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THE EFFECT OF PUBLIC SECTOR LABOR LAWS ON COLLECTIVE BARGAINING, WAGES, AND EMPLOYMENT

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ABSTRACT

This paper examines the effect of the different legal environments for bargaining faced by public employees across the states on wage and employment outcomes for union and nonunion employees, and also on the extent of bargaining, using cross-section, within-city, and longitudinal analyses based on a newly-derived data set on public sector labor laws.

We find that: (1) the legal environment is a significant determinant of the probability of collective bargaining coverage; (2) collective bargaining coverage raises wages and employment for covered employees; (3) a more favorable legal environment increases wages for all employees, but substantially reduces employment for employees not covered by a contract, while slightly reducing employment for employees who are covered by a contract. We also find evidence of significant spillovers of union wage effects to non-covered departments. We conclude by focusing on the effects of two specific legal provisions - arbitration and strike permitted clauses - on wages and employment.

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Unionism in the public sector of the United States differs in two fundamental ways from unionism in the private sector. First, in the American federal system states enact separate laws to regulate public sector labor relations in different departments, creating vastly different legal environments for collective bargaining by state and department. Provisions range from prohibitions on bargaining to duty-to-bargain requirements; the latter are often combined with impasse resolution procedures such as compulsory arbitration or strike permitted clauses. Even within a particular state, different public employee groups are frequently covered by different provisions. Second, because of the political context of public sector labor relations, public sector unionism is likely to affect a very different set of economic outcomes than private sector unionism. Whereas private sector unions are usually viewed as raising wages, thereby reducing employment, public sector unions are best thought of as increasing demand for the services of union members, raising employment, public output, and taxes, as well as wages.

This paper uses new data on labor relations law by state and department from the NBER Public Sector Collective Bargaining Law Data Set, together with data from the Survey of

Governments and the Current Population Survey, to examine the economic relationships between these two distinct features of U.S. public sector unionism. It seeks to determine in what ways, if at all, differing legal environments affect the outcomes in public sector labor markets, where outcomes include obtaining a contract and levels of employment as well as wages. It also examines whether legal environments favorable to public sector collective bargaining affect outcomes largely through increasing the probability jurisdictions end up with collective contracts or largely through affecting the terms of contracts.

The principal finding of the paper is that the legal environment has significant direct and indirect influences on the economic outcomes of public sector labor markets. The indirect effects occur because legal environments favorable to bargaining increase the likelihood that a city-department is covered by a collective contract. The direct effects, defined as the impact of the law on outcomes holding fixed coverage, differ from the effect of contract coverage: whereas coverage raises wages and employment,[1] a legal environment favorable to bargaining raises wages but tends to reduce employment in the relevant department. A plausible interpretation is that coverage measures both the outward shift in demand for public sector labor due to lobbying and the results of collective bargaining, while the collective bargaining law variable reflects more what unions can do at the bargaining table and thus their ability to raise wages along demand curves, with consequent reductions in employment. We also find evidence for significant "spillovers" of union effects on non-covered departments across and within cities, with wages higher and employment lower in departments without collective bargaining contracts in states with strong collective bargaining laws.

I. Measuring the Legal Environment for Public Sector Labor Relations

In the 1950s, the legal environment for public sector collective bargaining was, save for the exception of a few cities and states, largely undefined; where specific public sector legal provisions existed, they either outlawed strikes or bargaining, or provided the "right to work" to non-union employees. In ensuing years, the environment changed dramatically. In the sixties there was a substantial body of legislation legalizing public sector unionism. A second wave of legislation in the seventies imposed a duty to bargain on many public sector employers and developed procedures to resolve impasses -- either by mandatory arbitration or, in some cases, by allowing strikes. By the 1980s, the legal environment was drastically different than it had been 20-30 years earlier, with many states having enacted laws highly favorable to public sector collective bargaining while others had unfavorable laws.

How might the legal environment affect public sector labor market outcomes?

A priori, laws regulating public sector collective bargaining might be expected to influence outcomes indirectly by encouraging unionisation and the eventual signing of collective contracts and directly by influencing the outcomes of bargaining itself.

With respect to the "indirect effects" there is a sizeable literature indicating that the legal environment exerts an influence on unionisation in the public sector.[2] As the literature on public sector unionism has generally found that signed contracts have more of an impact on outcomes than unionism per se, we expect a favorable legal environment to affect outcomes through increasing the probability that bargaining units are established and agreements signed. Unfavorable laws, by contrast, are expected to have the converse effect, with laws that prohibit bargaining having especially large negative effects on the existence of contracts.

In addition to influencing the prevalence of contracts, however, a favorable legal environment is likely to alter outcomes directly. Unions are likely to do better at the bargaining table with a "duty to bargain" provision, which requires public employers to meet and bargain in good faith with elected union representatives over terms and conditions of employment, than with weaker collective bargaining legislation, and may do best when such provisions include impasse resolution procedures. Whether they do better with "strike permitted" or "compulsory arbitration" clauses is, by contrast, less obvious. Initially, most public sector unions favored the right-to-strike; more recently, attitudes have changed. From the perspective of states, the question is not only whether these clauses benefit unions in bargaining but how they affect settlements in non-union cities as well. Comparison of settlements reached through compulsory arbitration with results from negotiated settlements in the same legal environment is unlikely, as Farber and Katz (1979) have noted, to yield good estimates of how arbitration affects outcomes because arbitration is likely to bias negotiated settlements away from the settlements that would have been negotiated in the absence of the arbitration law. The same can be said of the situation for strike laws. Hence, to evaluate the overall effects of the legal environment it is necessary to make contrasts across environments, as we do in this study.

The Data Set

To measure the legal environment for public sector collective bargaining, the NBER, building on the work of earlier analysts (the Department of Labor, the American Federation of State, County, and Municipal Employees, John Burton, Berkeley Miller, and Joyce Najita),[3] put together in 1985 a comprehensive data set on collective bargaining laws across states for five basic groups: state employees, municipal police, municipal fire fighters, non-college teachers, and other local employees. The data set, described in detail in Valletta and Freeman (1985), covers five basic legal categories: Contract Negotiation (bargaining rights), Union Recognition, Union Security, Impasse Procedures, and Strike Policy for the years 1955-1984. Depending on the ultimate application, one can select certain variables from this data set to measure the legal environment facing unions. We focus on three main categories likely to affect economic outcomes: the requirements for bargaining; the provisions for dispute resolution; and strike provisions. Within each of these categories, the legal provisions can be ordered from those which most constrain the scope of public sector union activities to those which allow the broadest scope for such activities and hence provide the greatest probability of obtaining a collective bargaining contract and influencing outcomes.

In the area of bargaining rights, we distinguish between five types of provisions, ordered from least to most favorable toward public sector union activity: prohibition of bargaining; no provision on bargaining; bargaining permitted; right to "meet and confer" or "present proposals;" and duty-to-bargain. The bargaining prohibited category <u>requires</u> public employers to reach a unilateral decision on the terms and conditions of employment. The other categories allow public employers to make unilateral decisions but permit bargaining, with express permission encouraging bargaining more than does the implied permission of "no provision," while "meet and confer" or "present proposals" requirements ensure that unions have a voice in determining the terms and conditions of employment. Finally, duty to bargain is the most favorable provision for unions, guaranteeing that public employers will meet them at the bargaining table.

In the area of dispute resolution, we distinguish between non-binding intervention mechanisms (mediation and "fact-finding") and binding arbitration. The non-binding mechanisms each involve a neutral third party (individual or group) whose role is to investigate and provide information about the disputed issues (fact-finding) or simply attempt to conciliate the parties to the dispute (mediation); but in neither case is the third party empowered to force a settlement. In binding arbitration, the neutral third party's decision must be adhered to; we make no distinction here between conventional "fact-finding"-type arbitration and final offer variants. Even with non-binding intervention, risk-averse public employers are more likely to deal with unions than in the absence of potential intervention. Binding intervention assures a signed contract.

Finally, in the area of strikes, we distinguish between states which permit strikes and those that expressly prohibit strikes. Most states fall into the former category. Many attach specific penalties for individuals and unions that violate the law; others leave this decision to the discretion of the courts. Since it is difficult to know in which case the penalties are more severe, we have grouped all strike prohibition laws into one category. Where strikes are permitted, unions can force a contract from public employers; in addition, unions may be able to utilize strikes and the strike threat to obtain more favorable wage and employment outcomes.

These laws are of course closely interrelated; for simplicity we have chosen to represent them by a single hierarchical index. Specifically, we associate with each departmental group in each municipality in a particular year a single figure indicating the legal scope for public sector union bargaining activities in the state in which the municipality is located. This hierarchical ordering is shown in table 1. At the low end of our hierarchy are groups covered by "bargaining prohibited" provisions; these groups are allowed no scope for union activities. As we move toward the top of the hierarchy, there is an increase in unions' ability to create and exploit bargaining power. First, we have no provision on bargaining, then bargaining permitted, followed by the right to meet and confer, and required mediation or fact-finding; then we get to bargaining categories, where employers are required to bargain in good faith with employee representatives; the addition of closure properties in the bargaining process, in the form of strike permitted provisions or binding arbitration, ensures a signed contract.

Table 1: Distribution of Survey of Governments and Current Population Survey Observations Across Legal Categories, by Collective Bargaining Coverage

Legal Category	Z Score Value	Fraction of SOG Obs. in Category	Fraction Covered of SOG Obs. in Category	Fraction of CPS Obs. in Category	Fraction Covered of CPS Obs. in Category
9. Duty to Bargain & Required[1] Arbitration	1.29	.14	.67	.062	.74
8. Duty to Bargain & Strikes Permitted	.94	.052	.28	.12	.60
7. Duty to Bargain & Required Fact-Finding or Mediation	.58	.29	.21	.28	.65
6. Duty to Bargain	.23	.040	.11	.072	.56
5. Conferral Rights & Required Fact-Finding or Mediation	-.12	.033	.049	.022	.54
4. Right to Meet and Confer or Present Proposals	-.48	.030	.023	.020	.43
3. Bargaining Permitted	-.83	.15	.13	.14	.32
2. No Provision for Bargaining	-1.19	.18	.11	.14	.28
1. Bargaining Prohibited	-1.54	.088	.001	.14	.19
Number of Observations	-	18,541	3884	17,195	8160

1 The term "required" in this table indicates that the impasse procedure is initiated automatically at some point in the impasse or by request of at least one of the parties.

As a means of scaling our index, we have performed a z-score transformation of the nine categories, so that for each state-department-year we have a measure of the standard deviations from the mean legal category across states for all departments. The main advantage of this scaling technique, which exploits the standard normal distribution, is that it is sensitive to the number of groups in a legal category. A legal environment which is particularly rare (such as "bargaining prohibited") causes the z-scores to have more dispersion than they would if "bargaining prohibited" was as common as the other legal categories; this is consistent with the view that a "bargaining prohibited" environment is more qualitatively distinct than other environments. The z-score values corresponding to each legal category are shown in table 1. As an example of how this variable should be used, a change from "no provision" (category 2) to "duty-to-bargain & strikes permitted" (category 8) represents a 2.13 standard deviation change in a department's legal environment; similar calculations can be made for other changes in the legal environment. As the change from "no provision" to "strikes permitted" was quite typical of states which adopted bargaining legislation in the 1960s and 1970s, in our empirical work we will use a two standard deviation change in the legal environment as the base unit for assessing the impact of the law on labor market outcomes.

To analyse the effects of the laws on economic outcomes we have added our law variables to two data sets which contain information on public sector workers. The first is the Current Population Survey (CPS), which is the Census Bureau's main monthly survey of individuals. It has information on economic and demographic characteristics of workers, and contains data on whether a worker is a union member or is nonunion but covered by a contract. Unfortunately, these data fail to allow for the situation in which workers who are union members do not have a contract - an infrequent but not unheard of situation in the public sector. The second data set - which is more useful for our purposes - is the annual Survey of Governments (SOG), also conducted by the Census Bureau. It contains data on government employment (including various measures of unionism), wages, and finances across all levels of government but the federal, with detailed data by municipal department, making it an excellent source of data to test our hypotheses on the effects of the legal environment on coverage, wage, and employment outcomes. We analyse an extract containing data on 1153 cities in the United States for five municipal departments: police, fire, sanitation other than sewerage, streets and highways, and finance and general control personnel (grouped together) for the years 1972-1980;[4] however, since some data are unavailable in some years, we do not have a complete 9-year panel. We also have data for the same cities from the 1980 Census Summary Tape Files 1 and 3, which provide a wide range of economic and demographic characteristics by city to use as control variables.

Our principal unionism variable is the collective bargaining coverage of the department. As no coverage data is available by department on the Survey of Governments, we had to impute this variable from other information. Our estimate was obtained by looking at whether or not a city has a collective bargaining policy, the number of contractual agreements in effect in the city, and the number of existing bargaining units. Where the number of contracts exceed or are equal to the number of bargaining units, each department with a bargaining unit is considered covered.[5] If the city does not have a collective bargaining policy, or there are no bargaining units in the city, or there are no contracts in effect, each department in the city is considered not covered. In this way, coverage was imputed for 86% of cities; the 14% of the pooled sample for whom we could not make such imputations were not used in the analysis. Since we only have consistent bargaining unit data for the years 1977-1980, most of our analysis is conducted on a pooled sample of the five departments across the 1153 cities for those four years. Approximately 21% of the total pooled sample were covered by collective contracts. The distribution of our legal measure across departments and states in the two data sets is given in columns two and four of table 1. The Survey of Governments data record the proportion of city-department-years in our various categories. The table reveals a wide variation, with significant numbers of city-departments or workers in most of the nine groupings. The CPS data show a similar pattern for the distribution of individuals.

As our analysis seeks to differentiate between the indirect and direct effects of legal environments on outcomes, it is important to note that city-departments and individuals with and without collective bargaining contracts are found under all of the various legal environments. Columns three and five of table 1 relate our nine legal environment categories to the existence of a collective contract in the SOG and CPS data. Considerable "off-diagonal" variation is demonstrated. There are departments and individuals without contracts in states with legal environments favorable to collective bargaining and departments and individual with contracts in states with unfavorable legal environments.

Given the differing legal environments and the existence of departments and individuals with and without collective contracts in favorable and in unfavorable environments, what economic outcomes is the environment likely to affect?

II. Outcomes of Public Sector Union Activity

Following the standard "monopoly face" model of unions in the private sector most studies of public sector unionism focus on the effect of those unions on wages (see Freeman, 1986b; Ehrenberg and Schwarz, 1985; Lewis, this volume). While obviously valuable in understanding one aspect of union impacts, the concentration on wage effects can lead to an understatement of what public sector unions do and an incorrect analysis of their welfare consequences.

The political context of public employment creates a distinct environment for labor relations in which unions can influence not only wage levels but also the overall demand for labor and public sector output in a jurisdiction. Public sector unions can directly affect the goals of elected officials (or their representatives) sitting across the bargaining table by campaigning for, or against, those officials. While public sector unions can, and sometimes do, campaign directly for higher pay, they are likely to do better in the political arena by supporting candidates favorable to public spending in areas where their members work. There are numerous examples of such activity. Teachers campaign for increased school expenditures and services. Policemen favor candidates who want to spend more on law and order. Firemen favor candidates likely to increase expenditures on fire protection. That teachers are concerned with quality of education, policemen with law and order, and firemen with adequate fire protection, as well as with the effect of expenditures on those activities on their pay, we do not doubt. The point is not that these groups are trying to "trick" voters into supporting additional expenditures to increase pay but that increases in demand for services is the easiest way to obtain increases in pay.

Not only does the political process provide public sector unions with greater opportunity to alter levels of demand for services of their members than the market process permits private sector unions to do, but it also necessitates such behavior in order for collective settlements to be funded. In the public sector -- unlike the private sector -- collective negotiations do not guarantee the funding of contracts: so-called legislative vetoes can vitiate bargains, as legislatures or councils refuse to raise the money to fund signed contracts. For example, in the 1970s, despite signed contracts college professors in the University of Massachusetts system did not receive salary increases for several years because the legislature did not allocate the funds. Taxpayer revolts, as evidenced in Proposition 13 (California) and 2 1/2 (Massachusetts), have also been used by opponents of public spending to limit potential union wage gains by capping tax revenues or budgets. Hence, public sector unions must operate in a wider sphere than do private sector unions. They must convince voters to fund increased budgets, which invariably will include more than just wages, whereas in the private sector they have only to gain agreement with management at the bargaining table. The terms "multilateral bargaining" and "end run bargaining" are commonly used to refer to the situation in which public sector unions bargain not simply with those across the table from them but with other interested public parties as well. In such bargaining, need for public services, public expenditures, and quality of services, as well as wage packages, are often at stake.

The hypothesis that public sector collective bargaining induces unions to seek to alter the demand for labor as well as wages has several implications for evaluating the economics of public sector labor relations. First, if unions shift demand for services, they can <u>increase</u> rather than reduce employment, potentially raising total expenditures for a given department and taxes. Their economic effects on local governments may be much greater than indicated from application of the standard monopoly model analysis of union effects on wages. Second, to the extent that unions succeed in raising demand for public services as well as raising wages, a very different welfare calculus must be used to evaluate their social impact. In particular, whereas the normal welfare calculus of monopoly unionism stresses the misallocation of labor due to <u>reduction</u> of employment in the union sector, the appropriate analysis of public sector unionism may have to examine changes in welfare due to <u>increased</u> employment and output. When public sector output is below the social optimum (due to inaccurate revelation of preferences for public goods, for example), the welfare effects are likely to be positive. When public sector output is above the social optimum (due to "special interest" pressures, for example) the welfare effects are likely to be negative.

All told, if the argument that public sector unionism significantly affects demand for public output is correct, current belief in the relatively modest impact of those unions, based on wage studies, will have to be re-evaluated.

The Role of Legal Regulations

Given that public sector unions are likely to shift demand for labor schedules as well as bargain for higher wages along a given schedule, how might our measure of the legal environment influence unions' ability to shift demand as opposed to altering the wage and employment settlement on a demand curve? Since our measure focuses on the extent to which the environment is favorable to collective bargaining settlements, it seems likely that it will have a greater effect on wages than on the level of demand. For example, there is nothing in a "strikes permitted" or "compulsory arbitration" clause in a duty-to-bargain law that could be expected to increase the political power of unions, as these laws are distinctly oriented toward the outcome at the bargaining table. Hence, we expect the legal environment to have a relatively greater impact on wages than on the level of demand, and thus on wages as opposed to employment, per se.

Formally, to model the economic impact of public sector unions and the direct impact of the legal environment we postulate:

(1) A Demand Curve, which can be shifted by union lobbying and other non-bargaining table activity:

$$E = -\eta W + aX + bR_s$$

where

E = log (employment)

W = log (wages) with elasticity of labor demand $-\eta$

X = log (level of demand due to other factors)

R_s = log (resources spent by union to shift demand)

(2) A union objective function, dependent on wages and employment, $U = U(W,E)$

(3) A function relating union wage gains in bargaining to the resources devoted to bargaining, $W = W(R_w, L, S)$, where R_w = resources devoted to bargaining, L = legal factors that alter the effectiveness of bargaining resources, and S = labor supply factors that alter union effectiveness. For simplicity we assume that the resources spent by the union to shift demand have a constant proportional impact (b) on demand whereas the resources used at the bargaining table have declining marginal productivity ($dW/dR_w>0$ and $d^2W/dR_w{}^2<0$), and where $dW/dL>0$, $dW/dS<0$, and $d^2W/dLdR_w>0$.

(4) A resource constraint on unions, $R_w + R_s = R$.

The union problem is to maximize (2) subject to (1), (3), and (4). It does this by selecting an appropriate level of R_w according to the following equilibrium relation:

$$(5)\ U_1/U_2 = (\eta W' + b)/W' = \eta + b/W'$$

That is, the union divides its resources so that the marginal rate of substitution in utility is equated to the relevant marginal opportunity costs.

Given the unions' selection of R_w (and R_s with the fixed resource constraint), the model yields wage and employment levels as functions of the factors affecting demand (X) and those that alter the effectiveness of unions in bargaining (L and S):[6]

$$(6)\quad W = f(X,L,S);\ E = g(X,L,S)$$

To analyse the effect of the legal environment on the union allocation of R and outcomes, we consider the "substitution effect" when a favorable legal environment alters the relative effectiveness of R_s and R_w by changing W' and thus b/W'. When a more favorable environment increases the relative ability of unions' to raise wages at the bargaining table, b/W' falls; when it decreases the relative ability of unions' to raise wages at the bargaining table, b/W' rises. Thus, the law acts as if it was shifting the elasticity of demand. If, as we assume, our measure of the favorableness of the law to collective bargaining has a greater impact on union's ability to raise wages at the bargaining table, there will be a tendency for the law to induce a shift toward wages, as opposed to employment. Note, moreover, that since in this model unionization does not affect the elasticity pe se, a given wage increase will reduce employment according to η, not η + b/W'.

Rather than seeking to estimate a union utility function in our empirical work, we focus on the reduced form of the model, contrasting log-linear versions of employment and wage equations in union and nonunion settings. In the nonunion setting we assume that employment and wages are set by the interaction of supply and demand schedules, yielding reduced form equations comparable to those in (6), though with different interpretations on the coefficients. In the simplest version our estimated model is of the following form:

$$(7)\ W = a_W X + b_W C + c_W L + d_W S$$

$$(8)\ E = a_E X + b_E C + c_E L + d_E S$$

where C = the 0-1 collective bargaining coverage variable; L = our measure of the legal environment; and S reflects labor supply factors, which affect nonunion settings via normal supply and demand interactions and which affect union settings through the effectiveness of the bargaining equation. Here b_E and c_E reflect the full impact of coverage and the law on employment; they could be negative if the induced reduction in employment due to wage increases counterbalance any increase in employment due to a shift in demand. Similarly, the coefficients b_W and c_W reflect the full effects of coverage and the law on wages; they are expected to be positive.

Finally, to allow for possible "spillovers" of legal effects from covered to non-covered departments we also employ an interaction model much like that suggested by H. Gregg Lewis:

$$(9) \quad W = a_W X + b_W C + c_W L + d_W S + e_W CL$$

$$(10) \quad E = a_E X + b_E C + c_E L + d_E S + e_E CL$$

where the coefficients e_w and e_E reflect interactions. In the Lewis analysis, use of a coverage variable alone yields a biased estimate of the true impact of unionism on wage and employment outcomes, since it fails to account for union "threat" effects on non-covered groups. In our formulation, the legal environment is used to identify departments for whom this "threat" effect is important. To identify separately the "full" effect of unionism (without threat effects), the threat-adjusted union effect, and the threat (legal) effect, it is sufficient to interact and include separately the legal and coverage variables, and to have covered and not-covered departments in all of our nine legal categories. The latter requirement has been demonstrated in table 1; our model is constructed to meet the former.

Finally, note that equations (9)-(10) do not allow for supply or demand factors beyond the law to have differential effects in union and nonunion environments. Since the supply and demand factors operate through different routes in the two settings one might expect them to have different impact coefficients, with in the extreme case some variables affecting outcomes in one setting but not on the other. While we will test for such interactive effects (reported in note 18), our focus is on the overall law and collective bargaining coefficients, justifying the simplifications in these equations.

III. Estimates of the Effect of the Legal Environment and Contract Coverage on Labor Market Outcomes

In this section we present our estimates of the effects of the legal environment on labor market outcomes. First, we show that contract coverage is closely related to the legal environment; then we examine the effect of the legal environment on employment and wages, by department, with/without controls for coverage. Next we compare departments within a city by inclusion of city dummies and then use a longitudinal (before-after) design to deal with the potential problem of omitted department-specific variables. We conclude with an examination of the effects of arbitration and strike permitted clauses on wages and employment.

Coverage

Table 2 reports the results of linear regression model estimates of the relation between the legal environment and collective bargaining coverage, using CPS data for 1984, and SOG data for 1977-1980. The CPS regressions were performed on four public employee groups: state employees, teachers, police and firefighters, and "other local" employees, with workers in managerial occupations deleted since managerial employees are typically excluded from coverage by public sector collective bargaining law. The Survey of Governments sample includes observations for the five municipal employment groups described earlier (police, fire, sanitation other than sewerage, streets and highways employees, and finance and general control personnel)[7] across a sample of 1153 cities with populations greater than 10,000 (in the 1980 Census) for each of the years 1977-1980. In each regression the dependent variable takes the value 1 if the department or individual is covered by a collective bargaining contract, and the value 0 if not.[8] In general, models with dichotomous dependent variables are best estimated with probit or logit models since each constrains the fitted values to lie between 0 and 1, while the linear probability model does not. However, the linear model performs well where the dependent variable has a mean which is bounded well away from the endpoints of the (0,1) interval, as is the case for most of the coverage variables in our sample. The linear model is also computationally less expensive than the non-linear models, particularly in large samples. We therefore use the linear model in our coverage estimations.

The top panel of table 2 records the results of our analysis for the CPS; the bottom panel gives the results for the SOG. As can be seen in the table, the legal index has a significant positive effect on the probability of coverage in each cross-section linear model. In the CPS calculations the estimated coefficients range from approximately .10 to .13. Thus, a two standard deviation increase in the legal index increases the probability of coverage by about 20-26 percentage points (as noted in Section I, we use a two standard deviation change as our base to assess the impact of changes in the legal environment). Because the means of the variables are roughly equi-distant from one-half this also implies approximately equal logistic coefficients.[9] The SOG coefficients range from .014 to .21, roughly parallel to the range in coverage rates; the implied logistic coefficients, while wider in dispersion than in the CPS, again imply large positive effects for the legal index on the probability of coverage, with the exception of sanitation workers.

An important question to ask is whether or not the law per se increases collective bargaining coverage or whether it

Table 2: Regression Coefficients and Standard Errors (in parentheses) for the Relation Between the Legal Environment and Collective Bargaining Coverage, CPS and SOG Data

	CPS Cross-Section (1984)[1]			
	State Employees	Teachers	Police & 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	5340	3591	741	7523

Other variables controlled for in each regression are: dummy variables for educational attainment (4), age (5), region (3), female, black, city size (2), and firefighters in the police and fire regression, and alternative wages in the individual's SMSA.

[1] The CPS file used includes outgoing rotation group observations from each of the 12 monthly samples for 1984.

	SOG (Pooled Sample, 1977-80)				
	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	3904	3505	3247	3906	3957

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 (3), and year dummies (3).

simply reflects strong unionism in the area. To test this we include in our coverage regressions a variable measuring the percentage organized by department (i.e., the percentage of full time employees in the department who are <u>members</u> of a union or employee association). The results of this calculation for the SOG data set are shown in table 3. For simplicity in presentation we use a pooled sample for all departments, which yields coefficients intermediate between those for the departments in table 2 (see column 1). Inclusion of the percentage organized in column 2 shows that percentage organized has a large effect on the probability of coverage, but its inclusion does not eliminate the effect of the law, indicating that <u>even where union and association membership is high, a bargaining law serves to legitimize the bargaining role of public sector unions</u>. This is consistent with the findings of Saltzman (this volume) for Ohio and Illinois, where passage of laws was followed by significant increases in contract coverage, even in highly organized jurisdictions.

Finally, it may be argued that our cross-section results are biased due to the omission of a variable which is positively related to both municipal employee coverage and the legal environment. The best method to correct for this potential problem would be to perform a longitudinal analysis on departments. To do so requires variance over time in the coverage and legal index variables. However, we only have coverage data for the years 1977-1980, when there was virtually

Table 3: Regression Coefficients and Standard Errors (in parentheses) for the Relation Between the Legal Environment and Collective Bargaining Coverage, Controlling for Percentage Organized in the Department, SOG Data

	Pooled, 1977-80		Within City Analysis[1]	
Legal Index	.12 (.004)	.071 (.004)	.11 (.010)	.097 (.010)
Percent Union Members in Department	-	.42 (.008)	-	.20 (.009)
Police	.28 (.009)	.25 (.008)	A	A
Fire	.28 (.009)	.22 (.008)	A	A
Streets and Highways	.064 (.008)	.063 (.008)	A	A
Sanitation	-	-	-	-
City Dummies	no	no	yes	yes
Mean of 0-1 Coverage Variable	.23	.23	-	-
R^2	.29	.41	.37	.40
Number of Observations	13744	13744	11612	11612

A: Included but not reported; coefficient not comparable to basic cross-section coefficients due to the inclusion of department-demographic variable interactions.

Other variables controlled for in each regression are: population (and interactions with 3 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 (3), and year dummies (3).

In addition, the within city regressions include interactions between the demographic controls and the police, fire, and streets & highways department dummies.

[1] Standard errors corrected for inclusion of city dummies.

no longitudinal variation in the legal variable. Instead, we construct a different but similar experiment, which involves calculating the means for our basic cross-section variables within cities and differencing these means from the department specific values to sort out coverage and the law within cities. Letting C=coverage, X=measured city characteristics, L=the legal index, O=the omitted city characteristic, and the subscripts d and c denote department and city respectively, we have (ignoring the constant and the disturbance term):

$$(11)\ C_{dc} = A_{dc}X_c + bL_{dc} + O_c$$

$$(12)\ \overline{C}_c = A_cX_c + b\overline{L}_c + O_c$$

where $\overline{C}_c$ and $\overline{L}_c$ are city averages and A_c is a vector of the average across departments of the coefficients on the city characteristic variables. Differencing (12) from (11), we obtain:

$$(13)\ C_{dc} - \overline{C}_c = (A_{dc} - A_c)X_c + b(L_{dc} - \overline{L}_c)$$

The omitted city effect disappears, and we can accurately estimate the coefficient b. This technique is essentially equivalent to the inclusion of city dummies in a full sample cross-section regression. In practice, allowing the A parameters to vary by department in the pooled regression requires inclusion of interactions between the department dummies and city characteristic variables; this is done in the reported regressions. The results from this model are shown in columns 3 and 4 of table 3, and they confirm the basic cross-section results. An employee group with a relatively more favorable legal environment is more likely to be covered than other groups in the same city. Without controlling for percentage organized, the magnitude of this probability difference is approximately the same (.22 for a two standard deviation change in the law) as that obtained in the cross-section regressions. Inclusion of percentage organized reduces the estimated coefficient by only a small amount, from .11 to .097.

The results of our experiments using both CPS and SOG data sets, and using percentage organized as a separate variable, are clear: the legal environment in a state is a key determinant of whether or not a particular city-department is covered by a collective bargaining contract.

Wages and Employment

We now turn to our reduced-form estimates of the direct and indirect impact of the legal environment on wage and employment outcomes. By direct we mean effects holding fixed contract coverage; indirect effects are obtained by equations which exclude the coverage variable. Table 4 gives the results of our basic wage regression for the CPS. Here we regress the log of usual hourly earnings for each individual in the sample on the legal index, contract coverage, a legal index-contract coverage interaction variable, human capital and demographic

Table 4: Regression Coefficients and Standard Errors (in parentheses) for the Effect of the Legal Environment and Collective Bargaining Coverage on Ln(Usual Hourly Wage), Current Population Survey, 1984[1]

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

Other variables controlled for in each regression are: dummy variables for educational attainment (4), age (5), region (3), female, black, city size (2), and firefighters in the police and fire regressions, and alternative wages in the individual's SMSA.

[1] The CPS file used includes outgoing rotation group observations from each of the 12 monthly samples for 1984.

controls, and an "alternative wage" variable for each individual based on his department and SMSA to reflect the opportunities for those workers in the private sector.[10] The direct and indirect impact of the legal environment is estimated to be on the order of 6-8% for a two standard deviation change in the variable. The coverage effect on wages is about 12-15%. Its inclusion approximately halves the coefficient on the legal index. Finally, the coefficient on the legal index-coverage interaction suggests that for state employees and teachers most of the benefits of a more favorable legal environment are captured by groups which actively bargain, but that for police & fire and other local employees, there are considerable across-city spillovers.

As noted above, the SOG data set is more suited to our purposes, as it enables investigation of both the wage and employment effects of labor laws and public sector union activities. Table 5 records the results of our cross-section wage and employment analysis for the SOG data set. Panel A shows the wage results. Here we regress the log of average full-time monthly earnings in a municipal department on the legal index, contract coverage, a legal index-contract coverage interaction variable, demographic characteristics of the city, and year dummies (a complete variable list is provided in the table). Our estimate of the direct and indirect effects of the legal environment on wages is approximately 8% for a two standard deviation change in our scale.[11] The regression with

Table 5: Regression Coefficients and Standard Errors (in parentheses) for the Effect of the Legal Environment and Collective Bargaining Coverage on Wages and Employment, SOG Data

Pooled Cross-Section, 1977-1980 (N = 18,382)

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

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 (3), year dummies (3), and department dummies (4).

coverage in column 3 shows that contract coverage raises wages by about 6%, and attenuates the effect of the law variables only moderately, implying that the bulk of the effect of the laws on wages is direct rather than indirect through affecting coverage. The interaction term coefficient is small and insignificant, suggesting that there are substantial spillover effects from covered to not covered departments.[12]

The analysis in section 2 suggested that public sector unions are especially likely to use their political and lobbying influence to raise demand for labor and thus increase employment as well as wages. To test this notion we have used the SOG data set to estimate employment equations comparable to our wage equations.[13]

Panel B of table 5 presents the results of our cross-section investigation. The dependent variable is the log of full-time employment in each department-city unit;[14] the independent variables are the same set of city characteristics as used in the Panel A wage regressions, including city population and our measures of the legal environment and contract coverage.

The table shows the expected positive impact of strong unionism - in the form of departments with collective contracts - on employment: coefficients of about 21 to 24%. This is consistent with the findings of Zax and others on employment in unionized city-departments using other data sets. The big surprise in the calculation is the substantial <u>negative</u>

impact of a favorable legal environment on employment, which is estimated to reduce employment by about 11 to 17% for a two standard deviation change in the legal environment. Regressions in which the observations are weighted by city population yield modestly smaller but comparable negative coefficients. The interaction between the legal index and coverage shows, moreover, that the negative impact of favorable laws on employment occurs largely in non-covered departments, with a two standard deviation change in the legal environment decreasing employment by only a small amount in covered departments.[15]

How can we interpret these results?

There are two possible explanations. First, using our Section II model, it may be that the legal environment does indeed enhance union power largely at the bargaining table, so that the legal index reflects movement along a demand curve to a relatively greater extent than shifts in the demand curve, compared to the effect of collective bargaining coverage. The negative impact of favorable laws on employment in non-covered cities, might, moreover, be attributable to the impact of the wage spillovers found in our wage regressions, which workers are unable to offset through lobbying in the absence of a strong union. As we found the wage effects of the legal index to be larger in the noncovered sector, however, this explanation requires one of two additional facts: that the elasticity of demand for labor is greater in the non-covered cities (a pattern consistent with findings for private sector unions, in Freeman & Medoff 1981, and Allen 1983, among others); or that unions are able to shift out the demand curve more when legal environments are more favorable, leading to a smaller estimated reduction in employment for the covered sector.

A second possible interpretation is that the results are spurious, due to inadequate specification of the factors that determine public sector employment levels and omission of important department-specific employment determinants across cities (i.e. crime rates, potential fire loss, tons of sanitation generated, etc.). To assess the validity of the estimated negative effect of the legal environment on employment, and to further test the wage results, we have performed two additional analyses of wages and employment: a within-city comparison analogous to our earlier within-city coverage analysis, and a longitudinal analysis.

Panel A of table 6 presents the results of our wage analysis using the same type of within city model as discussed in our coverage analysis. The dependent variable in this regression is the difference between the log of department wages and the average log wages for all 5 departments in the city. The independent variables (legal index, coverage, and the legal-coverage interaction) were formed in a similar fashion.[16] As can be seen in the table, the coefficient estimates from this model show much smaller effects of the

Table 6: Regression Coefficients and Standard Errors[1] (in parentheses) for the Effect of the Legal Environment and Collective Bargaining Coverage on Wages and Employment, SOG Data (with City Dummies)

Within City Analysis (Pooled Data, 1977-1980. N = 13,960)

Panel A - Ln (Monthly Salary per Full-Time Employee in Dept.)			
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 Dept.)			
Legal Index	.030 (.020)	.004 (.020)	.002 (.020)
Coverage	-	.24 (.019)	.24 (.019)
Legal-Coverage Interaction	-	-	.019 (.047)

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 (3), year dummies (3), department dummies (4), and interactions between the city characteristic variables and four department dummies.

[1] Corrected for inclusion of city dummies.

legal environment and of coverage on wages than do the equations without city effects. One possible interpretation is that, in fact, our cross-section regressions overstate the true union and legal effects; as a longitudinal analysis to be discussed shortly shows little diminution of legal effects, we did not believe this to be the correct interpretation. A second interpretation is that there are considerable within-city spillovers among departments, an issue developed in depth by Ichniowski and Zax in this volume. Our findings in panel A are consistent with their evidence of considerable within-city wage spillovers.

The results of the within-city employment regressions are given in panel B of table 6. They show slight positive rather than negative effects of the legal index on employment and again reveal large positive effects of coverage on employment. As just noted, the former should not be taken as strong evidence against any negative employment effects for the legal environment, due to the apparently sizeable across-department within-city spillovers found by Ichniowski and Zax. We place greater weight on the longitudinal calculations contained in table 7; these are discussed below.

Longitudinal Analyses

A standard objection to union wage studies based on cross-section data is that the coefficients on unionization are biased because of omitted characteristics correlated with unionism and wages.

To deal with this problem we have performed a longitudinal analysis relating changes in wages (employment) to changes in the legal environment, conditional on wages (employment) in the base year. We perform this analysis on SOG department data between 1972 and 1980. Over this period we find considerable changes in legal environments; approximately 40% of the sample changed legal categories over the period, with about 15% of those experiencing a change of five or more legal categories. The changes are largely shifts from duty to bargain provisions to arbitration and from meet and confer provisions to duty to bargain provisions. There is unfortunately a cost to extending our analysis back to 1972; because the 1972 SOG neglected to gather data on bargaining units, we cannot control for changes in coverage in the period and thus are unable to divide our legal environment effects between their direct and indirect impacts.

Our specific longitudinal model is based on the following equations (with department subscripts omitted):

$$(14) \quad W_1 = a_w X + c_w L_1 + \lambda D + \mu_1$$

$$(15) \quad W_0 = a_w X + c_w L_0 + D + \mu_0$$

where the subscripts 0,1 refer to the beginning and end periods of our analysis; where D is the omitted department factor;

where μ_1 and μ_0 are independent disturbances; and where we allow the coefficients on the control variables X (which have no time subscript) and on the omitted factor to vary over time. By substitution, we obtain

$$(16)\ \Delta W = (\Delta a_w)X + c_w L_1 - \lambda c_w L_0 + (\lambda - 1)W_0 + (\mu_1 - \lambda\mu_0)$$

Least squares estimates of Equation 16 will not yield unbiased or consistent parameter estimates since the residual $-\lambda\mu_0$ is negatively correlated with W_0. The coefficient on W_0 will be biased downward and, given a correlation between W_0 and L, the coefficient on L_1 will also be biased. While there is no easy way around this problem, the extent of the bias can be assessed by treating $-\lambda\mu_0$ as an omitted variable correlated with w_0 and applying standard bias formulae together with prior estimates of the extent to which the variation in W_0 is due to μ_0. Our analysis, described in detail in Appendix A to the paper, suggests that the resultant bias is relatively modest, reducing the estimated coefficient on L_1 by about 4% and the estimated coefficient on W_0 by about 6% (in absolute value). As the change in the estimated coefficients is negligible (it is never more than .001 for the legal variable coefficients), we only report uncorrected coefficients in table 7.

Turning to the results of our longitudinal analysis in table 7, panel A gives the estimated coefficients for equation (16) with the change in wages as the dependent variable. The regression shows that the 1980 legal index variable has a positive effect; the estimated coefficient is .024 with a standard error of .004. Partitioning the sample into those departments that were covered and those that were not covered by a contract in 1980, we find that departments covered in 1980 had a slightly larger estimated wage gain than those not covered in 1980; these results are shown in columns (2) and (3) of table 7.

Overall, comparing these figures with our earlier cross-section estimates, we see that the longitudinal analyses yield somewhat smaller figures than those in the cross-section,[17] implying that there is some omitted variable bias in the cross-section regressions due to department factors, but that the legal effect is still significant. This should not be surprising: the legal variable is a <u>state</u>-based measure, whereas any omitted department factor is city-department based and hence unlikely to be highly correlated with a state-level variable.

Panel B of table 7 presents similar regression results for employment. In these calculations we relate 1972-80 changes in log employment across city-departments to the legal index in 1972 and 1980, using the model set out in equations (14)-(16). As noted for the wage analysis, we lack a coverage variable from the 1972 data set and thus are unable to test for coverage effects on employment along with the effects of the legal index. We find modest negative effects of the legal

Table 7: Regression Coefficients and Standard Errors (in parentheses) for the Effect of the Legal Environment and Collective Bargaining Coverage on Wages and Employment, SOG Data, Longitudinal Model

Longitudinal Analysis (Change from 1972 to 1980)

Panel A - Ln (Δ Monthly Salary per Full-Time Employee in Dept.)

	Full Sample (N=5281)	Covered[1] Depts. (N=1044)	Not Covered Depts. (N=3474)
Legal Index 1980	.024	.028	.021
	(.004)	(.008)	(.005)
Legal Index 1972	-.002	.002	-.013
	(.005)	(.008)	(.007)
Ln (1972 wages)	-.66	-.62	-.69
	(.012)	(.026)	(.015)

Panel B - Ln (Δ Number of Full-Time Employees in Dept.)

	Full Sample (N=5281)	Covered Depts. (N=1044)	Not Covered Depts. (N=3474)
Legal Index 1980	-.037	-.029	-.037
	(.010)	(.016)	(.014)
Legal Index 1972	-.010	-.016	-.029
	(.013)	(.017)	(.019)
Ln (1972 wages)	-.31	-.40	-.31
	(.010)	(.022)	(.012)

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 (3), and department dummies (4).

1 Refers to departments covered by a contract in 1980; data on 1972 coverage is unavailable.

environment on employment, consistent with our basic cross-section analysis.

To see whether the negative effect of employment is largely a phenomenon in non-covered departments, we further decompose the data set by 1980 coverage. The results, shown in columns (2) and (3) of table 7, indicate that employment is indeed reduced more in departments that are not covered than in departments that are covered.

Overall, the findings from the longitudinal analyses are consistent with the cross-section finding that a more favorable legal environment increases wages but reduces employment in both covered and non-covered departments, and that the wage effect is perhaps slightly larger while the negative employment effect is smaller in covered departments. Although these results are not reproduced in the within-city analysis, we attribute the difference to wage and employment spillovers similar to those found by Ichniowski and Zax (using similar data) in this volume.[18,19]

Differences Between CPS and SOG Wage Results

Comparing the CPS-based and SOG-based analyses in tables 4 and 5, the reader will notice that while the direct and indirect effect of the legal environment on wages is of comparable magnitude between the data sets, the effect of coverage differs noticeably, with the CPS yielding markedly larger coverage coefficients than the SOG. What might explain the difference in results?

One possibility is that the CPS data, based on individuals, gives greater weight to large departments than does the SOG and that coverage effects differ by size of department. To test this we re-estimated the equations in table 5 weighting the department observations by city size; the coverage coefficients fell rather than increased in size, indicating if anything that union effects were larger in smaller cities. Indeed, decomposing the SOG data by city size and running the same model as in column 2 of table 5 shows a coverage effect on wages of .00 in cities with a population of 500,000 or more; a coverage effect of .019 in cities of 250,000-500,000; a coverage effect of .015 in cities of 50,000-250,000; and an effect of .071 in cities with less than 50,000 in population. In short, we reject the notion that our different wage results are due to city size effects.

A second possibility is that the lower coverage effects in the SOG are the result of a different mix of occupations than in the CPS. To test this we estimate separately coverage effects for the two SOG groups for which the data sets overlap: police and fire, and other local employees.[20] Our estimates show coverage effects comparable to those in table 5 (.061 for police and fire; .049 for other local employees). Hence, this is not the reason for the differences.

A third possibility is that the results differ because of timing; perhaps public sector union effects increased from 1980 (the last year of our SOG analysis) to 1984. To test this we used the 1980 May CPS to estimate union coverage effects for the groups with significant sample size: teachers, state employees, and other local employees. Our results show that the estimated CPS coverage effects are in fact larger in 1980 than in 1984, which rejects a change-over-time explanation of our findings.[21]

In sum, we find that one obtains larger estimates of the effect of coverage on wages in CPS than in SOG data for reasons that are not readily explicable. Similar differences have been obtained in comparing CPS-based private sector union wage effects and effects from establishment-based surveys (Freeman, 1986a), and also in comparing CPS-based estimates of public/private pay differentials with estimates from establishment-based surveys (Freeman, 1985). The lesson is that to evaluate public sector union wage effects, one must examine both types of data, and must be careful not to mix the two types of data in comparisons over time.

The Impact of Arbitration and Strike Permitted Provisions

Throughout our analysis of public sector bargaining law and its impact on public sector union power, we have focused on our broad index measure of the legal environment. However, the frontier of current public sector labor law debate concerns arbitration and to a lesser extent (because they are less common) strike permitted clauses in public sector labor relations. What does our data tell us about the effects of these provisions on wages and employment?

To answer this question we have estimated wage and employment equations on a sample limited to departments covered by duty-to-bargain or stronger clauses in 1980, with 0-1 arbitration and strike-permitted dummy variables as the key independent variables. We estimate equations using both cross-section data for 1977-80 and longitudinal data from 1972-80. Our results, shown in table 8, indicate that strike permitted clauses tend to increase wages and to reduce employment in the cross-section, indicative of movement along a demand curve. However, both effects are positive in the longitudinal estimation, suggesting the presence in strike permitted departments of an omitted effect which is negatively related to employment levels.

For the quantitatively more important arbitration clauses, we obtain unexpected cross-section results: reductions in wages and in employment. Disaggregating our sample by whether or not a department is covered by a contract (in 1980 for the longitudinal model), we see that for covered departments, arbitration has essentially no effect on wages and a <u>positive</u> effect on employment. In light of other research which finds little or no effect of arbitration on wage settlements (Ashenfelter and Bloom 1984; studies cited in Freeman 1986b), but shows great effects on illegal strikes (Ichniowski 1986), this result will come as no surprise. What is surprising is

Table 8: OLS Regression Coefficients and Standard Errors (in parentheses) for Comparison of the Effect of Required Arbitration and Strike Permitted Laws on Wages and Employment, SOG Cross-Section (Pooled, 1977-1980) and Longitudinal Data (1980-1972), Duty-to-Bargain Sample[1]

Panel A - Wage Effects

	Arbi-tration 1980	Strikes Perm. 1980	Arbi-tration 1972	Strikes Perm. 1972	all other cate-gories 1972	W 1972	E 1972
Full Sample							
Cross Section (N=11,396)	-.023 (.005)	.014 (.007)	-	-	-	-	-
Longitudinal (N=2922)	.005 (.010)	.032 (.015)	-.014 (.012)	-.061 (.024)	.003 (.011)	-.69 (.017)	-
Covered[2] Departments							
Cross Section (N=3131)	-.002 (.009)	.024 (.012)	-	-	-	-	-
Longitudinal (N=853)	.011 (.016)	.093 (.026)	-.020 (.016)	-.12 (.045)	-.020 (.015)	-.64 (.027)	-
Not Covered Depts.							
Cross-Section (N=6410)	-.073 (.008)	.006 (.008)	-	-	-	-	-
Longitudinal (N=1468)	-.026 (.018)	.004 (.022)	-.029 (.026)	-.055 (.034)	.032 (.019)	-.73 (.025)	-

Panel B - Employment Effects

	Arbi-tration 1980	Strikes Perm. 1980	Arbi-tration 1972	Strikes Perm. 1972	all other cate-gories 1972	W 1972	E 1972
Full Sample							
Cross Section (N=11,396)	-.034 (.019)	-.21 (.027)	-	-	-	-	-
Longitudinal (N=2922)	-.027 (.027)	.080 (.041)	-.044 (.032)	-.21 (.062)	-.091 (.029)	-	-.26 (.013)
Covered Departments							
Cross Section (N=3131)	.032 (.024)	-.15 (.032)	-	-	-	-	-
Longitudinal (N=853)	.083 (.034)	.066 (.056)	-.092 (.034)	-.087 (.097)	-.077 (.034)	-	-.37 (.026)
Not Covered Depts.							
Cross-Section (N=6410)	-.25 (.035)	-.27 (.036)	-	-	-	-	-
Longitudinal (N=1468)	-.10 (.054)	.12 (.065)	.055 (.075)	-.29 (.098)	-.11 (.056)	-	-.25 (.018)

(con.)

Table 8 (con.)

Legal Category Variable Means

	Arbi-tration 1980	Strikes Perm. 1980	Arbi-tration 1972	Strikes Perm. 1972	all other cate-gories 1972
Full Sample					
Cross-Section	.30	.096	-	-	-
Longitudinal	.33	.092	.096	.033	.27
Covered Departments					
Cross Section	.54	.086	-	-	-
Longitudinal	.54	.068	.17	.016	.28
Not Covered Depts.					
Cross-Section	.13	.11	-	-	-
Longitudinal	.16	.11	.050	.040	.31

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 (3), and department dummies (4). Also, the cross-section regressions include year dummies (3).

1 The sample only includes departments in legal categories 6-9 (see table 1) in 1980.

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

the negative effects of arbitration clauses on wages and employment for non-covered departments who do not use the arbitration machinery (since they have no contract disputes to be resolved by arbitration), which holds up even in the longitudinal analysis. We do not have a good explanation for this. Clearly it will require a model focusing not on union behavior, as ours does, but on behavior of city-departments that are not covered by contracts, particularly with regard to their "spillover" behavior.

IV. Conclusion

This paper has examined the effect of the different legal environments faced by public employees across the states on wage and employment outcomes, using cross-section analyses, within-city analyses, and longitudinal analyses. While the investigation turned up some puzzles, the general tone of the results is consistent with the notion that the legal environment which governs public sector collective bargaining influences outcomes through its impact on contract coverage, and through its impact on wages and employment. In particular, we find:

(1) State laws governing collective bargaining are a major determinant of whether or not workers have contracts, even controlling for the proportion of workers who are organized and comparing departments within the same city.

(2) Collective bargaining coverage raises wages and

employment, consistent with a model of public sector unionism in which the unions use some of their resources to raise demand for labor.

(3) A more favorable legal environment raises wages but has virtually no effect on employment in departments covered by collective bargaining, presumably because the laws strengthen unions' ability to negotiate wage increases at the bargaining table, which offsets any union pressure to raise employment. For departments lacking collective bargaining contracts, a more favorable legal environment induces wage gains similar to those for departments with contracts, but reduces employment.

(4) Within cities, wage differences between departments with contracts and those without contracts and between those with more and less favorable collective bargaining laws are quite modest, consistent with the Ichniowski-Zax analysis of within-city spillovers. On the employment side, the impact of differing legal environments within cities is again small, consistent with the Ichniowski-Zax analysis. However, the large positive effect of contract coverage on employment persists even when we compare departments within the same city.

(5) Among city departments covered by collective contracts, arbitration clauses have little effect on wages but positive effects on employment, while strike permitted clauses raise wages but have an ambiguous effect on employment.

Turning to the puzzles that our analysis has uncovered but failed to resolve, we found: that the CPS individual-based data gives markedly higher estimates of union wage effects than does the SOG establishment-based data; and that noncovered departments with arbitration or the right to strike do worse in terms of wages and employment than those with duty to bargain but lacking closure laws.

All told, our analysis shows that the legal environment for collective bargaining is an important determinant of the presence of contracts and of outcomes in public sector labor markets, and that what public sector unions do to their employers differs in some important respects from what private sector unions do, along lines consistent with a model in which public sector unions use their resources to shift demand for labor as well as to raise wages.

Appendix A - Derivation of the Longitudinal Bias Correction

The longitudinal model from the text is:

(14) $W_1 = a_w X + c_w L_1 + \lambda D + \mu_1$

(15) $W_0 = a_w X + c_w L_0 + D + \mu_0$

(16) $\Delta W = (\Delta a_w) X + c_w L_1 - \lambda c_w L_0 + (\lambda - 1) W_0 + (\mu_1 - \lambda \mu_0)$

In the text we note that this model does not yield unbiased or consistent parameter estimates due to the negative correlation between the residual $-\lambda\mu_0$ and W_0. The correction for this problem involves application of standard omitted variable bias formulae.

Let α be the auxiliary regression coefficient of W_0 on L_1 conditional on all other variables, let r be the accompanying partial correlation coefficient, and let P $(0 \leq P \leq 1)$ be the ratio of the variance of μ_0 to the variance of W. Then the bias on c_w due to the omission of μ_0 from the calculation is determined by:

(A1) $\text{plim } \underline{c} = (\alpha/(1-r^2))P\lambda + c_w$

where $\underline{c}$ is the estimate of c_w and the plim is taken as the sample size tends to infinite.

The bias in estimating λ is:

(A2) $\text{plim}\ \underline{\lambda} = \lambda[1-(P/(1-r^2))]$

Regressing W_0 on L_1 and all of the variables in Equation 16 yields α=.031 and r=.0093 for the full sample wage regressions reported in table 7. With these magnitudes, the coefficient on L_1 will not be greatly affected by the omission of μ_0 unless P is a very large number. The parameter P is the ratio of the random (measurement error) variation of W_0 to the total variation in W_0. Assume, as a reasonable approximation, that one-tenth of the variance in W_0 is due to μ_0, so that P=1/10. With this value of P equations A1 and A2 imply that λ is understated by .04 and that c_w is overstated by .001 in the uncorrected estimates. As similarly small corrections were derived for the employment regressions, we report uncorrected estimates in table 7.

Notes

1. Consistent with other studies -- see Zax 1985b, references in Freeman 1986b.

2. See table 4 in Freeman 1986b.

3. AFSCME and John Burton provided us with their own lists of the laws; see References for others.

4. We thank Jeffrey Zax for providing us with this extract. Note that aggregation of the finance and control categories can be justified on a priori grounds, due to the similarity of services provided by the two categories (see Ehrenberg 1973, p. 370) and the lack of department-specific bargaining unit data for them (see note 5).

5. The relevant bargaining unit for the finance and control category is clerical, since the finance and control departments typically have a high percentage of clerical workers, and no department-specific bargaining unit data was available for them.

6. Note that while we have given all unions the same utility function, factors that produce different utility functions (differences in the age of union members, for example) would enter the reduced form relations (6) just as do the L factors.

7. The latter category is excluded from some regressions, as no data on percentage organized was available for finance and control personnel; regressions without this category, and without percentage organized, are included for purposes of comparison.

8. Individuals who answer "yes" to the CPS union membership question are automatically counted as "covered;" those who answer "no" are then asked the coverage question. Thus, some union members who are not covered are considered covered in the CPS. The SOG coverage variable construction is described in Section I.

9. We can crudely obtain logistic coefficients from the linear model by dividing the linear coefficients by P(1-P), where P denotes the mean coverage level for the sample.

10. This variable was formed by calculating the average log usual hourly earnings for employed individuals with 2 and 4-year college degrees separately in each SMSA (and also for

those "not in an SMSA"); the 4-year graduate average was matched by SMSA for the teacher sample, while the other 3 groups were matched with the 2-year graduate average.

11. We ran the same regressions including revenue variables (total tax revenues per capita and intergovernmental aid per capita) as regressors. As inclusion of these variables did not change the substantive results, and as there surely is a simultaneous relation between taxes and municipal wages, which we do not explore in this paper, we exclude the revenue variables from ensuing calculations.

12. We ran the same regressions adding percentage organized in the department as an additional control for unionism. Controlling for the legal environment and coverage, the estimated coefficient (standard error) on percentage organized was .082 (.004) for the pooled sample, .079 (0.13) for covered departments, and .085 (.005) for not covered departments. These results are consistent with our model in which unions can use resources to shift and/or move along a demand curve. Results for the effect of percentage organized on employment are given in note 15. We thank Charles Brown for suggesting the use of this variable in the wage and employment equations.

13. Since the CPS data are based on individuals, with small numbers by city, we cannot readily use the data for analysing employment in demand relations.

14. Full-time equivalents were not used, since on average part-time employment comprises less than 2% of total full-time equivalent employment in our sample; thus, any union-induced substitution from part-time to full-time employment can have only negligible effects. Regressions using full-time equivalents yielded similar results to those shown.

15. As with wages, we ran the same regressions with percentage organized included. Controlling for the legal environment and coverage, the estimated coefficient (standard error) on percentage organized was .018 (.016) for the pooled sample, .24 (.033) for covered departments, and .025 (.019) for not covered departments. These results, along with the wage results from footnote 12, suggest that in covered departments, a greater percentage organized substantially increases unions' ability to both shift and move along a demand curve, while in not covered departments percentage organized primarily increases unions' ability to move along a demand curve (possibly through threat effects). More research is needed in this area before definitive conclusions can be drawn.

16. As with the coverage regressions from columns 3 and 4 of table 3, we ran a pooled regression which included the usual city characteristic variables and interactions between these

and the four department dummies, to account for differences in the effects of the city characteristics on wages and employment in different departments. However, we constrain the omitted city-specific effect to be the same for all departments.

17. A more exact comparison is provided by a cross-section wage regression for only 1980 observations; it yielded a legal index coefficient of .039.

18. One important related result is that covered and not covered departments are subject to different wage and employment determination processes. Chow tests on the reduced form wage and employment equations, with the sample broken into covered and not covered department sub-samples, yielded F statistics which each attained the .01 significance level. Although our substantive results are not changed by breaking the sample into covered-not covered groups, future researchers should be careful to consider this point.

19. We also attempted to estimate a structural demand equation. However, due to lack of adequate supply instruments, we chose not to pursue this avenue of inquiry in the current paper.

20. The overlap is imperfect for the "other local" category, as we only have 3 other local groups (sanitation, streets and highways, and finance & control personnel) in the SOG, while the CPS contains more.

21. In particular, we find that the estimated coefficients (standard errors) in 1980 are .21 (.085) for state employees, .22 (.071) for other local employees, and .19 (.044) for teachers. These results are not strictly comparable to the 1984 results since the May 1980 sample is much smaller than our 1984 sample, and since extraction of the state and other local groups prior to 1983 requires restriction of the sample to "public administration" employees.

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