) Fringe Benefits/ Total Comp.
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1. Contract (C _i)	-.005 (.020)	-.010 (.022)	.181*** (.058)	--	--	-.068 (.079)	-.003 (.007)
2. Percent Contract (PC _s)	.245*** (.035)	.271*** (.037)	--	.481*** (.087)	--	.544*** (.138)	.023*** (.013)
3. LAW	-.012 (.024)	.002 (.037)	--	--	.255*** (.072)	.010 (.094)	.011 (.009)
4. Other Control variables	b	b	b	b	b	b	b

a - standard errors in parentheses.

b - other control variables include those listed in footnotes b and d in Table 2

*** - indicates significance at the .10 level, one-tailed test;

** - indicates significance at the .05 level, one-tailed test;

* - indicates significance at the .01 level, one-tailed test.

would indicate a somewhat larger effect of state unionization on fringe benefits than on salaries. The results from the fringe benefit equation in columns (3) through (6) also support the hypothesis that state unionization measures have a larger effect on fringe benefits than on salaries. Absent any state-level measures of unionization, contract cities appear to pay significantly higher fringe benefits than non contract cities. Once PC is also included (column 6), one sees the same pattern observed in the salary equations in Table 2: municipalities in states with high levels of unionization pay significantly higher fringe benefits than do municipalities in generally "nonunion" states but no significant difference between fringe benefits paid in contract and noncontract municipalities. Moreover, the PC effect on fringe benefits (column 6) is nearly double the effect of PC on salaries. These results further suggest that cities in states with high levels of police unionization have compensation packages more heavily weighted toward fringe benefits than salaries. In column (7), the ratio of fringe benefits to total compensation is used as a dependent variable to illustrate this point more directly. The coefficient on PC_s is positive and significant. The magnitude of the coefficient suggests that when a state is entirely unionized ($PC=1.0$), municipal police departments in the state will have 2.3% more of their compensation package made up of fringe benefits than do police departments in virtually nonunion states ($PC=0.0$).

Conclusion

The model developed in this paper and the various empirical equations estimated to test that model suggest that the effect of unionization on compensation in the public sector depends on a number of environmental and institutional features:

1. In environments where unionization is rare in the public sector (i.e. in states which do not have legal provisions permitting bargaining), unionization has little impact on compensation. This principle is illustrated by the absence of any sort of union effect on compensation in 1965 in the United States when only 4.8% of all municipal police departments engaged in collective bargaining and only four states had any legal provisions for collective bargaining. This principle is also illustrated by the very modest impact of collective bargaining in 1978 in states without bargaining laws. In these environments, only 6.8% of all departments bargained (almost always with the protection of municipal-level bargaining ordinances), and the "union effect" on salaries here was 5.5%.
2. In environments where collective bargaining is the dominant mechanism by which compensation is determined (usually in states with protective legislation), unionization exerts a strong upward pressure on salaries on both union and nonunion departments. Because of the threat effect of unionization on salaries in nonunion departments, a simple comparison of salaries in union and nonunion departments does not reveal the full "union effect." Our cross-section estimates for 1978 provide an upper bound of the estimate of the effect of unionization on salaries in union and nonunion departments (29.7%). Our "before-after" estimates of this "union effect" are somewhat more modest (9.9% to 18.1%).

While police departments in highly unionized states pay much higher salaries than do departments in nonunion states, there appears to be no salary differential enjoyed solely by unionized departments in the highly organized states.

3. Finally, the "union effect" on fringe benefits appears to be even greater than the effect on salaries. The net result is that in the unionized states, a greater proportion of the larger compensation packages is paid in fringe benefit contributions to health and retirement plans.

The results here suggest the value of developing models specific to the public sector for analysing union wage effect rather than simply replicating models used in the private sector. They also point out certain interesting puzzles, which merit further attention. In particular, while bargaining laws almost always lead to increased municipal organization, there are still six states where bargaining policies have not lead to a majority of the states' municipalities being organized. While these six states (Indiana, Kansas, Louisiana, New Mexico, North Dakota, and Vermont) have "weaker" meet-and-confer sorts of laws, other states with these weaker provisions have seen significant increases in the proportion of police departments that bargain. And finally, if municipal organization generally follows the passage of state laws, how do we conceptualize the kind of union strength in the state that may lead legislators to enact such favorable statutes?

Footnotes

1. David Lewin, "Public Sector Labor Relations," Labor History, (Winter, 1977), p. 138.
2. Daniel J. Mitchell, "Collective Bargaining and Wage Determination in the Public Sector: Is Armageddon Really at Hand?" Public Personnel Management, Vol. 7(2), March-April 1978, p. 89.
3. Ronald Ehrenberg and Gerald Goldstein, "A Model of Public Sector Wage Determination," 2 Journal of Urban Economics 223, July 1975.
4. 1978 compensation data are from International City Management Association, "1978 Police Salary" Data Tape, Washington, D.C., 1978. 1973 compensation data are from International Management Association, "Personnel Policies in Municipal Police Departments," Washington, D.C., 1973. 1965 compensation data are from International City Management Association, Municipal Yearbook, 1965 (Chicago, Illinois: I.C.M.A.), 1965.
5. Ronald Ehrenberg, "Municipal Government Structure, Unionization, and the Wages of Firefighters," Industrial and Labor Relations Review, Vol. 27 (1), October, 1973, pp. 36-48.
6. Bureau of National Affairs, "Summary of State Labor Laws," Washington, D.C., 1978. Allen Gammage and Stanley Sachs, "Digest of State Legal Authority," Police Unions, (Springfield, Illinois: Charles C. Thomas), 1972. U.S. Department of Labor, Summary of Public Sector Labor Relations, 1978, supplemented by reference to texts of state laws, court cases and attorney generals' opinions. Additionally, for several cities in states

where no bargaining policies or laws exist, local ordinances permit bargaining. The LAW variable is coded as 1 in the cases.

7. Government-type and central city status data are from International City Managers Association, "Master Code" Data Tape, (Washington, D.C.: I.C.M.A), 1978. Crime rate and land area data are from U.S. Bureau of the Census City and County Data Book, 1972, (Washington, D.C.: G.P.O.), 1972. International City Management Association, "Master Code" Data Tape (Washington, D.C.: I.C.M.A.) provides population data; International City Management Association, Municipal Yearbook, 1978, (Washington, D.C.: I.C.M.A), 1978 provides data on per capita income and per capita city revenue. U.S. Bureau of the Census, City and County Data Book, 1972, and U.S. Bureau of the Census, City and County Data Book, 1967, provides data on average education, percentage black in population, income per capita, per capita revenue, and property per capita. U.S. Bureau of the Census, 1970 Census of the Population, Volume 1, (Washington, D.C.: G.P.O.), 1973, and U.S. Bureau of the Census, 1960 Census of Population, Volume 1, (Washington, D.C.: G.P.O.), 1964 provide data on craftsmen and kindred workers salaries.
8. Casey Ichniowski, "Bargaining Legislation and Public Sector Union Growth: The Case of Police," NBER Summer Institute Conference Paper, (Summer, 1982), Cambridge, Massachusetts.
9. Results in Table 1 and throughout the analysis are obtained using average departmental salary as the dependent variable. When the other salary measures are used, patrolmen's minimum salary and patrolman's

maximum salary, estimated coefficients are similar.

For a recent study examining the relationship of unionism to police compensation, see Ann Bartel and David Lewin, "Wages and Unionism in the Public Sector: The Case of Police," Review of Economics and Statistics, (February 1981), vol. 63, no. 1, pp. 53-59. While these researchers also employ a two-stage procedure in which the union variable is instrumented on other equations, their simple cross-section union coefficients using data on individuals in 1973 range from 2% to 4% -- similar to our cross-section union coefficient of 4.0% in 1973 (Table 2, column 6).

10. Recent evidence of this trend can be observed in William H. Baugh and Joe E. Stone, "Teachers, Unions, and Wages in the 1970's: Unionism Now Pays," Industrial and Labor Relations Review, Vol. 35 (3), April 1982, pp. 372-373.
11. One method for assessing multicollinearity is by assessing statistics in the "variance decomposition matrix" and "condition indices" associated with that matrix. These regression diagnostics were evaluated for the column (4) specification for the 1978 equation. These diagnostics are developed and described in David Belsley, Edwin Kuhn, and Roy Welsch, Regression Diagnostics: Identifying Influential Data and Sources of Collinearity, (John Wiley and Sons: New York, 1980), pp. 85-191. According to this discussion, the CONTRACT, LAW and PCT CONTRACT variables demonstrate a clear case of "competing collinearity."
12. Ideally, we would have also liked to use the early period data to track transitions into unionization for several years after paying relatively low

salaries. Unfortunately, the smaller samples for 1965 and 1973 contain larger cities (populations over 25,000) which according to our 1978 data are relatively high-paying cities within their own states. Therefore, this hazard function analysis is not performed with the data from the earlier period.

13. The salary statistics used in this study are those in effect on January 1 of the given year.
14. Ehrenberg and Goldstein, pp. 223-248.