COMPARISONS BETWEEN PUBLIC AND PRIVATE SECTOR UNION WAGE DIFFERENTIALS:
DOES THE LEGAL ENVIRONMENT MATTER?

Joseph Tracy

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ABSTRACT

A stylized fact in the growing literature on public sector labor mar-

kets is that estimates of public sector union wage premia are significantly

lower than estimates of private sector union wage premia. In this paper I

investigate the hypothesis that this difference may in part be due to the

differing legal environments in which public and private sector unions oper-

ate. Using data from the Current Population Survey and the Census of Popu-

lation, I find that public sector union wage differentials increase signifi-

cantly with the degree of legal protection afforded to the union in bargain-

ing. However, the estimated public sector union wage premia when no legal

controls are included in the specification are close to the estimated premia

under the strongest legal environment. Consequently, while controlling for

the legal environment in the public sector is important, it may not recon-

cile the differences between estimated public and private sector union wage

premia.

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One of the "stylized facts" which has emerged from the growing

literature on public sector labor markets is that estimates of public sector

union wage differentials are significantly lower than estimates of private

sector union wage differentials. In a recent survey, Ehrenberg and Schwarz

(1986) comment on this comparison.

> What is most striking is how small these numbers typically are!
> The estimated relative wage differentials associated with union
> membership or collective bargaining coverage are typically smaller
> than 10 percent and rarely exceed 20 percent. These estimates are
> considerably lower than the estimates obtained from private sector
> studies and they suggest that the relative wage effects of unions have
> been less in the public-sector than the private-sector. (p. 1228)

A key issue is whether the differences in the magnitudes of union wage

premium across sectors are real or illusory.

Several arguments exist as to why at least part of this observed

difference may be illusory. Public and private sector union wage differen-

tials are typically derived from different wage equation specifications

estimated on different types of data. The basic difference in the level of

aggregation of the data used in public and private sector studies has

resulted in important differences in the types of control variables included

in these studies. Public sector wage equations have focused on economic,

demographic, and political variables relating to the geographic area

containing the bargaining unit. In contrast, private sector wage equations

have focused on individual worker characteristics. Several other specifica-

tion differences include the treatment of the endogeneity of the union

status of workers, the treatment of "threat" or "spillover" effects, and the

treatment of the occupational structure of the work force.[1]

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[1]Details of these arguments as well as specific references to the empirical
literature can be found in Ehrenberg & Schwarz (1986) and Freeman (1986).

Assuming that all of the gap between private and public sector union wage differentials can not be explained by data or specification differences, a second issue is what factors explain the inability of public sector unions to generate comparable premiums for their workers. A natural starting point is to examine the relative bargaining power between public and private sector unions. Previous studies have emphasized the nonprofit nature of government employers and the likely effects this may have on the bargaining process. A second distinction between public and private sector bargaining is the legal environment which governs the bargaining process. Private sector bargaining is governed by a largely uniform set of laws and court decisions. The only source of variation is the ability of a state to pass a Right-to-Work law. In contrast, public sector bargaining is governed by a set of laws which can vary both between states for a particular group of workers and between groups of workers within a particular state. Public sector bargaining laws range from one extreme of outright prohibitions against bargaining to the other extreme of a duty of public employers to bargain which is backed up with the right to strike or access to arbitration. In the best situation, public sector unions can negotiate contracts in a legal environment comparable to the private sector. In the worst situation, public sector unions can not negotiate a contract at all.

The potential importance of investigating the role of variations in public sector bargaining laws in explaining variations in public sector union wage premiums was emphasized by Ehrenberg & Schwarz (1986).

> Somewhat surprisingly, however, there are no studies that have empirically looked at how the nature of impasse procedures affects the union/nonunion differential ... the smaller estimated differentials in the public sector may reflect smaller actual differentials caused by the different nature of laws governing bargaining in that sector. (p. 1229)

In this paper I examine the influence of the legal environment on the union differentials which are negotiated in the public sector. I compare union wage differentials for teachers and police under a variety of different bargaining law configurations. Particular attention is paid to the issue of the potential endogeneity of the bargaining law variables. The basic findings are that public sector union wage premiums increase significantly with the degree of legal protection afforded to the union in bargaining. However, the estimated union wage premia when no legal controls are included in the specification are close to the estimated union wage premia under Duty-to-Bargain laws. Consequently, while controlling for the legal environment in the public sector is important, it may not reconcile differences between estimated public and private union wage differentials.

SUMMARY OF PREVIOUS STUDIES OF BARGAINING LAWS

Considerable attention has been focused on assessing the impact of Right-to-Work laws on unionization rates in the private sector. Far less effort has been directed toward measuring the impact of Right-to-Work laws on the union wage differential in the private sector. Two econometric concerns show up in each of these lines of research. The first concern is that there exists unobserved heterogeneity in states which is correlated both with the presence of a Right-to-Work law and with the level of unionization. For example, states with strong anti union "sentiments" may be likely to have both a Right-to-Work law as well as lower unionization rates. This creates a left-out-variables bias in the coefficient estimate of the Right-to-Work law. The second concern is that a simultaneity bias exists. That is, the level of unionization in a state may affect the probability that a Right-to-Work law is passed. States with high levels of unionization

may be less likely to pass such a law due to lobbying efforts by the unions.

A variety of methods have been used in previous studies to address one or both of these concerns. Lumsden & Petersen (1975) analyze state aggregate unionization rates for the years 1939, 1953, and 1968. The coefficient on the Right-to-Work indicator in the regression using the 1968 data was negative and significant. Lumsden & Petersen test for left-out-variable bias by including the same Right-to-Work indicator variable in a regression using the 1939 data. At this point in time no states had Right-to-Work laws. The coefficient estimate was negative, significant, and of roughly the same order of magnitude as the coefficient estimate from the 1968 regression. They conclude that there is no significant effect of the law per se on the level of unionization. The negative effect measured using the 1968 data simply reflected lower prelaw levels of unionization due to other unobserved factors prevailing in those states.

Moore & Newman (1975) analyze state aggregate unionization rates for the years 1950, 1960, and 1970. They address the issue of simultaneity by estimating the unionization equation using Two Stage Least Squares. They fit a model for the process governing the probability that a Right-to-Work law is passed in a state. This model is used to generate predicted probabilities which are used in place of the Right-to-Work indicator variable. They find a negative and significant coefficient for the Right-to-Work indicator but a negative and insignificant coefficient for the predicted value of the indicator. They conclude like Lumsden & Petersen that there is no effect of the law per se on unionization rates.

Finally, Ellwood & Fine (1983) address the problem by examining flows of unionization generated by NLRB elections. They argue that flows will be more sensitive than stocks to changes in the legal environment making it

easier to measure the effect of the law using flow data. The data indicate
that the flows into unionization drop immediately following the passage of a
Right-to-Work law and continue to be below the prelaw levels for up to ten
years. Their estimates imply a long run reduction of 5-10% in the stock of
union members or equivalently a drop of 1-3% in the unionization rate. For
a sample of seven states they have flow data both prior to and following
adoption of a Right-to-Work law. They estimate a fixed effect model on data
from these states to test for left-out-variable bias. Their fixed effect
estimates were virtually identical to their earlier results. Finally, they
test for simultaneity by including in the specification an indicator that
takes a value of one for the five years prior to the year the state passed
its Right-to-Work law. The simultaneity argument would predict a negative
coefficient on this variable. They find a positive and insignificant
coefficient. They conclude that the legal environment does have an
independent effect on the level of private sector unionization in a state.

The effect of Right-to-Work laws on private sector union wage
differentials has received less attention in the literature. Moore (1980)
examines a single cross section of workers in 1970 drawn from the Income
Dynamics Panel data. He finds negative and insignificant point estimates on
the Right-to-Work variable in both the nonunion and union wage equations.
The implied effect of the law on the union wage differential is positive but
insignificant. Farber (1984) reexamines this issue using data drawn from
the May 1977 Current Population Survey (CPS). He finds negative and signi-
ficant point estimates on the Right-to-Work variable in both the nonunion
and union wage equations. The implied effect of the law on the union wage
differential is positive and significant. No clear picture emerges as to
the magnitude of the effect of the legal environment on private sector union

wage differentials. In addition, neither study explicitly tests for the omitted variable bias or simultaneity bias.

Studies of the effects of the legal environment on unionization rates and wages in the public sector are of more recent vintage. This reflects the fact that until recently no comprehensive data set on public sector bargaining laws existed. Ichniowski (1986) examines the effect of bargaining laws on municipal police unionization. Hazard models are estimated for the duration until formation of a police bargaining unit in a city. He finds that unionization rarely proceeds passage of a favorable bargaining law. In addition, the bargaining laws are the most important determinants of the conditional probability of unionization. Passage of a Duty-to-Bargain law significantly increases the likelihood of the formation of a bargaining unit. Access to arbitration, though, does not significantly affect unionization.

Freeman & Valletta (1987) investigate the effect of the legal environment on unionization, employment, and wages. Union coverage and employment data are obtained from the Annual Survey of Government. Wage data are obtained from the 1984 May CPS. The legal environment is proxied by a single index summarizing the bargaining laws in effect. This is in contrast to other studies which have tried to estimate separate effects for different types of bargaining laws. They find that more "prounion" legal environments are associated with significantly higher union coverage rates, with significantly higher employment levels for covered groups of workers, and with significantly higher union wages. They do not test to see if the legal environment significantly affects union wage premiums. No specification tests are carried out to check for possible endogeneity of their legal index.

Indirect evidence on the effect of bargaining laws on public sector union wage premiums is given in Zax (1985). In this study, Zax examines the effect of different "modes" of unionization on the resulting union wage premium. The modes vary from one extreme of an unrecognized union in a city with no recognized bargaining units to the other extreme of a recognized bargaining unit. He finds that going from the weakest to the strongest mode of unionization, the measured union wage premiums increase from 3.8% to 11.8%. While controlling for the mode of unionization is not equivalent to controlling for the prevailing bargaining laws, the findings by Ichniowski (1986) would suggest that they are strongly correlated.

ECONOMETRIC SPECIFICATION ISSUES

The impression one is left with after reviewing the literature on bargaining laws is that no consistent set of results exists. Results vary with the type of data used and the type of specification estimated. No evidence has been found in support of the simultaneity hypothesis while mixed evidence has been found in support of the omitted variable hypothesis. In light of these studies, it remains plausible that the legal environment is an important factor in explaining the differences in union wage differentials between the public and private sector. While an explicit treatment of omitted variable bias is warranted, there is less justification for concern about simultaneity.[2]

[2]This follows for two reasons. First, the connection between unionization rates and passage of bargaining laws seems more plausible than a similar connection for union wage differentials. Unionization rates directly translate into electoral pressure. Second, as already pointed out, there is no strong evidence to date supporting the direction of causality from unionization rates to bargaining laws.

Each of the techniques used in previous studies to correct for omitted variable bias has been criticized. The first method is to estimate a fixed effect model using panel data. Two problems arise here. This method can not be used on a single cross section of data, and the method assumes that the omitted variables are constant through time. The second method is to explicitly model the legislative process which determines the legal environment and then to estimate the outcome (i.e. unionization or wage) equation using Two Stage Least Squares. The fundamental problem with this method is the lack of a convincing model of the legislative process.[3] This leads to questionable identification of the model through functional form or arbitrary exclusion restrictions.

Most estimates of private sector union wage differentials have been based on cross section data such as the CPS. Comparisons with these previous private sector estimates would be facilitated by using similar data sources. This raises the problem of how to deal with omitted variable bias issues using cross section data. In this paper I examine three approaches to this problem. The first approach is to use the group structure (ie by city) which exists in the data. I assume that the wage for individual i in city j is determined by the following stochastic model.

(1) $\ln W_{ij} = X_{ij} \, \beta + Z_j \delta + u_{ij}$

$u_{ij} = \alpha_j + \varepsilon_{ij}$

Where $\alpha_j \sim N(0, \sigma_\alpha^2)$ and $\varepsilon_{ij} \sim N(0, \sigma_\varepsilon^2)$.

The vector X_{ij} contains individual worker characteristics while the vector Z_j contains variables which are constant for all public workers in the city.

[3]Several different models have been estimated. See for example Faber & Martin (1979), Farber (1986), and Kochan (1973).

The composite error term for an individual consists of a city specific and an individual specific error component. I assume that the α's and ε's are uncorrelated with each other and that the ε's are uncorrelated with the included variables.

The city specific error component captures the combined effect of the unobserved state and city heterogeneity (ie omitted variables in Z) on wages. If $\sigma_\alpha^2 > 0$ and the α's are uncorrelated with X and Z, then OLS will produce consistent coefficient estimates but biased standard error estimates. Moulton (1986) demonstrates that the magnitude of this standard error bias is likely to be largest for the Z variables since they have no within group variance. Since the vector Z includes the bargaining law variables, this can lead to incorrect inferences as to the significance of the legal environment. This problem can be corrected either by recalculat-ing the OLS standard errors using estimates of σ_α^2 and σ_ε^2 or by estimating a random effects model.

A more important problem exists if the α's are correlated with variables in X and/or Z. Previous studies of bargaining laws have argued that factors such as public sentiment toward unions will affect both the type of bargaining laws in the state as well as union and/or nonunion wages. If this sentiment is picked up in the α's, then both OLS and random effects will be produce biased estimates of the bargaining law coefficients.

The advantage of the wage specification given in (1) is that the hypothesis that the α's are correlated with the included variables can be investigated using a Hausman test. The basic idea behind the test is that the "within" or "fixed effect" estimates of β are consistent regardless of the correlation between the α's and the included variables, while the GLS are consistent only under the null that no correlation exists. The test

consists of determining if there is a statistically significant difference between the Within and the GLS estimates of β.

If the specification test indicates that the α's are correlated with the included variables, then it may still be possible to obtain consistent coefficient estimates by following the Instrumental Variable Random Effects procedure (IVGLS) outlined in Hausman & Taylor (1981). Assume that the X's are exogenous and that the source of the correlation with the α's is the set of bargaining law variables contained in Z. In this case, β can be estimated using the within variation in the X's while the between variation in the X's can serve as instruments for the Z's. A second Hausman test can be carried out to check the validity of this assumption.[4]

The second approach to the left-out-variable problem is to augment specification (1) with proxies for the likely omitted variables. As mentioned earlier, states with greater tastes for unionism may be more likely to pass permissive public sector bargaining laws. In this case, the coefficient on the bargaining laws measures both the direct effect of the legal environment as well as the indirect effect of the prevailing union sentiment in the state. The second test of this hypothesis is to add the level of private sector unionism to the public sector wage equations. If private sector union coverage in the state is a good proxy for the prevailing union sentiment, then controlling for private union coverage should reduce the degree of bias in the bargaining law coefficients. If there is little change in these coefficients, then this is further evidence against the left-out-variable hypothesis.

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[4]Other Instrumental Variable procedures have recently been discussed in the literature. See Amemiya & MaCurdy (1986); and Breusch, Mizon and Schmidt (1987).

The third approach to the problem is to include the public sector

bargaining laws in a private sector wage specification.[5] Since public

sector laws are not germane to private sector bargaining, they should not

have any direct effect on the estimated union wage differentials. Any

effect of the public sector law variables should be attributed to the

indirect effect of the prevailing sentiment toward unionism. If these vari-

ables have the same qualitative effect on private union wage differentials

as they do on public union wage differentials, then this is evidence

supportive of the left-out-variable hypothesis.[6]

DATA DESCRIPTION

A necessary ingredient for this type of study is a comprehensive data

set of public sector bargaining laws. The compilation of this data was

undertaken by the National Bureau of Economic Research and is discussed in

detail in Valletta & Freeman (1985). For this study, the wealth of legal

information provided in the data have been collapsed into a few key varia-

bles. The collective bargaining rights of the union have been summarized in

three basic categories. The left-out category consists of states that

either have no laws dealing explicitly with bargaining rights or that have

laws which provide only "weak" bargaining rights.[7] The second category

[5]I would like to thank Henry Farber for this suggestion.
[6]In an earlier paper I focused on the issue of selection bias instead of
omitted variable bias. I estimated a multinomial logit selection model
where workers selected into one of four labor markets: private/nonunion,
private/union, public/nonunion, and public/union. I assumed that all
bargaining laws, public and private, affected a worker's choice of sector
but that wages were affected only by the bargaining laws applicable to that
sector. This led to exclusion restrictions between the choice and the
outcome equations. None of the selection effects were significantly diffe-
rent from zero. The public/union selection effect was large in magnitude
but imprecisely measured. In light of these findings, it seems more fruit-
ful to investigate sources of biases other than selection effects.
[7]Weak bargaining rights include situations where the public employer is

consists of states which have taken strong positions against public sector
unions by prohibiting collective bargaining. At the other extreme, the
third category consists of states which have imposed a "Duty-to-Bargain" on
their public employers. In addition to the bargaining rights variables, two
variables have been constructed which indicate if the union has the right to
strike or access to arbitration if bargaining results in an impasse.
Finally, similar to the private sector, public sector union security is
summarized by a Right-to-Work indicator variable.

The bargaining data is coded separately for state, police, fire,
teachers, and other local employees. Table 1 gives the overall distribution
of the bargaining rights variables as well as the distribution by region.
The unit of observation in TableS 1 through 3 is a function unit in a
particular state, ie police in Illinois, teachers is New Jersey, etc.[8] As
of 1976 only around 7% of the function units were prohibited from collective
bargaining. Distinct regional patterns to the bargaining rights exist. All
function units in the Northeast have Duty-to-Bargain rights. In contrast,
only 19% of function units in the South had Duty-to-Bargain rights, and none
had the additional right to strike or arbitration. In addition nearly all
prohibitions against collective bargaining were found in the South.

The distribution of public sector Right-to-Work laws in aggregate and
by region is given in Table 2. Slightly over 31% of the function units were
covered by these laws. A similar regional pattern exits for Right-to-Work
laws as for bargaining rights laws. No function units in the Northeast are
covered by Right-to-Work laws while over 43% of the function units in the

authorized but not obligated to negotiate with the union. situations where
the union has the right to present proposals, and situations where the
union has the right to "meet and confer" with the employer.
[8]Duty-to-bargain laws have been classified as "strong" if strike or
arbitration rights are also provided and "weak" if they are not provided.

South are covered. However, despite this similarity, it is not the case that Right-to-Work laws are necessarily synonymous with anti-public sector unionism. To see this, the distribution of public sector Right-to-Work laws by type of bargaining rights is presented in Table 3. The _same_ percentage of function units covered by Right-to-Work laws have strong Duty-to-Bargain rights as prohibitions against collective bargaining.

Unfortunately, there are serious limitations associated with the major sources of cross section data on public sector workers which can be matched with the bargaining law data. The two sources used in this analysis are the May CPS data and the 1980 Census of Population (CoP) data. Data on union and nonunion workers is available each year from the May CPS; however, several restrictions must be met before this data can be analyzed for any given year. Since the bargaining law data is gathered at the state level, we must be able to identify the state associated with a public employee's place of work in order to merge in the appropriate set of laws.[9] For reasons of confidentiality, the CPS grouped some states together up until the 1977 survey; a worker's specific state within the group can only be identified if the worker resides in an identified SMSA. To utilize the full range of variation in public sector bargaining laws, we must restrict ourselves to analyzing data from 1977 onwards. The second restriction is that we must have wage data for the worker. Starting in 1979 the CPS asked wage questions only for the two outgoing rotation groups. As a result the effective sample size drops drastically, making it impossible to estimate separate wage equations for union and nonunion samples of public employees.

[9]The appropriate bargaining laws is a function of a worker's place of work and not necessarily his/her place of residence. Unfortunately the CPS only asks a place of residence question. This will result in some mismatching of bargaining laws with workers.

Consequently, only the 1977 and 1978 data can be analyzed as separate cross sections without resorting to pooling across years. This study will analyze the 1977 CPS data.

The advantage of the CPS data is that it identifies the union status of each public sector worker. The disadvantage of the CPS is that it contains small samples of workers from each type of local function unit. The problem of small sample sizes can be overcome by using the 1980 CoP data. Combining the A and B samples gives a combined 6% random sample of the population. Each local public sector employee was selected from the 1980 CoP as well as a 1 in 100 subsample of private sector workers. The principal disadvantage of the CoP data is that no union question is asked. This problem can be addressed by restricting the sample to public employees working in central cities. Information on unionization rates for central city public employees is available in the 1979 Census of Government (CoG) data. For each city, the CoG data gives both the total number of employees and the number of organized employees in each function unit. This data was used to construct a city/function unit specific union coverage rate. As will be discussed in the next section, separate union and nonunion wage equations can not be estimated using the CoP data. Instead, a single wage equation with the union coverage rate as well as the coverage rate interacted with the bargaining law variables will be estimated.

The following selection criteria will be used in creating each sample used in the estimation. Workers must be employed full time or part time for economic reasons in a nonagricultural job. All observations with "allocated" or missing responses for variables used in the analysis are deleted. For the CPS data, the definition of labor union used is membership in a union or employee association. For reasons to be discussed below, the

analysis will be limited to teachers and police. Summary statistics are given in Table 4.

EMPIRICAL RESULTS

Three groups of local public workers were selected for potential use in this study: teachers, police, and fire fighters. These three groups were focused on for two reasons. First, each group can be identified in both the CPS and the CoP data. Second, a close link exists for these groups between the collective bargaining and the employment files of the SoG data. This improves the reliability of the unionization imputations to be used with the CoP data.[10] Tests were carried out to see if it would be permissible to aggregate some or all of these three groups. In no case did the data indicate that aggregation was justified. The sample of fire fighters from the CPS data was too small to warrant an independent investigation. As a consequence, further analysis was restricted to the samples of teachers and police. In addition, the CPS sample sizes for the teachers and police were too small to estimate a random effects model. Specification tests for group error structures were carried out only for the CoP samples.

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[10]The employment file of the SoG data gives both the total number of full-time workers as well as the number of organized workers in the function unit. The collective bargaining file gives the number of workers in the bargaining unit. Both the percent organized as well as the percent in the bargaining unit were calculated. If the percent organized was positive, then the unionization rate was set equal to the percent organized. If the percent organized was zero and the percent in the bargaining unit was positive, then the unionization rate was set equal to the percent in the bargaining unit. For some groups of workers such as clerical workers, the bargaining unit can contain workers from several function units in the employment file. In these cases, the percent in the bargaining unit can not be accurately calculated.

A one-sided LM test was calculated to test the hypothesis $H_0: \sigma_{\alpha}^2 = 0$ vs the alternative $H_1: \sigma_{\alpha}^2 > 0$. The test statistic is defined as follows.

$$LM = \frac{\sum_j (N_j \bar{u}_j)^2 - \sum_j \sum_i (\hat{u}_{ij}^2)}{\hat{\sigma}^2 [2(\sum_j N_j^2 - N)]^{1/2}}$$

Where $\hat{u}_{ij}$ = OLS residual for worker i in city (i.e. group) j

N = Total sample size

N_j = Size of jth city

$\bar{u}_j = \frac{1}{N_j} \sum_i \hat{u}_{ij}$

$\hat{\sigma}^2 = \frac{1}{N} \sum_j \sum_i \hat{u}_{ij}^2$

The LM statistic is distributed as a standard normal variate. The values of the LM statistic for the teacher and police CoP samples were 9.61 and 9.50 respectively. Clear evidence of a group error structure is indicated. Both OLS and GLS estimates are reported in Tables 5 and 6 for the CoP samples. The variance of the city-specific error component is small relative to the variance of the individual specific error component. Similar to the findings in Moulton (1986), the most significant difference between OLS and GLS is the precision of the coefficient estimates for the variables with no within group variance. Allowing for omitted state and/or city variables through inclusion of a city specific error component can make an important difference in the inferences drawn from the data.

The last specification issue to investigate is the possible correlation between the city error component and the independent variables. The test of whether a common set of omitted variables influences both wages and

bargaining laws is a test of this correlation. The Hausman Chi Square

statistics (and the probability under the null hypothesis of observing a

statistic at least this large) for the teacher and police CoP samples are

$X^2_{2} = 7.23$ (0.78) and $X^2_{2} = 14.77$ (0.10) respectively.[11] The evidence of

correlation is not strong enough in either case to warrant using instru-

mental random effects. This finding is consistent with the Ellwood & Fine

(1983) results for unionization rates. The two alternative methods for

addressing the issue of left-out-variable bias in the bargaining law coeffi-

cients yield similar findings and will be discussed below.

While no formal model of public sector wage determination has been

presented, we can speculate on the likely wage effects of each type of

public sector bargaining law. The legal environment may affect public

nonunion wages through their influence of union "threat" effects. If threat

effects exist, then a prohibition against collective bargaining eliminates

the threat of unionization and consequently should lower nonunion wages. On

the other hand, Duty-to-Bargain laws make the threat of unionization more

credible and consequently should lead to higher nonunion wages. A similar

pattern of coefficients would be predicted for the union sector through the

influence of the legal environment on the relative bargaining power of

unions. The likely coefficients associated with access to strikes or arbi-

tration is less clear. These added provisions do give unions recourse if

negotiations with the employer are not progressing. In this sense one might

predict a positive wage effect. However, once negotiations have led to a

strike or to arbitration. it is not clear that the union should be

[11]The test only indicates whether this correlation exists or not. It does
not indicate the source of the correlation. The investigator must
identify the endogenous variables and instrument them. A followup
Hausman test will indicate if the correlation still exists or not.

systematically better off.

The regression results for the CPS and CoP samples of teachers and police are given in Tables 5 and 6. A summary of the effect of the legal environment on public sector union wage differentials is presented in Table 7. Examination of the coefficients for the nonlegal variables will be left to the reader. When no bargaining law variables are included in the specifications, the estimated union wage differentials range from 7-11% for teachers and 9-20% for police. With the exception of the CPS teacher sample, these estimates are close to the estimates which assume that a Duty-to-Bargain law (with no access to strikes or arbitration) is in effect. This suggests that the failure to control for the legal environment is not the principal explanation of why public union wage differentials are below private sector estimates.

The similarity between these two sets of union wage differential estimates, however, masks the important role that the legal environment plays in determining wage outcomes. Prohibitions against collective bargaining generally result in union wage premiums which are insignificantly different from zero. The exceptions to this result are the sizeable positive differentials produced by the separate union and nonunion wage equations. Public sector union wage differentials are positive in states with Meet-and-Confer laws (or no specific laws) and statistically signifi-cant for the CPS teacher sample and the CoP police sample. In almost every case, the union wage differential increases when Meet-and-Confer laws are strengthened to Duty-to-Bargain laws. The one exception is the police differential calculated using the separate union and nonunion wage coeffi-cients. The magnitude of the increase in the union premium varies both by type of worker and sample. For teachers, the union premium increases from

13.9% to 20.3% in the CPS sample and from 2.5% to 9.6% in the CoP sample.
For police, the union premium increases from 11.8% to 12.4% in the CPS
sample and from 12.8% to 21.7% in the CoP sample. Expanding the legal
rights of public sector unions from simple Meet-and-Confer laws to Duty-to-
Bargain laws is associated with a 6-8% higher union wage differential. The
one exception is the modest 0.4% increase in the premium for the CPS sample
of police.

    While the separate nonunion and union samples from the CPS are small
and may produce unreliable estimates, it is still interesting to look at the
differential effects of the legal variables by union status of workers.
Results differ by type of worker. For teachers, there does not appear to
be any evidence that bargaining laws add or detract from union threat
effects. For nonunion teachers, prohibitions against collective bargaining
do not significantly lower wages (by eliminating the threat effect) and
Duty-to-Bargain obligations do not significantly raise wages (by strength-
ening the threat effect). Instead, the bargaining laws affect union wage
differentials through their influence on union wages. In contrast, for
police there is more evidence of a threat effect. Since no police
bargaining units existed in states which prohibited collective bargaining,
no union coefficient could be estimated for this variable. There is no
evidence that prohibiting collective bargaining significantly reduces non-
union police wages. However, nonunion police in states with Duty-to-Bargain
laws receive significantly higher wages than other nonunion police. This
later finding is consistent with a union threat effect.

    The final aspects of the legal environment to examine are the access by
the union to strikes or arbitration and limitations placed on union security
clauses in states with right-to-work laws. The evidence on the effect of a

strike threat on wage outcomes is limited to the CPS sample of teachers.
Although the CoP sample of teachers is considerable larger, no teachers from
this sample worked in states with right-to-strike provisions. The evidence
indicates that teacher union premia are significantly lower in states which
allow teachers to strike. Disaggregating by nonunion and union indicates
that the strongest effect is wages of unionized teachers. Similarly, the
data indicate that union wage premia in general are lower in states which
allow access to arbitration. This effect is significant, though, only for
the CPS sample of teachers. Finally, in contrast to Farber's (1984) finding
that private sector union wage differentials are higher in states with
Right-to-Work laws, the data indicate that public sector union wage differ-
entials are generally lower in states with Right-to-Work laws. This effect
is significant only for the CPS sample of teachers.

Recall that the Hausman specification test for correlation between the
city-specific error component and the included variables was the first of
three types of tests for left-out-variable bias. No strong evidence of
correlation was indicated by the data. The second test is to include the
level of private sector unionization in the state as a proxy for the
prevailing attitude in the state toward unionism. If the bargaining laws
are primarily proxies for these underlying attitudes and if private sector
union coverage is strongly correlated with these attitudes, then controlling
for private union coverage should significantly diminish the explanatory
power of the bargaining law variables. Examining columns (6) of Tables 6
and 7 indicate that including private sector coverage in the specification
primarily affects the coefficient on public sector coverage and does not in
general diminish the importance of the legal variables. In most cases, in
fact, the coefficients on the legal variables increase in absolute value.

The results of the second test do not reject the null hypothesis of no significant left-out-variables bias. The third test is to include the public sector bargaining law variables in a private sector wage regression. The null hypothesis would suggest that public sector bargaining laws should have no effect on private sector union wage differentials. The alternative hypothesis would suggest that they would affect these differentials in the same way as they do in the public sector. The data indicate that public sector bargaining laws do significantly affect private sector wages, but do not significantly affect private sector union wage differentials. Specifically, private sector wages (private sector union wage differentials) are insignificantly lower (insignificantly higher) in states which prohibit collective bargaining, significantly higher (insignificantly lower) in states which impose a Duty-to-Bargain, and significantly lower (insignificantly lower) in states which provide access to arbitration.[12] Again, no evidence supporting the left-out-variable hypothesis is indicated in the data.

CONCLUSION

The aim of this paper has been to examine the role of the legal environment in determining the magnitude of public sector union wage differentials. Specifically, I investigated the hypothesis that estimates of public sector union wage differentials are lower than estimates of private sector union wage differentials due to the weaker legal rights afforded to most public sector unions at the bargaining table. Empirical estimates of the effects of the legal environment were obtained by taking advantage of the wide variation in public sector bargaining laws across

[12]A full set of private sector regression results is available upon request.

states. The possibility that these bargaining laws are correlated with city
or state-specific left-out-variables such as sentiment toward unions was
addressed in three separate fashions.

The results indicated that there was little difference in public sector
union wage differentials when no bargaining laws were controlled for and
when a Duty-to-Bargain law was assumed. This indicates that the legal
environment is not the likely explanation for the observed disparity in
union wage differential estimates across sectors. However, the data did
indicate that the legal environment was an important determinant of public
sector union wage differentials. Generally, the measured differential
increased as the bargaining rights of the union were strengthened. Finally,
no significant evidence of left-out-variable bias in the bargaining law
coefficients was indicated by the three tests which were carried out.

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TABLE 1   Frequency Distribution of Public Sector Barganing Laws By Region

Region	Collective Bargaining Prohibited	"Weak" Bargaining	"Weak" Duty-to-Bargain	"Strong" Duty-to-Bargain
Aggregate	7.35	45.59	26.47	20.59
Northeast	0.00	0.00	47.22	52.78
Central	0.00	47.92	27.08	25.00
South	20.58	60.30	19.12	0.00
West	1.92	57.70	19.23	

TABLE 2   Frequency Distribution of Union Security By Region

Region	No Public Sector Right-to-Work Law	Public Sector Right-to-Work Law
Aggregate	68.75	31.25
Northeast	100.00	0.00
Central	58.33	41.67
South	56.25	43.75
Weat	72.73	27.27

TABLE 3   Frequency Distribution of Union Security By Bargaining Rights

Union Security	Collective Bargaining Prohibited	Bargaining Rights	"Weak" Duty-to-Bargain	"Strong" Duty-to-Bargain
Right-to-Work Law	11.67	58.33	18.33	11.67
No Right-to-Work Law	6.06	35.61	31.82	26.52

TABLE 4   Summary Statistics

| | Teachers | | | | Police | | | |
| | CPS | | | CoP | CPS | | | CoP |
Variable	Pooled	NonUnion	Union	Pooled	Pooled	NonUnion	Union	Pooled
Some College				0.062 (0.241)	0.374 (0.485)	0.368 (0.484)	0.382 (0.488)	0.419 (0.493
College Grad	0.480 (0.499)	0.574 (0.495)	0.416 (0.493)	0.294 (0.456)	0.102 (0.304)	0.083 (0.277)	0.127 (0.335)	0.119 (0.324
1st Yr Post Grad	0.228 (0.420)	0.169 (0.375)	0.268 (0.443)	0.237 (0.425)				0.024 (0.153
2nd Yr Post Grad	0.267 (0.443)	0.218 (0.413)	0.301 (0.459)	0.208 (0.406)				0.025 (0.156
3rd Yr Post Grad				(0.092) (0.290)				
4th Yr Post Grad				(0.076) (0.265)				
Experience	13.693 (10.702)	12.948 (10.934)	14.203 (10.518)	15.388 (10.712)	16.335 (10.251)	17.222 (11.130)	15.173 (8.890)	16.303 (9.795
Male	0.355 (0.479)	0.304 (0.461)	0.389 (0.488)	0.333 (0.471)	0.968 (0.175)	0.965 (0.184)	0.973 (0.164)	0.930 (0.255
White	0.917 (0.276)	0.908 (0.290)	0.924 (0.265)	0.765 (0.424)	0.917 (0.276)	0.930 (0.255)	0.900 (0.301)	0.834 (0.372
Married	0.707 (0.455)	0.705 (0.456)	0.708 (0.455)	0.629 (0.483)	0.819 (0.386)	0.840 (0.368)	0.791 (0.409)	0.758 (0.428
Northeast	0.205 (0.404)	0.133 (0.340)	0.254 (0.436)		0.236 (0.426)	0.104 (0.306)	0.409 (0.494)	
South	0.290 (0.454)	0.439 (0.497)	0.188 (0.391)		0.268 (0.444)	0.382 (0.488)	0.118 (0.324)	
West	0.217 (0.413)	0.181 (0.385)	0.242 (0.429)		0.252 (0.435)	0.250 (0.434)	0.254 (0.438)	
Non SMSA	0.493 (0.500)	0.578 (0.494)	0.436 (0.496)		0.350 (0.478)	0.403 (0.492)	0.282 (0.452)	
Private Sector Wage Residual				0.061 (0.109)				0.080 (0.103

TABLE 4: Continued

		Teachers					Police		
		CPS		CoP			CPS		CoP
Variable	Pooled	NonUnion	Union	Pooled	Pooled	NonUnion	Union	Pooled
# Sch Districts				68.560 (74.977)				
# Cities in SMSA								88.840 (81.867
Public Unionism	0.594 (0.491)			0.499 (0.267)	0.433 (0.496)			0.635 (0.280
Priv Unionism				0.126 (0.088)				0.172 (0.095
Collective Barg Prohibited	0.019 (0.136)	0.106 (0.306)	0.032 (0.175)	0.017 (0.098)	0.055 (0.229)	0.090 (0.288)		0.009 (0.081
Duty-to-Barg	0.342 (0.475)	0.374 (0.484)	0.576 (0.494)	0.309 (0.328)	0.315 (0.465)	0.417 (0.495)	0.727 (0.447)	0.453 (0.400
Right-to-Arbit	0.035 (0.184)	0.046 (0.210)	0.059 (0.236)	0.011 (0.079)	0.106 (0.309)	0.111 (0.315)	0.245 (0.432)	0.302 (0.408
Right-to-Strike	0.059 (0.235)	0.069 (0.254)	0.099 (0.298)					
Right-to-Work	0.139 (0.346)	0.366 (0.482)	0.234 (0.424)	0.067 (0.199)	0.071 (0.257)	0.285 0.453)	0.164 (0.372)	0.090 (0.230
Ln Wage	1.761 (0.361)	1.845 (0.357)	1.840 (0.342)	5.710 (0.567)	1.810 (0.342)	1.740 (0.361)	5.837 (0.292)	(0.451

Note: Table lists variable means (standard deviations) for the variables used in the analysis. Bargaining law variables in the pooled specifacations have been interacted with the public unionism variable.

TABLE 5   Teacher Wage Regressions

| | 1977 CPS Data | | | 1980 CoP Data | | |
Variable	(1) Pooled OLS	(2) NonUnion OLS	(3) Union OLS	(4) Pooled OLS	(5) Pooled GLS	(6) Pooled GLS
Intercep	1.420 (0.070)	1.325 (0.105)	1.693 (0.099)	4.375 (0.028)	4.367 (0.032)	4.369 (0.032)
Some College				0.317 (0.029)	0.320 (0.025)	0.322 (0.029)
College Grad	0.046 (0.056)	0.116 (0.076)	-0.109 (0.088)	0.669 (0.025)	0.674 (0.025)	0.675 (0.025)
1st Yr Post Grad	0.127 (0.057)	0.193 (0.081)	-0.028 (0.089)	0.824 (0.025)	0.828 (0.025)	0.828 (0.026)
2nd Yr Post Grad	0.212 (0.057)	0.302 (0.079)	0.045 (0.088)	0.900 (0.026)	0.893 (0.025)	0.893 (0.025)
3rd Yr Post Grad				0.918 (0.027)	0.920 (0.027)	0.921 (0.027)
4th Yr Post Grad				0.941 (0.028)	0.941 (0.028)	0.942 (0.028)
Experience	0.022 (0.003)	0.020 (0.005)	0.025 (0.004)	0.040 (0.001)	0.039 (0.001)	0.039 (0.001)
Experience Sq (X100)	-0.041 (0.008)	-0.039 (0.013)	-0.047 (0.009)	-0.067 (0.004)	-0.067 (0.004)	-0.067 (0.004)
Male	0.094 (0.018)	0.120 (0.032)	0.076 (0.023)	0.139 (0.009)	0.139 (0.009)	0.139 (0.009)
White	-0.057 (0.032)	-0.072 (0.051)	-0.024 (0.040)	0.040 (0.009)	0.044 (0.010)	0.044 (0.010)
Married	0.016 (0.019)	0.053 (0.032)	-0.006 (0.024)	0.026 (0.009)	0.028 (0.009)	0.028 (0.009)
Northeast	0.069 (0.026)	0.001 (0.054)	0.114 (0.030)			
South	0.049 (0.024)	0.062 (0.045)	0.000 (0.034)			
West	0.097 (0.025)	0.098 (0.047)	0.094 (0.029)			

TABLE 5: Continued

	1977 CPS Data			1980 CoP Data		
Variable	(1) Pooled OLS	(2) NonUnion OLS	(3) Union OLS	(4) Pooled OLS	(5) Pooled GLS	(6) Pooled GLS
Non SMSA	-0.135 (0.018)	-0.100 (0.030)	-0.161 (0.022)			
Private Sector Wage				0.385 (0.044)	0.404 (0.070)	0.360 (0.070)
# Sch Districts (X100)				0.086 (0.007)	0.091 (0.017)	0.085 (0.016)
Public Unionism	0.139 (0.025)			0.025 (0.021)	0.020 (0.038)	-0.113 (0.057)
Priv Unionism						0.519 (0.166)
Collective Barg Prohibited	-0.114 (0.066)	-0.005 (0.053)	-0.056 (0.065)	-0.026 (0.044)	-0.022 (0.059)	-0.031 (0.058)
Duty-to-Barg	0.064 (0.026)	0.021 (0.039)	0.045 (0.026)	0.071 (0.018)	0.059 (0.030)	0.055 (0.029)
Right-to-Arbit	-0.079 (0.049)	-0.047 (0.073)	-0.096 (0.047)	-0.035 (0.052)	-0.011 (0.070)	-0.014 (0.069)
Right-to-Strike	-0.131 (0.039)	-0.050 (0.063)	-0.123 (0.037)			
Right-to-Work	-0.125 (0.029)	0.019 (0.036)	-0.094 (0.029)	-0.015 (0.026)	-0.015 (0.042)	0.040 (0.044)
N	1,279	519	760	13,569	13,569	13,569
R-Sq	0.327	0.215	0.346			
σ^2_ϵ	0.0888	0.1033	0.0781	0.2236	0.2205	0.2205
σ^2_α					0.0038	0.0034

Note: Standard errors are in parentheses. Bargaining law variables in pooled regressions
are interacted with the public unionism variable.

TABLE 6:   Police Wage Regressions

	1977 CPS Data			1980 CoP Data		
Variable	(1) Pooled OLS	(2) NonUnion OLS	(3) Union OLS	(4) Pooled OLS	(5) Pooled GLS	(6) Pooled GLS
Intercep	1.439 (0.127)	1.385 (0.178)	1.622 (0.191)	4.927 (0.036)	4.923 (0.042)	4.927 (0.042)
Some College	0.076 (0.040)	0.105 (0.056)	0.035 (0.059)	0.070 (0.014)	0.063 (0.014)	0.063 (0.014)
College Grad	0.188 (0.063)	0.267 (0.098)	0.109 (0.083)	0.138 (0.021)	0.137 (0.021)	0.136 (0.021)
1st Yr Post Grad				0.173 (0.041)	0.165 (0.040)	0.165 (0.040)
2nd Yr Post Grad				0.262 (0.040)	0.245 (0.040)	0.246 (0.040)
Experience	0.014 (0.006)	0.014 (0.008)	0.014 (0.010)	0.035 (0.002)	0.034 (0.002)	0.034 (0.002)
Experience Sq (X100)	-0.015 (0.014)	-0.014 (0.018)	-0.015 (0.023)	-0.065 (0.005)	-0.062 (0.005)	-0.062 (0.005)
Male	0.247 (0.101)	0.246 (0.139)	0.247 (0.158)	0.214 (0.025)	0.217 (0.024)	0.216 (0.025)
White	-0.053 (0.066)	-0.007 (0.098)	-0.124 (0.094)	0.089 (0.017)	0.092 (0.016)	0.092 (0.017)
Married	0.008 (0.047)	-0.049 (0.070)	0.071 (0.069)	0.039 (0.015)	0.043 (0.015)	0.043 (0.015)
Northeast	-0.004 (0.058)	-0.048 (0.103)	-0.073 (0.082)			
South	-0.112 (0.054)	-0.146 (0.078)	-0.124 (0.101)			
West	0.207 (0.053)	0.196 (0.075)	0.123 (0.086)			

TABLE 6: Continued

	1977 CPS Data			1980 CoP Data		
Variable	(1) Pooled OLS	(2) NonUnion OLS	(3) Union OLS	(4) Pooled OLS	(5) Pooled GLS	(6) Pooled GLS
Non SMSA	-0.285 (0.038)	-0.283 (0.053)	-0.222 (0.061)			
Private Sector Wage				0.864 (0.072)	0.795 (0.115)	0.719 (0.121)
# Cities in SMSA (X100)				0.042 (0.009)	0.045 (0.026)	0.043 (0.026)
Public Unionism	0.118 (0.064)			0.128 (0.037)	0.133 (0.057)	-0.059 (0.111)
Private Unionism						0.657 (0.327)
Collective Barg Prohibited	-0.096 (0.083)	-0.020 (0.097)		-0.216 (0.082)	-0.183 (0.112)	-0.210 (0.112)
Duty-to-Barg	0.006 (0.071)	0.155 (0.063)	0.042 (0.073)	0.089 (0.030)	0.078 (0.053)	0.123 (0.057)
Right-to-Arbit	-0.069 (0.069)	-0.090 (0.095)	-0.117 (0.070)	-0.022 (0.026)	-0.003 (0.049)	-0.049 (0.054)
Right-to-Work	-0.083 (0.079)	-0.012 (0.063)	-0.133 (0.080)	-0.047 (0.032)	-0.062 (0.058)	0.008 (0.067)
N	254	144	110	4,353	4,353	4,353
R-Sq	0.407	0.436	0.354			
σ^2_ϵ	0.0740	0.0820	0.0633	0.1605	0.1528	0.1529
σ^2_α					0.0100	0.0097

Note: Standard errors are in parentheses. Bargaining law variables in the pooled
regressions are interacted with the public unionism variable.

TABLE 7: Comparisons of Public and Private Sector
Union Wage Differentials

Legal Environment	Teachers		Police	
	CPS	CoP	CPS	CoP
No Legal Controls				
Pooled OLS	0.112	0.074	0.090	0.201
	(0.018)	(0.015)	(0.038)	(0.024)
Separate OLS	0.150		0.071	
	(0.028)		(0.052)	
Legal Controls				
Collective Bargaining Prohibited				
Pooled OLS	0.025	0.000	0.021	-0.089
	(0.065)	(0.046)	(0.105)	(0.083)
Pooled GLS		-0.002		-0.050
		(0.065)		(0.114)
Separate OLS	0.115		0.190	
	(0.086)		(0.118)	
Meet and Confer				
Pooled OLS	0.139	0.025	0.118	0.128
	(0.025)	(0.021)	(0.064)	(0.038)
Pooled GLS		0.020		0.133
		(0.038)		(0.057)
Separate OLS	0.172		0.170	
	(0.038)		(0.082)	
Duty-to-Bargain				
No Strikes or Arbit				
Pooled OLS	0.203	0.096	0.124	0.217
	(0.024)	(0.016)	(0.054)	(0.033)
Pooled GLS		0.079		0.210
		(0.030)		(0.056)
Separate OLS	0.180		0.057	
	(0.038)		(0.070)	

TABLE 7: Continued

Legal Environment	Teachers		Police	
	CPS	CoP	CPS	CoP
Duty-to-Bargain				
Access to Strikes or Arbitration				
Pooled OLS	0.123 (0.049)	0.061 (0.054)	0.054 (0.065)	0.195 (0.025)
Pooled GLS		0.068 (0.075)		0.208 (0.025)
Separate OLS	0.140 (0.093)		0.030 (0.100)	

Note: Standard errors are in parentheses