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PUBLIC SECTOR BARGAINING AND THE LOCAL BUDGETARY PROCESS

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ABSTRACT

This paper investigates how the fiscal environment and the budgetary process affect wage and employment determination in the local public sector. The structure of the local tax system is found to be influential with significantly higher wages occurring in cities with access to local sales and/or income taxes. State-imposed property tax limits are found to be associated with lower wages (but not overall payrolls per capita). We find evidence that skill enhancement may be an important policy tool. Local governments appear to successfully use it to mitigate the wage premia associated with strong state collective bargaining legislation. We also find that controlling for the human capital of teachers substantially reduces the well-known positive correlation between teacher wages and community income.

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I. INTRODUCTION

The last thirty years have witnessed dramatic changes in both local public sector labor markets and in the structure of local public finance. Since the 1950's, the local public sector has grown rapidly in size and has become increasingly unionized. Local public sector employment as a percentage of private nonagricultural employment has risen from 9.0% in 1950 to 12.4% in 1986. Unionization of local public employees began in 1959 following Wisconsin's passage of collective bargaining legislation. By 1982 the local public sector unionization rate reached 48.9%. These changes have brought about significant increases in city payrolls. From 1967 to 1982, local payrolls increased by 36% in real terms (Census of Governments, Guide to the 1982 Census of Governments, Table 4). Not only have overall taxes been increased in order to finance the increased local expenditures, but the composition of tax revenues has also changed dramatically. From 1967 to 1982, the fraction of overall municipal tax revenues raised through property taxes declined from 51.8% to 32.7%, reflecting a growing reliance on income taxes, sales taxes, and special charges (1982 Census of Government, Vol. 4, Finances of Municipal and Township Governments, Table 1). In addition, residents in some states have responded by passing legislation which places a cap on their property tax rate or which limits the allowable annual percentage increase in expenditures.

The rapid transformation of local public sector labor markets has prompted considerable research investigating the impact of local public sector unions on employment, wages, and productivity.[1] Following Inman (1981, 1982), we extend this literature by analyzing how the fiscal environment and the budgetary process affect wage and employment outcomes in the local public sector. Our work incorporates a broader spectrum of

economic, fiscal, and political variables than has been analyzed previously in the literature. Our specifications use micro wage data and aggregate wage, employment, and payroll data for a broad cross section of teachers, police, and fire personnel. The paper does not attempt to distinguish between competing bargaining models (See Ashenfelter & Brown (1986) and MaCurdy & Pencavel (1986)). However, our results provide important new evidence on the influence of the fiscal environment on wages and employment in the local public sector.

Three central findings emerge from our empirical work. First, we find that the structure of the local tax system has a significant impact on bargaining outcomes. The willingness and ability of residents to pay higher wages to their public employees is influenced by the extent to which they bear the burden of local taxation. It has long been thought that sales and income taxes may shift some of the local tax burden to nonresidents.[2] Conversely, the burden of special charges for schools is likely to be concentrated on resident users of the school system. We find that localities employing sales and/or income taxes tend to pay their public workers higher wages. Police and fire personnel aggregate wage rates are from 7-9% higher in communities with access to nonproperty taxes. Teachers appear to gain the most from nonproperty taxes with their aggregate wages being 20% higher. Only teacher payrolls are significantly higher due to the use of these taxes. However, teachers have been harmed by the recent trend toward the use of educational special charges. School districts that rely more heavily on these special charges pay lower wages, hire fewer teachers per capita, and have lower aggregate teacher payrolls per capita. A one percentage point increase in the fraction of total educational expenditures financed through education special charges is associated with a 16% drop in

teacher employment and a 17% drop in teacher payrolls.

The structure of the local tax system is determined not only by the composition of tax instruments available to localities, but also by legislated maximum allowable property tax rates. Both the micro and aggregate wage data indicate that the presence of rate caps is associated with lower wages for teachers, police, and fire personnel. After a ten year period of time (roughly three bargaining cycles), a rate cap is associated with a 2-5% reduction in teacher wages, a 2-3% reduction in police wages, and roughly a 7% reduction in fire wages. For fire personnel, there is also a significant reduction in employment associated with the presence of a rate cap.

The second important result involves how the structure of the local labor market influences local bargaining outcomes. A key labor market characteristic is the number of school districts (for teachers) and communities (for police and firemen) in the SMSA. There are two potential ways in which the number of jurisdictions can affect local public wages and employment levels. The labor literature suggests that the potential monopsony power of local governments diminishes as the number of competing government jurisdictions increases (call this the monopsony effect). This effect implies that a greater number of jurisdictions within a given labor market area should be associated with higher wages and employment levels. In contrast, the urban literature suggests that the number of alternative locational choices for residents increases with the number of school districts and communities in the SMSA. This reduces the ability of local public sector unions to extract locational rents from residents in the form of higher wages (call this the Tiebout effect). This effect implies that a greater number of jurisdictions within a given labor market should be

associated with lower wages and higher employment levels.

The data indicate that the monopsony effect generally dominates the Tiebout effect. A one standard deviation increase in the number of jurisdictions (an increase of 40 school districts for teachers and 41 communities for police and fire personnel) generates aggregate wage increases of 9% for teachers, 3% for police, and 5% for firemen. Employment and aggregate payrolls for police and firemen also are increasing in the number of communities.

The third finding from our empirical work is the important role for skill enhancement as a policy tool for local governments. Previous studies typically have used aggregate data which does not contain information on worker skill levels. The advantage of using micro data to study local public sector wages is that we can control for the education and experience of each worker. A well established empirical relationship in the aggregate data is the positive association between the median family income level in a community and the wages paid to teachers. This may reflect the ability of local public sector unions to appropriate higher wages from communities with a higher ability to pay. Alternatively, the positive correlation may be the consequence of richer communities paying compensating wage differentials to higher quality teachers. In the teacher wage regression, we find that the coefficient on median family income decreases by 40% when we control for the human capital characteristics of the teachers hired.

A related finding is that local governments appear to be successful in using skill enhancement to offset wage premia negotiated by teacher unions in states with strong public sector collective bargaining laws. When teacher characteristics are not controlled for in the wage regression, we find that teacher wages are about 10% higher in states with laws mandating a

duty-to-bargain by the local government. When we do control for teacher characteristics in the wage regression, we find that the duty-to-bargain effect on wages is reduced by over 50% and loses its statistical significance. A substantial portion of the duty-to-bargain wage premia is eliminated through skill enhancement.

II. A LOCAL PUBLIC SECTOR BARGAINING MODEL

The analytical framework established for studying private sector union/nonunion wage differentials is useful as well for investigating public sector wage determination. The "market structure" hypothesis states that union wage differentials will vary systematically with the firm's (industry's) ability to pay and with the structure of the bargaining. Using various measures of the ability to pay and the bargaining environment, Hendricks (1975), Mishel (1986), and Abowd & Tracy (1988) have had some success in explaining the determination of wages negotiated by private sector unions.

It is important to realize that public sector wages and employment are set as part of a broader budgetary process in which tax and spending policies are also established. A host of local economic factors help to determine the locality's fiscal policies and, thereby, its ability to pay its workers. In addition to communities having the ability to finance higher public sector payrolls, local public employees must also have the necessary bargaining power to successfully garner higher compensation. The relative bargaining strength of local public employees will be influenced by factors such as the size of their voting coalition in the community, the percent of owner-occupied housing in the community, and the nature of state laws governing public sector collective bargaining.[3] Inman (1981, 1982)

first developed a public sector bargaining model incorporating the local budgetary process. His model treats the local government as analogous to a firm and the public employees as the firm's workers. The model presented here borrows heavily from his initial formulation.

City-resident preferences are represented by a city "administrative" preference function over wages (ω) and public employment per resident (ℓ). Different behavioral models could justify the city administration's concern for ω and ℓ. The most natural has to do with the need for officials to be reelected. Voters are assumed to desire public services per resident (G) at the least cost to them in terms of private income (Y). The details of the political choice process itself are left undefined here and a general administrative preference function V(G,Y) is postulated as characterizing local fiscal decision-making. We make no attempt to distinguish empirically among various political choice processes. Our ultimate focus is on reduced form wage and employment equations. We assume that $V_G > 0$, $V_Y > 0$, and $V_{YY} < 0$.

The administrative preference function is mapped into (ω,ℓ) space as follows. The public sector production technology is assumed as given in (1)

$$G = g(\ell); \quad g_\ell > 0, \quad g_{\ell\ell} < 0. \tag{1}$$

Net resident income per capita is defined in (2) as gross (after federal and state taxes) resident income per capita (I) minus the resident's share of per capita local taxes (s) times total local taxes per capita (T),[4]

$$Y = I - sT , \tag{2}$$

where $s < 1$ if residents do not bear the full burden of local taxes. For simplicity, it is assumed that there is a single tax and that the local

budget constraint is given by (3) as

$$T \equiv \tau B = \omega \ell - z + d \tag{3}$$

where τ is the effective tax rate, B is the tax base, z is intergovernmental aid per capita, and d is the debt interest per capita. Again for simplicity, it is assumed that tax revenues go solely to pay the per capita wage bill $\omega \ell$.[5]

The administrative preference function V(G,Y) can now be transformed into (ω, ℓ) space as shown in (4)

$$V(G, Y) = v(\omega, \ell) = V[g(\ell), I - s(\omega \ell - z + d)] \quad . \tag{4}$$

The demand function for public workers is implicitly defined by the first-order condition for employment per capita, $v_\ell = 0$. Rearranging the first-order condition results in

$$(V_G/V_Y)g_\ell = s\omega \quad . \tag{5}$$

This is the standard relationship between labor's marginal revenue product and its marginal factor cost. Using Inman's analogy between local governments and firms, administrators act as if they hire labor at a subsidized wage rate (if $s < 1$) and sell government services for a price given by V_G/V_Y. The demand curve for public sector workers traces out the maximum points of the administrative indifference curves as illustrated in Figure 1. The appendix details the conditions under which this demand curve is downward sloping.

The union is assumed to value the excess of its negotiated wage over the alternative wage, ω_a, and the level of union employment so that union preferences are as given in (6).

$$U(\omega, \ell) = U(\omega - \omega_a, \ell) \tag{6}$$

We assume further that $U(0, \ell) = 0$ so that the alternative private sector wage sets the lower bound for negotiated wages.

The negotiated wage and employment levels depend upon the structure of the bargaining. The labor literature recently has focused attention on two specific bargaining models. In the monopoly wage model the union sets the contract wage and the firm sets the employment level. This bargaining outcome is along the demand curve for public sector workers as illustrated in Figure 2. In the efficient bargaining model the firm gains some bargaining power over wages but loses complete discretion to set employment. The bargaining outcome is along a contract curve as illustrated in Figure 3. The greater the union's relative bargaining power, ϕ, the further up the contract curve the negotiated outcome will be located.[6] The reduced form wage and employment equations are given by (7) and (8)

$$\omega = \omega[\omega_a, I, z, s, d, B, \phi] \tag{7}$$

$$\ell = \ell[\omega_a, I, z, s, d, B, \phi] \quad . \tag{8}$$

Neither the monopoly wage model nor the efficient bargaining model produces sign restrictions on the effects of parameter changes on public sector employment levels. Under reasonable assumptions for preferences and technologies, unambiguous sign restrictions for wage changes exist only for the monopoly wage model. Normality of government services and private consumption in the administrative preference function and normality of wages in the union preference function are sufficient for the following set of comparative static results: $\partial\omega/\partial I > 0$, $\partial\omega/\partial z > 0$, $\partial\omega/\partial d < 0$, $\partial\omega/\partial s < 0$ and $\partial\omega/\partial\omega_a > 0$.[7] The comparative static results for the efficient bargaining

model depend on the slope of the contract curve which is indeterminate in the case where the resident's demand for government services is normal and leisure is normal in the union utility function.[8]

The tax base enters the reduced form wage and employment equations only when binding tax rate limits exist in the locality. The wage and employment effects arising from a tax rate cap are illustrated in Figure 4. The budget constraint before the rate cap is put into effect is $\tau B = \omega L - z + d$. Assume now that an effective property tax rate cap is imposed which requires that $\tau \leq \bar{\tau}$. The local budget constraint becomes $\omega L \leq \bar{\tau} B + z - d$. Payrolls above this level are indicated as being in the restricted space. If a bargaining outcome would have ended up at a point such as A on the contract curve or such as B on the labor demand curve, then the imposition of the rate cap would force the bargaining outcomes down the relevant curves. If the rate cap is binding, wages should be lower. However, the employment effect is ambiguous as it depends upon the specific bargaining model.

III. DATA SOURCES AND DESCRIPTION

A. Wage, Employment, and Payroll Data

Data on individual workers are taken from the 1980 Census of Population (CoP) public use tapes. Variables include measures of education, job experience, race, sex, and marital status. By combining the A and the B samples (for which the Census reports there is 'negligible' overlap) and selecting every local public sector employee working in a central city, we construct a sample of 12,741 teachers in 131 cities, 4,251 police and 2,210 fire personnel in 140 cities.[9] The sample includes workers from forty-six states. Local public workers in other government functions were not analyzed primarily due to their relatively small sample sizes.[10]

The reduced form wage equation given by (8) refers to a worker's total compensation. Data on fringe benefits such as pensions are not available for the broad cross-section of local public sector workers used in our analysis. Using a wage versus a total compensation measure in the empirical work would bias the coefficient estimates of (8) if a wage/pension trade off exists in the labor market, and if worker preferences are correlated with our independent variables. However, Inman (1982) found that local public sector pensions were unit elastic with respect to wages. This implies that wages are generally proportional to total compensation. In a log wage regression, the factor of proportionality will be absorbed in the constant term leaving the remaining coefficient estimates unaffected.

Neither the CoP nor the SoG data contain a direct wage measure. For the CoP, a weekly wage was constructed by dividing reported annual earnings by the number of weeks worked in 1979.[11] For the SoG, a monthly wage was constructed by dividing reported full-time monthly payroll by full-time employment. The payroll data are for October, 1979 and represent gross dollars before deductions. They include employee contributions to retirement plans and the like, but they do not include employer costs of fringe benefits.

The CoP data are also used to construct a private sector alternative wage for each city. A 1/100 subsample of private sector workers in each central city was used to calculate the measure. We regressed each private sector worker's wage on a set of individual characteristics and then calculated the median wage residual by city. This alternative wage measure also serves as a proxy for intercity differences in the cost-of-living. No direct cost-of-living index exists for all of the cities in our sample.

The aggregate employment data are taken from the employment file of the 1979 SoG. We calculate full-time equivalent employment levels for each function unit. For teachers and to a lesser degree fire personnel, data for some central cities had to be gathered from special district records. The Bureau of the Census provided us with a mapping of these special district records back into their corresponding central cities. We aggregated the central city records and all of the pertinent special district records to obtain overall employment and payroll figures.

A weakness of the CoP data is that no union coverage variable is reported. We use unionization data for each function unit (i.e. teachers, police, and fire) from the 1979 Survey of Governments (SoG). For each city and function unit, we computed the percentage of workers organized as well as the percentage in bargaining units.[12]

B. Fiscal and Other City-Specific Variables

All city-specific variables used in this study apply specifically to the central city and are not SMSA averages. Some of these variables have been analyzed in previous empirical studies of public sector unionism while others have been used in different contexts in the urban and local public finance literatures. Constraints on the local budget process can be divided into three basic categories: legal, political, and economic.

The legal constraints include tax and revenue restrictions as well as collective bargaining legislation. The structure of local taxation can affect who bears the burden of local taxation. Data on tax instrument availability for each state are provided in the 1979-80 Significant Features of Fiscal Federalism, a publication of the Advisory Commission on Intergovernmental Relations (ACIR). We constructed an indicator variable

for the presence of a local income/wage or sales (general or selective) tax from these data.[13]

We also collected data on education special charges which are becoming an increasingly important revenue source for local school districts. These often take the form of tuition or textbook fees. We measure the importance of education charges by calculating from the 1979 SoG finance data the ratio of the revenue raised by education special charges to total educational expenditures.

Besides constraining the choice of taxes available to a city, states sometimes limit rates of taxation or rates of increases in expenditures for local governments. Based on data from the 1979-1980 Significant Features of Fiscal Federalism, we constructed an indicator variable for the presence of a property tax limitation. A tax limit can be nominal or effective in nature. A nominal limit exists if the total nominal millage is capped but not the assessment-sales ratio. An effective limit exists if the product of the nominal millage rate times the assessment-sales ratio is capped. It is possible that states enact these rate caps in response to existing high local public sector wages. To account for the possible endogeneity, we interact the indicator variable with the number of years prior to 1979 that the rate limit was put into effect. We also constrain this interaction to have a maximum value of ten. If contracts run for three years, we are assuming that the impact of these rate limits is fully realized after three contract periods.

The final type of legal constraint considered involves the nature of public sector bargaining laws. We classify these laws into four categories ranging from prohibitions against collective bargaining to a strong duty-to-bargain law. A strong duty-to-bargain law requires the city to negotiate a

contract and provides the union either the right to strike or access to arbitration. A weak duty-to-bargain law requires the city to negotiate a contract, but does not provide the union with access to strikes or arbitration. The fourth category includes states with either no explicit laws governing collective bargaining or with meet-and-confer laws which do not require a written contract. In addition, we include an indicator variable for whether the state has a right-to-work law covering its public employees. Data on the states legal environment are discussed in detail in Valletta & Freeman (1986).[14]

Political constraints include measures of public employee voting strength. Local public workers can constitute a large voting and lobbying block in a city or state. From the SoG, we calculate the proportion of each city's voting population accounted for by its public employees. Homeowners comprise a second important voting coalition. To the extent that efficient local land markets capitalize the discounted costs of public sector unionism, homeowners have a financial incentive to closely monitor public officials. From the 1983 County and City Data Book, we compiled data on the percentage of housing in a city which was owner-occupied in 1980.

The final political constraint investigated involves the form of the city government. Others have speculated that governments with city managers may be more efficient and superior negotiators (See Adrian & Press, 1968). We include an indicator variable for the presence of a city manager. This information was collected from the 1979 Municipal Year Book published by the International City Management Association.

A variety of variables are included to control for economic constraints on local public bargaining outcomes. We characterize the structure of the local labor market by either the number of school districts in the SMSA or

by the number of communities in the SMSA. These data are taken from the 1982 Census of Governments. The number of alternative school districts or communities can help identify monopsony and/or Tiebout effects.

A second category of economic variables proxies for the locality's ability to pay for public services. Our model indicates that the three factors which determine this ability to pay are income, intergovernmental aid, and debt service. We collected data on 1979 median family income for each city in our sample from the 1983 County and City Data Book. Annual per capita data on aid from state and federal sources are provided in City Government Finances. We computed averages of these aid figures over the 1976-1979 fiscal years to better reflect the average long run level of intergovernmental aid to the city. We computed similar averages for a city's overall annual debt service per capita using data in various issues of City Government Finances. Federal and state aid to local school districts were obtained from the finance file of the 1979 SoG. Overall figures were calculated by aggregating the central city records and all relevant special district records.

IV. ECONOMETRIC SPECIFICATIONS AND EMPIRICAL RESULTS

Separate wage, employment, and payroll specifications using aggregate data were estimated. They provide a base for comparison with previous results using aggregate data. For the micro wage regressions we assume that the wage for individual i working in the public sector in city j is determined as follows

$$\ln W_{ij} = \beta_0 + \beta_1 X_i + \beta_2 Z_j + \beta_3 U_{ij} + u_{ij} , \qquad (9)$$

$$u_{ij} = \alpha_j + \varepsilon_i, \quad \alpha_j \sim N(0, \sigma_\alpha^2), \quad \varepsilon_i \sim N(0, \sigma_\varepsilon^2)$$

where X_i represents a vector of individual characteristics; Z_j represents a vector of city-specific legal, political and economic variables; and U_{ij} is the unionization rate of individual i's type of government function in city j.

Recent studies of wage determination often estimate separate union and nonunion wage equations. A third equation describing the individual worker's "choice" of sectors is also estimated in order to correct for any selection bias. In many cases, individual coefficient estimates in the union wage equation differ significantly from their values in the nonunion wage equation. In particular, the marginal wage effects from education and job experience are generally found to be smaller in unionized jobs. Unfortunately, we could not successfully estimate separate union and nonunion wage equations for (9). Recall that the CoP data does not ask any question regarding a worker's union status. In order to estimate separate union and nonunion equations, we would have to fully interact our unionization measure with the variables in X and Z. This introduced too much collinearity into the estimation and produced unreliable coefficient estimates. Experimentation with limited interactions proved unsuccessful as well. Consequently, we chose to estimate a pooled union/nonunion wage equation with the level of unionization entered additively. We separately estimate wage equations for teachers, police, and firemen.[15]

In estimating (9) we allow the error term to have both an individual and a city-specific component. The city component, α_j, is common to all public sector workers in a given function unit in city j, and is assumed to

be uncorrelated across workers from different cities. There are several reasons why a random effects specification may be appropriate. The α's may represent the composite effect of left-out city attributes which affect public sector wages. Finally, for those workers in bargaining units with written contracts, the α's may reflect contract-specific wage effects.[16]

The empirical findings are presented in Tables 1-3. The coefficient estimates on the individual worker characteristics from the micro wage regression were consistent with those reported in other studies and are not listed here. Summary statistics for all variables are provided in Table 4. We focus on the results for the city-specific variables using the three categories of constraints presented in Section II as an organizational framework.

Consider first the legal constraints. Our findings are consistent with the hypothesis that the exportability of the burden of financing the local public sector is an important determinant of local bargaining outcomes. The aggregate wage results (in column three of each table) indicate that access to nonproperty taxes is associated with 7-20% higher wages. The largest effect is for teachers and all three effects are statistically significant. The micro wage results (in the first two columns of each table) indicate a positive relationship between wages and nonproperty taxes as well, but they are significant at standard levels only in the case of fire personnel. Employment levels (in column four of each table) in cities which have access to nonproperty taxes are significantly higher only for teachers. When combined with the wage effect, nonproperty taxes give rise to a 30% increase in overall teacher payrolls.[17] Police and fire payrolls are not significantly impacted.

We also analyzed the impacts of education special charges but do not report the results for space reasons.[18] The burden of these charges is far more likely to be borne by resident users of the schools. Although not precisely measured, the estimated coefficient indicates that a one percentage point increase in the fraction of overall educational expenditures financed via special charges (the sample mean and standard deviation of this variable is 2.2% and 1.1%, respectively) is associated with a one percent decrease in teacher wages. Associated with this one percent drop in wages is a statistically significant 16% drop in employment and 17% drop in payrolls.

The composition of the local revenue sources is only one of the important aspects of the structure of local finances. Property tax rate limits are consistently associated with lower wages both in the micro and the aggregate data. However, it is interesting to note that we find no significant impacts on employment or payrolls. This is in contrast to Inman (1982) who reported generally counter intuitive effects of rate limits on police and fire wages and employment. We suspect that Inman's findings suffer from endogeneity problems as he did not interact his contemporaneous rate limit variable with an indicator of how long the limit had been in force.

Our specification includes a single tax limit variable indicating the number of years prior to 1979 during which either a nominal or an effective limit existed in each city. We found no difference between the effects of nominal versus effective tax limits. This suggests that the costs of reassessment are high enough that, in the short run, nominal limits are equivalent to effective limits.

The data indicate that when these tax limits have been in place for at least 10 years they act to lower wages by 2-7% depending upon the function unit. The largest reductions were observed for firemen where both the micro and the aggregate data suggest roughly a 7% wage reduction. For teachers the effect is only significant in the micro data, while for police the effect is only significant in the micro data when we do not control for worker characteristics.

The state can also affect bargaining outcomes through enactment of legislation dealing with public sector collective bargaining. Our results are broadly consistent with earlier studies of the effects of collective bargaining laws; that is, wages tend to be higher the stronger the bargaining rights given to the local union, but there does not seem to be a significant wage advantage for bargaining units that can strike or demand arbitration.[19]

An issue which has not been previously studied is the effect of controlling for worker characteristics in assessing the wage impact of these bargaining laws. The high wages negotiated by unions in states with strong bargaining laws appear to be in large part offset by local school districts through skill enhancement of their employed teachers. Duty-to-bargain laws are found to be associated with 10% higher teacher wages (as compared to states with meet-and-confer laws or no specific legislation) when we estimate the regression without controlling for individual teacher characteristics (column two, Table 1). When we control for teacher characteristics in the regression, the weak and strong duty-to-bargain wage effects lose their statistical significance and are reduced by 62% and 76%, respectively. This same pattern shows up for police but does not exist for firemen.[20]

Farber (1984) contends that right-to-work (RTW) laws create a free-rider problem for private sector unions which diminishes their bargaining power and consequently the wages which they can negotiate for their members. We find no evidence of this effect for local public workers. The wage effects associated with RTW laws tend to be positive, but are never statistically significant. The positive wage effects are offset by negative negative employment effects leaving payrolls generally unchanged.

The second category of variables examined involves economic constraints on bargaining. A key economic constraint that has been examined in earlier studies is the structure of the local public sector labor market. Landon & Baird (1971), Hall & Vanderporten (1977), Inman (1981, 1982), and Feldman & Scheffler (1982) have documented a positive relationship between wages for various public sector workers and some proxy for local government monopsony power (often the number of competing jurisdictions within the local labor market). However, the Tiebout literature suggests that wages might decrease with the number of competing jurisdictions. Increases in the number of jurisdictions give rise to better locational alternatives for residents, and consequently greater effective mobility. The ability of local unions to appropriate locational rents may be a declining function of the resident's mobility costs.

It is possible that the Tiebout effect exists in the data but is masked in specifications which only look for linear relationships between wages and the number of jurisdictions. To pursue this we estimated specifications with linear and quadratic terms in the number of school districts or communities.[21] If the Tiebout effect eventually dominates the monopsony effect, then this should show up in the wage regressions as a negative coefficient on the quadratic term. The data provide little or no supporting

evidence for a Tiebout effect. For the teachers and police micro and aggregate data, the coefficient on the quadratic term was negative in sign but small in magnitude and statistically insignificant. For fire personnel, the coefficient on the quadratic term was negative in sign and statistically significant in the aggregate data, and negative in sign and statistically insignificant in the micro data. We report coefficient estimates for quadratic specifications only when the data rejected the simpler linear specification. However, in all three cases the turnaround point implied by the quadratic specification was outside the range of communities for all but a few central cities in our data. Associated with these wage effects are positive employment and payroll effects for police and fire personnel.

Another economic constraint suggested by the ability-to-pay hypothesis involves community income. A result that has consistently found support in previous studies is that holding constant the union's relative bargaining power, public sector unions can generate higher wages in communities with higher average incomes.[22] An issue which has not been addressed in the literature is whether this observed relationship reflects rent-sharing or simply compensating wage differentials for better quality public employees. Our model assumes homogeneous workers and deals only with rent-sharing. Previous studies have not been able to address this issue as they have primarily used aggregate data with no controls for worker quality. An advantage of using the CoP micro data is that proxies for the human capital stock of workers are available. We estimated the micro wage regressions with and without individual worker characteristics.

Our results indicate that the positive correlation between wages and community income partially reflects compensating differentials for teachers, but not for public safety employees. For teachers, controlling for worker

characteristics reduces the marginal impact of family income on wages by 40%. For police and fire personnel, there is no significant reduction in the income coefficient due to controlling for worker characteristics.

This finding can be interpreted in two ways. If the rent-sharing hypothesis is correct, then local school districts appear to be able to mitigate in large part the union-negotiated wage premia by upgrading the skills of its employed teachers. This is consistent with the earlier results on the smaller wage effects associated with duty-to-bargain laws as we control for worker characteristics. Another interpretation is that residents in wealthier communities desire higher quality teachers, and use compensating wage differentials to attract these teachers. In contrast, the data suggest that for police and firemen the observed positive relationship between income and wages arises mainly from rent-sharing.

The final set of economic variables considered is the level of federal and state aid to a community and its debt obligations. We find that higher federal aid levels raise wages slightly. Aggregate police and fire wages are significantly positively correlated with federal aid. The employment effects are positive as well and large in magnitude. The wage and employment effects for federal aid interact to generate large positive police and fire payroll effects. In contrast, variations in state aid has either no effect on wages or has a negative effect as in the micro data for police. Inman (1982) also found a negative correlation between state aid and police wages. The effect of debt obligations is generally to lower wages, although never in a precisely measured fashion. The associated employment and payroll effects are more mixed in their direction and are never statistically significant.

While not reported in the tables, we performed a test of the fungibility of government aid for the case of teachers. We find evidence of fungibility of federal aid for teacher employment decisions but not for teacher wages. The 1979 SoG finance data lists federal and state aid which is specifically directed toward local public school systems. If aid money is fungible, then one would expect to see a positive coefficient on the overall aid variable in a regression including the directed aid variable. That is, holding constant the level of aid directed toward education, cities with higher overall aid levels pay their teachers more and/or hire more teachers. The wage coefficients on the federal and state general aid variables are never significantly greater than zero. However, the employment coefficient on the federal general aid variable is both significantly positive and equal in magnitude to the coefficient on the federal directed aid variable (F-statistic = 0.003, p-value = 0.96). The coefficients indicate that an additional $100 per capita in either general or directed federal aid raises teacher employment by 21%-24%. This suggests a high degree of fungibility of federal aid for teacher employment decisions.

The final category of variables to consider involves political constraints and union organization levels. The ability of homeowners and public workers to discipline government officials through the ballot box depends in part on the size of their respective voting coalitions. Generally we find that the larger the proportion of residents who own homes (i.e. have an equity stake in the community), the lower the observed wage. However, this effect is never significant at standard levels. The larger the size of the overall local public sector voting coalition, the higher the observed wage. This effect is significant for teachers and firemen when

individual characteristics are not controlled for in the regression. The biggest impact is on teacher wages where a one percentage point increase in the proportion of voters who are local public employees is associated with about a 0.5% increase in teacher wages.[23]

Several studies have examined the wage and employment effects from having a city manager versus a mayor or city council. Ehrenberg (1973, 1980) finds positive and significant effects of city managers on the salary structure of firemen and police. Zax (1985) finds that city managers tend to reduce employment and to increase compensation. We find a similar pattern of effects in our data. Both police and firemen aggregate wages are significantly higher under city managers, with the magnitude of the increase being similar in magnitude to the increase associated with duty-to-bargain laws. This wage effect generally does not show up in the micro data. Associated with these wage increases are significant employment reductions which leave overall police and firemen payrolls unchanged. We find that aggregate teacher wages are unaffected by the form of city government in cities with independent school systems. In contrast, city managers are associated with around 11% higher aggregate teacher wages in city run school systems.[24]

The coefficient on the extent of public sector organization roughly measures the union wage premium holding constant the bargaining environment and other conditions. For the teachers and fire personnel, we find no statistically significant wage or employment impact of union organization that is independent of the bargaining environment. The estimated police union wage premium is 13-16%, with an associated employment reduction of over 25%. Both of these effects are statistically significant.[25] While no formal test of efficient bargaining models has been attempted in this study,

the sizeable employment reductions (holding constant a measure of the alternative wage) suggests that bargaining outcomes for police may not fall on a vertical contract curve (i.e. the "strong" efficient bargaining model). Note also that although unions generate wage premiums for police, they do not appear to be successful in raising overall police payrolls.

The model presented in Section II did not distinguish between different government functions within the overall local public budget. As a result, it did not address the issue of spillover effects from unionization in one government function affecting bargaining outcomes in another government function. In a careful analysis of this issue, Zax & Ichinowski (1988) find that higher levels of organization in other government function generally have positive wage and negative employment spillover effects.

To test for possible spillover effects in our data, we included in our specifications the overall percent organized in the city. Greater levels of unionization in the city were associated with higher wages for police and firemen, and lower wages for teachers. The employment effects were negative for teachers and police, and positive for firemen. None of these effects were statistically significant. For the specific groups of workers we examine, there is no evidence that they are adversely affected by competition from other public sector unions for a share of the local budget. This may reflect an ability of the unions to collectively raise overall budget expenditures.

V. CONCLUSION

We examined the influence of a wide variety of legal, political, and economic factors on public sector wages, employment, and payrolls. Bargaining was placed in the context of a local budgetary process. We found

that the structure of local taxes has important ramifications for bargaining outcomes. Cities that are successful in shifting some of the incidence of their local taxes to nonresidents are observed to pay higher wages to their public employees. States which have placed effective caps on local property tax rates have consequently exerted downward pressure on local public wages but not on overall payrolls. Protective collective bargaining legislation passed by states also has important wage effects, although there is evidence that city governments mitigate these wage effects through skill enhancement for some groups of its workers. Cities with higher levels of family income pay higher wages to their public employees. However, for teachers this wage effect is drastically reduced when we control for the human capital characteristics of the teachers. This suggests that either the wage premium negotiated for teachers is being offset by skill enhancement, or that these cities are voluntarily paying a wage premium in order to attract higher quality teachers. The structure of the local labor market is important with wages and employment levels generally increasing in the number of school districts or communities in the SMSA. This is consistent with a monopsony view but not with a Tiebout view. Voting coalition measures produced a consistent pattern of wage effects but they often were never precisely measured. Finally, city managers were found to raise wages and reduce employment leaving overall payrolls unchanged for police and firemen, and to raise wages for teachers in city run school systems.

APPENDIX

I. DEMAND FOR PUBLIC SECTOR EMPLOYMENT

The first order condition for labor from the administrative preference function is restated from (5)

$$V_{\ell}(\omega, \ell) = 0 \Rightarrow \frac{1}{s}\frac{V_G}{V_Y} g_{\ell} = \omega .$$

Totally differentiating this expression with respect to ℓ and ω and rearranging yields

$$\frac{d\omega}{d\ell} = \frac{\frac{V_G}{V_Y} g_{\ell\ell} + g_{\ell}\frac{1}{V_Y}\left[(V_{GG} - \frac{V_G}{V_Y} V_{GY}) g_{\ell} - (V_{GY} - \frac{V_G}{V_Y} V_{YY})\right] s\omega}{s\left[1 + g_{\ell}\frac{1}{V_Y}(V_{GY} - \frac{V_G}{V_Y} V_{YY})\ \ell\right]}$$

This expression is negative if government services and leisure are normal in residents' preferences. Normality of government services implies that $V_{GY} - (V_G/V_Y)V_{YY} > 0$. Normality of leisure implies that $V_{GG} - (V_G/V_Y)V_{GY} < 0$. Thus, if G is inferior it is possible for $d\omega/d\ell > 0$.

II. MONOPOLY WAGE MODEL: COMPARATIVE STATICS

The sign restrictions on employment and wages can be seen in the following example considering a small change in income (I). Holding constant the initial equilibrium (ω^*, ℓ^*) combination, consider the effect of an increase in I. At the initial equilibrium, we know that $(1/s)(V_G/V_Y)g_{\ell} = \omega$. Because G is not directly a function of I, the term V_G is unaffected. However, $dY/dI = 1$, so that V_Y has fallen in value

$(V_{YY} < 0)$. At the original equilibrium, it is now the case that $(1/s)(V_G/V_Y)g_{\ell} > \omega$. It is also the case that

$$\frac{\partial}{\partial \ell}\left\{\frac{1}{s}\frac{V_G}{V_Y}\,g_{\ell}\right\} = \frac{1}{s}\left\{\frac{V_G}{V_Y}\,g_{\ell\ell} + g_{\ell}\,\frac{1}{V_Y}\left[(V_{GG} - \frac{V_G}{V_Y}\,V_{YG})\,g_{\ell} - (V_{GY} - \frac{V_G}{V_Y}\,V_{YY})s\omega\right]\right\}$$

This expression is unambiguously negative if G and Y are both normal. That is, at a constant wage the demand curve rotates out and ℓ rises to restore equilibrium. This rotation of the demand curve generates income and substitution effects which result in a higher equilibrium wage so long as wages are a normal commodity in the union's preference function. Note that if G is inferior, all signs could be reversed.

III. EFFICIENT BARGAINING MODEL: COMPARATIVE STATICS

Because we have no good reason to impose a particular bargaining outcome on the problem, it is uncertain where the negotiations will end up along the contract curve. To simplify the comparative statics, we assume that in response to a parameter change the parties remain in the same relative position on the new contract curve. Holding employment (wages) constant, the shift in the contract curve determines the change in wages (employment).

To carry out this procedure, we differentiated the first-order condition, $-U_{\ell}/U_{\omega} = -\omega/\ell + (g_{\ell}/s\ell)(V_G/V_Y)$, with respect to the relevant model parameters. To isolate the effect of a change in income on wages holding employment constant, differentiating the first-order condition yields

$$\frac{\partial \omega}{\partial \bar{l}} = \frac{(\frac{g_{\bar{l}}}{s_{\bar{l}} V_Y})\ (V_{GY} - \frac{V_G}{V_Y} V_{YY})}{\frac{1}{\bar{l}} + \frac{g_{\bar{l}}}{V_Y}\ (V_{GY} - \frac{V_G}{V_Y} V_{YY}) - \frac{1}{U_\omega}\ (U_{\bar{l}\omega} - \frac{U_{\bar{l}}}{U_\omega} U_{\omega\omega})} \gtrless 0 \quad .$$

If G and $\bar{l}$ are normal in the administrative and union preferences, respectively, the expression cannot readily be signed. The numerator is unambiguously positive but the denominator is not signed. However, the sign can be determined if we also know the slope of the contract curve.

The slope of the contract curve is determined by totally differentiating the above first-order condition with respect to both wages and employment. This yields

$$\frac{\partial \omega}{\partial \bar{l}} = \frac{\frac{1}{U_\omega}\ (U_{\bar{l}\bar{l}} - \frac{U_{\bar{l}}}{U_\omega} U_{\omega\bar{l}}) + \frac{V_G}{V_Y}\ (\frac{g_{\bar{l}\bar{l}}}{(s_{\bar{l}})^2} - \frac{g_{\bar{l}}}{s_{\bar{l}}^2}) + \frac{\omega}{\bar{l}^2} + A}{\frac{1}{\bar{l}} + \frac{g_{\bar{l}}}{V_Y}\ (\ V_{GY} - \frac{V_G}{V_Y} V_{YY}) - \frac{1}{U_\omega}\ (U_{\bar{l}\omega} - \frac{U_{\bar{l}}}{U_\omega} U_{\omega\omega})}$$

where

$$A = (\frac{g_{\bar{l}}}{s_{\bar{l}}} \frac{1}{V_Y})\ [(V_{GG} - \frac{V_G}{V_Y} V_{YG})\ g_{\bar{l}} - (V_{GY} - \frac{V_G}{V_Y} V_{YY})\ s\omega] \quad .$$

Note that the denominator term for the slope of the contract curve is identical to the denominator term for the comparative static expression. If we can sign the numerator term for the slope, then prior information about the slope of the contract curve will be sufficient to determine sign of the comparative static expression.

Assuming normality of wages and employment in the union preferences and normality of services and income in the administrative preferences implies

that the numerator is negative. This is not immediately apparent since there is one positive term, ω/ℓ^2, while all the remaining terms in the numerator are negative by assumption. The additional information required to determine the overall sign is provided by the restriction that the administrative indifference curves be downward sloping and concave to the origin at the points along the contract curve. These restrictions yield

$$\frac{\partial \omega}{\partial \ell} = -\frac{\omega}{\ell} + \frac{g_\ell}{s\ell}\frac{V_G}{V_Y} < 0$$

$$\frac{\partial^2 \omega}{\partial \ell^2} = \frac{\omega}{\ell^2} + A - \left[\frac{1}{\ell} + \left(V_{GY} - \frac{V_G}{V_Y} V_{YY}\right) g_\ell \frac{1}{V_Y}\right] \frac{\partial \omega}{\partial \ell} < 0 \quad .$$

Concavity implies that the first two terms in $\partial^2\omega/\partial\ell^2$ must be negative. This is sufficient to guarantee that the numerator for the slope of the contract curve is also negative. The implication for the comparative statics is as follows. If the contract curve is positively (negatively) sloped, then an increase in income causes an outward (inward) shift of the contract curve.

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FOOTNOTES

[1]See the surveys by Ehrenberg & Schwarz (1986) and Freeman (1986) for excellent reviews of the existing literature.

[2]See McLure (1967) for an early empirical investigation into exporting the burden of state and local taxes. Mutti & Morgan (1983) provide a more recent overview of what they term indirect exportation via deductability and direct exportation via taxation of nonresidents' income and purchases.

[3]Other interested groups such as landowners and renters have an incentive to see that wage premia are not paid. To the extent that local public wage premia are financed either through higher taxes and/or lower effective service levels, efficient local land markets should capitalize these costs into lower land prices. Gyourko & Tracy (1989b) present empirical evidence that land markets do capitalize the costs of public sector unionism. This gives homeowners a clear financial incentive to monitor the manner in which public officials negotiate with public sector unions.

[4]This framework can be generalized to incorporate capitalization of future expected taxes and/or service reductions into current land prices. This only complicates the model without altering any of its qualitative features.

[5]This simple constraint abstracts from any matching rate for wage bill expenses (m), capital expenditures per capita (k), and new borrowing per capita (b) for capital spending. Including these factors expands the budget constraint such that $T = (1-m)\omega l - z + d + k - b$. Omitting these additional variables has no material effect on the relevant implications of the model for this paper.

[6]There is no role for the union's relative bargaining strength in the traditional monopoly wage bargaining model. The model can be modified to incorporate ϕ in the following manner. Assume that the negotiated outcome is located along the demand curve between the union's most preferred position (given by the tangency point) and the firm's most preferred position (given by ω_a). Higher values of ϕ move the negotiated contract up the demand curve toward point A in figure 2.

[7]Generally, if a parameter change causes the demand for public workers to rotate out, the substitution and income effects work in the same direction for wages but in opposite directions for employment in the monopoly theory model. See the appendix for details.

[8]This amplifies the argument by Harrison (1987) which states that prior information is needed about the technology of production or about union preferences to be able to distinguish between the monopoly wage and the efficient bargaining models. The appendix provides the details of the various assumptions required to yield comparative static results for the efficient bargaining model.

[9]There are potentially over 200 cities which can be identified in the CoP data. We drop those cities from SMSA's with multiple central cities due to

the need to accurately match a city's local public workers with city-specific variables. Problems with missing fiscal data further reduced the sample of cities.

[10]An exception is the clerical workers who were well represented in the data. These workers were not included in the analysis because no reliable union coverage measure could be calculated. See the discussion in footnote #12 for details.

[11]Annual earnings is reported as a continuous variable with a ceiling of $75,000. None of the public sector workers in our sample were affected by this truncation.

[12]In the empirical work we use only the percentage organized variable since it appeared to be the more reliable unionization measure. The percentage organized is based exclusively on employment file data. The percentage in bargaining units is calculated using data from separate files of the SoG. The number of workers in bargaining units (the numerator) is taken from the bargaining unit file while the number of workers employed (the denominator) is taken from the employment file. In some cases, the SoG definition of a bargaining unit did not conform to a specific function unit in the employment file. For example, bargaining units for clerical workers are often organized across more than one function unit. This led to numerous cases of the percentage in bargaining units exceeding 100%. We use the percentage organized to avoid this problem. The percent in bargaining units measure was used on occassion to adjust the percent organized measure. In states where collective bargaining is prohibited, if the percentage organized was greater than zero and the percentage in bargaining units was equal to zero, then the percentage organized was set equal to zero. If the percentage organized was equal to zero while the percentage in bargaining units was significantly greater than zero, then the percentage organized was set equal to the percentage in bargaining units.

[13]We have not been successful in locating data on the extent of nonresident income or consumption. Available data on nonresident income taxes is available only at the state and not the city level.

[14]We thank them for providing and assisting us with the data.

[15]We tested for the appropriateness of pooling across these three government functions. Complete pooling as well as all pairwise pooling were rejected by the data.

[16]To test for the appropriateness of OLS vs Random Effects, we calculated the one-sided Lagrange Multiplier (LM) for the null hypothesis that $\sigma_\alpha^2 = 0$ (See Moulton (1987)). For each government function, the data strongly rejected the null hypothesis of zero group variance. We also tested for possible correlation between the city error term and the included variables. If this correlation were present, then the Random Effects coefficient estimates would be biased. The Hausman tests did not reject the null hypothesis of zero correlation (See Hausman and Taylor (1981)).

[17]Inman (1982) did not examine the effect of access to nonproperty taxes on

wages and employment, but he did find limited evidence that pensions per capita were impacted. He reported that the percentage of tax revenues raised from the property tax had a significantly negative effect on police pensions and an insignificantly positive effect on fire pensions. Variation in the percentage of tax revenues arising from sales taxes was not found to have any significant influence on pensions.

[18]Including special charges in the specification cuts the number of cities in the sample to ninety. The coefficients on other variables are not materially affected.

[19]See Freeman & Valletta (1988) and Freeman & Ichniowski (1988). The limited nature of our unionization information prevent us from examining issues of threat effects arising from bargaining legislation. See Freeman & Ichniowski (1988) for an analysis of this issue for police.

[20]The interpretation given here to the bargaining law variables is that they affect wages through altering the union's relative bargaining power. An alternative interpretation is that they reflect prevailing attitudes in communities toward unionism. That is, communities which pass protective labor legislation may also be more willing in general to finance higher wages to its local public workers. To address this issue we included the level of private sector unionization in the state as a proxy for the public sentiment toward unionism. Adding this variable to the specification only slightly diminished the impact of the duty-to-bargain laws. This is in sharp contrast to the affect of controlling for individual worker characteristics as discussed above.

[21]We thank Bob Inman for suggesting this extension to us.

[22]See for example Edwards & Edwards (1982), Ehrenberg (1980), Ehrenberg, Sherman, & Schwarz (1983), Hall & Carroll (1973), Hall & Vanderporten (1977) and Inman (1981).

[23]We excluded the public sector voting strength measure from the aggregate employment and payroll specifications because the dependent variables are directly related by construction to the political measure.

[24]Approximately 81% of our cities have independent school systems. We estimated teacher specifications which excluded the city run school systems, as well as specifications which included additional interaction terms. These specification changes had only marginal effects on the results reported in Table 1.

[25]We experimented with the percent in bargaining units measure of unionization and obtained qualitatively similar results. See footnote #12 for a discussion of why we report results based on the precent organized.

Table 1: Teacher Regressions

Variable	CoP Data		SoG Data		
	Ln Wage (1)	Ln Wage (2)	Ln Wage (3)	Ln Empl (4)	Ln Payroll (5)
Log City Population				0.893**	0.960**
				(0.064)	(0.070)
Non Property Tax	0.009	0.020	0.192**	0.141	0.306**
	(0.026)	(0.021)	(0.066)	(0.121)	(0.133)
Property Tax Rate Limit	-0.0045**	-0.0048**	-0.000	0.014	0.015
	(0.0021)	(0.0017)	(0.006)	(0.011)	(0.012)
Number of School Districts	0.0010**	0.0012**	0.0022**	-0.0020	-0.0009
	(0.0002)	(0.0001)	(0.0006)	(0.0014)	(0.0016)
Private Sector Wage Residual	0.117	0.096	-0.079	0.416	0.379
	(0.109)	(0.089)	(0.274)	(0.472)	(0.518)
Median Family Income ($000)	0.013**	0.020**	0.029*	0.036	0.063**
	(0.006)	(0.005)	(0.015)	(0.027)	(0.030)
Federal Aid Per Capita ($00)	0.012	-0.004	-0.062	0.161*	0.107
	(0.018)	(0.012)	(0.051)	(0.086)	(0.095)
State Aid Per Capita ($00)	0.002	0.000	0.008	-0.002	0.000
	(0.008)	(0.005)	(0.025)	(0.044)	(0.048)
Debt Interest Per Capita ($00)	0.015	-0.017	0.210	0.120	0.289
	(0.061)	(0.044)	(0.176)	(0.310)	(0.341)
Percent Owner Occupied Housing	-0.0014	-0.0011	-0.0012	-0.0092	-0.0108
	(0.0013)	(0.0010)	(0.0037)	(0.0064)	(0.0070)
Public Sector Voting Strength	0.002	0.011**	0.030		
	(0.007)	(0.005)	(0.019)		
City Manager	0.050	0.070**	-0.113	0.030	0.119
	(0.040)	(0.034)	(0.113)	(0.203)	(0.222)
City Manager X Indep. School District	-0.035	-0.036	-0.149	0.109	-0.060
	(0.043)	(0.036)	(0.122)	(0.216)	(0.237)
Percent Private Sector Unionization	0.442**	0.537**	-0.011	-0.818	-0.814
	(0.121)	(0.099)	(0.323)	(0.567)	(0.623)

Table 1: Teacher Regressions Continued

Variable	CoP Data		SoG Data		
	Ln Wage (1)	Ln Wage (2)	Ln Wage (3)	Ln Empl (4)	Ln Payroll (5)
Percent Organized	0.014	-0.063*	-0.074	-0.097	-0.223
	(0.041)	(0.033)	(0.107)	(0.184)	(0.201)
Collective Bargaining Prohibited	0.016	-0.038	-0.212**	-0.094	-0.318
	(0.041)	(0.035)	(0.105)	(0.185)	(0.203)
Weak Duty-to-Bargain	0.029	0.117**	0.047	-0.104	-0.064
	(0.021)	(0.016)	(0.059)	(0.103)	(0.113)
Strong Duty-to-Bargain	0.042	0.097**	-0.018	0.116	0.143
	(0.031)	(0.023)	(0.082)	(0.141)	(0.155)
Right-to-Work	0.037	0.029	-0.041	-0.061	-0.139
	(0.025)	(0.020)	(0.067)	(0.120)	(0.131)
Percent College Educated	-0.007**	-0.008**	-0.028**	-0.012	-0.039**
	(0.002)	(0.002)	(0.006)	(0.011)	(0.012)
Control For Worker Characteristics	Yes	No	No	No	No
σ_1^2	0.222	0.306			
σ_α^2	0.003		0.056	0.169	0.204
R-Sq	0.502 [a]	0.562 [a]	0.422	0.836	0.846
N	12,741	12,741	131	131	131

Note: Standard errors are given in parentheses. Significance levels are denoted as follows: * denotes significance at a 10% level, ** denotes significance at a 5% level.

a) R-square statistic is taken from a second stage regression with estimated city fixed effects as the dependent variable and city-specific variables as the dependent variables. For specification (1) the city fixed effects are estimated in a first-stage regression including controls for worker characteristics. For specification (2) no controls for worker characteristics are included in the first-stage regression.

Table 2: Police Regressions

Variable	CoP Data		SoG Data		
	Ln Wage (1)	Ln Wage (2)	Ln Wage (3)	Ln Empl (4)	Ln Payroll (5)
Log City Population				0.997**	1.067**
				(0.026)	(0.036)
Non Property Tax	0.052	0.021	0.071*	-0.077	-0.047
	(0.044)	(0.032)	(0.042)	(0.051)	(0.070)
Property Tax Rate Limit	-0.0022	-0.0052**	-0.0033	0.0018	-0.0003
	(0.0034)	(0.0022)	(0.0036)	(0.0043)	(0.0058)
Number of Communities	0.0004	0.0009**	0.0007*	0.0029**	0.0028**
	(0.0003)	(0.0002)	(0.0004)	(0.0006)	(0.0008)
Private Sector Wage Residual	0.516**	0.609**	0.363**	0.544**	0.835**
	(0.161)	(0.111)	(0.153)	(0.184)	(0.249)
Median Family Income ($000)	0.008	0.008*	0.019**	-0.030**	-0.014
	(0.006)	(0.004)	(0.006)	(0.008)	(0.010)
Federal Aid Per Capita ($00)	0.017	0.010	0.057**	0.110**	0.162**
	(0.026)	(0.015)	(0.028)	(0.034)	(0.045)
State Aid Per Capita ($00)	-0.011	-0.012*	0.010	0.036*	0.032
	(0.013)	(0.007)	(0.015)	(0.019)	(0.025)
Debt Interest Per Capita ($00)	0.095	0.081	-0.087	0.049	-0.048
	(0.095)	(0.051)	(0.103)	(0.123)	(0.167)
Percent Owner Occupied Housing	-0.0021	-0.0007	0.0018	-0.0031	-0.0043
	(0.0018)	(0.0010)	(0.0019)	(0.0023)	(0.0031)
Public Sector Voting Strength	0.0087	0.0115	-0.0046		
	(0.0112)	(0.0073)	(0.0019)		
City Manager	0.0480*	0.0396**	0.100**	-0.112**	-0.006
	(0.0291)	(0.0193)	(0.030)	(0.036)	(0.049)
Percent Private Sector Unionization	0.342	0.304	0.359	0.104	0.545
	(0.235)	(0.162)	(0.232)	(0.280)	(0.379)
Percent Organized	0.162**	0.161**	0.131**	-0.256**	-0.156*
	(0.058)	(0.039)	(0.058)	(0.068)	(0.092)

Table 2: Police Regressions Continued

Variable	CoP Data		SoG Data		
	Ln Wage (1)	Ln Wage (2)	Ln Wage (3)	Ln Empl (4)	Ln Payroll (5)
Collective Bargaining Prohibited	-0.073	-0.059	0.055	0.038	0.113
	(0.068)	(0.053)	(0.065)	(0.079)	(0.106)
Weak Duty-to-Bargain	0.012	0.052**	0.101**	0.052	0.154**
	(0.039)	(0.024)	(0.042)	(0.050)	(0.068)
Strong Duty-to-Bargain	0.050	0.087**	0.124**	0.046	0.192**
	(0.043)	(0.025)	(0.042)	(0.050)	(0.068)
Right-to-Work	0.028	0.004	0.045	-0.009	0.014
	(0.156)	(0.029)	(0.044)	(0.053)	(0.072)
Control For Worker Characteristics	Yes	No	No	No	No
σ_i^2	0.155	0.181			
σ_α^2	0.010		0.024	0.034	0.062
R-sq	0.468 [a]	0.476 [a]	0.452	0.975	0.962
N	4,251	4,251	131	131	131

Note: Standard errors are given in parentheses. Significance levels are denoted as follows: * denotes significance at a 10% level, ** denotes significance at a 5% level.

a) R-square statistic is taken from a second stage regression with estimated city fixed effects as the dependent variable and city-specific variables as the dependent variables. For specification (1) the city fixed effects are estimated in a first-stage regression including controls for worker characteristics. For specification (2) no controls for worker characteristics are included in the first-stage regression.

Table 3: Fire Regressions

Variable	CoP Data		SoG Data		
	Ln Wage (1)	Ln Wage (2)	Ln Wage (3)	Ln Empl (4)	Ln Payroll (5)
Log City Population				0.871**	0.962**
				(0.056)	(0.064)
Non Property Tax	0.090*	0.078**	0.088*	-0.132	-0.085
	(0.052)	(0.036)	(0.046)	(0.108)	(0.122)
Property Tax Rate Limit	-0.0071*	-0.0063**	-0.0068*	-0.0079	-0.0129
	(0.0043)	(0.0028)	(0.0038)	(0.0090)	(0.0102)
Number of Communities	0.0004	0.0002	0.0018*	0.0089**	0.0064*
	(0.0004)	(0.0002)	(0.0011)	(0.0029)	(0.0033)
Number of Communities Squared (x100)			-0.0006	-0.0024**	-0.0025*
			(0.0005)	(0.0012)	(0.0014)
Private Sector Wage Residual	0.800**	0.625**	0.426**	0.800**	1.140*
	(0.197)	(0.137)	(0.165)	(0.386)	(0.438)
Median Family Income ($000)	0.012	0.010*	0.019**	-0.037**	-0.023
	(0.008)	(0.005)	(0.007)	(0.016)	(0.018)
Federal Aid Per Capita ($00)	0.017	0.016	0.060**	0.088	0.146*
	(0.033)	(0.021)	(0.030)	(0.070)	(0.080)
State Aid Per Capita ($00)	0.001	0.002	0.006	0.047	0.035
	(0.017)	(0.010)	(0.016)	(0.040)	(0.045)
Debt Interest Per Capita ($00)	-0.008	0.040	-0.045	-0.196	-0.216
	(0.115)	(0.064)	(0.116)	(0.273)	(0.310)
Percent Owner Occupied Housing	-0.0011	-0.0021	-0.0022	0.0046	0.0030
	(0.0023)	(0.0014)	(0.0021)	(0.0048)	(0.0055)
Public Sector Voting Strength	0.0158	0.0162*	-0.0005		
	(0.0144)	(0.0097)	(0.0128)		
City Manager	-0.0025	-0.013	0.075**	-0.145*	-0.060
	(0.0353)	(0.024)	(0.033)	(0.077)	(0.087)
Percent Private Sector Unionization	0.128	0.690**	0.310	-0.866	-0.433
	(0.280)	(0.191)	(0.244)	(0.587)	(0.656)

Table 3: Fire Regressions Continued

Variable	CoP Data		SoG Data		
	Ln Wage (1)	Ln Wage (2)	Ln Wage (3)	Ln Empl (4)	Ln Payroll (5)
Percent Organized	0.061	0.067*	0.069	-0.158	-0.098
	(0.053)	(0.038)	(0.047)	(0.105)	(0.119)
Collective Bargaining Prohibited	0.033	0.009	0.023	0.271*	0.315*
	(0.082)	(0.058)	(0.070)	(0.166)	(0.188)
Weak Duty-to-Bargain	0.068	0.106**	0.111**	-0.081	0.025
	(0.046)	(0.030)	(0.043)	(0.102)	(0.115)
Strong Duty-to-Bargain	0.103**	0.096**	0.109**	0.091	0.225**
	(0.046)	(0.029)	(0.044)	(0.101)	(0.115)
Right-to-Work	0.047	0.049	0.019	-0.129	-0.137
	(0.054)	(0.039)	(0.047)	(0.111)	(0.126)
Control For Worker Characteristics	Yes	No	No	No	No
σ_i^2	0.146	0.178			
σ_α^2	0.016		0.027	0.152	0.194
R-Sq	0.281 [a]	0.282 [a]	0.479	0.872	0.866
N	2,210	2,210	131	131	131

Note: Standard errors are given in parentheses. Significance levels are denoted as follows: * denotes significance at a 10% level, ** denotes significance at a 5% level.

a) R-square statistic is taken from a second stage regression with estimated city fixed effects as the dependent variable and city-specific variables as the dependent variables. For specification (1) the city fixed effects are estimated in a first-stage regression including controls for worker characteristics. For specification (2) no controls for worker characteristics are included in the first-stage regression.

Table 4: Summary Statistics

Variable	Teachers		Police		Firemen	
	CoP (1)	SoG (2)	CoP (3)	SoG (4)	CoP (5)	SoG (6)
Ln Wage	5.715 (0.569)	7.212 (0.286)	5.838 (0.452)	7.213 (0.207)	5.847 (0.446)	7.272 (0.213)
Ln Empl		7.819 (0.942)		6.147 (1.099)		5.820 (1.020)
Ln Payroll		15.030 (1.068)		7.213 (0.207)		13.092 (1.128)
Ln City Population		12.114 (0.930)		12.103 (0.933)		12.103 (0.933)
Non Property Tax	0.919 (0.272)	0.817 (0.388)	0.953 (0.250)	0.829 (0.378)	0.891 (0.311)	0.829 (0.378)
Property Tax Rate Limit	4.918 (4.133)	4.672 (4.007)	5.698 (4.210)	4.864 (4.054)	5.287 (4.182)	4.864 (4.054)
Number of School Districts	72.520 (75.677)	27.382 (39.795)				
Number of Communities			90.634 (81.995)	29.957 (41.028)	72.565 (73.420)	29.957 (41.028)
Private Sector Wage Residual	0.064 (0.103)	0.020 (0.112)	0.080 (0.101)	0.014 (0.113)	0.058 (0.104)	0.014 (0.113)
Median Family Income ($000)	18.417 (2.331)	18.616 (2.598)	18.056 (2.194)	18.365 (2.618)	18.008 (2.298)	18.365 (2.618)
Federal Aid Per Capita ($00)	1.217 (0.589)	0.988 (0.568)	1.313 (0.600)	1.030 (0.598)	1.264 (0.607)	1.030 (0.598)
State Aid Per Capita ($00)	1.968 (2.564)	1.070 (1.391)	1.818 (2.309)	1.041 (1.349)	1.718 (2.198)	1.041 (1.349)
Debt Interest Per Capita ($00)	0.303 (0.238)	0.213 (0.159)	0.307 (0.223)	0.225 (0.169)	0.303 (0.218)	0.225 (0.169)
Percent Owner Occupied Housing	48.078 (13.171)	54.568 (9.874)	48.081 (12.603)	54.211 (9.983)	48.988 (12.492)	54.211 (9.983)

Table 4: Continued

Variable	Teachers		Police		Firemen	
	CoP (1)	SoG (2)	CoP (3)	SoG (4)	CoP (5)	SoG (6)
Public Sector Voting Strength	4.653 (1.240)	4.387 (1.254)	4.600 (1.174)	4.307 (1.269)	4.530 (1.221)	4.307 (1.269)
City Manager	0.286 (0.452)	0.412 (0.494)	0.227 (0.419)	0.421 (0.496)	0.275 (0.446)	0.421 (0.496)
% Private Sector Unionization	0.247 (0.082)	0.236 (0.088)	0.259 (0.085)	0.228 (0.093)	0.247 (0.086)	0.228 (0.093)
Percent Organized	0.601 (0.221)	0.550 (0.280)	0.634 (0.262)	0.546 (0.300)	0.735 (0.349)	0.728 (0.374)
Collective Bargaining Prohib.	0.043 (0.204)	0.092 (0.290)	0.022 (0.147)	0.064 (0.246)	0.040 (0.197)	0.064 (0.246)
Weak Duty-to-Bargain	0.530 (0.499)	0.419 (0.495)	0.227 (0.419)	0.257 (0.439)	0.239 (0.426)	0.257 (0.439)
Strong Duty-to Bargain	0.098 (0.297)	0.122 (0.329)	0.371 (0.483)	0.300 (0.460)	0.361 (0.480)	0.300 (0.460)
Right-to-Work	0.201 (0.401)	0.221 (0.417)	0.163 (0.369)	0.250 (0.435)	0.200 (0.400)	0.250 (0.435)
Percent College Educated	15.349 (4.684)	14.969 (5.544)				

Note: Figures in the table correspond to the variable sample means (standard deviations).

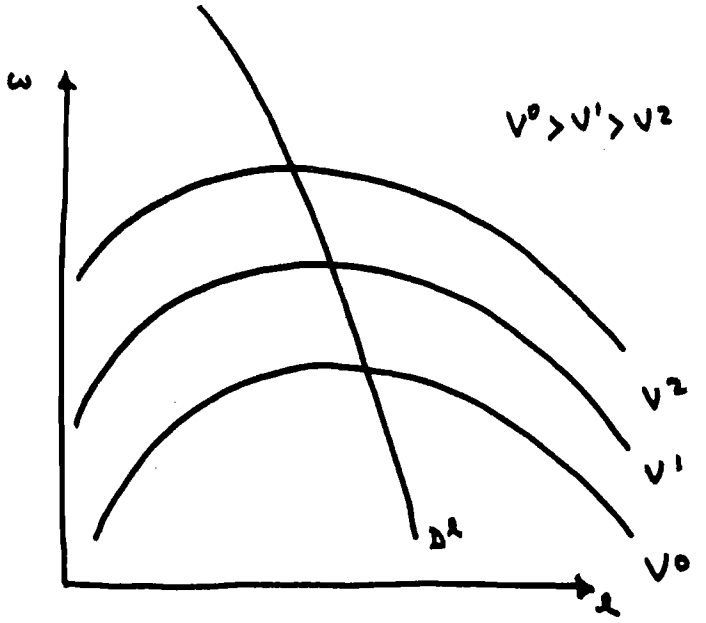

FIGURE 1

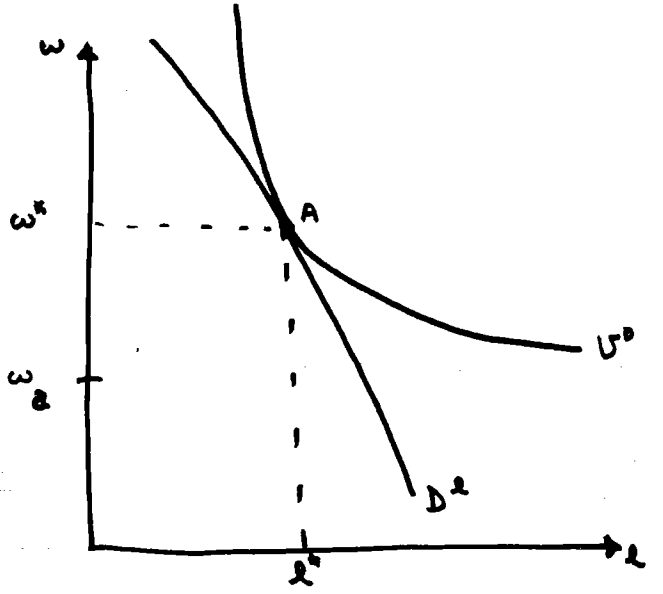

FIGURE 2

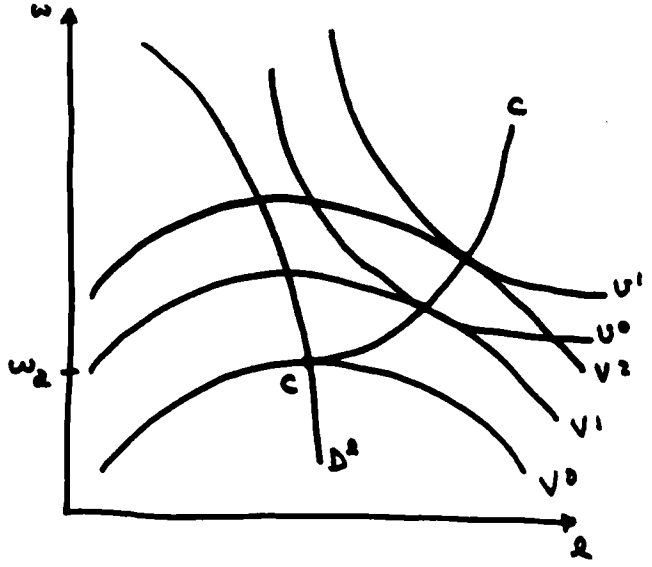

FIGURE 3

FIGURE 4