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PUBLIC SECTOR LABOR MARKETS

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ABSTRACT

This paper provides a critical survey of the literature dealing with public sector labor markets. It discusses the research by economists on wage determination in the state and local sector (including the effects of unions), on the estimaton of compensating wage differentials for pecuniary and nonpecuniary job characteristics, on the effects of unions on productivity, on the estimation of public sector demand and for labor functions, on dispute resolution, on public/private pay differentials, and on gender and race discrimination in the public sector. Numerous suggestions for future research are offered.

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I. Introduction

Why does the study of public sector labor markets in the United States warrant a separate chapter in this Handbook of Labor Economics? One reason is that federal, state, and local governments are differentiated from most (but not all) private-sector employers in that profit maximization is unlikely to be an objective of governmental units.[1] As such, labor-market models based upon the assumption of profit maximization are clearly inappropriate for the government sector; alternative models must be developed.

A second is that employment expanded more rapidly between 1950 and 1975 in the state and local government (SLG) sector than in any other sector of the economy. While civilian employment by the federal government (when expressed as a percentage of total nonagricultural employment) actually declined slightly during the period, SLG employment rose from 9.1 percent to 15.5 percent of total nonagricultural payroll employment. Indeed, the absolute number of state- and local-government employees almost tripled during this period, rising from 4.1 to 11.9 million. Although the share of SLG employment in total employment has declined slightly since 1975, the absolute number of SLG employees has continued to rise, reaching over 13 million in 1982. The growing importance of the sector suggests that attention should be directed to analyses of it.

A third is that the pattern of unionization and the laws governing collective bargaining, dispute resolution, and wage determination differ between the public and private sectors. In contrast to the declining fraction of private sector workers who are union members, union membership is growing rapidly in the public sector in both absolute and percentage terms. For example, between 1964 and 1978, the proportion of federal employees who belonged to unions increased from 38.2 percent to 50.2 percent.[2] Similarly, the proportion of SLG employees belonging to unions rose from 7.7 percent to 17.4 percent during the same period. If one includes membership in bargaining organizations--which include professional organizations such as the National Education Association (NEA) that over time have behaved more and more like unions--an even more rapid increse is observed, with the percentage of SLG employees rising to 36.2 percent in 1978.

One factor that affected this growth in public-sector unionization was changing public attitudes and legislation governing bargaining in the public sector. Unlike the private sector where the rights of workers to organize and bargain collectively have been guaranteed since the National Labor Relations Act, laws governing bargaining in the public sector are of much more recent vintage. Executive Order 10988 issued by President John F. Kennedy in 1962 legitimized collective bargaining in the federal sector for the first time, providing federal workers with the rights to join unions and bargain over working conditions--but not wages. While this executive order has been modified several times since then, most federal employees' wages are still not determined by the collective bargaining process.[3] Instead they are determined via comparability legislation, first passed in 1962, which ties the wages of most federal civilian workers to the results of government surveys of wages of "comparable" private workers, subject to possible Presidential or Congressional modification.[4] The influence of federal unions on wages operates, then primarily through the political pressure they can exert on the President and Congress to approve wage increases that the surveys suggest are warranted.

Favorable state legislation for SLG employee collective bargaining began with a 1959 law in Wisconsin; prior to that date collective bargaining was effectively prohibited in the state and local sector. By the late 1970s most industrial states had adopted statutes that permitted SLG employees to participate in the determination of their wages and conditions of employment, although not all employees in each state were covered by the laws.[5] While these statutes were being adopted, and at the same time that employment and unionization were growing in the SLG sector, SLG employees' earnings also started to rise relative to the earnings of private sector employees. From the mid-1950s to 1970, SLG employees' average earnings improved relative to those of private employees by some 15 to 20 percent. (However, during the 1970s the trend was reversed, and SLG employees' earnings fell relative to those of their private sector counterparts.)

The growth in the relative earnings position of SLG employees during the 1960s, coupled with the growing strength of public-employee unions, their increased militancy, and the trend towards allowing SLG employees to bargain over wage issues, led to fears that inflationary wage settlements would continue in the sector and aggravate the financial problems faced by state and local governments. These fears were explicitly based upon the belief that many public services are essential and this implied that the wage elasticity of demand for public employees was very inelastic. To many, the logical conclusion was that, in the absence of market constraints that would limit the wage demands of public employees, limitations should be placed on the collective bargaining rights of these groups.[6]

Although by 1981 eight states did grant the right to strike in one form or another to selected employee groups, most continued historic prohibitions against strikes.[7] The states that prohibited strikes, however, often provided assistance to local governments and unions in settling contract disputes, with a number of states adopting forms of binding arbitration as the terminal stage in their impasse procedures. How these alternative institutional arrangements operate and affect economic outcomes is, of course, worthy of discussion.

A final reason why public-sector labor markets warrant separate treatment is that they represent an area toward which much of our public policy has recently been directed. To take one example, during the decade of the '70s attempts were made to reduce unemployment by means of public service employment (PSE) programs. Starting with the Emergency Employment Act of 1971 and then continuing under the Comprehensive Employment and Training Act (CETA), the federal government provided funds to state and local governments to increase their employment levels, in the hope that the availability of extra public sector jobs would provide job opportunities for the unemployed. By 1978, 569,000 individuals were reported employed on PSE program funds; these employees comprised some 3.3 percent of total SLG employment.[8] To take another example, growing concern over the fiscal condition of state and local governments and the increased state and local tax burden borne by taxpayers led to the passage of expenditure- and tax-limitation legislation in a number of states in the late 1970s. The most notable was the enactment of Proposition 13 in California, which drastically reduced local property taxes and limited the ability of all governmental units in the state to increase their revenues.

The unique nature of the agents in public sector labor markets (nonprofit organizations), of the institutional arrangements governing these markets, and of the public policies that have been directed towards them, suggest then that research relating to them warrants separate treatment in this volume. The discussion that follows is structured along topical lines. We begin with a discussion of the research on wage determination in the state and local government (SLG) sector. Although our focus is on attempts to estimate union/nonunion wage and total compensation differentials, we also emphasize the importance of various characteristics of the environment in which bargaining takes place. This discussion is followed by brief discussions of the research on the effects of unions on productivity and on the estimation of compensating wage differentials for various pecuniary and nonpecuniary job characteristics in the sector.

The literature on wage determination in the SLG sector is based, at least implicitly, on some notion of the forces that affect the demand for labor in the public sector. In Section V, we explicitly focus on studies of public sector labor demand that have sought to provide estimates both of wage elasticities of demand for various categories of SLG employees and of the net job creation effects of PSE programs. The former studies are important because they shed light on the question of whether the market forces that constrain union power in the private sector would exist in the public sector if the same institutional rules governed collective bargaining in both sectors. The latter are important because they address the issue of "fiscal substitution"; to what extent were PSE funds used to hire SLG employees who would have been hired (in the aggregate) even in the absence of the program?

The long section that follows analyzes the research relating to dispute resolution in the SLG sector. It first discusses normative models of different impasse procedures (conventional arbitration, final-offer arbitration, the right to strike) and then the empirical research on the determinants of the use of the various procedures. Finally, it discusses studies of the effects of the availability, and the use, of the various procedures on wage and nonwage outcomes.

Given the variety of institutional arrangements that determine compensation in the public sector, it is natural to ask whether they generate settlements that leave "comparably qualified" workers performing "comparable" work in the public and private sectors receiving roughly equal total compensation. Answers to such a question are of more than academic interest; especially in the federal sector where wages for a majority of the civilian workers are established via "comparability" surveys. The next section evaluates the research on this question. It is followed by a discussion of the research on gender and race discrimination in public sector labor markets and then some brief concluding remarks.

Both space and time constraints have caused us to limit the scope of our survey and three omissions warrant special mention. First, except in passing, we have limited our discussion to public sector labor markets in the United States and ignored studies of other countries.[9] Second, our discussion is limited to nonmilitary employees; we have ignored the important literature on military manpower and compensation problems.[10] Third, in the main, we have not discussed the research relating to the compensation of top government officials, especially those in the federal sector.[11]

II. Wage Determination in the State and Local Government Sector[12]

Several features distinguish studies of the effects of collective bargaining on SLG employees' wages from similar studies of union wage effects in the private sector. First, the unit of observation in the public sector studies is typically an individual bargaining unit where, in the presence of collective bargaining, the same negotiated union wage scale covers nonunion employees as well as union employees. In contrast the private sector studies tend to use either individual workers or industry aggregates as the units of observation. In the latter case, data on workers covered by union contracts are merged with data on workers not covered by union contracts and the observed average industry wage is typically not the result of any single negotiation.

Second, in an attempt to control for the forces other than collective bargaining that might influence wages, public sector studies tend to stress economic, demographic, and political variables relating to the geographic area that the bargaining unit is in, while typically ignoring the personal characteristics of the public employees. In contrast, the private sector studies stress the personal characteristics of employees and only occasionally incorporate characteristics of the employer or the industry (e.g., establishment size, concentration ratios, capital/labor ratios).

Finally, because public sector studies tend to utilize bargaining unit data, their focus is often on how various characteristics of the environment in which bargaining takes place (e.g., city size, form of government, formal parity agreements) influence the effects of collective bargaining. In contrast, although a few private sector studies have looked at how the structure of collective bargaining (multiemployer, union competition, etc.) affect union/nonunion wage differential estimates, most have stressed how these differentials vary with individual worker characteristics (e.g., race, sex, age, education, occupation).

Most studies of the effects of collective bargaining on SLG employees' wages are based explicitly, or implicitly, on a rather simple conceptual framework. Based upon a utility maximizing model of government behavior, the demand for public employees is specified to be a function of the wage costs of public employees (W) and a vector of sociodemographic and economic variables (Z) that represent the determinants of both the fiscal capacity (or ability to pay) of residents of the jurisdiction and the relative preferences of the community for various public services. Similarly, the supply of public employees is specified to be a function of the wages paid to public employees and another vector of sociodemographic and economic variables (V), that reflect alternative wages in the private sector and those forces that influence applicants' relative nonpecuniary preferences and qualifications for public sector employment. In the absence of imperfections in the labor market, one can then solve for the market clearing wage (W^c). However, given the presence of unions and political and institutional forces (e.g., form of government, monopsony power, parity agreements), which may be represented by a vector of variables (X), the actual wage equation to be estimated is specified as

$$(1) \qquad W_i = F(Z_i, V_i, X_i, U_i) + \varepsilon_i$$

where the i^{th} subscript is used to denote a bargaining unit, U_i is some measure of collective bargaining (to be discussed shortly) and ε_i is a random error term.

Tables 1 and 2 present a nonexhaustive survey of studies published between 1970 and 1983 that estimate equations similar to equation (1). The former contains estimates for public school teachers, while the latter focuses on various categories of noneducational employees. Most of the studies use individual bargaining units as the units of observation, although some of the early teacher studies used statewide data, one study of hospital employees did some analyses using SMSA-wide data, and several studies use data on individuals from the Current Population Survey.[13] In the main they are cross-section studies in which the extent of unionization or collective bargaining coverage is taken as exogenous. However, a number of the studies allow the unionization variable to be endogenous, in the context of models that seek to control for selection bias.[14] In addition, at least one of the studies performs some analyses using two years' data and a fixed effects model, to eliminate the biases caused by unobserved variables that may be correlated with collective bargaining coverage.[15]

The unionization variable in these equations varies across studies. Some use a (1,0) variable to indicate whether collective bargaining negotiations take place. Others use a (1,0) variable to signify whether a formal contract governs wages and conditions of employment. Still others look at union membership, focusing on either the percentage of employees who are union members or whether any employees are union members. In each case, however, the estimates reported in the final column of the tables may be interpreted as our estimates (based upon their results) of what Jacob Mincer (1981) has called the "wage gap;" the relative wage differential associated with the union variable taking on the value of one rather than zero.[16]

What is most striking is how small most of these numbers 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.[17] In addition, the two studies that use data on individuals, rather than on bargaining units, tend to find that the union/nonunion relative wage differential increased between the early and late 1970s. Most students of public sector labor relations find this latter result strange since their consensus was that while public sector unions may have won large wage gains in the early years of bargaining when municipal employers were not fully prepared to bargain, over time these gains have eroded.

What accounts for these two findings? Does collective bargaining really have a smaller effect on union/nonunion wage differentials in the public than private sector and have the public sector differentials grown over time? Or are there methodological problems with these studies which may account at least partially for the results?

Turning first to the question of the size of the union/nonunion wage differential in the public sector, on the one hand we should stress once again that the laws governing dispute resolution in the public sector differ from those in the private sector. Most states prohibit strikes by public employees which may weaken their bargaining power and should lead to lower observed union/nonunion differentials. Other states, however, provide for alternative forms of dispute resolution and, as discussed below, some provide for binding arbitration, either of a conventional or a final offer form, as the terminal stage of their impasse procedures. The literature we review below suggests that the nature of the impasse procedures available may well affect the bargaining power of unions. Somewhat surprisingly, however, there are no studies that have empirically looked at how the nature of impasse procedures affects the union/nonunion wage differential; this represents a fertile area for future research. In any case, the smaller estimated differentials in the public sector <u>may</u> reflect smaller actual differentials caused by the different nature of the laws governing bargaining in the sector.[18]

On the other hand, several methodological problems may cause these studies to understate public sector unions' impact on their members' relative wages. First, most of these studies ignore the interdependence of wage settlements across different public sector bargaining units in the same city (e.g., police, fire, sanitation) and the interdependence of wage settlements across geographic areas (e.g., cities in an SMSA) for a given category of employees (e.g., police). Such occupational and geographic wage interrelationships lead union wage gains to "spillover" across bargaining units in a given city and across contiguous cities.

Studies that take account of these spillovers often find much larger union relative wage effects. For example, Ehrenberg and Goldstein (1975) found union/nonunion wage differentials in the range of 6 to 16 percent for various categories of municipal noneducational employees in 1967 when they considered only the unionization of employees in the category. In contrast, when occupational spillovers of union wage gains were permitted, the estimated union/nonunion wage differentials in cities where all categories of municipal employees were organized rose to the range of 20 to 32 percent. The same study also illustrated the importance of geographic spillovers finding that when a matched set of central city and suburban observations were used that, on average, the presence of a public sector union in a category in one city of the pair caused wages of employees in the category in the other city in the pair to be significantly higher.

More recent studies have confirmed these findings suggesting that unionization of police influences firefighter and sanitation wages (Victor (1979)), that unionization of one category of hospital employees leads to higher relative wages for other categories of hospital employees (Feldman and Scheffler (1982), Cain, et al. (1981), Becker (1981)) and that the extent of collective bargaining coverage in a geographic area (e.g., an SMSA) often has a larger effect on public sector wages in a bargaining unit than does the extent of organization in that unit (Chambers (1977), Cain, et al. (1981)). In sum, ignoring the presence of occupational and geographic wage spillovers may well have caused most researchers to underestimate the magnitude of public sector union/nonunion wage differentials.

Second, most public sector wage studies have treated collective bargaining coverage as exogenous. However, one might expect employee pressure for organization to come first in cities where public sector wages are below average for cities with comparable sociodemographic and economic characteristics. If this is the case, subsequently contrasting wages in those cities where unions are present to those in which unions are not present will understate the true public sector union/nonunion wage differential.

Attempts to allow for the endogeneity of collective bargaining coverage have not all yielded similar results. Cain, et al. (1981) and Ehrenberg, et al. (1983) estimated union/nonunion wage differentials in the context of models that corrected for selectivity bias; neither found systematically larger differentials using such models. Ichniowski (1980) used a fixed-effects model and panel data to eliminte the effects of any unobservable variables that might be correlated with both collective bargaining coverage and wages and again found that such a method did not significantly affect his estimated differential. However, Bartel and Lewin (1981) did find much larger estimated differentials when unionization was made endogenous.

Third, most of the studies summarized in Tables 1 and 2 have tended to focus on hourly or annual earnings, rather than fringe benefits or total compensation. Might such a focus understate the relative union/ nonunion compensation differentials in the public sector? Studies of union effects in the private sector have often found that union/nonunion relative total compensation or fringe benefit differentials exceed union/ nonunion wage differentials (e.g., Freeman (1981)); an explanation for this result is that unions serve a collective voice function and can help to aggregate the preferences of individual workers for fringes and communicate these preferences to management. Since in the public sector the ultimate financers of settlements (taxpayers) are not explicitly represented at the bargaining table and since it may be easier to hide the true costs of generous fringe benefit settlements than wage settlements from taxpayers (via underfunding of pensions or withholding information on the "true" costs of fringes), one might expect union compensation gains to be skewed even more towards fringes in the public sector.

In fact, there is some evidence that this may have occurred. For example, Ichniowski (1980) found the relative union/nonunion fringe benefit (as measured by contributions to retirement and insurance benefits) differentials for firefighters to be roughly four times as large as the comparable wage differential; a relationship which is much larger than that found by Freeman (1981) in his private sector studies. Edwards and Edwards (1982b) similarly found much larger fringe differentials than wage differentials for sanitation workers. Other investigators, however (e.g., Bartel and Lewin (1981), Becker (1979), Feldman and Scheffler (1982), Cain, et al. (1981)), found union/nonunion fringe differentials for hospital employees and police that appear to exceed the union/nonunion wage differential by only a small amount.[19] So while union/nonunion relative wage differential estimates in the public sector probably do understate the comparable total compensation differentials, it is not obvious that the understatement is greater here than in the private sector.

Finally, while most recent studies of union relative wage effects in the private sector have used data on the wages paid to specific individuals, studies in the public sector have tended to focus on minimum, maximum, or average salaries for a particular category of public employees. If public sector unions are dominated by senior workers, one might expect to observe smaller union/nonunion differentials at the entrance than at the maximum salary levels. Moreover, if public employers respond to union induced wage gains that are skewed to favor older workers by seeking to increase the proportion of their workforces that are younger workers, estimated union/nonunion average salary differentials will also be less than maximum salary differentials and may actually be less than minimum salary differentials.[20] If public sector wage studies focused primarily on average salaries, this might partially explain the lower union/nonunion wage differentials observed in the public sector.

Although many public sector studies, especially those for noneducational employees have focused on average earnings, others have focused on minimum and maximum salaries. A quick reading of Tables 1 and 2 also suggests that the union/nonunion wage differentials estimated from the latter studies are not appreciably higher than those estimated from the former. Moreover, the evidence on whether these differentials are actually larger for older than for younger workers is mixed. For example, Ehrenberg, et al. (1980) found differentials that were larger at the maximum salary level than at the minimum salary level for both police and firefighters. However, Ehrenberg (1973c) and Ichniowski (1980), for firefighters, and Bartel and Lewin (1981), for police, all found larger differentials at the minimum than at the maximum level. Similarly, with respect to teachers, while Thornton (1971) found larger differentials at the maximum level and Gustman and Segal (1977) found that teachers' unions were associated with a shorter length of time to reach maximum salaries, Chambers (1977) found positive union/nonunion differentials for starting salaries but not for increments, and Gallagher (1978) found differentials at the minimum and maximum level that were roughly equivalent. All in all then, the evidence on union effects on the seniority structure of compensation in the public sector is mixed.

We turn next to the question of whether the public sector union/nonunion wage differential has grown over time. It must be stressed that the estimates that suggest it has come from studies that utilize data on individuals and that completely ignore information on the economic and sociodemographic characteristics of the cities in which public sector employees are located. We know from the bargaining unit studies cited in the tables that, ceteris paribus, public sector wages tend to be higher in large, densely populated cities. These cities also tend to be the cities in which collective bargaining arose first and in which it has grown most rapidly. Hence, even if the actual union/nonunion wage differential is constant across high and low wage cities at a point in time and does not vary over time for each type of city, it is straightforward to show that the observed average union/nonunion wage differential will increase over time.[21,22] Put another way, omitted variable problems may well have biased the studies based on individual data and we do not find the evidence that public sector union/nonunion wage differentials have increased over time compelling.

As noted above, in addition to providing estimates of union/nonunion wage differentials, many of the studies of public sector wages have taken great care to emphasize the importance of various characteristics of the environment in which bargaining takes place. To take one example, numerous studies have sought to estimate whether public employers have monopsony power, focusing on measures like the number of school districts in a county (Baird and Landon (1972)), the percentage of a SMSA's population which resides in a jurisdiction (Ehrenberg and Goldstein (1975)), or concentration ratios such as the fraction of an area's hospital beds in the four largest hospitals in an area (Feldman and Scheffler (1982)). To the extent that variables like these are good proxies for monopsony power, the first should be positively associated with wages and the latter two negatively associated; these and other studies suggest this it the case.

To take another example, a set of studies has focused on the effects of form of government on wages. Historically, at least partially because of the belief that professionally trained managers could "produce" a desired set of services at lower cost than could an elected nonprofessional, a substantial proportion of all U.S. cities have chosen to employ a city-manager as the principle operating officer rather than an elected mayor or set of commissioners. If professionally trained managers are better negotiators, are more aware of market conditions, and/or are more efficient than elected officials in "producing" public services from a given number of employees, one might expect that city-manager cities would have lower wage costs than other municipalities and that union/nonunion wage differentials would be lower in them, ceteris paribus, for employees of comparable quality.

To date, however, only one study, Edwards and Edwards (1982b) found that the form of local government affects the size of union/nonunion wage differentials. In particular, it found that union/nonunion differentials for municipal sanitation workers are zero in city-manager run cities but positive in other cities. Moreover, the evidence on the effect of governmental form on salary <u>levels</u> is mixed. Ehrenberg (1973) found city-manager cities had lower hourly wages for firefighters but higher annual salaries, while Ehrenberg and Goldstein (1975) replicated the latter result for ten different categories of noneducational municipal employees. More recently, Edwards and Edwards (1982b) and Bartel and Lewin (1981) have studied sanitation and police employees, respectively, finding city-manager cities had higher wages and fringes in the former case and that the form of government did not affect salary levels in the latter case.

Finally, a number of studies have examined how the effects of specific collective bargaining contract provisions, such as parity provisions that require that two groups of employees (e.g., police patrolmen and firefighters) receive the same wages, affect wage settlements. Some have also begun to look at how municipal laws, such as civil service laws, residency requirements that mandate that public employees live in the municipality where they work, or prevailing wage laws that at least partially determine wages via reference to comparability studies, affect wages.

The evidence on the effects of parity provisions not surprisingly suggests that they positively influence the wages of groups that are in excess supply, but negatively influence the wages of groups that are in excess demand (e.g., Ehrenberg (1973), Hall and Vanderporten (1977)). The evidence on the effects of civil service laws suggests they are associated with higher wages. Finally, with respect to the effects of municipal laws, studies have been conducted by Werner Hirsch and Anthony Rufolo (1975; 1983a; 1983b); a conceptual framework is presented in their first paper and some empirical results in their latter papers. So far, however, they have not found strong evidence that these laws have statistically significant effects.

The last group of studies are suggestive of a direction in which future research on public sector wage determination might proceed--to more fully analyze the relationship between the legal environment, public sector wages, and public sector union/nonunion wage differentials. The legal environment includes state statutes governing public sector dispute resolution; as noted above there have been no studies of the effects of dispute resolution statutes on union/nonunion differentials and only a limited number (to be discussed below) of their effects on the level of wage settlements.

Dispute resolution statutes are only one part of the environment, however. Surprisingly no one has addressed the effects of a host of other laws. Some states require taxpayers to approve local school budgets at annual budget referenda, while others do not. In the wake of the passage of Proposition 13 in California in the early 1970s, some states now have limitations on state or local taxes and/or expenditure levels, while others do not. Some state constitutions require that state governments operate balanced budgets, while others do not. Finally, some states have agency shop provisions in their public sector bargaining laws that require public employees to join the union representing them or pay the equivalent of dues, while other states explicitly prohibit such provisions (Hanslowe, et al. (1978)). Surely these laws should all be expected to influence public sector union bargaining power and hence the level of wages.

The fact that these laws vary across states provides a form of natural experiment that should allow researchers to investigate their effects on wage levels and differentials. Of course, the possibility that the laws are endogenous should be considered and appropriate econometric methodologies used. To analyze the effects of such laws obviously requires a national sample of bargaining units; it is interesting that many of the studies cities in Table 1 were confined to a single state.

III. Unions and Productivity

Recently a number of economists have directed their attention to estimating the effects of unions and collective bargaining on nonwage outcomes in the public sector. As is well known, the traditional neoclassical view of unions is that by creating noncompensating wage differentials and negotiating work rules that limit employers' flexibility to allocate resources, unions cause efficiency losses. In contrast, drawing on hypotheses put forth long ago by institutional economists, the "Harvard School" holds that unions may well increase productivity via a number of routes including reducing turnover, increasing morale and motivation, and expanding formal and informal on-the-job training.[23] Indeed, several studies, summarized in Chapter ___ of this Handbook suggest that union/nonunion productivity differentials in the private sector are often positive. It is natural to similarly ask then, what the effects of unions have been on productivity in the public sector.

Figure 1 presents a simple schematic diagram that illustrates the routes via which unions affect productivity. Unionization and the collective bargaining process per se leads to the establishment of union contract provisions (grievance, seniority, staffing, sick leave, wages, etc.). These provisions directly influence both employer and employee resource allocation decisions in areas such as turnover, training, absenteeism, and the nature of the production process and managerial behavior. Unionization per se may also influence these decisions independent of any specific contract provisions; for example, management behavior may be altered due to the mere presence of a union. Finally, the sum of these resource allocation decisions may affect output.

This figure highlights a number of important points. First, unions affect productivity both through the specific union contract provisions they negotiate and administer and via the unions' mere presence. A complete analysis would focus on both routes, however, as will be seen below, most public sector studies have focused on estimating the effects of a specific set of provisions or the sum of the effects of unionization across the two routes. Second, because the unit of observation in the public sector is the bargaining unit and public sector labor contracts are often readily available, one would expect much more analyses of the effects of contract provisions to have occurred in the public sector than in the private sector. To some extent this has occurred; while private sector studies have focused on (1,0) collective bargaining coverage variables, a number of the public sector studies have examined contract provisions. Third, the difficulties involved in trying to measure output and specify production functions in the public sector are well-known. As such, one might expect much of the public sector research to focus on the effect of unions on resource allocation decisions, rather than on productivity, per se, and this in fact has also occurred.

Turning first to studies that have attempted to estimate the effects of unions on productivity in the public sector, these have all been single year cross-section studies that treat collective bargaining coverage or unionization only as a (1,0) variable and that use a production-function or derived demand for public services approach. For example, Ehrenberg and Schwarz (1983) and Ehrenberg, Sherman and Schwarz (1983) focused on municipal public libraries because of the availability of various measures of output (circulation, interlibrary loans, borrowers, etc.) and found no union/nonunion productivity differential.[24] A similar result was found by Noam (1983), who studied municipal building departments, using the number of building permits granted and the volume of construction supervised as measures of output.

Sherman (1983) and Eberts and Stone (forthcoming) studied elementary and secondary schools, using various student test scores as measures of output. The former found that unions were associated with significantly lower mean test scores for students and a higher variance of test scores. The latter, who used data on individuals, found that unions were associated with higher test scores for "average" students but lower test scores for both below and above average students. Both of these studies were based upon the work of Brown and Saks (1975), who emphasized that school districts and teachers must make decisions about the allocation of resources across different categories of students. As a result, when one observes unions being associated with both the mean and variance of educational outcomes in aggregate district-wide data or having different associations with various categories of students when individual data is used, it is difficult to disentangle unions' effects on the educational production function from their effects on how resources are allocated across students.

In principle, such production function estimates could be extended to other public services, for example, police, fire and sanitation, for which measures of "output" can be obtained.[25] One must remember, however, that the association of union coverage with productivity does not imply a causal relationship; of the above studies, only Ehrenberg and Schwarz (1983) and Ehrenberg, Sherman and Schwarz (1983) considered the endogeneity of unionization, modelling it in a sample selection framework. Future research in this tradition must continue to consider this problem, using either a similar approach and/or longitudinal data, that would permit one to use a fixed-effects model to control for unobservables correlated with both union coverage and output.

Turning next to the studies of union effects on resource allocation, these have focused almost exclusively on public education and again have been primarily cross-sectional in nature. Eberts (forthcoming) used data from a national sample of elementary school teachers and principals in the mid-1970s and found that teachers in unionized schools spent less time per school year in instructional activities and more time on preparation, administration and parent conferences. He also found that unionized districts had more teachers and administrators per student (but fewer secretaries and aids), a result that contrasts with Hall and Carroll's (1973) finding of lower teacher/student ratios in unionized school districts in Cook County, Illinois in 1968-69.

Most of the studies in this area, however, have focused on the effects of specific contract provisions. For example, Winkler (1980) found that various contract provisions relating to sick leave policy were associated with the number of short-term absences observed for a sample of Wisconsin and California teachers. Murnane (1981a) studied the turnover of public school teachers in a system in which pay was determined strictly by seniority and found that the seniority provision did not cause the more productive teachers, as measured by principals' evaluations and/or the teachers' effects on student performance, to quit their jobs more frequently.[26]

Eberts (1982b) also focused on teacher turnover, studying whether contract provisions that specify maximum class sizes, and those that specify that reductions in force (RIF) due to declining enrollment be governed by seniority, affect the probability either that teachers voluntarily leave their school district or that they transfer from one school to another within a district. Using data from a sample of 19,000 teachers in New York State over the 1972-76 period, he found that class size provisions were associated with fewer quits but more within-district transfers. As might be expected, RIF provisions were associated with a lower probability that experienced teachers would leave the school district (these teachers would have relatively more job security under RIF's) but a higher probability that teachers with little seniority would leave. In related work, using data at the school district level in New York State, Eberts (1982a) found that districts with RIF provisions experienced, ceteris paribus, a smaller reduction in the level of resources available for education during a period of declining enrollments and fiscal stress (1972-76). Similarly, Eberts (1983) found that an index of the number of contract provisions contained in a contract and the presence of a set of specific provisions were all associated with a larger share of the school budget being devoted to instructional purposes, ceteris paribus, during the 1976-77 school year.

While these latter studies are useful first efforts, they have at least three limitations. First, we have been careful to use the words "associated with" rather than "cause" when talking about the contract provision studies because, save for Eberts (1982a), none of them allow for the possibility that some omitted variables influence both the contract provisions and the resource allocation decisions. Again, what seems called for is an explicit simultaneous equations approach or the use of longitudinal data that would permit the estimation of a fixed effects model. If the latter approach is used, one would want to focus on how changes in contract provisions affect changes in outcomes, differencing out the fixed effects. If the former approach is used, one could explicitly address the issue of how state laws governing public sector collective bargaining and dispute resolution affect resource allocation.

Second, while these studies are a substantial improvement over prior private and public sector studies that focus on (1,0) union variables, they do not go quite as far as they might. Virtually all use (1,0) variables to parameterize specific contract provisions when often more detailed data is available.[27] Is it the presence of a maximum class size provision or the level of the maximum student/teacher ratio that matters? Future studies should try to parameterize contract provisions in a more detailed fashion.

Finally, all of these contract provision studies deal with resource allocation decisions, but most make no attempt to evaluate how these decisions subsequently affect output or productivity.[28] What is obviously needed, for example, is estimates of public sector "production functions" that include variables such as absentee rate or turnover rates in the production function. Alternatively, one might ignore such variables and directly estimate "quasi reduced-form" production functions in which the underlying contract provisions appeared explicitly. But again, here account must be taken of possible simultaneity between contract provisions and productivity.

IV. Compensating Wage Differentials in the Public Sector

The realization that the total compensation of labor includes a host of pecuniary and nonpecuniary job characteristics, as well as money wages, naturally led students of public sector labor markets to consider the issue of compensating wage differentials. The studies in this area break down neatly into two sets; the first deals with the trade-off between public school teachers' wages and nonpecuniary job characteristics, while the second deals with the trade-off between wages and retirement system characteristics for police, fire, and sanitation workers.

The teacher studies were motivated at least partially by concerns over educational equity. If teachers in school districts with, say, a preponderance of low-income or minority students demanded, and received, higher wages to compensate them for the disamenities they perceived to be associated with working with such students, then equal expenditures per pupil across school districts would leave these districts able to afford only lower teacher/student ratios than other districts. Thus, knowledge of whether such compensating wage differentials exist is essential for the formulation of state-aid-to-education policies.

The methodological approach used in these studies, and those discussed below, on the wage/fringe trade-off, is a straightforward application of Rosen's (1974) hedonic price approach. Using cross-section data, equations of the form

$$\text{(2)} \quad \log W_i = f(X_i) + \gamma C_i + \varepsilon_i$$

are estimated, where W_i is some measure of earnings for the i^{th} teacher or school district, X_i is a vector of teacher and community characteristics expected to influence earnings in the absence of any compensating wage differentials, C_i is a pecuniary or nonpecuniary job characteristic (in practice a vector of such characteristics is often used) and ε_i is a random error term. A positive (negative) coefficient on a perceived job disamenity (amenity) is interpreted as indicating the presence of a compensating wage differential. Rosen is careful to stress, as have other researchers that followed (e.g., R. Smith (1979)), that the estimated trade-off curve per se reflects only a market-equilibrium curve; in itself it identifies neither employees' demand prices nor employers' supply prices for the characteristics. We will return to this point below.

The best known of these studies, Antos and Rosen (1975), used cross-section data on individual teachers from the 1965 Equality of Educational Opportunity Survey and included teacher, school, neighborhood, and geographic characteristics, as explanatory variables. A major finding was that white teachers were paid higher wages in areas with high proportions of nonwhite students. Similar results were found by Gustman and Clement (1977), who used data for 83 inner city school districts, and Toder (1972), who used data for Massachusetts cities and towns; both studies suggested that average teacher salaries in a district were higher, the higher the proportion of nonwhite students. Kenny and Denslow (1980) failed to find such a relationship in data spanning 1419 southern school districts, but they did find that (dis)amenities such as the crime rate and the climate in an area were significantly related to salaries.

Studies of the wage/retirement system characteristics trade-off for police, fire, and sanitation employees were motivated by concerns about whether taxpayers or public employees would pay the costs of proposed public sector pension reform legislation which, like the Employee Retirement Income Security Act that applies to the private sector, might call for improved vesting and funding requirements. One needs to know the extent to which such rules, that would increase employers' pension costs, would be shifted onto employees in the form of lower wages. Estimation of an equation in which public employee wage scales are regressed on retirement system characteristics and variables that previous studies have shown to influence public employee wages (see section II) would permit one to ascertain whether public employers actually do shift pension costs on to their employees.

While it is straightforward to show that an increase in any pension plan characteristic that increases an employer's costs should lead, ceteris paribus, to decreased wages, as should a decrease in the employees' pension contribution, it is somewhat less obvious why and how pension funding should affect wages. The answer depends upon how employers and employees perceive underfunding.[29] Employers may regard underfunding as merely borrowing from the future--that is, creating a future liability with a present value equal to the amount of underfunding.[30] With such a perception employers would not offer high wages in the event of underfunding; no wage-underfunding trade-off would exist.

Public sector employers, however, may regard underfunding as cost-saving, at least to the currently elected administration.[31] They may, for example, believe that higher levels of government will "bail-out" funds whose pensioners face nonreceipt of benefits. They may also reason that the financial crisis is 15 to 20 years in the future and therefore well past the time when they will be in office. In either case, employers regarding underfunding as cost-saving will be willing to pay higher wages if they choose to underfund.

Now if employees are unaware of underfunding or believe it will have no effect on their expected pension benefits, they will essentially ignore underfunding in their choice of employers and go for the highest paying job (ceteris paribus). The highest wages, other things equal, will be paid by the biggest underfunders. Large-scale underfunders would dominate in their ability to attract employees and a Gresham's Law of pensions would exist: poorly funded retirement systems would drive out well funded ones. We would observe near-total underfunding by all public employers.

If employees are aware, however, of underfunding and perceive it to reduce their expected benefits, they would demand higher wages to compensate for additional underfunding. Employees who require a large wage increase for a given increment of underfunding would choose to work for the better-funded employers, while those who require only a small wage increase would work for the poorest funders. We would observe both a positive wage-underfunding trade-off in the labor market and the coexistence of retirement systems in which funding practices vary widely. In fact, this is the only case where a wage-underfunding trade-off would be observed; in the other cases employers are either unwilling to make the trade-off or are clustered at some near-maximum level of underfunding.

Attempts to test for the trade-off between wages and retirement system characteristics, including the degree of underfunding have been made by a number of investigators who have estimated variants of equation (2). Ehrenberg (1980a) used data on police and firefighters from roughly 130 cities of populations of 50,000 or more, drawn from a 1973 International City Management Association survey and other sources, to test for the effects of several pension plan characteristics--minimum age and service requirements for regular retirement, percentage of salary received for regular retirement, and employees' pension contributions as a fraction of their salary--on public-sector wages. His strongest finding was that, holding promised pension benefits and other variables expected to affect wages constant, police and firefighters appeared to be fully compensated in the form of higher wages, on virtually a dollar-for-dollar basis for increases in their own pension contributions. He also performed a limited analysis of the effect of underfunding on wages, finding that a set of proxy variables for the extent of underfunding was correlated with wages.[32]

In the same paper, Ehrenberg also analyzed data from a 1975 U.S. Conference of Mayors survey of 262 cities with populations of 25,000 or more to test for wage-retirement system characteristics trade-offs among fire, police, and sanitation workers. Perhaps his most important finding was that, ceteris paribus, the presence of vesting led to a 3-9 percent decrease in wages.

R. Smith (1981) tested the predictions of the theory on data for non-uniformed employees enrolled in Pennsylvania's city and county retirement systems. These data include actuarial calculations of the "normal cost of pension promises" and the extent of underfunding. Smith found that, ceteris paribus, increases in normal service costs reduced wages virtually dollar-for-dollar and increases in the extent of underfunding increased wages, again virtually dollar-for-dollar. In a second paper he performed similar analyses for uniformed employees finding that again underfunding led to higher wages, but this time at a less than dollar-for-dollar trade-off.[33]

Finally, Inman (1981) used pooled data for police and firefighters in 60 large cities for the 1970-73 fiscal years and provided estimates of a simultaneous equations system that included a wage equation (like equation (2)), an employment equation, a pension contribution per employee, and a unionization equation. He found that underfunding led to higher wages for police, but not for firefighters. However, even in the former case, the trade-off was less than dollar-for-dollar.

While providing useful insights, these studies of the wage-nonpecuniary job characteristic and wage-retirement system characteristics trade-offs suffer from a number of methodological short-comings, which are common to virtually all private sector studies of compensating wage differentials as well. First, in spite of the fact that the underlying hedonic structure yields no strong implications about the functional form of the equation to be estimated, little experimentation is typically done with alternative functional forms to see how robust the findings actually are.[34] Second, although in theory all pecuniary and nonpecuniary job characteristics should appear in equations like (2), in practice only a subset actually appear. So, for example, most of the teacher studies, which focus on nonpecuniary job characteristics, omit fringe benefit data, and most of the retirement characteristics studies omit nonpecuniary job characteristics data. To the extent that various dimensions of pecuniary and nonpecuniary compensation are correlated, the potential arises for biased estimates of the coefficients of the trade-offs.

Third, there is an obvious simultaneity problem that most studies have totally ignored. Returning to equation (2), the true model is that total compensation, which is assumed to be a linear combination of the logarithm of wages and the job characteristic (C), is a function of the vector X plus a random error term (ε). For estimation purposes, however, C is moved to the right-hand side and equation (2) is estimated. Since increases in the random error term lead to increases in total compensation, the possibility that the error term is positively correlated with C and thus that the coefficient γ is biased in a positive direction is strong. In cases where the job characteristics is an amenity (disamenity) and this coefficient is hypothesized to be negative (positive), one will consequently understate (overstate) the absolute magnitude of the trade-off.

Three attempts have been made to handle this latter problem. Ehrenberg (1980) returned the vector C back to the left-hand side of the equation and estimated the resulting equation using canonical correlation analysis. While such an approach seemed to "improve" his results, since no tests of significance are available for individual coefficients in canonical correlation analysis, he could not draw any firm conclusions. Eberts and Stone (1983) investigated the trade-off between wages and other job characteristics for teachers in New York State using panel data and a fixed effects model; the hope is that any omitted variables were constant over time.[35]

Finally, Woodbury (1983) used data from a sample of 2,500 school districts in 1977 to estimate wage-fringe trade-offs. However, rather than estimating market trade-off curves like (2), he used a translog indirect utility function and attempted to directly estimate teachers' marginal rates of substitution of wages for fringes. As Rosen (1974) emphasizes, the the conditions under which market observations can be used to directly infer marginal rates of substitution for one side of the market (i.e., employee or employer) is rather stringent and it is not clear that they are met in this case. In particular, Woodbury's approach seems to require that employers will be willing to trade-off fringes for wages in a constant linear manner, a condition which is unlikely to hold if fringes affect teacher productivity.[36]

V. The Demand for Labor in the Public Sector

The motivation for existing studies of the demand for labor in the state and local government sector is two-fold. First, to provide estimates of wage elasticities of demand for various categories of SLG employees to shed light on the question of whether the same market forces that constrain union power in the private sector would exist in the public sector, if the same institutional rules governed collective bargaining in both sectors. Second, to provide estimates of the extent to which federal funds provided to state and local governments under public service employment programs were actually used to increase SLG employment. That is, to what extent did these funds create new jobs and to what extent were they used to hire people who would have been hired anyway?

Turning first to the estimates of wage elasticities of demand, the earliest studies are surprisingly more faithful to economic theory than the ones that followed, in the sense that they provide estimates of complete systems of demand equations based upon utility maximization models that permit one to test, or impose, the restrictions suggested by classical demand theory. For example, Ehrenberg (1972) (1973a) provided estimates of the demand for eleven categories of SLG employees based upon a utility-maximizing model of a representative decision-maker who derives utility from various categories of public and private produced goods and services.[37] Ehrenberg's estimates were based on a variant of the Stone-Geary utility function that allowed for minimum required employment levels in each category; these were specified to be a function of lagged employment levels and this specification allowed him to test for the presence of incremental budgeting.[38] Similarly, Ashenfelter and Ehrenberg (1975) used a variant of the Rotterdam, or differential demand system to test if the restrictions implied by classical demand theory (homogeneity, symmetry, and Engel aggregation) were met.[39]

Each of these studies used pooled cross-section time-series data at the state level for the 1958-59 period. They attempted to control for differences in tastes for public services across areas by including a vector of sociodemographic variables (population density, school age population, etc.) as controls in the estimating equations and/or by segmenting the data by the values of these variables. Aggregate time-series evidence for 1929 to 1973 on the demand for all SLG employees was provided by Ashenfelter (1979) in a later paper. Finally, two recent papers have focused on specific groups of educational employees; Thornton (1979) used cross-section state data for academic years 1968-69 to 1973-74 to study the demand for public school teachers, while Chang and Hsing (1982) used pooled cross-section time-series data for 12 southeastern states during the 1967-76 period to study the demand for college faculty in public universities. All of these studies, save for Ashenfelter (1975) treated public employees' wages as predetermined and ignored supply side considerations.

The estimated wage elasticities from these studies are summarized in Table 3. In the main they suggest that demand curves for labor in the SLG sector are inelastic. However, the estimated elasticities do not appear to be substantially lower in absolute value than the private sector wage elasticities, summarized in the Hamermesh paper in this volume.

Before one draws any conclusions from this about whether the market forces that constrain union wage demands are similar in the public and private sector, one should remember that an objective of unions is to make demand curves less elastic in order to improve the wage/employment trade-offs they face. To the extent that current public sector bargaining legislation limits unions' ability to pursue this objective, it is plausible that in a less restrictive environment public sector labor demand curves would prove to be less elastic.

In fact, this suggests an obvious deficiency in the public sector labor demand literature. It is well-known that public sector unions seek through the collective bargaining process to reduce the substitutability of capital for labor; for example, by establishing maximum student/teacher ratios in education or minimum patrolmen per patrol car ratios.[40] Public employees are also voters and, through the political process, may seek to increase the demand for their own services.[41] One might expect that public employee unions, via the lobbying route and their support of favorable legislation would further seek to increase the demand for public employees. Yet in spite of these observations, there have been virtually no studies that explicitly deal with the effects of unions on the levels, or wage elasticities, of public sector labor demand curves. There have also been virtually no studies which examine whether the form of local government similarly influences the public sector demand for labor; why city managers might affect the demand for public employees was discussed in section II.[42]

Several other omissions in this literature are also obvious. Most of the existing studies have used data from the 1960s and early 1970s.

Somewhat surprisingly, in the wake of proposition 13 in California and tax and expenditure limitation legislation subsequently passed in other states, there have been no studies that examine whether the presence of such legislation per se influences the demand for labor in the public sector.[43] The updated studies required to answer such a question would also be useful in that they would enable one to test if public sector wage elasticities are more elastic in times of fiscal stringency than they are in expansionary periods. Finally, as in the case of the public sector wage determination studies cited earlier, none of the demand studies have examined the role of other state legal or constitutional statutes (e.g., annual municipal budget referendum or balanced budget rules) on labor demand in the public sector.

Turning next to the studies of the <u>net job creation</u> effects of public sector employment programs, the approaches here have been varied. One of the early studies, Johnson and Tomola (1977) used quarterly aggregate data for the period 1966 to 1975 and asked what the effect of providing additional PSE positions would be on the aggregate level of SLG employment. Their estimating equation included seasonal dummies, a time trend, the real wage of public employees, personal income net of taxes and the percentage of the population that was of school age (to control for tastes for public education). Johnson and Tomola found that while initially PSE funds stimulated increased employment, the net job creation effects seemed to be close to zero after five quarters. That is, eventually the federal funds simply displaced, or were substituted for, local resources. Borus and Hamermesh (1978) then performed some reanalyses of the same data that illustrated how sensitive the aggregate time-series results were to choice of lag structure, functional form, and sample period. That is, they concluded that little can be concluded from the aggregate time-series data.

Two parallel studies using different methodologies also found quite different results. The first, an Urban Institute study (Bassi (1979), Bassi and Fechter (1979)), used cross-section data for cities, counties, and states for fiscal years 1976 and 1977, and a structural econometric model of SLG decision-making and found net job creation effects in the range of 40 to 50 percent.[44] The second, Richard Nathan, et al. (1981), was a noneconometric study based on the perceptions of field observers in 40 local governments and concluded that net job creation effects were in the range of 80 percent in fiscal years 1977 and 1978. Finally, a third study, Charles Adams, et al. (1983), used pooled time-series cross-section data for 30 cities from FY 1970 to FY 1979 and concluded that of every dollar of PSE program funds, 30 percent actually went to increase local government wage bills in FY 1977, with the estimate rising to 70 percent in FY 1978 and FY 1979.

Although the methodologies varies across these studies, the consensus appears to be that the net job creation effects of the program increased over time. Put another way, as the PSE program evolved from its onset in 1973, the number of new jobs actually created per each 100 positions funded seemed to increase. This is not surprising for, as Congress increasingly became aware of the possibility that federal funds could be used to substitute for, or displace, local funds, it continued to redesign the program in a way that limited such substitutions. For example, in the latter years of the program it became more difficult to switch employees from regular municipal payrolls to the PSE ones and to employ people on PSE projects for extended periods of time.

Several cautions are in order here, however. First, the Adams, et al. (1983) study focused on the total wage bill (payroll), not the municipal employment level. Thus, we have no way of knowing whether this study's results imply that PSE funds went for increased employment or for increased wages for existing employees. Indeed, one relatively unresearched area is the effect of federal grants on SLG employees wage levels, and the role unions play in this process. Second, since the Nathan, et al. (1981) study did not use formal statistical methods, no formal statements about statistical significance or confidence intervals can be associated with it. Finally, as Borus and Hamermesh (1978) note in the time-series context and Bassi and Fechter (1979) hint at in the cross-section context, many of these results are very sensitive to model specification, sample period, and choice of variables. Prudent researchers probably should not draw strong conclusions from this literature.[45]

VI. Dispute Resolution

As noted in Section I, most states prohibit strikes by public employees, substituting instead a formal system of impasse procedures in which assistance is provided to local governments and unions to help them resolve collective bargaining disputes. For those categories of public services that are often thought to be essential (police and firefighters), a number of states have adopted forms of binding arbitration as the terminal stage of the impasse procedures. This takes the form of either conventional arbitration where the parties present their final positions and supporting evidence to an arbitrator (or panel of arbitrators) who fashions a binding final settlement based upon the evidence and any other factors deemed to be relevant, or of final offer arbitration where the arbitrator is bound to issue a settlement that corresponds to the final position of one of the parties, either on a package (one party "wins") or issue-by-issue (each party may "win" on a number of issues) basis.

These unique forms of public sector dispute resolution lead to a number of empirical research questions that economists and industrial relations specialists have devoted considerable resources to answering. For example, in spite of prohibitions against strikes in the public sector, strikes do occur and it is natural to study their determinants, including state laws governing dispute resolution.[46]

To take another example, conventional arbitration statutes were introduced in the hope that they would reduce strike activity. But concern is often expressed that these statutes will have a chilling effect on bargaining; if the parties believe that arbitrators' decisions tend to "split-the-difference" between their final positions, the parties will have reduced incentives to make concessions during bargaining since any concession would come back to haunt them if the dispute went to arbitration. As a result, conventional arbitration statutes may lead to a reduced level of bargaining and heavy use of the arbitration procedures. Final offer arbitration, where the "reasonableness" of a party's position influences the likelihood that the arbitrator chooses it was developed to avoid this problem.[47]

These alternative forms of arbitration and the issues they raise lead naturally to the study of whether arbitrators tend to split the difference under conventional arbitration, whether a conventional arbitration statute increases the probability of a dispute going to arbitration vis-a-vis a final offer statute, and whether arbitration statutes tend to have a narcotic or addictive effect, in the sense that once the parties go to arbitration, this increases the probability that they will go to arbitration again in future rounds?

The empirical research addresses the outcomes of bargaining as well as the process itself. It is again natural to study whether the existence of an impasse procedure per se affects either the mean level or dispersion of contract settlements in a state, whether settlements systematically differ between bargaining units that use the procedures and those that settle on their own, and whether arbitrators exhibit bias in the sense that most of the cases that go to arbitration are won by one party (e.g., unions)?

These questions are all important because industrial relations specialists tend to evaluate public sector impasse procedures by their effectiveness in inducing the parties to settle on their own (i.e., to not use the procedures) and by their effectiveness in not influencing the nature of the settlements. Before turning to the empirical evidence on these points, however, it is useful to remember that these are all somewhat ad hoc criteria. Indeed, recently a number of economists have provided analytical models of the arbitration process that suggest that some of these criteria may not be useful ones to focus on and we turn first to a discussion of these models.

In a series of papers (Farber and Katz (1979), Farber (1980a)(1980b) (1981)) simple two-party zero-sum models of parties' bargaining over a single outcome are presented. The first, Farber and Katz (1979), considers the case where the parties form expectations of a conventional arbitrator's award; each's expectation is assumed to be normally distributed and to have a specified mean and variance. Each party seeks to maximize its expected utility from the negotiations and risk aversion of the parties leads to a contract zone; a range of settlements that is preferred by both parties to facing the uncertainty of the arbitrator's decision. In this framework, uncertainty is the cost of the arbitration process that leads to the contract zone and a key assumption of the model is that the larger the contract zone is, the more likely the parties will settle on their own. As we shall discuss below, this is not an innocuous assumption.

The Farber-Katz model leads immediately to two important implications. First, if over time the parties' uncertainty about arbitrators' decisions diminishes, the size of the contract zone and thus the probability the parties will settle on their own will also decrease. To avoid ever-increasing use of the arbitration process, one must increase the cost to the parties of using the process.[48] Second, if one assumes that the bargaining power of each party (the "share" of the contract zone the party will win in bargaining) is fixed, then necessarily the settlements that go to arbitration will differ from those where the parties settle on their own. This occurs because the more risk adverse party will willingly settle on its own for a smaller share of the pie to avoid the risks of going to arbitration. Thus, any difference observed between arbitrated and negotiated settlements does not indicate that the process is unfair. Rather, it suggests only that the arbitration process per se necessarily affects the nature of negotiated settlements.

The Farber-Katz model takes the arbitrator's notion of a fair settlement as given. Suppose instead that the arbitrator considers both the "intrinsic" fairness and the parties' positions in framing his award, with deviations of the parties' positions from his notion of intrinsic fairness reducing the weight he assigns to their offers in determining his award. Farber (1981) shows that such a model will lead the parties to endogenously select their offers in an attempt to influence the arbitrator's decision and that, in equilibrium, the offers will be structured so that it appears that the arbitrator is "splitting the difference". Evidence that arbitrators are splitting the difference thus may imply only that expectations of arbitrators' decisions influence the parties' positions.[49]

Finally, Farber (1980a)(1980b) models final offer arbitration (FOA) as well as conventional arbitration. Under FOA a party has incentives to make concessions because, although such concessions reduce a party's expected utility if its position is chosen, they increase the probability that the party's position will be chosen. Thus, just as they are under conventional arbitration, the parties' offers are endogenous under FOA (Farber (1981)). As such, Farber shows that it is not necessarily the case that FOA will lead to more uncertainty about the arbitrator's decision, and thus one cannot conclude that FOA provides the parties with more of an incentive to settle on their own than does conventional arbitration. Moreover, he also shows (1980b) that if the arbitrator awards the final offer closest to his notion of a fair settlement and the parties choose their final offers to maximize their expected utility, then the contract zone will be skewed against the more risk averse party. Put another way, the more risk averse party will win a greater share of the arbitrated awards but the awards it wins will be closer to the arbitrator's notion of intrinsic fairness than will the awards that the other party wins. Hence, evidence that one party (e.g., unions) win most of the cases that go to arbitration may imply only greater risk aversion on that party's part, not that arbitrators are systematically biased in favor of that party.

This series of papers illustrates how simple economic models can be used to contrast alternative institutional arrangements and to call into question the criteria by which industrial relations specialists evaluate the effectiveness of public sector impasse procedures. However, lest we appear too sanguine about the papers' importance, we should note that they have been subject to a number of criticisms. Crawford (1981) stresses that there is no theoretical justification for the key assumption that the size of the contract zone is positively related to the probability of reaching a negotiated settlement. To see this, suppose that there is only one point on the contract zone--only one bargaining outcome that both parties consider preferable to an arbitrated solution. Surely it should be easier for the parties to agree on that point than it would be for them to agree on one point out of five hundred on a contract zone, which all differed in their distribution of the pie between the parties. Without the assumption of the positive correlation between the size of the contract zone and the probability of reaching a negotiated settlement, many of the model's results concerning dependence on impasse procedures vanish.

Similarly, an important assumption in a number of the models is that the "bargaining power" of each party is fixed, in the sense that a negotiated settlement would give each bargainer a fixed proportion of the difference between the two end points of the contract zone. This assumption leads to the result that settlements will be skewed against the more risk averse party under both conventional and final offer arbitration.

However, Crawford (1981) challenges the idea that it is meaningful to talk about bargaining power independently of the parties' risk aversion; more risk averse parties surely have less bargaining power.[50] Moreover, Bloom (1981a) raises the possibility that uncertainty will be present about the nature of negotiated settlements and also generalizes the Farber-Katz (1979) model to allow for resource costs of both the negotiations and arbitration processes. One key result of Bloom's is that increased uncertainty about where the negotiated settlement will wind up leads to early use of the arbitration process (given the existence of direct costs of the negotiations process).

One should not, however, go too far in dismissing the usefulness of the Farber-Katz line of research. In recent work, Ashenfelter and Bloom (1983a)(1983b) have looked at data on police wage settlements under the first three years of a binding arbitration procedure in New Jersey. This procedure allows for conventional arbitration if the parties agree to it and otherwise mandates final offer arbitration. The raw data suggest that unions win over two-thirds of the final offer arbitration cases, that there is about a two to three percentage point spread between the typical union and employer final offers, and that the means of the conventional arbitration awards in each year are very close to the means of the union offers under final offer arbitration. These data are very suggestive (assuming that conventional arbitration awards are good measures of arbitrators' intrinsic notions of fair awards) of a Farber (1980a) view of the world in which the more risk averse party (the union) is positioning its offers closest to arbitrators' intrinsic views of fairness and thus winning the majority of the cases.

Ashenfelter and Bloom formally test whether this is occurring. They assume that an arbitrator has a normally distributed set of preferred settlements and chooses the party's offer which is closest to a random draw from this distribution. This leads directly to a simple probit model of which party's offer is chosen that can be estimated from data on the parties' final offers, the arbitrator's decision, and the variables that determine the expected value of the arbitrator's preferred decision (such as private wage settlements). From such a model one can infer the mean and variance of the distribution of arbitrators' preferences.

They show that it is straightforward to extend the model to allow for the arbitrator's preferred award to be influenced by the parties' final offers, as suggested in Farber's models, and to test if arbitrators are unbiased in the sense that they weigh both parties' offers equally in arriving at their preferred award. Finally, from conventional arbitration awards they show that one can again estimate the determinants of arbitrators' preferred settlements and see if these are the same under conventional and final offer arbitration. Without going into the details of their work, suffice it to say that strong support is found for the underlying framework.

Turning to the literature on strike activity in the public sector, it suffers, as does its private sector counterpart (see the chapter by John Keenan in this volume), from the lack of any single analytical model that is universally accepted as providing an explanation of strike activity. Although many public sector studies draw on existing theories of bargaining and strike activity, such as those found in Hicks (1966) and Ashenfelter and Johnson (1969), for our purposes it is best to think of them as quasi-experimental designs in which some measure of strike activity is regressed on a set of variables designed to capture the effects of public policy in an area, and a set of variables included to "control" for other factors. That is, the studies in the main seek to estimate the effects of public policies on the level of public sector strike activity.

One early study, Thornton and Weintraub (1974), examined the level of strike activity in the twelve-month periods before and after the adoption of "permissive" public sector bargaining legislation for teachers in twenty-seven states, concluding that the level of strike activity tended to increase after the adoption of a statute.[51] No conclusions can be drawn as to the casual nature of the relationship, since they failed to control for any other factors that may have influenced both the propensity of public employees to strike and the passage of a state law. A second paper of theirs, Weintraub and Thornton (1976) tried to improve upon the methodology, using aggregate time series data on various dimensions of teachers' strike activity (number of strikes, number of teachers involved, man-days idle, duration) over the 1946-73 period. These were specified to be a function of the percentage of school districts in states with permissive bargaining legislation and a vector of control variables. While the bargaining legislation variable tended to be positively associated with the various strike measures, it also tended to move like a time trend over the 1960-73 period; this makes it difficult to separate out its effects from those of changes in any other variables (e.g., teacher militancy).[52]

A second set of studies uses cross-section or pooled cross-section time-series data at the state level and focuses on more aspects of state bargaining laws. Burton and Krider (1975) used data on four dimensions of local government non-teacher strikes (strikes per employee, striking employees per employee, man-days idle per employee, and duration of strikes) for the 1968-71 period and regressed these outcomes on a vector of control variables, as well as a vector of public sector bargaining law characteristics. The latter included dichotomous variables for the existence of a permissive bargaining law, the requirement that the parties meet and confer, the requirement that the parties bargain in good faith, the existence of third-party impasse procedures, and the existence of laws penalizing strikers. The authors found no consistent pattern of significant effects for any aspects of the laws, either in individual year cross-section or in the pooled data, and noted the low explanatory power of their models. The latter result is a characteristic of virtually all studies of public employee strikes.

Subsequent studies using state-level data have followed in the Burton and Krider (1975) tradition. Perry (1977) used 1973 data for teachers, other local government employees (excluding police and firefighters), and state government employees and concluded that permissive strike policies tended to increase the frequency of teacher strikes. Rogers (1980) used data on all local government employees (including teachers) for 1974 and 1975 and concluded that "meet and confer" laws were associated with a greater frequency of strike activity and that laws that made bargaining illegal and those that provided for third-party impasse procedures were associated with a lower frequency.[53] Finally, Partridge (1983) sought to replicate the Bruton and Krider analyses using data for all nonuniformed noneducational employees for the 1974-78 period. For the most part his results were consistent with Burton and Krider's (no significant associations), although he did find that limited right-to-strike provisions were positively associated with strike frequency.

The usefulness of these interstate studies is limited by their treatment of state statutes governing bargaining as exogenously determined, by the collinearity of various provisions of the laws which makes it difficult to disentangle their independent effects, and by the fact that the unit of observation does not correspond to the bargaining unit which makes it difficult to control for other forces that might influence the level of strike activity. Several recent studies use data on individual bargaining units and go at least part of the way towards resolving these difficulties.

Olson, et al. (1981) used data on teachers, nonuniformed municipal employees, and police and firefighter negotiations in a number of states in 1975 and 1976. Logit probability of a strike occurring equations were estimated separately for each employee group, with an arbitration dummy variable included in the police and firefighter equations and a state dummy variable in the other equations to control for state public policies. The magnitudes of the estimated coefficients suggested that in states where strike penalties were harsh <u>and</u> frequently enforced the frequency of strikes was lower, as it was for police and firefighters in states with an arbitration statute. While this study treated state laws as exogenous, Ichniowski (1982b) used data from 863 municipalities in 13 states for a number of years on police work stoppages and estimated a fixed effects model to control for the endogeneity of statutes. His results suggest that a change from no law to a "duty to bargain" law increased strike activity, while a shift from the latter to a compulsory arbitration statute decreased strike frequency.

A final study that utilized bargaining unit data has moved the analyses away from estimating the effects of state statutes back towards understanding the economic determinants of strikes. Olsen (1983) argues that most theories of strike activity imply that higher costs to the parties of strikes will lead to a lower level of strike activity. In the case of teachers, these costs will be inversely related to the probability that a school district will opt to reschedule school days lost during a strike. He models the latter as a function of the community's demand for education and the penalties imposed by the state if the length of the school year falls below a mandated state minimum.

Using data on all school districts in Pennsylvania over a four-year period, a two-equation bivariate probit model was estimated that simultaneously determined the probabilities that lost school days will be rescheduled and that a strike will occur. The latter was specified to be a function of whether strike days were rescheduled in the past (if a strike occurred) and their probability of being rescheduled in the current round. His preliminary findings suggested that both of these variables positively influenced the probability of observing a strike.

Turning next to the studies of the usage of third-party impasse procedures, these have been of three types.[54] The first addresses the issue of the "chilling" effects of arbitration statutes, asking how the statutes influence the amount of bargaining that occurs. A number of studies have analyzed a modified final offer arbitration statute introduced in Iowa in the mid 1970s. This statute replaced a prior system that had factfinding as the terminal stage in the procedure and it permits the arbitrator to choose as the final award either of the parties' offers or the recommendation made by a factfinder. Gallagher and Pegnetter (1979) found the number of issues taken to impasse declined and Gallagher and Cahubey (1982) found the parties' willingness to compromise once at impasse increased after this statute was introduced. Similarly, Gallagher, Feuille, and Chaubey (1979) found that if a factfinder's report was issued, the parties tended to take fewer issues to arbitration and to compromise more on those issues prior to arbitration; a not unexpected result if one interprets the factfinder's report as an estimate of what the arbitrator will consider to be an intrinsically fair solution, which reduces the parties' uncertainty about this parameter (Farber (1980)).

Other studies used data from several states. Feuille (1975) studied the introduction of arbitration statutes in Michigan, Pennsylvania, and Wisconsin, found that the states with final offer arbitration had fewer issues in each case taken to impasse than did the states with conventional arbitration, and concluded that conventional arbitration had more of a chilling effect on bargaining. In contrast, Wheeler (1978) looked at the gap between the parties' final offers in states with and without arbitration statutes and found the parties' range of disagreement was smaller in states with arbitration.

Each of these studies used a very simple quasi-experimental design --a before-after comparison or a comparison of differences across bargaining units--without any attempt to control for factors other than differences in the laws that might cause the outcomes to differ over time or across units. The same criticism can be directed at the second type of study; those which address the issue of the chilling effect by looking at variations in the frequency of impasse over time and across areas. Somers (1977) and Lipsky, Barocci, and Svojanen (1977) for Massachusetts, Kochan, et al. (1978) for New York State, and Olsen (1978) for Wisconsin, all found that the percentage of police and firefighter negotiations going to impasse increased after the passage of arbitration statutes. Similarly, Lipsky and Drotning (1977) found the shift from legislative determination to factfinding as the final stage of the impasse procedure for teachers in New York State was associated with an increased percentage of negotiations going to impasse. Finally, Wheeler (1975b) conducted an interstate analysis of the percentage of firefighter negotiations going to impasse and found it to be higher in states with arbitration statutes than in states where the procedures terminated with factfinding.

The third type of study addresses whether the procedures create a narcotic effect--a tendency once the parties use a procedure for them to become increasingly reliant upon it in future negotiations. The methodological approach used in these studies is somewhat more satisfactory than those used in the studies cited above. Equations of the form

$$(3) \qquad Y_{it} = X_{it}B + \Theta Y_{it-1} + \varepsilon_{it}$$

were estimated where Y_{it} (Y_{it-1}) takes on the value of one if bargaining unit i goes to impasse in period t ($t-1$), X_{it} is a set of economic, political, structural and organizational variables expected to influence the probability that unit i goes to impasse in period t, B is a vector of regression coefficients and ε_{it} is a random error term.[55] A positive estimate for the coefficient Θ would suggest that prior impasse experience positively influences the probability of going to impasse in the current round.[56]

Estimates of variants of equation (3) are found in Kochan and Baderschneider (1978) for police and firefighters in New York, Olson (1978) for the same groups in Wisconsin, and Lipsky and Drotning (1977) for teachers in New York. All found evidence that Θ is positive, which they interpret as implying that prior impasse experience has a narcotic effect.[57] But does it really mean this?

The problem here is one of distinguishing between a true narcotic effect and unobservable heterogeneity across bargaining units. If any unobservable variables that influence the probability of going to impasse exist and remain roughly constant over time, estimates of Θ will be biased in a positive direction. Butler and Ehrenberg (1981) show how one can correct for this bias using either a fixed or random effects model, along with an instrumental variable approach. Indeed, when they reanalyzed the Kochan-Baderschneider data using these methods, the estimates of Θ they obtained proved to be negative--suggesting that a negative narcotic effect was present. That is, once unobservable heterogeneity was controlled for, the experience of going to impasse in the past appeared to <u>reduce</u> the probability of going to impasse in the current round.[58]

Studies of the <u>effects</u> of impasse procedures have focused on whether unions or management tends to win under arbitration, what the effects of the <u>use</u> of the procedures are on economic outcomes, and what the effects of the <u>availability</u> of the procedures per se are on economic outcomes. In the first group are studies by Ashenfelter and Bloom (1983a)(1983b) for New Jersey and Somers (1977) for Massachusetts that indicated that unions won over 60 percent of police and firefighter cases under final offer arbitration in the early years of the statutes. As we have previously discussed, Ashenfelter and Bloom have emphasized (following Farber (1980b)) that such a finding does <u>not</u> imply that arbitrators are biased in favor of unions.

Some of the studies of the effects of the use of impasse procedures have simply contrasted the mean wage levels or wage changes in a state of units that settle at different stages of the process.[59] Others have estimated wage level or wage change equations across bargaining units in a state, including a set of explanatory variables to control for other forces that might be expected to influence wages.[60] The consensus of these studies seems to be that the usage of arbitration per se, or the stage of the impasse procedure one settles at if an impasse is reached (mediation, factfinding, or arbitration), has no effect on wage levels or wage changes; the former result is not consistent with the Farber-Katz (1979) model. In contrast, in areas where strikes are at least de facto legal, there is some evidence that settlements arrived at in negotiations that do wind up in a strike are higher than those arrived at when the parties settle without reaching an impasse.[61]

Many of the studies of the effects of the availability of different forms of impasse procedures have used national samples (either at the individual or bargaining unit level) and estimated wage equations that included dummy variables for the form of impasse procedure present.[62] Others have estimated wage level or wage change equations across bargaining units within a single state, including a number of years' data and dummy variables for years after a (new) impasse procedure was in place.[63] While the latter suggest that the presence of an arbitration statute does not affect wages, the former strongly suggest that the availability of a statute increases wage levels by some 6 to 10 percent. Similarly, the availability of a strike option, also seems to be associated with higher wages.[64]

One must interpret these results with caution, however. The studies of the availability of the various procedures have not included data on actual use; it is therefore difficult to disentangle the effect of availability from that of use.[65] More importantly, virtually all of the studies treat both the use of impasse procedures and their availability as exogenous. The former treatment seems strange in light of the work of Kochan and Baderschneider (1978) and others described above, that model the usage of impasse procedures. The latter seems equally strange since the types of impasse procedures that exist are not randomly distributed across states. For example, arbitration statutes seem to have been enacted first in states where public sector unions are strong and where one might expect to observe above average wages even in the absence of the statutes. It is not surprising then, that the national cross-section "availability" studies show arbitration and/or the right to strike statutes having a positive effect on wages. Before these results can be taken at face value, the endogeneity of the availability and use of impasse procedures must be addressed.[66]

Finally, to reiterate a point made first in Section II, the availability studies focus on only one aspect of the legal environment governing public sector bargaining in a state. The effects of the "availability" of impasse procedures more appropriately should be estimated in the context of a model that permits consideration of other aspects such as budget referenda requirements, expenditure and tax limitation legislation, agency shop provisions, and constitutional requirements for balanced budgets.

VII. Public/Private Pay Comparisons

As noted in the introduction, the pay of most federal white- and blue-collar workers in the United States is determined through a comparability process that ties their wages to the results of a government surveys of "comparable private employees," subject to possible Presidential and Congressional modification. Other federal workers, for example postal workers, have their wages determined via collective bargaining, as do many state and local government employees. However, again as noted above, the dispute resolution procedures which govern collective bargaining in the public and private sectors differs substantially between sectors.

Given these differences between the public and private sector, many researchers have sought to ascertain whether comparably qualified workers performing comparable work in the public and private sector actually receive equal total compensation. That is, do the variety of institutional arrangements for determining wages that exist result in a compensation structure in which public employees are doing no better (or no worse) than they would if they were employed in the private sector? Even in the case of federal workers covered by the comparability surveys this question is difficult to answer because the comparability studies historically focused only on wages and ignored both nonwage benefits and nonpecuniary forms of compensation.[67] Moreover, the jobs performed in the public and private sectors are not always directly comparable and subjective decisions must often be made as to how a job should be classified.

As a result, instead of focusing on the earnings of workers with comparable job characteristics, researchers have focused on the earnings of workers with comparable measured personal characteristics in the two sectors. The basic methodological approach, which is discussed most fully in Sharon Smith (1977a), is identical to that used in studies of sex, race, or union wage differentials. Equations of the form

$$(4) \qquad Y_i = \sum_{j=1}^{n} \alpha_j X_{ji} + \alpha_{n+1} d_i + \varepsilon_i$$

are estimated over a sample of public and private sector workers, where Y_i is some measure of the natural logarithm of earnings, the X's are a vector of personal characteristics expected to influence earnings, d_i is a dichotomous variable which takes on the value of one if the individual is a public employee and zero otherwise, and ε_i is a random error.

Estimates of the parameter α_{n+1} provide information on the public/private earnings differential.[68] In practice, a vector of dummy variables is often used to indicate the level of government at which the individual is employed (federal, state, or local), separate estimates are obtained by race or sex, and/or separate earnings equations estimated for public and private employees. In the latter case, one can estimate the public/private differentials by the wage differentials that would exist if government employees with a given set of characteristics were paid according to the private wage equation, or by the differentials that would exist if private employees with such characteristics were paid according to the government wage equation.[69]

The major work in this area has been done by Sharon Smith in a series of articles and books; our summary of the estimates obtained by her and several other researchers for selected years between 1960 and

1978 appears in Table 4. The studies are not all directly comparable for a number of reasons. Some use annual earnings as the measure of earnings, others use hourly earnings. The variables included in the vector X vary across studies; for example only about a half of the studies include a measure of unionization. The definition of who is a public employee, especially at the state and local level also varies across studies. Finally, the private sector comparison group varies across studies; in most it is all private nonagricultural workers, but in the Wachter and Perloff (1981) study, it is taken to be private sector employees employed in service industries.

Despite these differences, these studies paint a fairly uniform picture.[70] The federal/private sector differential is positive but has appeared to diminish during the 1970s from over 20 to under 15 percent. Postal workers, whose salaries are determined via collective bargaining, receive earnings differentials relative to private sector workers, that are about equal to the differentials received by other federal workers.[71] The federal/private differentials appear to be larger for females than for males and for nonwhite males than for white males; this may reflect a lesser level of race and gender discrimination in the federal than in the private sector.[72]

A similar result occurs in both the state and local sectors, where again, public/private earnings differentials are larger for females than for males. Moreover, as we move from the federal, to the state, to the local government level, the size of the government/private earnings differential gets smaller. Indeed, after controlling for personal characteristics, on average males employed by local governments, and possibly also males employed by state governments earn less than their private sector counterparts.[73]

What factors cause the public/private earnings differentials to decline as we move from the federal to the state to the local level? One possibility is that taxpayer information about the effect of public employee wage increases on tax burdens is much easier to obtain and understand the smaller is the level of government. It may also be easier to hold local politicians accountable for such financial decisions; each federal legislator is just one out of hundreds of representatives who vote on scores of issues besides government employee pay. As such, pressure to hold down public-employee wage scales may be greater at the state and local than at the federal level.

A serious deficiency with most of these studies, which is often acknowledged by the authors, is that they tend to focus on measures of earnings or wages, rather than on total compensation. The latter should include all present and expected future forms of pecuniary (e.g., fringe benefits) and nonpecuniary (e.g., working conditions and stability of employment) conditions of employment. Presumably, if labor markets were fully competitive, one would observe equality of total compensation, as defined above, across sectors, not equality of current earnings.

Some limited research has been conducted that does focus on outcomes other than earnings levels. For example, several authors have tried to examine various components of fringe benefits. Bellante and Long (1981) and Quinn (1979b) use an estimate of fringes as a percentage of wages, and Quinn (1982) uses an estimate of pension wealth, and all find that fringe benefits in the public sector tend to exceed those in the private sector, with the difference being greatest for federal workers (one must be cautious in interpreting these results though, since some aspects of fringes are difficult to quantify and/or value). Similarly, Quinn (1977) uses survey data on disamenities of the workplace (pace of work, degree of supervision, danger, etc.) and finds that, ceteris paribus, private sector workers tend to be employed in situations with more disamenities than do public employees. These results suggest that positive public/private wage differentials are not compensating differentials for either lower fringe benefits or unfavorable working conditions.

Other authors have examined the question of stability of employment. Sharon Smith in a number of her studies (e.g., Smith (1976b)) uses both hourly and annual earnings as dependent variables to control for annual variations in hours of work. Bloch and Smith (1979) also directly examine the probability that an individual will be employed at a point in time, and find that it is higher for white male federal employees and for all race/sex groups of state and local government employees than it is for comparable private sector workers. Hence again, positive public/private wage differentials are apparently not compensating differentials for relative instability of employment in the public sector.

Somewhat surprisingly, there have been relatively few attempts to explain why public/private earnings differentials vary over time and across regions and states; the geographic variation has been noted by Smith in a number of her papers and by Borjas (1982b). A notable exception is Borjas (1982a) who presents and tests a theory of why federal/private wage differentials should vary over the electoral cycle and Borjas (1982b) who presents and tests a political model of a vote-maximizing bureaucrat to explain why state government/private differentials should vary across states.[74]

Neither of these studies, however, explicitly considers the role of institutional variables. To return to a previous theme, we are offered no insights about the effects of state laws such as those governing public sector impasse procedures, those establishing tax or expenditure limitations, or those governing public sector union security arrangements, on public/private pay differentials. Save for studies of postal workers, there have also been no studies of federal/private net wage differentials for federal workers whose wages are determined via collective bargaining; we have little evidence then about whether the comparability process leads to larger, or smaller, relative wage differentials than one would observe under collective bargaining.[75]

All of the studies of public/private wage differentials have treated individuals' sectors of employment as exogenous. However, if individuals nonrandomly sort themselves into public or private jobs because of differences in tastes for public service or preferences for nonrisky employment, then the possibility of sample section bias arises. In fact, evidence presented by Bellante and Link (1981) suggest that public sector workers are more risk averse than private sector workers and that, holding risk aversion constant, many of the same factors that influence wages in the public and private sector also influence the sector of employment.[76]

In view of all of the conceptual and measurement problems involved with trying to estimate public/private pay comparability, a number of investigators have suggested simply focusing on quit rates instead (e.g., Adie (1977)). Since, holding constant characteristics of individuals, better pecuniary and nonpecuniary conditions of employment should lead to lower quit rates, the argument is that public/private quit rate differentials would be prima facie evidence of public/private total compensation differentials. The evidence on quit rates seems compelling; both gross quit rates (e.g., Adie (1977), Wachter and Perloff (1981)) and net quit rates after controlling for personal characteristics (e.g., Long (1982)) are lower in the public than in the private sector.

A problem with these studies, however, is that they contain no controls for characteristics of jobs. One key variable is the size of the employer for, ceteris paribus, the larger the employer, the more likely that an unhappy employee can improve his lot by an intrafirm change. Put another way, quits should be negatively related to firm size (Utgoff (1981)). Since federal and state governments and some local governments are obviously large employers, their lower quit rates may at least partially be due to this fact.

A second key characteristic is the amount of specific training required for a particular job. As is well known, in situations where specific training is involved, an employer's goal is to minimize the sum of hiring, training and compensation costs, not simply compensation costs. A high-wage low-quit policy may contribute to the former objective. Thus, one can not simply focus on relative compensation or quit levels in judging comparability; one needs to know the savings in hiring and training costs from pursuing a high-wage policy. Although many researchers have looked at public/private quit and wage differentials, only Adie (1977) has examined (for postal workers) if the differentials could be possibly "justified" by lower hiring and training costs.[77]

VIII. Discrimination in Public Sector Labor Markets

The studies of public/private wage comparisons discussed in the previous section suggest that public/private wage ratios are higher for females than for males and for nonwhites than for whites (see Table 4). This *may* reflect a lesser extent of gender and race discrimination in public sector labor markets; a result that would not be totally unexpected for two reasons. First the highly structured nature of federal, state, and local government employment with civil service and/or collectively bargained work rules, often requires equal pay for all individuals with the same seniority and qualifications who are employed in a given job. Thus, discrimination can take primarily the form of slower promotion rates or unequal access to initial jobs, not of unequal pay for equal work.[78] Second, the oldest U.S. programs to combat race discrimination in employment are equal employment opportunity programs for government employees. The federal programs started during the New Deal and, by 1945, thirteen states had similar provisions for their employees--predating the Civil Rights Act by some twenty years. If these programs had any "teeth", one would expect to observe less race discrimination in the public sector.

A number of researchers have focused on estimating the extent of race or gender discrimination in the public sector and their methodologies are by now fairly standard. Returning to equation (4) of the previous section, let the sample now refer to white federal employees, let d_i now be a dichotomous variable that takes on the value of one if the individual is a male and zero if the individual is a female, and let all other variables be defined as before. Estimates of the parameter α_{n+1} now provide information on the male/female earnings differential that exists for whites employed in the federal sector, after one controls for the other variables in the analysis.

One can similarly do analyses of gender differentials for nonwhites employed in the federal sector, and for employees of state and local governments. By restricting the sample to employees of one gender and letting d_i stratify employees by race, one can obtain estimates of white/nonwhite earnings differentials for public employees. Finally, rather than inserting a dichotomous variable in (4), one can again estimate separate equations by gender (or race). In this case, the male/female (white/nonwhite) government employee earnings differential is estimated by the differential that would exist if female (nonwhite) government employees with a given set of characteristics were paid according to the male (white) wage equation.[79]

Studies that have utilized such approaches with various micro-data files suggest that, after controlling for personal characteristics of workers, the earnings of minorities and females employed in the government sector are often substantially lower than those of white males--although the magnitude of the differences may be slightly less than comparable gender and race differences in earnings found in the private sector. For example, Corazzini (1972) studied black federal employees in the Washington, D.C. area and found they earned approximately $1,700 less than their white counterparts, primarily due to slower rates of promotion.

Long (1976) used data from the 1970 Census and found adjusted black/white earnings ratios in the federal sector of roughly .76 for males and .74 for females; the former is about the size of the private sector ratio while the latter is considerably larger, implying that less gender discrimination occurs in the federal than in the private sector. Borjas (1978) used data collected by the Civil Service Commission for employees of the then Department of Health, Education and Welfare in 1977 and again found females and nonwhites paid significantly less than white males with identical personal characteristics; results that he found to hold true for federal employees in general in a later paper (Borjas (1983)).

As in the case of public/private pay comparisons, perhaps the most comprehensive study to date is S. Smith (1977a) who analyzed gender and race differentials in public employees' earnings at different levels of government. Some of her results, obtained using 1973 and 1975 Census of Population data, are summarized in Table 5. They confirm that, ceteris paribus, males appeared to get paid more than females and whites more than blacks at all levels of government.[80] However, the race and gender differentials she found were smaller at the state and local government level than they were at the federal level. Indeed, Antos and Rosen (1975), who confined their analyses to local government public school teachers, found virtually no evidence of gender differentials and only little evidence of race differentials.

To the extent that the male/female and white/nonwhite earnings differentials one observes in the federal sector can be interpreted as estimates of labor market discrimination, one must conclude that the federal government EEO programs directed at its own employees have not been completely effective. However, this does not imply that federal government employment per se has not reduced the extent of labor market discrimination in the economy. These studies suggest that gender and race differentials are smaller in the public sector and the studies summarized in the last section imply, ceteris paribus, that federal government employees earn more than private employees with comparable characteristics. As such, if the probabilities that females or nonwhites obtain employment in the federal sector exceed the comparable probabilities for males or whites, the presence of government employment will cause the average female (nonwhite) wage in the economy to rise relative to the average male (white) wage. D. Alton Smith (1980) demonstrates that this condition appears to have been met.

In a recent article, Borjas (1982c) has moved the discussion away from measuring the existence of gender and race earnings differentials in government per se to a discussion of discrimination in different federal government agencies. It is well known that both the fraction of an agency's employees who are minorities and the fraction of these minority employees who are in upper-level jobs varies widely across agencies. For example, in 1978 both fractions were low in the Defense Department and high in the Equal Employment Opportunity Commission. Similar observations can be made for female employees who in 1978 were underrepresented at Defense but overrepresented at the then Department of Health, Education and Welfare. After providing evidence that the magnitudes of gender and race earnings differentials also vary across agencies (Borjas (1982c) (1983)), Borjas seeks to provide an explanation for why this might occur.

Based on previous work (Borjas (1980a) (1980b), he presents a model of a government trying to maximize its political support. The constituency of each government agency is assumed to have a "taste" for discrimination and he shows that the vote maximization hypothesis predicts that the economic status of minorities (females) in an agency will depend upon how important minorities (females) are in generating political support for the agency. Operationally, the race (gender) composition of an agency's constituents is measured by the race (gender) composition of employment in the industry the agency "relates to" and/or the race (gender) composition of the population in states where the agency expends funds. The expenditures made by an agency on civil rights activities is also used as a measure of its affirmative action orientation. His empirical work, which uses individual personnel data from the Office of Personnel Management, does indeed lead to the conclusion that a portion of the interagency variation in race and gender earnings differentials can be explained by interagency variations in the above variables.

Finally two recent studies have sought to ascertain whether specific federal programs relating to public sector labor markets have significantly reduced gender discrimination. Simeral (1978) estimated wage equations for a sample of participants who held Public Service Employment (PSE) program jobs that were created under the Emergency Employment Act of 1971. She estimated separate wage equations for participants' pre-program, PSE, and post-program jobs and computed male/female wage differentials from each. These differentials actually rose over time; hence she concluded that the PSE program did not lead to less gender discrimination.

Eberts and Stone (1982) focused on gender differences in promotions of public school teachers to administrative positions in New York and Oregon. They sought to analyze the effects of Title IX legislation, that was passed in 1972 and enforced through guidelines starting in 1975, which prohibited gender discrimination against students and employees in public schools. Using longitudinal data on teachers in both states they estimated logit probability of promotion equations for periods prior to and after the legislation, finding that gender differentials in promotion rates to administrative positions tended to decline after the legislation. While they concluded that the legislation probably had an impact on females' promotion rates, one must caution that their approach did not permit them to disentangle the effects of the law from the effects of any other "macro-level" variables that changed at the same time. In particular since the decade of the 70's saw an ever increasing movement of women out of traditional female occupations, such as teaching, one might question whether higher female promotion rates to administrative positions would have occurred even in the absence of the law.

IX. Concluding Remarks

A long summary of the literature requires no summary. However, several themes have emerged from our review that are worth repeating.

First, one unique aspect of public sector labor markets is that the laws governing impasse resolution vary across states. This provides an opportunity for researchers to estimate their effects on union/nonunion wage (and nonwage) differentials, on wage levels, on the demand for labor, and on public/private pay differentials. However, other aspects of the legal environment that influence bargaining also differ across states; these include budget referenda requirements, expenditure and/or tax limitation legislation, balanced budget requirements, and agency shop provisions. Studies are required that consider all of these forces simultaneously and that allow for the possibility that many of them are endogenously determined.

A second unique aspect is that the unit of observation in public sector studies tends often to be a bargaining unit (e.g., a city or school district), and the underlying union contracts in areas where bargaining takes place are typically available to researchers. As such, in contrast to private sector studies that have focused on estimating union/nonunion productivity differentials, there is much more room in the public sector for studies of how specific contract provisions influence resource allocation decisions and productivity. One must stress here, though, both the need to model the determinants of contract provisions and the fact that unionization per se may influence productivity independently of specific contract provisions.

Third, studies of the tradeoffs between wage and nonwage conditions of employment in the public sector suffer from the same two methodological problems that virtually all private sector compensating wage differential studies suffer from (but rarely admit). On the one hand, these studies typically try to estimate the tradeoff between wages and one set of nonwage characteristics, for example fringes (working conditions), but omit other job characteristics, for example working conditions (fringes) from the analysis. On the other hand, their estimation methods typically treat the nonwage characteristics as predetermined. While in some cases neither one of these restrictions will cause problems, often econometric problems arise that only a few researchers have confronted (see Section VII).

Fourth, many of the studies of public sector labor markets make no mention of the role of unions. For example, while there are private sector studies that examine whether unions affect the demand for labor or the existence of compensating wage differentials for unfavorable job characteristics, virtually no public sector counterparts exist.[81] Clearly, there is room for research here.

Finally, many of the empirical studies of arbitration statutes use criteria such as whether arbitrated settlements are the same as negotiated ones, or whether unions and management each win roughly half of the cases that go to impasse, to evaluate how the statute is performing. However, simple economic models of the arbitration process suggest that a priori neither of those outcomes is likely to occur. This suggests that the empirical studies may have focused on inappropriate criteria and it emphasizes the general proposition that the criteria used in evaluations of social policies should be based on explicit conceptual models.

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Footnotes

1. For an interesting analysis of the private not-for-profit or voluntary sector, see Burton Weisbrod (1977).

2. John F. Burton, Jr. (1979), Table 3. After 1978 the data cease to be comparable, so we have terminated our comparisons as of this date.

3. There were some major exceptions--namely, postal workers and employees of federal government authorities, such as the Tennessee Valley Authority (TVA). In each of these cases the prices of the products or services produced (mail delivery, hydroelectric power) can be raised to cover the cost of the contract settlement, unlike other federal agencies where salaries are paid out of general revenues.

4. See Sharon Smith (1977a) for a more complete description of the comparability process in the federal sector.

5. See B. V. H. Schneider (1979) for a more complete discussion of the evolution of laws governing bargaining in the public sector.

6. See H. Wellington and R. Winter (1969).

7. For more details on dispute resolution in the public sector, see Thomas Kochan (1979) and John Burton (1981).

8. The PSE was terminated, however, by the Reagan Administration in the early 1980s.

9. For example, on the public/private pay comparability issue alone one could cite R. Layard, A. Matin, and A. Zabalza (1982) for Great Britain, and Morley Gunderson (1980) for Canada.

10. See, for example, Martin Binkin and Irene Kyriakopoulos (1981) for a study of executive compensation in the federal sector. At the local government level, Gerald Goldstein and Ronald Ehrenberg (1976) study whether the compensation of police chiefs, fire chiefs, and city managers is related to their "performance".

11. See, for example, Robert Hartman and Arnold Weber, eds. (1980).

12. This section owes much to previous surveys of public sector union wage effects including Lipsky (1982), Mitchell (1979), and Flanagan and Mitchell (1982).

13. The study using SMSA data is Cain, et al. (1981), while the studies using CPS data include Baugh and Stone (1982) and Moore and Raisian (1981).

14. In spite of the dramatic growth of public sector collective bargaining over the last 25 years, relatively few studies have been conducted on the determinants of the growth of public sector unionization over time or on why collective bargaining coverage in the sector varies across areas. This is surprising because the laws governing bargaining in the SLG sector differ across states and are continuously evolving.

A number of studies have estimated probit union coverage, or existence of a union contract, equations using cross-section data in the context of models that seek to control for selection bias in union outcomes equations. For example, Cain, et al. (1981) used individual data from the CPS for hospital workers, finding that region of the country and size of city were the key explanatory variables. Bartel and Lewin (1981) used data from a 1973 survey of police in about 200 cities with populations greater than 25,000 and found that the percentage of private sector workers organized in an area was significantly associated with the probability of union coverage. Finally, Ehrenberg, Sherman and Schwarz (1983) used data on 260 municipal libraries in cities over 50,000 in size in 1977 and found that the probability of observing a union was significantly related to the laws in the state governing collective bargaining for municipal employees. In particular, a state law that prohibited strikes reduced the probability of observing a union, while a state law providing mediation or factfinding services in the event of an impasse, increased the probability.

This latter study illustrated one type of natural experiment one can perform to analyze the effects of state laws on union coverage. A second type is found in Ichniowski (1982a) who used panel data, spanning the 1958-1978 period for a set of almost 1,000 cities, on the existence of a written contract for police and estimated a logit model of the determinants of union contracts. Even after controlling for a time trend and a set of city-specific variables, Ichniowski found that the number of years since a public sector bargaining law was passed in a state significantly was associated with the probability of observing a contract. Moreover, independent associations were found for several dimensions of the law--whether bargaining was permitted, whether bargaining was required, and whether an arbitration statute for police impasses existed.

A final study, Moore (1978) sought to explain both aggregate time-series (1919-1970) and interstate (1970) variation in teachers' unions membership. For some specifications of his cross-section work, he found that both the presence of a mandatory bargaining law and the proportion of private sector employees who were union members significantly associated with teacher union membership.

One senses from these studies that state laws governing public sector bargaining are significantly associated with union membership and collective bargaining coverage. However, an unresolved issue is the direction of causation; no one has seriously studied whether public sector union strength influences laws governing public sector bargaining. Room is clearly present here for more work, possibly involving Granger causality tests.

15. Ichniowski (1980).

16. In the case of the percentage unionization variable, the estimate should be interpreted as the relative wage differential between cities with some union members and those with none. We should note that Lewis (1983) has argued that the estimates obtained from macro level studies (studies that use grouped data) that use an extent of unionization variable should <u>not</u> be interpreted as wage gap estimates. His argument, however, seems to apply to the case when the units of observation are all in different industries. When they are in a single industry, as is the case for the public sector studies, one can show that the estimates can be interpreted as wage gap estimates (see Ashenfelter (1971), footnote 16) although they will not always be unbiased estimates.

17. A notable exception, however, is Edwards and Edwards (1982a) who find larger union/nonunion wage differentials for solid waste collection employees in cities with public collection systems than in cities with private systems.

18. Some people have also argued that the smaller estimated public sector union differentials may reflect the fragmented nature of bargaining in the public sector--with bargaining done often at the local level by "occupation." Since a similar bargaining structure exists in construction where union/nonunion differentials considerably exceed the private sector average differential, we are suspicious of this explanation.

19. In fact one study, Rogers (1979), found that union/nonunion fringe benefit differentials were sometimes <u>negative</u> for certain categories of fringes, suggesting a willingness of unions to trade off some benefits for others.

20. Suppose initially that there are two types of employees, junior workers who get paid W_j and senior workers who get paid $(1+s)W_j$ and that half of a city's workforce is in each category. Suppose also that a union increases wages of the two groups by α and αm percent respectively, $(m > 1)$ and the city responds by increasing the share of junior workers it hires to $\gamma(>\frac{1}{2})$. Then the union/nonunion average wage differential is $[W_j(1+\alpha)\gamma + W_j(1+s)(1+\alpha m)(1-\gamma)]/[W_j(\frac{1}{2}) + W_j(1+s)(\frac{1}{2})]$ or $[(1+\alpha)\gamma + (1+s)(1+\alpha m)(1-\gamma)]/[1 + (s/2)]$. This will be less than $(1+\alpha)$ provided that $[(1+s)(1+\alpha m)(1-\gamma)/(1+\alpha)] < 1 + (\frac{s}{2} - \gamma)$. For example, if $s = 1$ (initially senior workers get paid twice as much), $\alpha = .1$ and $m = 1.5$, then any value of $\gamma > .542$ will yield the union/nonunion average wage differential to be less than the comparable minimum wage differential.

21. To see this, suppose that there are two types of cities; low wage where public employees are paid W_n in the absence of unions and high wage where they are similarly paid hW_n $(h > 1)$. Suppose initially that all of the former are nonunion and the fraction f of the latter are unionized and that unions increase their members' wages by α percent. If there are an equal number of high and low wage cities, the observed union/nonunion wage differential is given by

$$[(1+\alpha)hW_n]/[\{(1-f)hW_n + W_n\}/(2-f)] \quad \text{or} \quad (1+\alpha)\left\{\frac{h(2-f)}{(1-f)h+1}\right\}$$

It is straightforward to see that as f increases the observed differential increases.

22. A number of studies have looked at whether union/nonunion wage differentials vary with characteristics of cities, such as city size. For example, Ehrenberg (1973) found annual salary differentials for firefighteres that did not vary with city size.

23. For a good nontechnical treatment of the argument, see Freeman and Medoff (1979).

24. The latter studies also attempted to ascertain if collective bargaining coverage affects the elasticity of substitution between capital and labor or the elasticities between different categories of labor in the production of library services.

25. In work in progress, Linda Edwards is studying productivity in sanitation.

26. Teachers' effectiveness was estimated via the estimation of educational production functions using individual student data, which permitted separate intercepts for each teacher in the sample.

27. Winkler (1980) is an exception.

28. Eberts and Stone (forthcoming) is an exception.

29. The next few paragraphs drawn heavily from Smith and Ehrenberg (1979). For more details, see also Ehrenberg and Smith (1981).

30. Gene Mumy (1978) treats underfunding as a temporary intergenerational transfer and models why the level of underfunding should vary across retirement systems.

31. Why underfunding may be viewed as permanent is discussed in Mumy (1983) and Inman (1981). The latter presents evidence that underfunding is not fully capitalized in the form of lower property values, which suggests that politicians are able to at least partially "hide" the underfunding from taxpayers.

32. A model deriving the set of correlates is found in Ehrenberg (1980b).

33. R. Smith (1983) also found that state (as opposed to local) funding of pensions was associated with higher wages, the level of pensions held constant; a not unexpected result.

34. For an elaboration of the functional form issue see R. Smith (1979).

35. Eberts and Stone's paper goes beyond the compensating differential question and attempts to test if teachers' contracts are "efficient".

36. For an elaboration of this point, see Smith and Ehrenberg (1983).

37. The use of such "median voter" or "representative decision-maker" models goes back at least as far as Downs (1957) and Tullock (1967). Not all economists believe, however, that public sector decision-making can be effectively modeled in such a way. See, for example, Reder (1975) and Brennan and Buchanan (1980) who postulate that bureaucrats seek to maximize their own welfare, not that of a median voter.

38. The Stone-Geary utility function and the resulting linear expenditure system is described in Stone (1954). Its modification to allow for minimum required consumption levels to be functions of prior consumption occurs in Pollack and Wales (1969). The system of employment demand equations actually estimated by Ehrenberg was

$$\log\left(\frac{M_j^{\,t}}{P} - \frac{\alpha_j M_j^{\,t-1}}{P}\right) = b_{oj} + b_{1j}\log w_j + b_{2j}\log\left(\left(B - \sum_{k=1}^{n} w_k \alpha_k M_k^{\,t-1}\right)/P\right)$$

$$+ \sum_{k=1}^{m} C_{Rj}\log SD_r \qquad j = 1,2,\ldots n$$

where M_j/P is per capita SLG employment in category j, α_j is the minimum fraction of last period's employment in the category that must be employed this period, w_j is a measure of the category's wage rate, B/P

is the per capita SLG employment budget, the SD_r are those sociodemographic variables expected to influence the demand for category j, there are n employment categories, and the b's and C's are parameters to be estimated. With this system, decisions are made about increments of employment above "committed" levels and only the total employment budget less "committed" expenditures is free to be allocated. To be consistent with the Stoney-Geary utility function b_{1j} should equal minus one and b_{2j} equal one for all j; a very severe restriction which was not imposed in the estimation. A separate equation was also fitted to explain the determinants of the per capita SLG employment budget.

39. See Barten (1968) or Theil (1971). Ashenfelter and Ehrenberg (AE) estimated the system of demand equations

$$f_j d\ \ln(M_j/P) = \sum_{k=1}^{n} \Pi_{jk} d\ \ln w_k + u_j\left[\sum_{k=1}^{n} f_k d\ \ln(M_k/P)\right] \quad \begin{matrix} j=1,\ldots n \\ k=1,\ldots n \end{matrix}$$

where w_j and M_j/P again represent the wage rate and per capita employment level of the j^{th} category of public employees and f_j is the average share of category j in the total SLG employment budget. The equation is expressed in terms of the change, over time, in the natural logs of the variables and the expression in brackets on the right-hand side can be shown to be approximately equal to the change in the real per capita employment budget. The u_j and Π_{jk} are parameters to be estimated; the former is interpreted as the marginal budget share allocated to category j. It is straightforward to show that to satisfy the budget constraint the u_j must sum to unity and that utility maximization implies that $\Pi_{jk} = \Pi_{kj}$ (symmetry) and $\sum_k \Pi_{jk} = 0$ (homogeneity). Thus, the restrictions imposed by the utility maximization hypothesis can be tested directly.

Since this system is expressed in first difference form, any socio-demographic variables that might affect only the intercept term of the demand equations drop out. However, to allow for the possibility that population density might affect other parameters in the model, separate estimates were provided by AE for high and low density states.

40. For evidence on union effects on class size, see Hall and Carroll (1973).

41. For a recent analytical treatment of this point, see Courant, Gramlich and Rubinfeld (1979). They also provide evidence, based on a survey of a random sample of 2001 Michigan residents in 1978 that dealt with voters support for tax limitation legislation, that SLG employees appear to want more SLG spending than do private and federal employees (see Courant, Gramlich, and Rubinfeld (1980)), although the difference is not large.

42. The only study we know that examined the effects of unions and form of government on the demand for municipal employees, was Ehrenberg (1973b) who looked at a sample of 90 cities in 1970-71 and found inconclusive results.

43. If the "median voter" approach was correct, the passage of such legislation would only reflect changes in other forces influencing the demand for public services and should have no independent effect. If, in contrast, a "bureaucratic-maximization" approach is correct, passage of restrictions based on voters' preferences might affect public sector outcomes. Some evidence in favor of the bureaucratic model is found in Shapiro and Sonstelie (1982), however, they do not focus on public sector labor demand.

44. A similar estimate is found in Perles (1983). Bassi (1981) updates the cross-section analyses to FY 78 and 79 and finds results similar to those in Adams, _et al_. (1983) that are cited immediately below.

45. One should also stress that the net job creation effect of a PSE program is not the sole criteria one should use in evaluating it. Even if fiscal substitution is complete (no new SLG employees are hired directly with program funds), as long as the PSE funds are spent (or used to reduce taxes) they will have a generally stimulative effect on labor markets. In addition, one is also interested in questions like "Did it shift the composition of SLG employees toward groups at which the program was targetted?" or "Did it substantially increase the earnings of program participants, both during and subsequent to their program participation?" Answers to these questions have been provided by many; these studies are outside the scope of our survey. For some evidence on the latter point, see Bassi (1982).

46. For example, in 1979 there were 593 work stoppages involving 205,000 state and local government workers (U.S. Bureau of Labor Statistics (1981)).

47. See Stevens (1966) for an early argument to this effect.

48. This point has also been made by Kochan, et al. (1979) and Hirsch and Donn (1982). It is often thought that final offer arbitration would increase uncertainty due to the all or nothing decision of the arbitrator. However, Farber and Katz suggest that such a view ignores the endogeneity of the parties' final offers; a point developed more fully in Farber (1980a)(1980b).

49. Farber (1981) also shows that the more weight the arbitrator puts on the parties' positions, the smaller the contract zone will be and thus the higher the probability of arbitrated settlements.

50. Crawford (1981) also questions the generalizability of these models when more than one outcome is being negotiated and raises the possibility that the equilibriums that the models assume to exist will not always exist. For his own work, which considers bargaining over a number of outcomes when arbitrators' decisions are assumed to be known with certainty, see Crawford (1979).

51. By "permissive" legislation, we mean legislation that explicitly permitted collective bargaining for teachers.

52. Nelson, Stone, and Swint (1981) is another aggregate time-series study, but the only public policy variable they included was a dummy variable for the passage of the Landrum-Griffin Act. Since the latter applies only to private sector unions, its relevance to the public sector is not obvious.

53. Wheeler (1975a) performed a simple comparison (without any controls) of firefighter strikes in states with arbitration statutes vis-a-vis those without such statutes and similarly found lower levels of strike activity in the former.

54. Due to space limitations our discussion here is necessarily brief. For a survey of the empirical arbitration literature, see Anderson (1981).

55. See Kochan and Baderschneider (1978) for a description of the variables that might enter into the vector X. Their effort to provide a behavioral model of the forces that determine whether negotiations go to impasse is an important one, which we shall return to below.

56. The model is easily extended to allow impasses in rounds prior to t-1 to have an effect. Some of the researchers cited below have also

simply looked at aggregate statistics on whether the conditional probability of going to impasse depends on prior impasse experience. It should be evident that this is equivalent to estimating equation (3) without any X variables, so all of our criticisms of that approach given below are equally applicable to these efforts.

57. Olson actually finds that if a union has won a prior arbitration hearing, the probability of going to arbitration in the current round increases.

58. Explanations for why a negative narcotic effect might occur are found in Butler and Ehrenberg (1981).

59. See for example, Gallagher, Feuille, and Chaubey (1979) who contrast outcomes under mediation, factfinding and final offer arbitration in Iowa, Subbarao (1979) who contrasts outcomes for Canadian federal sector workers who struck or who went to arbitration (Canadian law permits the choice of routes), Lipsky, Barocci, and Svojanen (1977) who contrasted outcomes at different stages of impasse resolution for Massachusetts police and firefighters, and Thompson and Cairnie (1973) who contrasted arbitrated and negotiated wage settlements for British Columbia school districts.

60. See for example, Bloom (1981b) who contrasted negotiated, conventionally arbitrated and final offer arbitrated settlements for police in New Jersey, Delaney (1983) who contrasted negotiated and arbitrated settlements for teachers in Iowa and negotiated settlements with and without strikes for teachers in Illinois, and Anderson (1979) and Auld, Christofides and Wilton (1981) who studied Canadian federal sector employees.

61. See Delaney (1983) and Subbara (1979).

62. For example, Olson (1980) used data for six years on 72 firefighter bargaining units and contrasted having an arbitration statute with not having one. Kochan and Wheeler (1975) similarly studied firefighters using data on 121 bargaining units in 1972. Delaney and Feuille (1983) used bargaining unit data on police in 698 cities in 1980. Finally, Delaney (1983) used 1979 Current Population Survey data on individual public school teachers.

63. For example, Kochan, et al. (1978) contrasted arbitration and factfinding for police and firefighters in New York, Lipsky and Drotning contrasted factfinding and legislative determination for teachers in New York and Stern, et al., contrasted arbitration and factfinding in Michigan, Wisconsin, and Pennsylvania.

64. Delaney (1983). Delaney also presents some evidence that an arbitration statute has not reduced the dispersion of outcomes across bargaining units in Iowa.

65. In principle those "availability" studies that used bargaining unit data should be able to get "use" data through retrospective questionnaires. The Delaney (1983) study that used CPS data obviously could not do this.

66. Only two studies have addressed this issue. Olson (1980) used a random effects model to try to handle the endogeneity of availability. Delaney (1983) reports some estimates of the effects of use of arbitration in the context of a model in which use is endogenous. However, he never specifies the variables that are included in his "use" equation so one can not evaluate what he has done.

67. Only recently have attempts been made to include a comparison of fringe benefits in the comparability surveys. For detail, see Hartman (1983).

68. More specifically, the percentage differential is given by $100(e^{\alpha_{n+1}}-1)$.

69. Whenever, authors used the latter approach, the estimates in Table 4 reflect their (or our) estimate of the average differential obtained from the two methods.

70. We exclude the Wachter-Perloff study from our discussion of the level of government/private differentials or of trends in the differential over time since by using only low wage private sector service workers as the reference group, their public/private differentials become noncomparable to those of other studies.

71. S. Smith (1977a), however, presents evidence that as of 1975 female postal workers received higher compensation than comparable females employed in other federal positions. See her Table 6.7.

72. This is not to say, however, that there is no evidence of race of sex discrimination in the government sector. On this, see the next section of this paper.

73. There are certain regions of the country and occupational categories, however, for which this result fails to hold. See S. Smith (1977a) Chapters IV and V.

74. In earlier work, Borjas (1980a) (1980b) uses a similar model to explain why wages should vary across government agencies for individuals with comparable personal characteristics.

75. For a comparison of gross wage differentials that do not control for personal characteristics of workers, see U.S. Comptroller General (1982).

76. These factors include education, age, marital status, family size, race, and sex.

77. See Ronald Ehrenberg (1979a), Chapter 3, for a more detailed discussion of this point. Another interesting turnover study is Borjas (1982a) who focuses on why turnover rates vary across federal agencies.

78. One should, however, not discount the possibility of different job titles and hence compensation for males and females who perform essentially the same work; for example, male prison guards and female prison matrons. This leads to the question of "comparable worth" which we will not discuss here.

79. As noted in the previous section, one could alternatively estimate gender (race) differentials by calculating male (white) earnings from the female (nonwhite) equations and contrasting these estimates with the actual earnings levels.

80. Black females were actually estimated to earn more than their white female colleagues in local government in 1975 (see Table 5), however, this differential was not statistically significant.

81. See, for example, Freeman and Medoff (1982) and Ehrenberg and Schumann (1982), Chapter 7, for private sector studies of union effects on the demand for labor and the tradeoff between wages and an unfavorable job characteristic (mandatory overtime).

Table 1

Estimated Union/Nonunion Earnings Differentials
for Public School Teachers: Selected Studies

Study	Year	Coverage	Outcomes	Union Variable	Estimated Earnings Differential
Baird & Landon (1972)	1966-67	national (D)	starting salary	(1,0) negotiations held	4.9%
Kasper (1970)	1967-68	national (S)	average salary	proportion of teachers or districts with union representation	0
Frey (1975)	1964-65 to 1969-75	New Jersey (D)	1) starting salary	(1,0) formal contract	1) 0 to 1.4%
Lipsky & Drotning (1973)	1967-68	New York (D)	B.S. min., plus various steps in salary schedule	(1,0) formal contract	0 to 3%
Hall & Carroll (1973)	1968-69	Illinois (D)	average salary	(1,0) formal contract	1.8%
Thornton (1971)	1969-70	national (D)	B.A. min. and max. M.A. min. and max.	(1,0) formal negotiations	0 to 5% save for M.A. max. which was >20%
Balfour (1974)	1969-70 1970-71	national (S)	average salary	% of teachers and/or districts covered by agreements	0%
Chambers (1977)	1970-71	California (D)	1) starting salary 2) increments	(1,0) formal contract	1) 5.7 to 12.2% 2) 0%
Moore (1976)	1970-71	Nebraska (D)	average secondary/ average elementary salary	(1,0) formal negotiations	negative
Zueike & Frohreich (1977)	1972-73	Wisconsin (D)	variety of salary variables	index of comprehensiveness of negotiations	negative
Gustman & Segal (1977)	1972-73	national (D)	minimum salary, maximum salary, number of steps between min. and max.	(1,0) formal agreement	0 on min. or max., but reduce number of steps
Holmes (1976)	1974-75	Oklahoma (D)	average salary	(1,0) any union activity	7%
Gallagher (1978)	1976-77	Illinois (D)	variety of measures	(1,0) presence of collective bargaining	1 to 4.5%
Schmenner (1973)	1962-70	9 large cities (D)	B.A. min. salary	% union members (1,0) formal agreement	12 to 14% 6 to 9%
Baugh & Stone (1982)	1) 1974 & 75 2) 1977 & 78	national (C)	annual earnings	(1,0) union member (1,0) union or employee association member	0 to 7% 12 to 22%

where D = school district level data
S = state level data
C = individual data from the CPS

Table 2

Estimated Union/Nonunion Earnings Differentials
Noneducational Employees: Selected Studies

Study	Year	Coverage	Outcomes	Union Variable	Estimated Earnings Differential
Ashenfelter (1971)	1960-66	firefighters	1) average hourly wage 2) annual hours 3) annual salary	(1,0) any union members	1) 2 to 10% 2) -3 to -9% 3) 0
Freund (1974)	1965 to 1971	noneducational employees	% change in average weekly earnings	% union members	0
Fottler (1977)	1966, 1969, 1972	hospital employees	average weekly	% union members	4 to 8%
Ehrenberg & Goldstein (1975)	1967	10 categories non-educational employees	average monthly earnings	% represented by a union	2 to 16%
Schmenner (1973)	1962 to 1970	police and fire-fighters in 9 large cities	minimum salary	% union members (1,0) formal bargaining	15% 0
Ehrenberg (1973c)	1969	firefighters	hourly and annual minimum, maximum and average salary	(1,0) some union members (1,0) formal contract	0 2 to 18% primarily due to lower hours
Shapiro (1978)	1971	noneducational (individual data)	hourly pay	(1,0) union member	0 to 20%
Cain, et al. (1981)	1973-76	hospital employees 1) individual data 2) by hospital 3) by SMSA	hourly pay hourly compensation	1) (1,0) union membership 2) (1,0) organized 3) % organized	on wages - 0 to 10%, on fringes somewhat larger
Ehrenberg (1980)	1973	police firefighters	minimum, maximum annual & hourly salary	(1,0) formal negotiations	3 to 10%
Bartel & Lewin (1981)	1973	police	minimum, maximum average annual salaries and hourly wage	(1,0) written contract	6% 10-21%[a]
Edwards & Edwards (1982a)(1982b)	1974	sanitation	average hourly wage	(1,0) any union members	10-17% 10-11%
Victor (1979)	1975	1) police 2) fire 3) sanitation	average hourly earnings	% organized (1,0) any union members (1,0) written contract	1) 7-12% 2) 9-13% 3) 7-14%
Hall & Vanderporten (1977)	1973	police	minimum, maximum average annual	(1,0) written contract (1,0) formal negotiations	0 < 10%
Becker (1979)	1975	hospital employees	average wage	(1,0) occupation covered by a contract (1,0) occupation is non-union job in hospital with some contracts	7% 8%
Ichniowski (1980)	1976	firefighters	min., max., average hourly & annual wages & contributions to fringes	(1,0) presence of contract	0-3½% (wages) 18% (fringes)
Feldman & Scheffler (1982)	1977	hospital employees	average salary average fringe costs	(1,0) presence of written agreement	salary 8-12% fringes
Ehrenberg, Sherman & Schwarz (1983)	1977	libraries	minimum, maximum annual salaries	(1,0) any collective bargaining agreement	0
Moore & Raisian (1981)	1967 to 1977	noneducational (individual data from CPS)	hourly wages	(1,0) union member	0 to 18% higher in later years

[a]Obtained from two-stage least squares framework.

Table 3

Estimates of Wage Elasticities of Demand for Labor in the State and Local Sector

Category	(1)	(2)	(3)	(4)	(5)
Education	-1.06	-0.08 to -0.57	-0.57 to -0.82	-0.89	
Noneducation	-0.38				
Streets and Highways	-0.09	-0.44 to -0.64			
Public Welfare	-0.32	-0.33 to -1.13			
Hospitals	-0.30	-0.30 to -0.51			
Public Health	-0.12	-0.26 to -0.32			
Police	-0.29	-0.01 to -0.35			
Fire	-0.53	-0.23 to -0.31			
Sanitation and Sewage	-0.23	-0.40 to -0.56			
Natural Resources	-0.39	-0.39 to -0.60			
General Control and Financial Administration	-0.28	-0.09 to -0.34			
All Categories					-0.53

Sources:

(1) Orley Ashenfelter and Ronald Ehrenberg, "The Demand for Labor in the Public Sector" in Labor in the Public and Nonprofit Sectors, ed. Daniel Hamermesh (Princeton, NJ: Princeton University Press, 1975), Table 6.

(2) Ronald G. Ehrenberg, "The Demand for State and Local Government Employees," American Economic Review 63 (June 1973): 366-79.

(3) Robert J. Thornton, "The Elasticity of Demand for Public School Teachers," Industrial Relations 18 (Winter 1979): 86-91.

(4) Hui S. Chang and Yu Hsing, "A Note on the Demand for Faculty in Public Higher Education," Industrial Relations (Spring 1982): 256-60.

(5) Orley Ashenfelter, "Demand and Supply Functions for State and Local Government Employment," in Ashenfelter and Wallace Oates, eds., Essays in Labor Market Analysis (New York: Halstead Press, 1979).

Table 4

Estimates of Public/Private Percentage Earnings Differentials:
by Level of Government, Race, Sex, and Year

Year	Study	Federal									State				Local			
Year/Study		A	M	F	WM	WF	NM	NF	P	WP	A	M	F	WM	A	M	F	WM
1960	S. Smith (1976abc) (1977ab)	21			8	19	15	27										
1970	Quinn[a] (1979a) (1979b)				22					20				12				-3
	S. Smith (1976abc) (1977ab) (1983)				4	8	15	15										
1973	S. Smith (1977a) (1982)		20	38								-3	14			-7	6	
1975	S. Smith (1977a) (1982) (1983)		15	21								-7	6			-7	1	
	Bellante and Long (1981)	20	18	24							2	-3	8		-5	-4	-2	
1978	S. Smith (1981) (1983)		11	21								2	11			-4	-2	
	Wachter & Perloff (1981)[b]	34							33		16				7			

A - all employees in group
M - male
F - female
WM - white male
WF - white female
NM - nonwhite male
NF - nonwhite female
P - postal workers
WP - white male postal workers

[a]Quinn studies focus on white males 45+.

[b]Wachter and Perloff study uses private sector service workers as reference group. As a result, their public/private earnings differential estimates are artificially high.

Table 5

A) Estimates of the Percentage by Which Male Employees' Earnings Exceed Those of Female Employees with Equivalent Personal Characteristics

Sector/Year	1973	1975
Federal	23	34
State	22	19
Local	19	16
Private	36	28

B) Estimates of the Percentage by Which Black Employees' Wages Are Less Than Those of White Employees with Equivalent Personal Characteristics in 1975

Sector/Gender	Male	Female
Federal	13	5
State	5	2
Local	8	-2
Private	11	4

Source: Authors' interpretation of results presented in S. Smith (1977a), Chapter 6. In cases where separate earnings equations were estimated by gender (or race) the male (white) equation was used by us to calculate the earnings differentials.

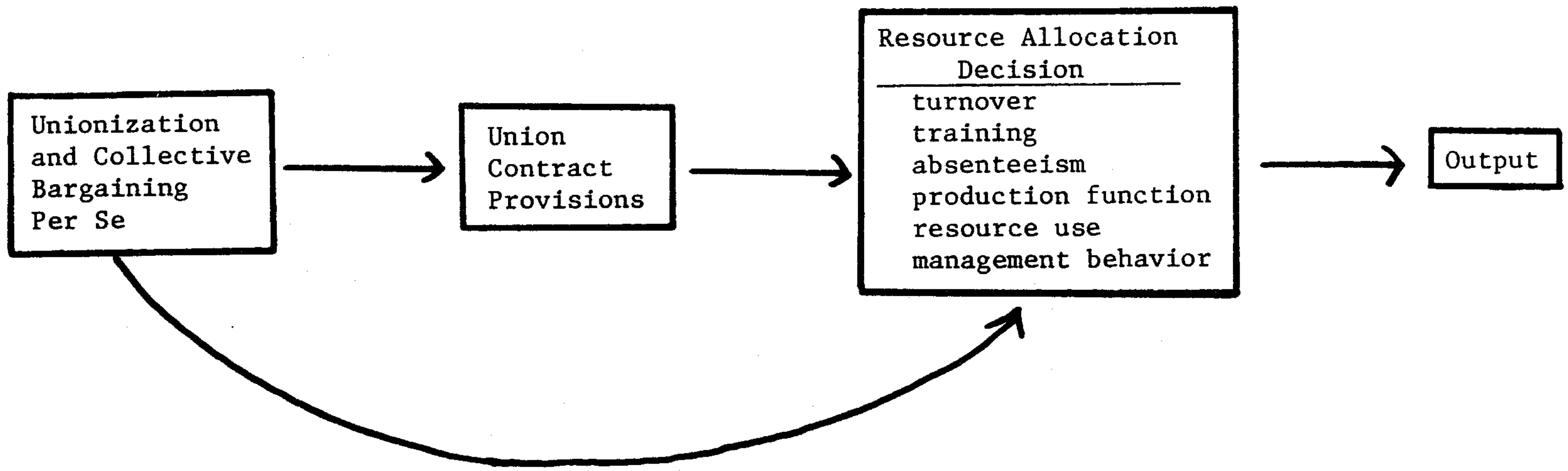

Figure 1

The Routes Via Which Unions Affect
Productivity in the Public Sector