
When Public Sector Workers Unionize

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The Effects of Public Sector Labor Laws on Labor Market Institutions and Outcomes

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In this paper we seek to determine the impact of labor laws on the collective bargaining status, wages, and employment of local government workers in the United States. We use the new data on state public sector labor laws described in detail in appendix B in this volume and information from the Survey of Governments (SOG) and the Current Population Survey (CPS) to examine how differences and changes in public sector labor laws across states and among departments in cities affect collective bargaining, wages, and employment.

The major finding is that state public sector laws are a prime determinant of the likelihood that municipal workers are covered by collective bargaining and have a moderate impact on the wages and employment of public sector workers. Comprehensive public sector labor laws raise the probability that workers are covered by collective bargaining contracts and, conditional on contracts, raise wages at the expense of employment. In addition, we find that employment and wages in otherwise identical departments are higher in those with collective bargaining contracts, supporting the notion that public sector unions raise demand for labor as well as increase wages along given demand curves (Zax 1985; Freeman 1986b).

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3.1 The Legal Environment for Public Sector Labor Relations

The legal environment for public sector labor relations changed greatly in the United States between the 1950s and 1980s. In the 1950s most states had no explicit legislation covering public sector workers, and the few laws that did exist outlawed strikes or bargaining. During the 1960s a large number of states enacted labor laws that legalized collective bargaining for different groups of public employees. In the 1970s many states amended these laws to impose a duty to bargain on governments, and often followed this with compulsory interest arbitration or, in some cases, right-to-strike provisions designed to resolve impasses in bargaining (see appendix B and Farber, this volume, chap. 5). Other states, by contrast, did not pass such legislation or in some instances enacted anti-union legislation.

For the purpose of analyzing the effects of these different legal settings on public sector labor markets, we develop an index of the favorableness of the state laws toward collective bargaining. Our index is based on provisions regulating bargaining rights and dispute resolution. In the area of bargaining rights, we categorize laws into five groups: bargaining prohibited; no provision for bargaining; bargaining permitted; "meet and confer" or "present proposals"; and duty to bargain. The bargaining prohibited category gives public employers recourse to the courts if workers form unions and try to negotiate over terms and conditions of employment. It is thus the least favorable legal environment for collective bargaining. The no provision for bargaining category is, however, close behind, as courts have often ruled that it also means that workers have no right to bargain collectively. The other legal categories treat collective bargaining more favorably: bargaining permitted allows bargaining but does not require employers to negotiate with workers; "meet and confer" or "present proposals" ensures that employers listen to unions though it still allows them to make unilateral decisions; finally, duty-to-bargain provisions are the most favorable to collective bargaining because they require employers to meet employee representatives at the bargaining table.

In the area of dispute resolution we distinguish between: nonbinding mediation and fact-finding mechanisms that call for a neutral third party to seek to resolve disputes without empowering them to fashion a settlement; compulsory interest arbitration, which gives the neutral party the right to determine the terms of agreement, guaranteeing closure of the process;¹ and laws that permit strikes, which are the traditional private sector mode for resolving bargaining impasses.²

The bargaining rights and dispute resolution laws form a hierarchy from least to most favorable to collective bargaining. We combine them into a single index for analysis. First, we divide state laws into the nine

categories shown in the first column of table 3.1, from bargaining prohibited at one extreme to strike permitted and compulsory arbitration at the other extreme.³ As can be seen in columns 2 and 3, there is a wide distribution of municipalities across the nine categories in the years for which we have observations. For reasons of parsimony we further summarize the laws with a single monotonic index of their favorableness to collective bargaining. Specifically, we associate with each legal environment in a city department a value from 9 (= most favorable to collective bargaining) to 1 (= least favorable); compute the mean and standard deviation of these numeric values across cities; and form a standardized Z score as our index measure. The advantage of standardizing categories in this way is that it allows us to simplify presentation of empirical results. The virtue of the Z score is that it gives more extreme values to categories that differ greatly from the mean in their rating and that are relatively rare. None of our results hinge, of course, on this particular way of summarizing the legal codes.

Column 4 of table 3.1 records the Z-score values that we associate with each law category given the distribution of municipal departments in column 2. To illustrate how to interpret the scores consider the movement from no provision ($Z = -1.19$) to duty to bargain with required mediation or fact-finding ($Z = 0.58$) or to arbitration ($Z = 1.29$),—both common changes in state law between the 1960s and

Table 3.1 Distribution of Survey of Governments (SOG) and Current Population Survey (CPS) Observations Across Legal Categories, by Collective Bargaining Coverage

Legal Category	% of Observation in Category		Z-Score Value
	SOG	CPS	
(1)	(2)	(3)	(4)
1. Duty to bargain and required arbitration	.14	.062	1.29
2. Duty to bargain and strikes permitted	.052	.12	.94
3. Duty to bargain and required fact-finding or mediation	.29	.28	.58
4. Duty to bargain	.040	.072	.23
5. Conferral rights and required fact-finding or mediation	.033	.022	-.12
6. Right to meet and confer or present proposals	.030	.020	-.48
7. Bargaining permitted	.15	.14	-.83
8. No provision for bargaining	.18	.14	-1.19
9. Bargaining prohibited	0.88	.14	-1.54
Number of observations	18,541	17,195	—

1970s. The first change is a 1.77 standard deviation improvement in the legal environment for collective bargaining; the second corresponds to a 2.48 standard deviation improvement. Given the frequency of these changes in the laws we will use a two standard deviation change in the legal index to evaluate the quantitative impact of legal changes on labor market outcomes.

3.1.1 Modelling Effects of the Legal Environment

There are two ways in which laws regarding collective bargaining can affect economic outcomes. First, they can affect outcomes by encouraging collective bargaining (Saltzman 1985; this volume, chap. 2; Ichniowski, this volume, chap. 1) and thus bear *indirect* responsibility for the bargaining-induced increases in wages (Lewis, this volume, chap. 6; Freeman 1986b) and employment (Zax 1985; Freeman 1986b). Second, given collective contracts, the laws can also affect outcomes *directly* by altering the results of bargaining and the decisions of non-union managements. This can occur by changing union power at bargaining tables and by creating greater or lesser threat effects on nonunion employers to match union wage gains.

To analyze the effects of public sector laws on outcomes we develop a model of union behavior in which the legal environment alters the resources the union expends to raise wages at the bargaining table. The model consists of:

i) A labor demand curve that the public sector union can shift through lobbying for greater public spending or other non-bargaining-table activity:

$$(1) \quad E = -\eta W + X + bRS,$$

where E = ln employment; W = ln wages; η = elasticity of labor demand; X = level of demand for labor; RS = resources devoted by union to lobbying or political activity to raise the demand for public services produced by union members. We assume for simplicity that this activity has a constant proportional effect on demand;

ii) A union objective function that depends on wages and employment:

$$(2) \quad U = U(W, E);$$

iii) A function relating wages obtained through collective bargaining to the resources devoted to bargaining (RB):

$$(3) \quad W = W(RB, L, S),$$

where L = the legal environment for collective bargaining, S = labor supply factors, and $RB + RS = R$, the total amount of resources available to the union.

To maximize utility the union divides its resources between bargaining and lobbying/political activity subject to (1) and (3) and the resource constraint. This yields the equilibrium condition that the union divide its resources to equate the marginal rate of substitution in utility to the relevant marginal opportunity costs:

$$(4) \quad U_w/U_e = \eta + b/W',$$

where U_w and U_e are the partial derivatives of the utility function with respect to wages and employment.

How does a more favorable labor law affect wage and employment outcomes in this model? If, as seems plausible, a legal environment favorable to collective bargaining increases the relative effectiveness of resources spent on bargaining as opposed to those spent on raising demand through the political process, W' will be greater at any given level of RB . Therefore, $\eta + b/W'$ will be smaller, inducing the union to shift resources to bargaining and thus to increase wages at the expense of employment. The law acts as if it reduced the elasticity of demand for labor. Since the actual elasticity remains unchanged, this has the consequence of lowering employment.

3.1.2 Reduced Form Estimating Equations

Rather than seeking to estimate a full-scale union maximizing model, we use the model in (1)–(4) as the framework for interpreting reduced-form employment and wage equations. The simplest reduced-form equations that we estimate have the log-linear form:

$$(5) \quad W = aX + cL + dS$$

$$(6) \quad E = a'X + c'L + d'S$$

In some calculations we also include a dummy variable for collective bargaining coverage.⁴ With this variable in the regressions, the coefficients on the legal index reflect the direct effects of the laws on outcomes as opposed to the “full” effects in (5) and (6).

3.2 Empirical Analysis

This section presents estimates of what the legal environment does to contract coverage, wages, and employment using data from the SOG for 1977–80 and data from the annual 1984 CPS for six local sector departments: police; fire fighters; sanitation other than sewerage; streets and highways; finance and general control; and teachers.

The SOG contains data on employment, wages, government finances, and diverse aspects of labor relations for municipal departments

in the United States. To determine whether a city department in the SOG has a collective bargaining contract, we made use of two pieces of information: the total number of contracts in the municipality and the number of bargaining units. When the number of contracts equals or exceeds the number of bargaining units we specify that each department with a bargaining unit had a collective agreement.⁵ When the SOG indicates that a city had no contracts, no bargaining units, and no collective bargaining policy, we infer that no departments have a collective agreement. These two rules enable us to specify the collective bargaining status of departments in 86 percent of the SOG sample. Departments in cities in which the data did not fit these rules have an ambiguous bargaining status, so we deleted them from the sample. By our procedure 21 percent of the sample of department-year observations were covered by collective bargaining. Finally, to take account of the diverse factors beyond state laws that affect municipal labor markets we supplemented the SOG data with detailed information on the economic and demographic characteristics of the populations of 1,153 U.S. cities from Summary Tape Files 1 and 3 of the 1980 Census of Population.⁶

The CPS May files contain information on the demographic and economic position of individual workers and whether they are union members or covered by collective bargaining contracts. In 1984 the file contained this information for the outgoing rotation group from each of the twelve monthly surveys of the year, which we used as our sample. A problem with the CPS is that it does not allow for the possibility that some public sector workers are union members but not covered by contracts, forcing us—like other researchers—to assume that all union members are covered. Because the CPS data are for individuals, moreover, they give greater weight to larger cities and departments than does the SOG. Since larger cities are more likely to be covered by collective contracts, the mean of the coverage variable in the CPS is considerably higher than the mean for the same occupational group in the SOG (see table 3.2).

3.2.1 The Relation of Laws to Coverage

The first issue to investigate is whether public sector laws favorable to collective bargaining are associated with greater contract coverage. If they are not, we would not expect them to have any impact on wages or employment.

To determine the effect of laws on contract status, we estimated linear probability equations linking a 0-1 contract coverage variable to our legal index and diverse controls for various public employee groupings in the CPS and SOG.⁷ In the CPS calculations our dependent variable takes on the value one if the worker is a union member or

Table 3.2 Regression Coefficients and Standard Errors (in parentheses) for the Effect of Laws on Collective Bargaining Coverage (CPS and SOG data)

<i>Panel A: CPS Cross-Section (1984)^a</i>				
	State Employees	Teachers	Police and Fire	Other Local
Legal index	.13 (.008)	.10 (.010)	.10 (.020)	.11 (.008)
Mean of 0-1 coverage variable	.39	.74	.75	.38
R ²	.23	.20	.25	.20
Number of observations	5,340	3,591	741	7,523

<i>Panel B: SOG Cross-Section (pooled sample, 1977–80)^b</i>					
	Police	Fire	Sanitation	Streets and Highways	Finance and Control
Legal index	.21 (.008)	.19 (.009)	.014 (.005)	.073 (.007)	.062 (.006)
Mean of 0-1 coverage variable	.40	.39	.052	.13	.073
R ²	.38	.32	.069	.17	.12
Number of observations	3,904	3,505	3,247	3,906	3,957

^aOther control variables are: dummy variables for educational attainment, age, region, female, black, city size; fire fighters in the police and fire regression; and alternative wages in the individual's SMSA.

^bOther control variables are: population (and interactions with three city-size dummies), per capita income, median household income, median property values, percent of population with income below 75% of poverty level, percent black, percent high school graduates, percent with 1 to 3 years college, percent college graduates, percent attended graduate school, region dummies, and year dummies.

covered by a collective contract. In the SOG calculations it takes the value one if the city department has a collective contract according to the procedure described earlier.

Panels A and B of table 3.2 summarize the results of these calculations for the two data sets. While there are differences in the magnitudes of the estimated coefficients on the legal index, both panels tell the same story: they show a significant positive relation between the favorableness of the public sector labor law to collective bargaining and contract coverage. In the CPS calculations the estimated coefficients on the legal index range from .10 to .13, implying that a two standard deviation improvement in the laws (roughly from no laws to duty to bargain or arbitration) would increase the probability of having a collective contract by 20 to 26 percentage points. In the SOG calculations the estimated coefficients vary more widely, from .19 and .21 for police and fire fighters to .07 and .06 for streets and highways, and finance and control, to a bare .01 in sanitation. With the sole exception of

sanitation, the estimated effects are quantitatively large relative to the mean coverage. Finally, if the SOG data are pooled into a single sample and dummy variables added for departments, the average effect of the legal index is .12 (column 1 of table 3.3), which is on the same order as the effects found in the CPS data.

The strong relation between laws and the presence of collective contracts that underlies these regressions is shown in figure 3.1, which reports the percentage of city departments or workers in city departments covered by contracts in different legal settings. The coverage proportions range from two-thirds of city departments in the most favorable category to virtually zero in the least favorable, and from three-fourths of workers in city departments in the most favorable category to 19 percent in the least favorable category. Because, as we noted earlier, the CPS data code all workers who are members of unions or associations as having contracts, the 19 percent is undoubtedly a substantial overestimate of the true proportion in that category. The data bias works to minimize the relation between the laws and coverage, making the observed patterns even more striking.

3.2.2 Probing the Law-Collective Bargaining Relation

Should the positive relation between the favorableness of public sector labor law and collective bargaining shown in table 3.2 and figure 3.1 be interpreted as causal, or might it be due to the effect of some omitted variables? One possible explanation of the results is that favorable laws encourage unionization, which leads to collective bargaining, but beyond stimulating workers to organize, the laws then have no further impact. If this were the case, we would expect the estimated coefficient on the laws to disappear if we controlled for union membership in the regressions. Accordingly, we added another labor relations variable from the SOG to our regressions: the percentage of full-time workers who are members of a union or employee association. As can be seen by comparing columns 1 and 2 of table 3.3, addition of this variable reduces the estimated impact of the legal index on coverage from .12 to .07. Still, .07 represents a substantial and statistically significant effect of a change in the legal index on coverage, which implies that even where union and association membership is high, a strong bargaining law serves to legitimize the collective bargaining process. Put differently, worker support for unionism is not enough to guarantee collective bargaining.

A second noncausal interpretation is that the positive relation between public sector labor laws and collective bargaining results from spurious correlation due to the omission of a city- or state-specific variable (call it pro-union sentiment) that is correlated with both the laws and bargaining. To test this explanation we controlled for city

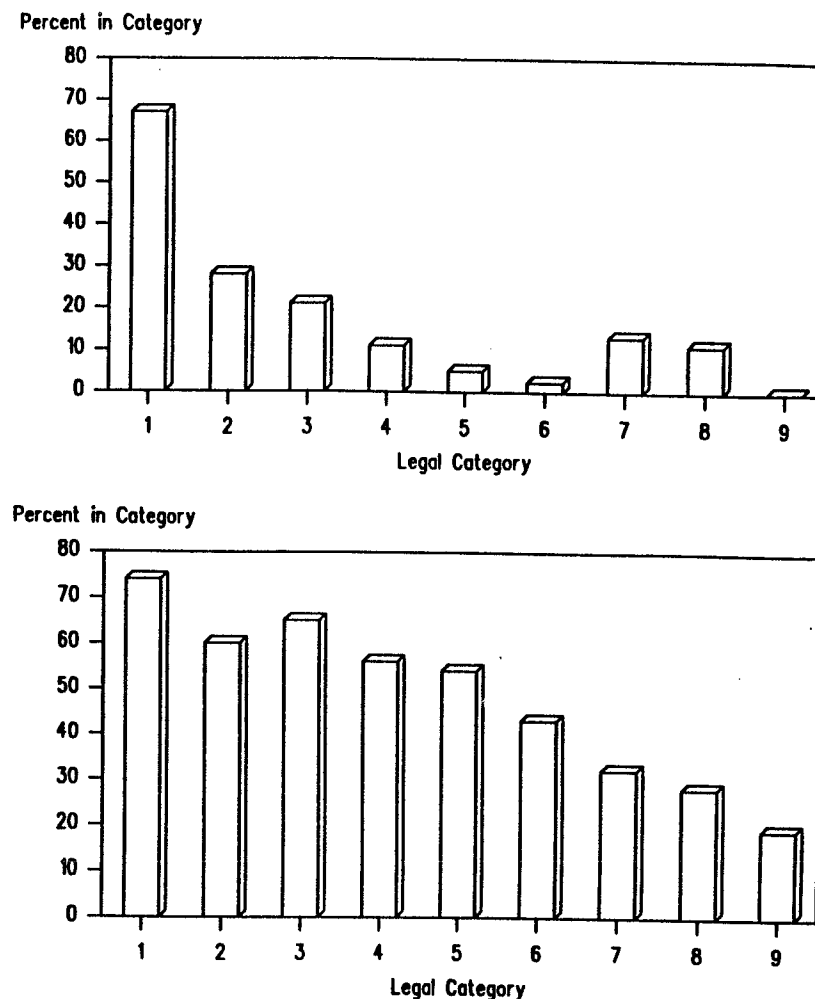


Fig. 3.1

Percent in category of government departments that are covered by collective bargaining (SOG data) (*top*); percent in category of workers that are covered by collective bargaining (CPS data) (*bottom*).

Legal categories are represented by numbers as follows:

- 1 = Duty to bargain and Required arbitration
- 2 = Duty to bargain and Strikes permitted
- 3 = Duty to bargain and Required fact-finding or mediation
- 4 = Duty to bargain
- 5 = Conferral rights and Required fact-finding or mediation
- 6 = Right to Meet and confer or Present proposals
- 7 = Bargaining permitted
- 8 = No provision for bargaining
- 9 = Bargaining prohibited

Table 3.3 Regression Coefficients and Standard Errors^a for the Effect of Laws on Collective Bargaining Coverage, Controlling for Percent Organized in the Department (SOG data)

	Pooled, 1977-80		Within City Analysis ^b	
	(1)	(2)	(3)	(4)
Legal index	.12 (.004)	.071 (.004)	.11 (.010)	.097 (.010)
Percent union members in department	—	.42 (.008)	—	.20 (.009)
Department dummies	yes	yes	yes	yes
City dummies	no	no	yes	yes
Mean of 0-1 coverage variable	.23	.23	—	—
R ²	.29	.41	.37	.40
Number of observations	13,744	13,744	11,612	11,612

Note: Other control variables are: population (and interactions with three city-size dummies), per capita income, median household income, median property values, percent of population with income below 75% of poverty level, percent black, percent high school graduates, percent with 1 to 3 years college, percent college graduates, percent attended graduate school, region dummies, and year dummies.

^aStandard errors in parentheses.

^bStandard errors in columns (3) and (4) are corrected to adjust for inclusion of city-specific control variables. In addition, the models in columns (3) and (4) include interactions between the demographic controls and the department dummy variables.

effects that influence all occupations by taking dependent and independent variables as deviations from city means, which is equivalent to including city dummy variables in the regressions. In these calculations we also included interactions between department dummy variables and measured city characteristics to allow measured control variables to affect different departments differently.

The results of our calculations, given in columns 3 and 4 of table 3.3, show that controlling for city effects has little impact on the estimated coefficient on the legal index, and thus the relation between laws and bargaining cannot be attributed to an omitted city factor. With no control for the percent organized, the coefficient on the legal index in the within-city analysis is comparable to that in the pooled regression, whereas after controlling for the percent organized, it is actually larger. Employee groups governed by more favorable collective bargaining laws are more likely to win contracts than employee groups governed by less favorable laws *in the same city*. We conclude that the relation between the legal environment and contract coverage is more likely to reflect causal factors than to be a spurious correlation due to omitted variable bias.

3.2.3 The Relation of Laws to Wages and Employment

What about the impact of the laws on market outcomes? Do wages and employment differ in departments operating under different collective bargaining laws? To answer these questions we estimated reduced-form equations relating wages to the legal environment in the CPS (table 3.4) and relating wages and employment to the legal environment in the SOG (table 3.5). In both cases we estimated three regressions for each occupation group. The first regression includes the legal index and control variables, but excludes a measure of whether workers are covered by collective bargaining. The coefficient on the legal index thus reflects the direct and indirect effects of the laws as described earlier.

The second regression in each set includes collective bargaining coverage as an additional independent variable. Since coverage is fixed, the coefficient on the legal index variable measures the direct effect of the laws on outcomes. While, as we have seen in figure 3.1, the legal environment and contract coverage are closely connected, there is sufficient variation in the data to allow us to disentangle the effects of the two variables.

The third regression in each set adds a coverage-legal index interaction term to allow for the possibility that the indirect effect of laws is different for workers who gain a contract and those who do not. Each regression also includes the full set of controls described in the table notes. The CPS regressions are limited to wage equations because data on individuals do not permit analysis of the effect of laws on departmental employment.

Turning to the estimates, the first regressions for each group in table 3.4 show that the legal environment has a statistically significant but moderate impact on ln hourly earnings for all groups. The regression coefficients on the legal index are on the order of .03, which translate into roughly 6 percent earnings differentials between states with no laws and those with favorable laws, given the approximately two standard deviation difference between the categories. The second regressions in the table show that much of this is due to the intervening coverage variable: the coefficients on the legal index variable are roughly halved in each case, while collective bargaining is estimated to raise wages by .12 to .15. This is consistent with Lewis's generalization that collective bargaining has sizeable impacts on wages at the local level (Lewis, this volume, chap. 6). Still, the legal index remains significant in all cases, with coefficients that suggest direct effects on earnings of 3 to 4 percent. Finally, the coefficients for the interaction terms in the third regressions in the table range from insignificantly negative to significantly positive, and thus give no indication of a differential effect of labor laws on organized and non-organized workers.

Table 3.4 Regression Coefficients and Standard Errors* for the Effect of the Legal Environment and Collective Bargaining Coverage on Ln(Hourly Wage) (CPS, 1984)

Group	Legal Index	Coverage	Coverage-Legal Interaction	R ²	N
State employees	.033	—	—	.36	5340
	(.008)				
	.014	.15	—	.37	5340
	(.008)	(.013)			
Teachers	.003	.15	.033	.37	5340
	(.009)	(.013)	(.033)		
	.029	—	—	.24	3591
	(.009)				
Police and fire fighters	.017	.12	—	.25	3591
	(.009)	(.015)			
	-.009	.14	.036	.25	3591
	(.015)	(.016)	(.016)		
Other local employees	.033	—	—	.30	741
	(.016)				
	.018	.14	—	.33	741
	(.016)	(.030)			
Other local employees	.038	.14	-.033	.33	741
	(.024)	(.030)	(.028)		
	.037	—	—	.35	7523
	(.007)				
Other local employees	.020	.15	—	.37	7523
	(.007)	(.010)			
	.015	.15	.015	.37	7523
	(.008)	(.011)	(.011)		

Note: Other control variables are: dummy variables for educational attainment, age, region, female, black, city size; fire fighters in the police and fire regressions; and alternative wages in the individual's SMSA.

*Standard errors in parentheses.

Panel A of table 3.5 reports the results of estimating the effect of the legal index on wages for city departments in the SOG data, pooled across departments and years. The estimated impact of the index is .04, which is of comparable magnitude to the estimate in the CPS data. The estimated impact of collective bargaining coverage is, however, noticeably smaller—.06 versus a range from .12 to .15—and its addition to the equation has a more modest impact in reducing the coefficient on the legal index. As in the CPS calculations, the interaction term is negligible, suggesting that state labor laws have a similar impact on the wages of workers with and without collective contracts.

Panel B of table 3.5 turns from the effect of laws and collective bargaining on wages to their effect on employment. In these calculations the dependent variable is the log of full-time departmental em-

Table 3.5 Regression Coefficients and Standard Errors* for the Effects of the Legal Environment and Collective Bargaining Coverage on Wages and Employment (SOG pooled cross-section data, 1977–80 (n = 18,382))

Panel A: Ln (Monthly Salary per Full-Time Employee in Department)			
Legal index	.039	.032	.032
	(.002)	(.002)	(.002)
Coverage	—	.058	.057
		(.004)	(.004)
Legal-coverage interaction	—	—	.002
			(.004)
R ²	.64	.65	.65
Panel B: Ln (Number of Full-Time Employees in Department)			
Legal index	-.056	-.082	-.099
	(.006)	(.006)	(.007)
Coverage	—	.24	.21
		(.014)	(.015)
Legal-coverage interaction	—	—	.076
			(.014)
R ²	.70	.71	.71

Note: Other control variables are: population (and interactions with three city-size dummies), per capita income, median household income, median property values, percent of population with income below 75% of poverty level, percent black, percent high school graduates, percent with 1 to 3 years college, percent college graduates, percent attended graduate school, region dummies, year dummies, and department dummies.

*Standard errors in parentheses.

ployment,⁸ while the independent variables are the same as in the pay regressions. Here we obtain a surprising result: the estimated coefficient on the legal index is substantial and negative, implying that employment is smaller when laws are more favorable to collective bargaining, whereas the effect of collective bargaining on employment is significantly positive, consistent with Zax (1985).

Why do strong labor laws, which affect wages in the same direction as collective bargaining contracts, have the opposite effect on employment? The positive coefficient on the interaction of collective bargaining coverage and legal enactment in the interaction regression offers one possible answer, for it indicates that the negative relation between the laws and employment occurs largely among city departments that do not sign contracts. If a favorable legal environment induces nonunion cities to raise wages to avoid unionization but does not create pressures for additional government services, one would expect the higher wages to reduce employment. Similarly, if favorable laws raise union power more at the bargaining table than in the political or lobbying arena, as

in our model of union behavior, they will induce greater wage increases at the expense of employment under collective bargaining. Put differently, under these interpretations the legal index will measure movements along a demand curve to a greater extent than shifts in labor demand due to union (or nonunion worker) political activity, and thus have negative or nonpositive effects on employment. Since the wage effects of the legal environment are estimated to be about the same in cities with and without contracts, however, this explanation requires the following: either the elasticity of labor demand must be greater in noncovered city departments (as in the private sector, Freeman and Medoff 1981; Allen 1983), or more favorable legal environments must enhance the ability of unions to shift out the demand curve, reducing the negative impact of higher wages on employment.

The second possible interpretation of the employment results is that they are spurious, due to an incomplete specification of the determinants of public sector employment. Perhaps employment is lower in noncovered city departments because these cities have an especially low demand for public services, keeping employment low and limiting the power of unions to obtain contracts.

3.2.4 Probing the Relation of Laws to Wages and Employment

To probe the estimated impact of laws on pay and to examine the possible causes of the inverse relation between the laws and employment, we performed two additional calculations designed to eliminate the potential impact of omitted city variables on the regressions. First, we added city dummy variables to the SOG wage and employment regressions,⁹ so that the estimated coefficients on the legal index and collective bargaining reflect the within-city effect of differences in the laws and outcomes among departments. Second, we made use of the 1972 SOG data file to perform a longitudinal analysis of the same city departments over time as state public sector labor laws changed.

Table 3.6 presents the results of our within-city calculations. The dependent variable in panel A is the difference between the log of pay in a department and the average pay in a city for all five departments in the sample. The dependent variable in panel B is the difference in the log of employment in a department and the average employment in the city for all departments. In each panel the independent variables also relate to differences between the variable for a department and the city average. In addition, we allow for likely differences in the effects of city characteristics on departments by including interaction terms between department dummy variables and those characteristics. Controlling for city effects in the wage equation greatly reduces the coefficients on the legal index and on collective bargaining; in the

Table 3.6 Regression Coefficients and Standard Errors^a for the Effect of the Legal Environment and Collective Bargaining Coverage on Wages and Employment Controlling for City-Specific Effects (SOG pooled cross-section data for 1977–80 (n = 13,960))

Panel A: Ln (Monthly Salary per Full-Time Employee in Department)			
Legal index	.009 (.004)	.008 (.004)	.008 (.004)
Coverage	—	.008 (.004)	.008 (.004)
Legal-coverage interaction	—	—	-.003 (.012)
Panel B: Ln (Number of Full-Time Employees in Department)			
Legal index	.030 (.020)	.004 (.020)	.002 (.020)
Coverage	—	.24 (.019)	.24 (.019)
Legal-coverage interaction	—	—	.019 (.047)

Note: Other control variables are: population (and interactions with three city-size dummies), per capita income, median household income, median property values, percent of population with income below 75% of poverty level, percent black, percent high school graduates, percent with 1 to 3 years college, percent college graduates, percent attended graduate school, region dummies, year dummies, department dummies, and interactions between the city characteristic variables and four department dummies.

^aStandard errors in parentheses, corrected for inclusion of controls for city-specific effects.

employment equation it reverses the sign on the coefficient on the legal index but has little impact on the estimated effect of collective bargaining compared to our earlier cross-section results. While these changes may be taken as evidence of a significant omitted city-factor bias in the cross-section regressions, they also can be interpreted as reflecting spillover effects across departments within cities that greatly reduce the estimated impact of the laws and of collective bargaining in within-city comparisons. This interpretation is consistent with the Zax and Ichniowski findings of substantial within-city spillovers of wages (this volume, chap. 12).

Table 3.7 presents the results of our longitudinal analysis from 1972 to 1980. Because the SOG did not collect good data on collective bargaining coverage for 1972 our analysis is limited to changes in the legal index over the period. In this eight-year interval approximately 40 percent of our sample changed legal categories, with most of the changes taking the form of movements from simple duty-to-bargain

provisions to arbitration, and from meet-and-confer provisions to duty-to-bargain statutes. The regressions we use for our analysis are derived from the following two-equation system:

$$(7) \quad Y_1 = a + bL_1 + cX + \lambda D + u_1$$

$$(8) \quad Y_0 = a' + bL_0 + c'X + D + u_0$$

Here, Y is the dependent variable, wages or employment; D is an omitted city-department variable that is expected to bias cross-section regressions; u_1 and u_0 are independent disturbances; L and X are defined as in eq. (1) and (3). The subscript 1 relates to 1980 and the subscript 0 relates to 1972. This specification imposes similar coefficients on the legal index in the two periods but allows coefficients on the omitted city-department factor and on the control variables (which are available only for one time period and thus have no time subscript) to differ over time. Solving for D in (8) and substituting in (7) yields our estimating equation:

$$(9) \quad \Delta Y = a - \lambda a' + bL_1 - \lambda bL_0 + (c - \lambda c')X + (\lambda - 1)Y_0 + (u_1 - \lambda u_0)$$

in which the omitted city-department factor has been eliminated.

Table 3.7 contains estimates of equation (9) for pay and employment.¹⁰ Panel A gives the coefficients when the dependent variable is the change in pay. The regression shows that the 1980 legal index variable has a positive impact on wages of .024, which is somewhat smaller than the .039 obtained in the comparable table 3.5 regression but still non-negligible and statistically significant. If the specification is correct, the coefficient on the 1972 legal index should be opposite in sign to that on the 1980 legal index, and roughly equal in magnitude to the coefficient on the 1980 index multiplied by one plus the coefficient on the lagged wage term, as is roughly the case. Dividing the departments between those that were and were not covered by collective contracts in 1980 shows similar results.

Panel B of the table gives the coefficients when the change in employment is the dependent variable. Here, we find moderate negative effects for the 1980 legal index in all the regressions, with the separate regressions for city departments by contract status showing greater negative effects to laws without contracts, consistent with the cross-section results given in table 3.5. Note, however, that the 1972 legal index has the same, rather than the opposite, signed impact on employment, suggesting that a more complex model with lagged employment responses is needed to capture the variation in the data. If one assumes that the negative coefficient on the lagged legal index reflects

Table 3.7 Regression Coefficients and Standard Errors^a for the Effects of the Legal Environment and Collective Bargaining Coverage on Wages and Employment (Longitudinal model controlling for department-specific effects; SOG data for changes between 1972 and 1980)

Panel A: $\Delta \text{Ln}(\text{Monthly Salary per Full-Time Employee in Department})$			
	Full Sample (N = 5281)	Covered ^b Departments (N = 1044)	Not Covered Departments (N = 3474)
Legal index 1980	.024 (.004)	.028 (.008)	.021 (.005)
Legal index 1972	-.002 (.005)	.002 (.008)	-.013 (.007)
Ln(1972 wages)	-.66 (.012)	-.62 (.026)	-.69 (.015)
Panel B: $\Delta \text{Ln}(\text{Number of Full-Time Employees in Department})$			
	Full Sample (N = 5281)	Covered Departments (N = 1044)	Not Covered Departments (N = 3474)
Legal index 1980	-.037 (.010)	-.029 (.016)	-.037 (.014)
Legal index 1972	-.010 (.013)	-.016 (.017)	-.029 (.019)
Ln(1972 employment)	-.31 (.010)	-.40 (.022)	-.31 (.012)

Note: Other control variables are: population (and interactions with three city-size dummies), per capita income, median household income, median property values, percent of population with income below 75% of poverty level, percent black, percent high school graduates, percent with 1 to 3 years college, percent college graduates, percent attended graduate school, region dummies, and department dummies.

^aStandard errors in parentheses.

^bRefers to departments covered by contract in 1980; coverage data for 1972 are unavailable.

time delays, one would add the coefficients on the two legal index variables to get a full impact of the legal environment.

3.2.5 Arbitration versus Permitting Strikes

The analysis thus far has focused on the relation between the index of the legal environment for collective bargaining and economic outcomes. In states that have enacted duty-to-bargain legislation, the policy-relevant question relates to the more specific issue of the impact of alternative dispute resolution laws on outcomes; specifically, on whether compulsory arbitration or strike-permitted laws raise pay. What does

our data tell us about the effects of these provisions on wages and employment in duty-to-bargain states?

To answer this question we estimated wage and employment equations analogous to those given earlier on a sample of departments with duty-to-bargain or stronger bargaining laws in 1980, with dummy variables for arbitration or strike-permitted legislation replacing our legal index. The results of these calculations are summarized in table 3.8 in terms of the coefficients on the key legal category dummy variables. Columns 1 and 2 record the results when the dependent variable is ln wages, while columns 3 and 4 record the results when it is ln employment. The principal finding in the table is the marked difference between the estimated impact of arbitration and strike-permitted laws. In the cross-section analysis, departments covered by compulsory arbitration laws appear to have somewhat lower wages than other depart-

Table 3.8 Regression Coefficients and Standard Errors (in parentheses) for the Effect of Arbitration and Strike-Permitted Laws on Wages and Employment (SOG cross-section [pooled, 1977-80] and longitudinal data [1980-1972], Duty-to-bargain sample¹)

	Dependent Variable			
	ln Wages		ln Employment	
	Cross Section (1)	Longitudinal (2)	Cross Section (3)	Longitudinal (4)
Arbitration 1980	-.023 (.005)	.005 (.010)	-.034 (.019)	-.027 (.027)
Strikes permitted 1980	.014 (.007)	.032 (.015)	-.21 (.027)	.080 (.041)
Arbitration 1972	—	-.014 (.012)	—	-.044 (.032)
Strikes permitted 1972	—	-.061 (.024)	—	-.21 (.062)
ln Wages 1972	—	-.69 (.017)	—	—
ln Employment 1972	—	—	—	-.26 (.013)
Sample size	11,396	2,922	11,396	2,922

Note: Other variables controlled for in each regression are: population (and interactions with three city-size dummies), per capita income, median household income, median property values, percent of population with income below 75% of poverty level, percent black, percent high school graduates, percent with 1 to 3 years college, percent college graduates, percent attended graduate school, region dummies, and department dummies. Also, the cross-section regressions include year dummies.

¹The sample only includes departments in legal categories 1-4 (see table 3.1) in 1980.

²Refers only to departments covered by a collective bargaining contract in 1980. Coverage data is unavailable for 1972.

ments. In the longitudinal analysis, departments covered by compulsory arbitration in 1980 have essentially the same pay as other departments, while those that were also covered by arbitration in 1972 are estimated to have slightly lower pay than other departments. At the least these calculations reject the notion that arbitration laws create pressures for higher pay in covered jurisdictions—a result consistent with other findings that compare collectively bargained and arbitrated settlements (Ashenfelter and Bloom 1984; studies cited in Freeman 1986b). By contrast, both the cross-section and longitudinal analyses suggest that pay is modestly higher under strike-permitted laws: in column 1 the coefficient on strike-permitted laws for 1980 is .014; in column 2 strike permitted in 1980 has a positive effect on ln pay, whereas strike permitted in 1972 has a negative effect, as expected given the difference equation. As for employment, the evidence is more mixed: the cross-section and longitudinal calculations show modest negative effects for arbitration laws on employment, but show drastically different effects of strike-permitted laws—negative in the cross-section analysis and positive in the longitudinal analysis—that suggest the need for more detailed investigation. Setting aside the employment results as ambiguous, the conclusion to be drawn from table 3.8 is that *arbitration laws have effectively no impact on pay, while strike-permitted laws raise pay.*

3.2.6 CPS versus SOG Results on Contracts

While it was not the purpose of this study to estimate the impact of union contracts on wages, the surprising difference in the estimated contract effect between the CPS data set in table 3.4—coefficients on coverage of .15—compared to those in the SOG data set—coefficients on coverage of .05—deserves further attention. What might explain the difference? Does collective bargaining in the local public sector raise pay as much as is suggested by the CPS or as little as suggested by the SOG?

We have explored several possible reasons for the divergent estimates of the collective bargaining effect between the two data sets. One possibility is that the CPS data give greater weight to large cities than the SOG data and that coverage effects vary by size of city. To assess this we re-estimated the table 3.5 wage equation weighting the department observations by city size. The estimated coverage coefficients decreased rather than increased, which indicates that union effects are larger for smaller cities, contrary to our hypothesis.¹¹ A second possibility is that the estimated coefficients differ because the data sets cover different occupations. To see if this is the case, we estimated our cross-section wage equation in the SOG for the two occupations found in both data sets, police and fire, and other local employees,¹²

and obtained estimates of coverage effects of .06 and .05, respectively, well below the CPS-based estimates in table 3.4. A third possibility is that the results differ because the SOG sample covers the years 1977–80, whereas the CPS sample covers 1984. To test this possibility we estimated the impact of coverage on wages using the May 1980 CPS. Because this file has fewer observations than the 1984 annual file, we estimated coverage effects for occupations with relatively large numbers of persons: teachers, state employees, and other local employees. The 1980 regression coefficients on coverage exceed the 1984 coefficients for the same groups, rejecting this explanation.¹³ In sum, we are unable to account for the difference in estimated coverage effects between the two data sets. Whatever the explanation, however, the message is clear: given the estimated divergence in estimates one should look at both in assessing the impact of collective bargaining on wages and be careful not to mix them in evaluating changes in union wage effects over time.¹⁴

3.3 Conclusion

This study has found that state labor laws have significant effects on collective bargaining and wage and employment outcomes in the public sector. While there are ambiguities in interpreting some of the empirical relations in the data, the evidence tends to support the following four claims:

1. State laws are a major determinant of whether workers obtain collective bargaining contracts, even after controlling for their union status and for unmeasured city-specific factors.
2. Laws favorable to collective bargaining produce higher wages by encouraging bargaining relations and by creating an environment in which covered and noncovered workers make wage gains. More favorable laws are, however, associated with lower employment, primarily in departments lacking collective bargaining contracts, which we attribute to the departments paying higher wages to avoid unionization.
3. Collective bargaining for local government workers is associated with higher wages and greater employment. The latter is consistent with models of public sector unionism that stress the lobbying and political activities of unions designed to increase demand for public services produced by members.
4. Within cities wage and employment differences between departments covered by more and less favorable public sector laws are relatively modest, consistent with the notion that there are sizeable spillovers of wage and employment decisions across department lines in a city.

Our analysis also raised questions about the magnitude of estimated effects of collective bargaining on wages, which is markedly smaller in the department-based SOG data set than in the individual-based CPS data set for reasons that we were unable to determine.

Notes

1. We do not distinguish between various forms of interest arbitration such as conventional, issue-by-issue last-best-offer, or package last-best-offer.
2. Some laws that outlaw strikes contain specific penalties while others allow courts to decide penalties. As it is difficult to determine which penalties are in practice more severe, we have not attempted to subdivide these laws further.
3. It is unclear whether strike permitted or compulsory arbitration should be viewed as the most favorable category for collective bargaining. In table 3.1 we classified compulsory arbitration as most favorable, but none of our results depend on this choice.
4. Equations (5) and (6) do not allow for supply or demand factors having different effects in cities with and without collective bargaining contracts. As these factors presumably operate differently in the two environments, one might expect them to have different coefficients, and in some of the empirical work we estimate separate equations for covered and uncovered cities to allow for this.
5. For finance and control departments (for which there was no bargaining unit data) we used the data for clerical employee bargaining units.
6. We thank Jeffrey Zax for providing us with this data extract.
7. Given the large samples that we are using it would have been expensive to do logit or probit equations, with little potential gain. As the dependent variables have means well within the 0–1 interval in all samples, we are unlikely to run into serious functional form problems using the linear model.
8. Use of full-time equivalent employment yielded essentially identical results to those reported below.
9. In fact, given the number of cities, we calculated city-specific means for all variables and performed regressions with the difference between a variable in a city and its mean, as in the within-city regressions in our table 3.3.
10. Least squares estimates of equation (9) may still yield inconsistent parameter estimates as the residual u_0 is negatively correlated with Y_0 . The coefficient on Y_0 will be biased downward, biasing the coefficient on L_0 as well. While there is no simple correction for this bias unless one is willing to develop a much more complex model, calculations given in Freeman and Valletta (1987) show it to be of negligible magnitude under plausible assumptions.
11. Separate wage equations for departments in cities of different sizes showed the same pattern. In cities with populations over 500,000 the coverage effect is 0.00; in cities with populations between 250,000 and 500,000 it is 0.019; in cities with populations between 50,000 and 250,000 it is 0.015; and in cities with populations less than 50,000, it is 0.071.
12. The other local category differs between the data sets, as there are only three such groups in the SOG: sanitation, streets and highways, and finance and control, while the CPS contains a wider range of departments.

13. The estimated coefficients (standard errors) for 1980 are: 0.21 (0.09) for state employees; 0.19 (0.04) for teachers; and 0.22 (0.07) for other local employees. The latter results are not strictly comparable to the 1984 regressions because prior to 1983 the other local group is confined to public administration employees.

14. Freeman (1986a) reports similar inconsistencies between estimates of changes in union wage effects between CPS- and establishment-based surveys, while Freeman (1985) reports inconsistencies between estimates of public/private sector pay differentials between CPS- and establishment-based surveys of federal employees. Hence, there is growing evidence of inconsistencies between wage differentials based on the CPS and those based on other data sets.

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Comment Harvey S. Rosen

The paper by Freeman and Valletta is an econometric investigation of the ways in which different legal environments affect the outcomes of public sector labor markets, where outcomes include: 1) obtaining a contract, 2) levels of unemployment, and 3) wages.

Whatever the model eventually chosen, in order to estimate the effect of the "legal environment" one must be able to measure it. Freeman and Valletta (F&V) devote section 3.1 of the paper to this issue. In this paper, the legal environment encompasses the requirements for bargaining, the provisions for dispute resolution, and strike provisions. Within each category, F&V can order provisions according to how much they constrain the scope of public sector union activity. They then rank all the possibilities along a single dimension. This hierarchy

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is described in their table 3.1. To turn this table into a number for each jurisdiction, F&V perform a Z-score transformation of the nine categories.

An alternative strategy would have been to use a system of dichotomous variables to represent the nine categories. There are over a thousand observations, so degrees of freedom would not be a problem. Tests on whether legal environment "matters" would then just be tests for the joint significance of the system of dummies. One advantage of such a procedure is that it would yield quantitative estimates of the impacts of the various arrangements. The other advantage is that it is more familiar to most economists than Z-tests.

F&V use two main sources of data for their analysis, the Current Population Survey and the Survey of Governments. The former is well known to labor economists, but a few words about the latter may be useful. The Survey of Governments is a rich source of data providing almost everything one would want to know about the components of jurisdictions' budget constraints. The main problem that I have had using it arises because various legal jurisdictions can overlap geographically. Consider, for example, a town which pays taxes to a "special district" whose responsibility is to provide education. The town's payments to the special district may be categorized as "intergovernmental grants," and the town's education expenditures recorded as a zero! Unfortunately, the data set provides no obvious way of determining whether such a situation is likely to be important for a given community. It does suggest, however, that some care should be taken in "cleaning" the data, and it would be nice to hear what steps were taken by F&V. (Perhaps such considerations help explain the difficulties that F&V have in reconciling the results from the two sets of data.)

F&V motivate their estimating equation by setting up a model in which the maximand is the union's objective function, which depends on the wage rate and employment. This is maximized subject to the community's demand curve for labor. The twist relative to more conventional models of wage-employment determination in unionized industries is that the union can commit resources to shift out the demand curve for its services. On the other hand, it can also devote resources to raising its wages via collective bargaining; hence, another constraint in the problem is that total union resources must sum to some pre-specified amount.

The equations actually estimated are the reduced form of this model. Thus, they estimate:

$$W = a_W X + b_W C + c_W L + d_W S + e_W CxL,$$

$$E = a_E X + b_E C + c_E L + d_E S + e_E CxL,$$

where E = employment, X = demand variables, S = labor supply variables, L = F&V's measure of the legal environment, and C = a dichotomous variable indicating whether or not there is coverage by collective bargaining. The interaction variables are present to allow for the possibility of "threat effects."

I can think of another way in which the union might affect the economic environment. It might lobby the state and/or federal governments for grants. This suggests that it might be interesting to estimate a grants equation in addition to those for W and E . It also suggests that treating grants as exogenous demand shifters may create econometric difficulties.

It is important to note that the wage variable W does not take into account the accrued value of pensions. In the current context, this point may be of importance for two reasons. First, local managers and politicians may find that the easiest way to deal with the demands of unions is to promise them more money in the future—when someone else will be in charge. Although the evidence is mixed with respect to whether pension underfunding is correctly perceived by current citizens (who might have to pay the price via capitalization), my guess is that this is an important consideration. This also suggests that some measure of the likelihood that current taxpayers will also be future taxpayers might have a role in the model. (Perhaps this could be measured by the proportion of the population that moves away from the community.) Second, if pension practices were more or less uniform across communities, then this issue wouldn't matter very much as a practical matter. However, Frant and Leonard (1984)¹ provide some estimates that there are large cross-community differences. Indeed, different occupations within a community can be treated very differently.

F&V's choices for the X and S variables seem altogether sensible. The only important omission is the community's tax price for local goods and services. In the median voter framework, this is determined in part by the property tax rate, the ratio of the median voter's house value to community property values, and the median voter's marginal federal income tax rate. Presumably, the lower the tax price, the greater the demand for local goods and services, and hence the greater the demand for public sector employees. Some people have argued that local officials' resistance to the elimination of deductibility is due to the fact that it would reduce public employment. Inclusion of such a variable would help shed light on this important issue. Whether the tax price is correlated with the C and L variables, and thus would affect their coefficients, I do not know.

1. Howard Frant and Herman Leonard, "State and local government pension plans: Labor economics or political economy." Memo, John F. Kennedy School of Government, Harvard University, 1984.

F&V estimate the equations using a variety of techniques, with alternative groups of right-hand-side variables. They make a rather strong case that their substantive results are robust. Among these results are that collective bargaining coverage raises wages *and* employment, which is consistent with the notion that public sector unions are able to shift out the demand for labor. However, while a more favorable legal environment leads to higher wages, it does not have much impact on employment in communities that are covered by collective bargaining. F&V explain this by arguing that the main effect of the favorable legal environment is to strengthen the union's position at the bargaining table, which increases its wage rate and moves it back along the demand curve.

F&V's essay joins the growing list of papers suggesting that political and legal institutions do matter in the analysis of a community's economic decisions. The paper will contribute to the debate on suitable ways to quantify such institutions and measure their impact.