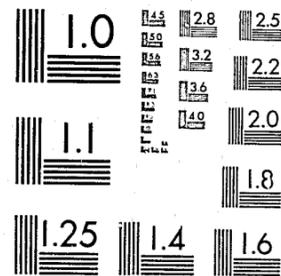


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THE IMPACT OF  
COLLECTIVE BARGAINING AND INTEREST ARBITRATION ON POLICING

by

PETER FEUILLE  
WALLACE HENDRICKS  
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Institute of Labor and Industrial Relations  
University of Illinois  
Champaign, Illinois

Final Report  
Prepared for and submitted to the  
National Institute of Justice  
U.S. Department of Justice  
Washington, D.C.  
in fulfillment of  
NIJ Grant 81-IJ-CX-0074

December 1983

92685

U.S. Department of Justice  
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## ABSTRACT

This report presents the results of a national study of the impact of collective bargaining, the availability of interest arbitration, and the use of arbitration upon police salaries, fringe benefits, and union contract provisions, and upon police department employment, reported crime rates, and clearance rates during the 1971-81 period. The researchers found that both collective bargaining and the availability of interest arbitration are clearly associated with higher salaries, higher fringe benefits, and contracts which are more favorable to the union. However, the results are somewhat less clear about whether bargaining and especially arbitration actually caused these higher salaries, fringes, and more favorable contracts. The results do show that, controlling for the availability of arbitration, the actual use of arbitration does not lead to any long term union (or employer) advantage compared to those unions (or employers) who did not use the procedure. The evidence also indicates that, after controlling for other influences, bargaining is associated with the employment of fewer sworn officers and more civilians in police departments but that arbitration has exactly the opposite effect (more officers and fewer civilians). In addition, bargaining is associated with lower reported crime rates, but this association disappears in arbitration states. Further, both bargaining and arbitration are associated with higher levels of total police department expenditures. However, as with the other findings, the results are less clear about whether bargaining and arbitration actually caused these changes in employment levels, crime rates, and total expenditures.

## ACKNOWLEDGEMENTS

This report represents the combined efforts of many people. The three of us designed the research and did all of the analysis and writing, but our efforts would never have occurred without the financial support of the National Institute of Justice of the U.S. Department of Justice. Our key Project Officer was Shirley Melnicoe, and she was responsible for getting the project underway and keeping it going. As our Washington Godmother, she constantly encouraged and assisted us in our work. All federally-supported researchers should be so lucky. Joe Kochanski was equally helpful during the project's later stages.

We also are grateful to the Institute of Labor and Industrial Relations at the University of Illinois, and in particular to Institute Director Walter Franke, for additional financial support. As with many research projects, this project expanded beyond what we originally proposed to do. This expansion (and concomitant improvement in the final results) would not have been possible without Director Franke's generous willingness to commit Institute resources, especially research assistant positions, to the research effort.

As part of our research we collected more than 2,200 police collective bargaining contracts and arbitration awards. To be usable, the provisions in these contracts and awards had to be carefully evaluated and then equally carefully coded into the computer. This work required great precision and accuracy yet was monotonously repetitive. We are very grateful to Research Assistants Steve Kawakami, Mark Phillips, Joe Schimansky, and Dick Williams for the invaluable work they did collecting, evaluating, and coding all these contracts and awards. Without their

efforts, we would have no results to report.

This project generated a huge amount of clerical work, and we were very fortunate to have Anice Birge, Brendon LeNoir, and Emma Jean Mahoney to perform these tasks. In addition to producing several thousand letters, envelopes, survey forms, and other documents, they expertly prepared the draft and final versions of this report--each with its dozens and dozens of tables and equations. We very much appreciate their timely and expert assistance. Similarly, we also are very grateful to Susan Schwochau for her output generation and retrieval assistance.

We are especially grateful to a large number of people who supplied us with necessary information: Ms. Karen Elwell, who informed us of the constitutional status of state and local arbitration laws; Mr. Ross Hofs of the International City Management Association, who supplied us with a mountain of already computerized police salary, fringe, and employment information; Mr. Paul Zolde of the Federal Bureau of Investigation, who supplied us with ten years' worth of already computerized crime and clearance information; Mr. Casey Ichniowski, of the National Bureau of Economic Research, who shared with us his information on the police bargaining status of many U.S. cities; the hundreds of city and police union respondents who filled out our survey questionnaires and sent us thousands of police contracts and arbitration awards; and the staffs of the state labor relations administrative and other agencies in Connecticut, Iowa, Massachusetts, Michigan, Minnesota, New Jersey, New York, Rhode Island, and Wisconsin who helped us gather additional police contracts and awards. Without the generous assistance of all these people, we would have nothing to report.

Finally, we are indebted to the members of our Advisory Committee - Arvid Anderson, John Anderson, Hervey Juris, Tom Kochan, and Cliff Van Meter - for all their advice, assistance, encouragement, and guidance. They monitored our efforts and gave us insightful and helpful feedback about pitfalls to avoid and improvements to make. In particular, they provided quite detailed and extremely useful comments on the draft version of this report, and they should receive a substantial share of the credit for whatever contributions this project has made toward an improved understanding of collective bargaining and interest arbitration in government.

Peter Feuille  
Wallace Hendricks  
John Thomas Delaney

## CHAPTER I

### INTRODUCTION AND EXECUTIVE SUMMARY

Managers usually hate it, unions usually love it, arbitrators (and arbitration researchers) naturally benefit from it, and the public knows little about it. "It," of course, is the compulsory arbitration of negotiating disputes between public employers and public employee unions. Compulsory arbitration is seldom used in private industry, but it has been widely adopted in the public sector. As a result, the absence or presence of this dispute resolution technique is one of the most substantial differences between private and public sector labor relations. However, research about arbitration has lagged behind practice with it, and thus we have relatively limited systematic knowledge of interest arbitration's impacts. In this report we attempt to (partly) remedy this information gap by analyzing some of arbitration's impacts on the police service. Because our report is rather long, we use this opening chapter to provide an executive summary of our research methods and findings.

### BACKGROUND

Before we summarize, though, we need to describe compulsory arbitration's place in American labor relations. Compulsory interest arbitration seeks to provide "labor peace" between unions and employers by substituting a quasi-judicial examination and resolution of disputed negotiating issues by a neutral third party in place of the strikes (and strike threats) which are frequently used to settle negotiating disputes. Instead of the strike vote and picket line, arbitration's hallmarks are the hearing room and written award.

more states passed compulsory arbitration laws of one kind or another (however, three of these laws -- in Massachusetts, South Dakota, and Utah -- are no longer on the books). Although a few of these laws apply to several public employee groups (Connecticut, Iowa, Nebraska, Wisconsin), most apply either only to firefighters (Hawaii, Montana, Nevada, Wyoming) or to firefighters and police officers (Alaska, Maine, Massachusetts, Michigan, Minnesota, New Jersey, New York, Oregon, Pennsylvania, Rhode Island, Washington). This experimentation is continuing; as this report was being written in 1983, Ohio implemented a compulsory arbitration statute covering police officers and firefighters. These laws have been implemented because the unions of the covered employees have lobbied vigorously and skillfully in their state legislatures and governors' chambers. In addition, most of this pro-arbitration lobbying has been done in the face of considerable anti-arbitration lobbying by municipal management groups.

Public employee unions have pressed for these arbitration laws for two key reasons. First, without the legal right to strike, they see themselves as being on the short end of a bargaining power imbalance when negotiating with management. They perceive that a compulsory arbitration arrangement would eliminate this power imbalance and enable them to sit at the negotiating table in a position of equal strength with management (i.e., they believe they can get more with arbitration available than without it). Second, public employee unions long have recognized that they can mount illegal strikes. Yet, they also have recognized that these strikes, especially in the public safety services, can be risky: although these strikes may generate lots of pressure to

states which have not yet had their arbitration laws judicially tested, but the information in this table represents the constitutional status of state arbitration laws as of June 1983.\*

The information in Table I-1 indicates that a properly drafted state arbitration law will pass constitutional muster with little difficulty. Fourteen of these 18 statutes have been upheld, three have been struck down, and one law is in effect but in a sort of constitutional limbo (Connecticut). We emphasize state laws because local arbitration laws have fared less well when judicially reviewed. Courts in California, Colorado, Kentucky, and Maryland have declared particular local arbitration laws to be unconstitutional, although courts in California, New York, and Texas have allowed other local arbitration arrangements to stand (i.e., in California, charter (or home rule) cities apparently can adopt arbitration but general law cities cannot). It appears, then, that state appellate courts are generally willing to defer to a state legislature's decision to delegate decision-making authority to arbitrators, but they are much less deferential to local decisions to install arbitration.

The Table I-1 listing of a lopsided constitutional batting average in favor of state arbitration laws indicates that the threshold issue of arbitration's compatibility with the American form of government has been rather decisively answered in the affirmative. This conclusion is reinforced by the fact that no state arbitration law has ever been directly repealed (although the Massachusetts statute was indirectly

---

\*We are grateful to Karen Elwell for supplying us with this information.

#### Private Sector

Interest arbitration has been available for the resolution of negotiating disputes ever since the nineteenth century, but it became widely known primarily during World War II. Since then, compulsory interest arbitration has been the source of a considerable divergence of opinion in the American labor relations community and especially among U.S. unions. On the one hand, private sector unionists and managers have been unalterably opposed to compulsory arbitration, primarily because it would involve the government determination of the terms and conditions of employment which unions and employers have been free to decide for themselves (Northrup, 1966; Phelps, 1964). There is considerable private sector support for voluntary interest arbitration (Stieber, 1970), for these voluntary arrangements -- such as the now-expired Experimental Negotiating Agreement in the steel industry -- reflect the mutual decisions of unions and employers to replace a strike threat negotiating system with a quasi-judicial arbitration threat system (though relatively few unions and employers have done so). Further, voluntary arbitration agreements continue only as long as the parties want them to continue. However, this willingness to consider the voluntary use of arbitration has not eroded the longstanding private sector opposition to compulsory arbitration.

#### Public Sector

On the other hand, the public sector has been the scene of considerable experimentation with compulsory interest arbitration. Wyoming passed a firefighters arbitration law in 1965; since then, at least 21

settle the dispute on favorable (to the union) terms, these strikes also may create considerable ill will which public officials can use against the unions. However, an arbitration procedure eliminates the need to mount a strike, and thus arbitration provides a mechanism to obtain satisfactory terms without the uncertainty and downside risk that a strike entails.

Constitutionality. Ever since the first public sector arbitration law was passed, practitioners, policy makers, and scholars have been debating whether such laws are constitutionally permissible within the American form of representative democracy. These debates tend to have a highly normative flavor, as the partisans on both sides of the arbitration fence argue strongly about whether or not compulsory interest arbitration should exist (for one example, see the exchange of views in Horton, 1975; Krislov, 1977; and Horton, 1977). The front line in this debate consists of the courtrooms in state courts around the country where the constitutionality of these arbitration statutes has been litigated.

Table I-1 presents a list of constitutional challenge cases decided in the appellate courts of 18 states (usually by the state's highest court). This table specifies the state, the case, whether or not the arbitration law was found constitutional, and the arguments raised against these laws. Because most legal challenges to arbitration statutes involve some sort of illegal delegation of legislative authority reasoning, we listed numerous specific arguments in addition to "illegal delegation" (see Grodin (1979) for a more detailed discussion of some of these arguments). There are a few lower court decisions which currently are working their way up the appellate ladder, and there are a few

TABLE I-1

CONSTITUTIONAL CHALLENGES TO  
STATE COMPULSORY ARBITRATION LAWS

STATE	CASE	CONSTITUTIONAL?	ARGUMENTS RAISED
ALASKA	Anchorage Educ. Assn. v. Anchorage School District, 648 P. 2d 993 (AK Sup. Ct. 1982)	Yes	6
CONNECTICUT	Town of Berlin v. Santaguida, 435 A. 2d 1980 (CT Sup. Ct. 1980)	Uncer. <sup>a</sup>	1,2,3,4,5
MAINE	City of Biddeford v. Biddeford Teachers Assn., 304 A. 2d 387 (ME Sup. Ct. 1973)	Yes <sup>b</sup>	1
MASSACHUSETTS	School Committee v. Bangor Educ. Assn., 443 A. 2d 383 (ME Sup. Ct. 1981)	Yes	2,3
	Town of Arlington v. Bd. of Concil. and Arb., 352 N.E. 2d 914 (MA Sup. Ct. 1976)	Yes	1,4,6,7
MICHIGAN	Dearborn Firefighters Local 412 v. Dearborn, 231 N.W. 2d 226 (MI Sup. Ct. 1975)	Yes <sup>b</sup>	1,2,3,4,7,8
	City of Detroit v. Detroit Police Officers Assn., 294 N.W. 2d 68 (MI Sup. Ct. 1980)	(E.D.)	
MINNESOTA	City of Richfield v. IAFF Local 1215, 276 N.W. 2d 42 (MN Sup. Ct. 1979)	Yes	2,4,6,7
NEBRASKA	Seward Educ. Assn. v. School District of Seward, 199 N.W. 2d 752 (NE Sup. Ct. 1972)	Yes	1,4
	Orleans Educ. Assn. v. School District of Orleans, 229 N.W. 2d 172 (NE Sup. Ct. 1975)	Yes	1
NEW JERSEY	Division 540 v. Mercer County Imp. Authority, 386 A. 2d 1290 (NJ Sup. Ct. 1978)	Yes	1,2
NEW YORK	City of Amsterdam v. Helsby, 332 N.E. 2d 290 (NY Ct. App. 1975)	Yes	3,6
OREGON	City of Roseburg v. Roseburg Fire Fighters, 639 P. 2d 90 (OR Sup. Ct. 1981)	Yes	1,6,7,8
PENNSYLVANIA	Harney v. Russo, 255 A. 2d 560 (PA Sup. Ct., 1969)	Yes	7
	Washington Arbitration Case, 259 A. 2d 437 (PA Sup. Ct. 1969)	Yes	2,5,6
RHODE ISLAND	City of Warwick v. Warwick Firemen's Assn., 256 A. 2d 206 (RI Sup. Ct. 1969)	Yes	5
	City of East Providence v. IAFF Local 850, 366 A. 2d 1151 (RI Sup. Ct. 1976)	Yes	1,2,4,10
SOUTH DAKOTA	City of Sioux Falls v. Sioux Falls Fire Fighters, 234 N.W. 2d 35 (SD Sup. Ct. 1975)	Yes	2,7
TEXAS	IAFF Local 2390 v. City of Kingsville, 568 S.W. 2d 391 (TX Civ. App. 1978)	No	1
UTAH	Salt Lake City v. IAFF Local 1645, 563 P. 2d 786 (UT Sup. Ct. 1977)	No <sup>c</sup>	2
WASHINGTON	City of Spokane v. Spokane Police Guild, 553 P. 2d 1316 (WA Sup. Ct. 1976)	No	1,2,3,4,9
	City of Everett v. Firefighters Local 350, 555 P. 2d 418 (WA Sup. Ct. 1976)	Yes	1,8
	Yakima County Deputy Sheriff's Assn. v. Board of Commissioners, 601 P. 2d 936 (WA Sup. Ct. 1979)	Yes	6,7
WISCONSIN	Hortonville Educ. Assn. v. Hortonville School Dist., 225 N.W. 2d 658 (WI Sup. Ct. 1975)	Yes	6,11
	Milwaukee County v. Milwaukee Dist. Council 48, 325 N.W. 2d 350 (WI App. 1982)	Yes	6
WYOMING	State v. City of Laramie, 437 P. 2d 295 (WY Sup. Ct. 1968)	Yes	1,3,4,5,6,11
		Yes	1,7,9,10,11

<sup>a</sup>A lower court decision overturning Connecticut's law was dismissed by the state supreme court on non-constitutional grounds.

<sup>b</sup>E.D. means equally divided, which has the effect of upholding the lower court decision.

<sup>c</sup>This decision struck down that portion of the Texas police and fire local option bargaining law which provided arbitration, but the same court later said that cities could enact their own arbitration systems (Jones v. IAFF Local 936, 601 S.W. 2d 454 (TX Civ. App. 1978)).

## ARGUMENTS RAISED:

- |  |  |
|--|--|
| 1 = Illegal delegation of legislative authority              | 7 = Interferes with home rule                                  |
| 2 = No standards or criteria for arbitrators' decisions      | 8 = Interferes with power to tax                               |
| 3 = Lack of procedural safeguards in the arbitration process | 9 = Ripper (i.e., non-delegation) clause in state constitution |
| 4 = Arbitrators have no political responsibility             | 10 = Violates separation of powers                             |
| 5 = Lack of due process guarantees                           | 11 = Other   |
| 6 = Denial of equal protection                               |  |

repealed in November 1980 by being attached to an enormously popular property tax limitation referendum measure). Accordingly, we believe that normative assessments of arbitration are no longer usefully made on the basis of constitutional issues. Instead, as others have noted (A. Anderson, 1981), they are more appropriately made on the basis of how well or poorly arbitration works in practice. Expressed another way, conclusions about the costs and benefits of arbitration are more usefully based on how the procedure affects public employees, public managers, and the public than on how the form of arbitration continues to comport with ever more refined legal abstractions.

Previous research. The role of the strike in collective bargaining has contributed to the primary research focus upon compulsory arbitration which has occurred to date: what impact has compulsory arbitration had on union and management bargaining incentives? Labor relations observers have theorized that because the costs of using arbitration are so low (compared to the costs of striking) arbitration may have a "chilling effect" on the parties' incentives to negotiate, and over time it may have a "narcotic effect" as the unions and employers adopt it as a habit-forming method of resolving their disagreements. Accordingly, during the past ten or so years labor relations scholars have performed a comparatively large amount of research on arbitration's influence on the public sector negotiating process.

In contrast, there have been relatively fewer investigations of arbitration's influence upon the terms of the employment relationship between the public employers and employees covered by arbitration procedures. Taken together, these studies indicate that the availability

of arbitration enables public employee unions to secure moderately higher wages, but that the actual use of arbitration procedures (measured by the issuance of arbitration awards) produces no net wage advantage (i.e., there is no statistically significant difference between arbitrated and negotiated wage settlements in the same jurisdiction).

These impact studies provide us with useful information about arbitration, but the generalizability of each study's findings is limited. In addition, none of these studies attempted to examine any service delivery influences that arbitration might have. Consequently, we simply do not know how arbitration might have affected a wide range of employment and service delivery conditions across a large sample of cities over a long period of time.

In our research, we have attempted to overcome some of the limitations of these earlier studies by performing an extensive and intensive examination of how arbitration has affected police officers, police unions, and municipal managers. In the next section we describe our research effort.

#### THE RESEARCH EFFORT

##### Research Objectives

In keeping with the interests of the National Institute of Justice (National Institute of Justice, 1981) and the desires of the researchers (Feuille and Hendricks, 1981), our primary objective has been to isolate what impact, if any, compulsory interest arbitration has had on a variety of police employment conditions. These employment conditions,

or outcomes variables, include salaries, fringe benefits, a wide variety of work rules, police employment levels, reported crimes, and crimes cleared by arrests (clearances). In an attempt to fully analyze arbitration's possible impacts, we have collected data for the 1971-81 period, and in particular we have emphasized the 1975-80 period. As a result, we have been able to perform longitudinal as well as cross-sectional analyses.

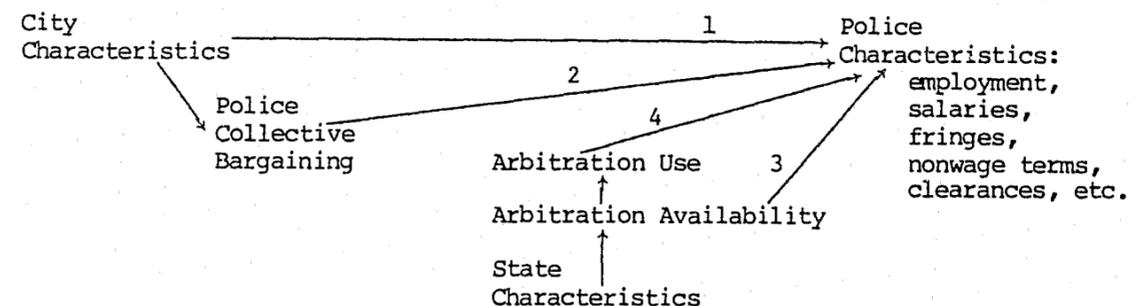
Bargaining vs. arbitration. In our analyses, one key objective has been to differentiate between any impacts that police unionism or collective bargaining has had versus any impacts that arbitration has had. Some previous research on police unionism (usually dealing with the unions' impacts on wages; see Bartel and Lewin, 1981; Victor, 1980) has differentiated only between union and nonunion police departments (usually measured by the presence or absence of a collective bargaining agreement). To the extent that some of the unionized cities exist in states with interest arbitration laws, and to the extent that arbitration has had an impact on such things as police wages, this research may have overestimated the impact that police collective bargaining by itself has had. As a result, we have taken care to differentiate among cities where police are nonunion and do not bargain at all, are unionized and bargain but without access to arbitration, and are unionized and bargain with guaranteed access to arbitration.

Arbitration availability vs. arbitration use. Most of the existing research on arbitration has focussed either on the impact of arbitration's availability (Delaney and Feuille, 1983; Kochan and Wheeler, 1975; Olson, 1980) or on the impact of actually using arbitration (Ashenfelter

and Bloom, 1983; Bloom, 1981; Kochan, et. al., 1979; Stern, et. al., 1975; Somers, 1977). In contrast, very few studies have attempted to simultaneously measure the impact of arbitration's availability and use (Delaney, 1983a, 1983b). This dual measurement is crucial, however, for arbitration's availability may have different effects from the actual use of arbitration (Kochan, et. al., 1979). In fact, there are theoretical and empirical reasons to expect that, within an arbitration state, arbitrated outcomes will not differ from negotiated outcomes (Farber and Katz, 1979; Bloom, 1981). However, the mere existence of an arbitration procedure may have an impact on police employment conditions. As a result, we have taken great care to separately measure the availability and use of arbitration.

Our analyses, then, are designed to isolate and measure the separate impacts that collective bargaining, the availability of arbitration, and the use of arbitration have had upon a variety of police characteristics. These objectives can be seen in Figure I-1:

FIGURE I-1



What are the impacts of 2, 3, and 4 in the presence of 1?

Multivariate analyses. We know from previous research that there are many factors, or variables, which affect police employment conditions (e.g., city size, location, wealth, etc.), and we also know that many of

these variables have nothing directly to do with collective bargaining or interest arbitration. As a result, in our analyses we must control for as many of these other influences as possible in order to isolate whatever impacts may be attributable to collective bargaining, or to arbitration's availability, or to arbitration's use. In turn, this need for multivariate analyses means that we have relied heavily on multiple regression statistical techniques. However, no one needs to be a statistician to understand the results presented in this report.

What we have not done. The summary in this chapter provides an accurate portrait of the subjects we have covered in our research; here we make explicitly clear what topics we did not include in our investigations.

(1) Arbitration's impact on the process of collective bargaining.

Other researchers have performed many studies of arbitration's process impacts (for two reviews, see J. Anderson, 1981a; and Feuille, 1979); we have not attempted to replicate any of those efforts here.

(2) Arbitration's impact on strikes. We know from previous research that the presence of arbitration substantially reduces strikes (Ichniowski, 1982; Olson, et. al., 1981; Wheeler, 1975). Therefore, we have not attempted to reinvent this particular wheel.

(3) How arbitrators make decisions. We have not attempted to peer into the minds of arbitrators to determine why they made particular awards. However, some of our arbitration use findings may shed some light on how arbitrators respond to various bargaining issues and environmental forces when making particular awards.

- (4) How arbitration laws were passed. The legislative histories of arbitration statutes have been inadequately researched (for one exception, see Kochan, 1978), and we have not attempted to correct this situation. However, our findings do help explain why police unions seek such laws and why managements resist them.
- (5) Handling an arbitration case. Union advocates, management advocates, state arbitration administrators, and arbitrators interested in the nuts-and-bolts details of processing arbitration cases must look elsewhere for guidance. We have not attempted to collect such information because (a) most of it is highly state-specific and hence of limited value elsewhere and (b) the existing collective wisdom of arbitration practitioners far exceeds what we could say in this report.

#### Data Collection

We collected data on as many as 1,015 cities for varying years during the 1971-1981 period with particular emphasis on the 1975-80 years. These data include city characteristics (such as population, density, per capita income, reported crimes, region, etc.), relevant bargaining and arbitration characteristics (police bargaining law, mandatory scope of bargaining, police arbitration law, etc.), police characteristics (police department expenditures, number of police employees, clearances, etc.), numerous police employment terms (minimum salaries, maximum salaries, fringe benefits, generic contractual provisions such as grievance procedures, police-specific contractual provisions such as weapons rules), and the police collective bargaining contracts

and arbitration awards which are the source of many of these employment terms.

Our key method of collecting data was a mail survey we conducted during January-July 1982 of almost all U.S. cities over 25,000 population. We used this mail survey to ask a respondent in each city (1) to fill out and return a questionnaire (see Appendix I-1 of the full report) which asked if the city bargains with a police union, how long a bargaining relationship has existed, if interest arbitration exists, how many contracts have been negotiated and arbitration awards issued, and if (and when) any police strikes have occurred; (2) to return police collective bargaining agreements covering the 1975-81 period; and (3) to return police interest arbitration awards covering the 1975-81 period. Most of these surveys were completed and returned (along with any contracts and awards) by city management personnel, and a few survey forms, contracts, and awards were returned by police union representatives. However, no management or union organization or individuals exercised any influence or control over the research effort, nor was any of our research performed on behalf of any advocate groups. We received 731 usable responses to our mail survey requests (which consisted of the original mailing plus one follow-up request to nonrespondents). We also collected information about the bargaining status of 284 additional cities from other sources, including Mr. Casey Ichniowski at the National Bureau of Economic Research, various state public employment relations boards (PERBs), and selected state leagues of cities. In addition, in late 1982 we made field visits to the state PERBs in nine arbitration states (Iowa, Minnesota, Wisconsin, Michigan, New York, New Jersey,

Connecticut, Rhode Island, and Massachusetts) to collect additional contracts and awards.

We collected a total of 1,963 contracts and 348 awards via our mail survey and field visits. Table I-2 describes the results of our primary data collection in more detail. Because we exercised extra collection efforts in the nine selected arbitration states, our sample of contracts is not a random representation of all the police contracts negotiated in the U.S. Instead, we have collected a purposive sample of contracts (and awards) so that we may more fully measure the impacts of arbitration.

Table I-3 describes the police bargaining and interest arbitration status of the cities in our sample. The figures in this table show that the number of cities in which the police bargain collectively more than doubled during the 1971-81 period, that the number of cities covered by an arbitration statute almost quadrupled during the same period, and that police bargaining and arbitration increasingly coexist. Table I-3 clearly shows, then, that (1) collective bargaining is quite widespread in the police service, (2) interest arbitration is hardly a transitory phenomenon confined to the periphery of police union-management relations, and (3) police union impacts can be accurately researched only by examining the effects of both collective bargaining and interest arbitration.

A disproportionate amount of the data we collected came from selected states. This data distribution means that the labor relations experiences in some states will be especially influential in the analyses presented in subsequent chapters. Accordingly, in Table I-4 we have provided a state-by-state breakdown of the salary, fringe benefit, and contract

TABLE I-2

## POLICE CONTRACTS AND AWARDS

U.S. cities over 25,000 population with own police force <sup>a</sup>	1,077
Number of cities in our sample in 1981 <sup>b</sup>	1,015
Number of bargaining cities	703
Number of nonbargaining cities	312
Cities whose 1981 police bargaining status is unknown	62
Number of contracts collected <sup>c</sup>	1,963
Number of contract-years included <sup>c</sup>	3,325
Number of cities which supplied all 1975-81 contract data	354
Number of cities which supplied at least one contract	534
Number of cities in arbitration states in 1981	413
Number of cities in nonarbitration states in 1981	664
Number of arbitration awards collected	348
Number of cities which supplied at least one award	205

Source: Mail survey and field visits by the researchers.

<sup>a</sup>To be included, a city needed a population of 25,000 at least once during the 1970-80 period and needed to have its own police force (i.e., cities which contract out for police services were excluded).

<sup>b</sup>This is the total number of cities whose bargaining status we have identified. Because of missing data, the actual number of cities included in particular analyses in subsequent chapters will be fewer than 1,015.

<sup>c</sup>Some of these contracts apply to years before 1975 or after 1981, and some apply to superior officer bargaining units. As a result, the number of contracts analyzed in later chapters will be smaller than reported here.

TABLE I-3  
BARGAINING AND ARBITRATION STATUS OF CITIES

Year	Number of Cities in Sample <sup>a</sup>	Number (Percent) of Cities Which Bargain With Police	Number of Cities in Arbitration States <sup>b</sup>	Bargaining Cities Covered By Arbitration Law		Number (Percent) of Covered Cities That Used Arbitration <sup>d</sup>
				State	Local <sup>c</sup>	
1971	944	332 (35.2%)	106 (82)	65	2	-
1972	949	394 (41.5)	133 (108)	90	4	-
1973	958	447 (46.7)	187 (162)	135	5	-
1974	972	512 (52.7)	291 (243)	218	5	-
1975	988	576 (58.3)	328 (290)	271	9	41 (14.6%)
1976	994	620 (62.4)	347 (314)	293	9	59 (19.5)
1977	1,002	654 (65.3)	412 (376)	363	9	64 (17.2)
1978	1,002	667 (66.6)	412 (376)	367	11	90 (23.8)
1979	1,008	682 (67.7)	412 (382)	376	15	69 (17.6)
1980	1,013	690 (68.1)	413 (386)	380	16	76 (19.2)
1981	1,015	703 (69.3)	413 (388)	382	17	79 (19.8)

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Source: Mail survey and field visits by the researchers.

<sup>a</sup>To be included in this column a city needed to have a population of 25,000 at least once during the 1970-80 period, needed to have its own police force, and needed to have its police bargaining status identified.

<sup>b</sup>The first figure describes the total number of cities in arbitration states, and the figures in parentheses describe the cities whose bargaining status we have identified. Our analyses in subsequent chapters are based on the numbers in parentheses. For example, in 1981 there were 413 cities in arbitration states: 382 bargained with the police, six did not bargain, and the bargaining status of 25 cities was unknown.

<sup>c</sup>The cities in this column are located in nonarbitration states.

<sup>d</sup>These are the reported numbers of police interest arbitration awards issued in each year, as identified through our mail survey and field visits. Because of missing data, the actual yearly totals will be larger.

TABLE I-4

## 1980 CITY DATA BY STATE

	Number of Cities in Each State <sup>a</sup>	Number of Bargaining Cities <sup>b</sup>	Number of Cities Which Supplied:		
			Salary Data	Fringe Data	Police Contracts
Alabama	16	2	10	9	1
Alaska (A)	3	2	2	2	2
Arizona	8	2	8	8	2
Arkansas	10	0	6	7	0
California	142	133	116	114	85
Colorado	18	4	13	13	4
Connecticut (A)	37	37	23	21	15
Delaware	3	3	2	2	2
District of Columbia (A)	1	1	0	0	1
Florida	49	30	39	39	21
Georgia	14	0	8	8	0
Hawaii	1	1	1	1	1
Idaho	5	2	4	4	1
Illinois	72	37	43	41	22
Indiana	25	10	11	9	8
Iowa (A)	19	19	12	12	15
Kansas	11	4	9	8	2
Kentucky	9	2	6	6	2
Louisiana	12	2	8	8	1
Maine	3	3	1	1	2
Maryland	6	2	4	6	2
Massachusetts (A)	65	60	27	18	37
Michigan (A)	55	41	33	29	26
Minnesota (A)	28	28	23	22	19
Mississippi	9	1	4	4	0
Missouri	18	1	11	10	0
Montana	5	5	2	3	4
Nebraska (A)	5	4	5	5	1
Nevada	5	4	4	4	4
New Hampshire	4	2	0	0	1

TABLE I-4 (cont.)

## 1980 CITY DATA BY STATE

	Number of Cities in Each State <sup>a</sup>	Number of Bargaining Cities <sup>b</sup>	Number of Cities Which Supplied:		
			Salary Data	Fringe Data	Police Contracts
New Jersey (A)	65	63	30	26	37
New Mexico	8	2	3	3	1
New York (A)	39	36	19	17	30
North Carolina	19	0	16	16	0
North Dakota	4	0	4	3	0
Ohio	55	31	39	39	19
Oklahoma	15	10	12	12	6
Oregon (A)	7	7	5	5	5
Pennsylvania (A)	34	31	21	19	18
Rhode Island (A)	12	11	6	6	7
South Carolina	10	0	8	8	0
South Dakota	3	2	0	0	1
Tennessee	13	2	6	5	1
Texas	54	8	41	39	5
Utah	6	0	3	3	0
Vermont	1	1	0	0	1
Virginia	21	0	18	17	0
Washington (A)	19	17	12	11	9
West Virginia	7	2	4	4	1
Wisconsin (A)	24	24	19	19	13
Wyoming	3	1	2	2	1
	1,077	690	703	668	436

(A) indicates arbitration state.

<sup>a</sup>These are the number of cities over 25,000 population in each state which have their own police force. The actual number of cities over 25,000 in some states, especially in California and New York, will be larger due to the contracting out of police services.

<sup>b</sup>These are the cities (as just defined) in each state which bargain with the police and for whom we know the year such bargaining started. Because of incomplete information, the actual number of bargaining cities in some states will be larger.

score data we collected for 1980. This distribution indicates that the bargaining and arbitration experiences in about 15 states will be the key determinants of our analytical results. (See Appendix I-2 of the full report for a complete list of our data sources.)

Data manipulation. In general, we processed our huge amount of information via the computer. In particular, we had to transform our hundreds of police contracts and awards from written documents into computer-usable formats. We did this during the July 1982 - May 1983 period by scoring the contracts with our Kochan-type contract scoring index (Kochan and Wheeler, 1975; Kochan and Block, 1977; Kochan, et. al., 1979; see Appendix I-3 of the full report for the scoring index).

In addition, we also developed an arbitration award scoring procedure (see Appendix I-4) which we used in conjunction with the contract scoring index. This procedure allowed us to precisely measure those contract provisions which were ruled upon by arbitrators and how these provisions were changed by the arbitral rulings. In turn, these scores allowed us to determine if the actual use of arbitration yields specific benefits to unions or employers. In addition, these contract and award scores allowed us to assess bargaining and arbitration's impacts on a wide variety of wage and nonwage employment terms. It is to a summary of these impacts that we now turn.

## SUMMARY

## Chapter II: Arbitration Awards

In this chapter we describe and analyze key characteristics of the

arbitration awards we collected.

Arbitrators. Of the 343 awards we obtained which apply to the patrol officers' bargaining unit, 60 percent were issued via a conventional arbitration procedure (which gives the arbitrator considerable discretion to fashion appropriate rulings), 15 percent were issued via a final offer by issue arbitration procedure (the arbitrator's discretion is limited to selecting a union or employer final offer on each separate issue), 11 percent were issued via a final offer by package arbitration procedure (these arbitrators have the least discretion, for they make only one all-or-nothing selection decision of one side's package of final offers on all the disputed issues), and the remaining 14 percent of the awards were issued through a mixture of decision mechanisms (such as conventional arbitration on some issues and final offer by issue arbitration on others). Consequently, the arbitrators who issued most of these awards had considerable discretion to tailor their rulings to fit the circumstances of each case.

These 343 awards were issued by 208 different arbitrators, and 143 individuals issued only a single award. Only two persons issued more than ten awards (the most prolific arbitrator in our sample wrote 15 awards). This dispersion suggests that nobody relies upon interest arbitration work as the mainstay of his or her dispute settlement practice.

Number of issues. The number of issues decided in each of these awards ranged from one to 57, with a median of ten issues and a mean of 13 issues per award. The fact that more than one-fifth of the awards included more than 20 issues strongly suggests that some of the unions

and employers have used arbitration as much more than a dispute settlement procedure of last resort. Instead, some of the parties seem to be using a "let's take these issues to arbitration and see what happens" approach to the process.

We also found that final offer arbitration with package selection, especially as it is practiced in Wisconsin, appears to encourage the parties to settle more issues during negotiations and hence bring fewer issues to the arbitral hearing room than do the other two kinds of arbitration.

Types of issues. The vast majority of the issues placed before arbitrators are economic. Specifically, 78 percent of the issues decided in these awards involved salaries, pay supplements, or fringe benefits, and all of the ten most frequently arbitrated specific issues fell into these three categories (patrol officer maximum salary was the most arbitrated specific issue). Issues with direct law enforcement implications, such as the number of officers in a squad car or the type of ammunition used, were infrequently taken to arbitration. Similarly, issues which affected management's ability to deploy and assign officers, such as shift assignment and transfer provisions, rarely appeared in these awards. However, monetary provisions which put price tags on these practices, such as court appearance pay, special assignment pay, and pay for out of title work, are regularly arbitrated.

Proposals and responses. On nonsalary issues taken to arbitration, unions propose most of the departures from the status quo, but on salary issues both sides propose changes. The arbitrators who issued these awards appeared reluctant to order wholesale revisions in the parties'

contracts. Only 14 percent of their rulings on nonsalary issues resulted in the inclusion of new issues in the contracts, and only 21 percent of these nonsalary rulings resulted in clear and significant contractual improvements for the unions. In short, police interest arbitrators seem to view their role in a conservative rather than innovative manner.

### Chapter III: Salaries

In this chapter we demonstrate that collective bargaining, the availability of arbitration, and the use of arbitration have different impacts on police salaries. In addition, these impacts vary according to the research methods we used to assess them.

Collective bargaining. After controlling for the influences of other factors, in our aggregate analysis we found that police minimum and maximum salaries are 4-10 percent higher in bargaining cities than in nonbargaining cities. However, when an arbitration variable is added to the measurement equation, the magnitude of the bargaining effect declines. It remains positive (in the 3-8 percent range) and statistically significant, but this decline in magnitude means that arbitration supplied some of the bargaining effect. This comparison indicates that any union wage impact study performed upon public sector occupations or jurisdictions covered by arbitration must also measure the influence of arbitration separately from the influence of bargaining; failure to do so means that any impact attributed to collective bargaining by itself might be overstated.

The availability of arbitration. Some of our analyses show that the availability of arbitration has strongly positive effects on salaries,

while other analyses show little or no effect. For example, our aggregate cross section analyses of 600-700 cities (i.e., when all the cities are grouped together and each year is examined in isolation from other years) show that the presence of a state arbitration law covering the police is associated with salaries which are 3-9 percent higher than they would be otherwise, ceteris paribus. Not only do these effects vary year by year, they become stronger during the 1980-81 years (i.e., minimum salaries are six percent higher in arbitration cities during those years, and maximum salaries are nine percent higher). In 1981, maximum annual salaries in arbitration cities were \$1,674 higher because of the availability of arbitration, ceteris paribus.

However, our disaggregated cross section analyses tell a very different story. When we control for the length of time that arbitration statutes have been on the books, our results show some large year by year changes. Then, when we disaggregate our cities and group them on a state by state basis for several arbitration states, we see that three of the states which paid high salaries after arbitration's arrival (Minnesota, New York, New Jersey) also paid high salaries before arbitration came along. The only clear exception to this pattern occurred in Washington, where salaries became noticeably higher after arbitration compared to the "before" years. (Salaries in Michigan and Pennsylvania were consistently higher than in most other states during the 1971-81 years, but because these two states acquired arbitration laws in 1969 and 1968, respectively, we were unable to do any before and after comparisons.) Salaries in Connecticut, Rhode Island, Massachusetts, Wisconsin, and Iowa did not become consistently higher after arbitration

arrived in those states. As a result, these state by state before and after analyses indicate that (1) arbitration's impact may vary substantially from state to state and that (2) arbitration is associated with high salaries in some states but did not cause these high salaries.

Our time series analyses provide still different results. These analyses tracked salaries over the 1971-81 years, and each year's results were grouped with the results of the other years. The time series results produced by ordinary least squares (OLS) and by generalized least squares (GLS) multiple regression analysis showed that, on average during the 1971-81 period, salary levels were somewhat higher in arbitration cities than elsewhere. However, these time series arbitration coefficients were in the 1.4-4 percent range, and that is considerably smaller than the 3-9 percent arbitration coefficients produced in the aggregate cross section analyses.

These disparate and sometimes inconsistent results may be annoying to those who prefer less rather than more ambiguity. However, all of these results appear to be accurate products of the different sample sizes and different calculation techniques used in these various analyses, and there is no precise formula for determining which of these results to accept and which to reject. When all of these findings are considered together, the weight of the evidence indicates that the availability of arbitration does have an independent and positive association with police salaries but that arbitration probably is not the cause of these higher salaries. In addition, arbitration's impacts also appear to vary on a state by state basis.

The use of arbitration. In contrast to the variability of our

arbitration availability results, our arbitration use findings consistently indicate that the actual use of an arbitration procedure does not have any significant impact on salaries. This is true for minimum and maximum salaries, for different samples of cities, and for current use (i.e., the use of arbitration in the year being examined) and prior use (i.e., the use of arbitration in years prior to the year being examined). In other words, our results show that when we control for the influence of bargaining, arbitration availability, and city characteristics, arbitrated salaries are not significantly different in any year from negotiated salaries.

This finding is very plausible, for it says that there is no long term net advantage which accrues to police unions (or to cities) from actually using the arbitration process. If this were not the case (i.e., if arbitrated salaries were significantly higher than negotiated salaries), we would expect to find that every police union in every arbitration state in every year had gone to arbitration. Instead, we actually found that only about one-fifth of the unions in our sample actually used arbitration in any year.

The combination of our arbitration availability and arbitration use results strongly support the hypothesis advanced by Farber and Katz (1979), namely, that the presence of an arbitration statute in a state may significantly alter the entire negotiating environment, but the actual use of arbitration will not lead to higher salaries than those negotiated in the same state. Our arbitration availability results suggest that an arbitration statute may exert upward pressure on all the police salaries in the state, but arbitrated salaries will show no

consistent advantage over negotiated salaries in that state.

However, this conclusion does not mean that there is no connection between arbitration's availability, arbitration's use, and salaries. Obviously, for arbitration to have any impact on anything it must be used from time to time. In that sense, it is very similar to the strike threat and strike use system. Most private sector unions in most negotiations do not go on strike; instead, they use the threat of a strike to obtain their goals. However, some strikes must occur some of the time for the strike threat to have any credence. Similarly, in each police negotiating round in each arbitration state, some police unions need to use the arbitration procedure so that it will retain whatever impact it has on the negotiation environment.

Levelling effects. Our data show that there is much natural dispersion of police salaries: large cities pay more than small ones, wealthy cities pay more than poor ones, and so on. Labor relations observers have predicted that the arbitration process' emphasis on comparability would cause arbitration to become the "visible hand" whereby salaries would become less dispersed or more levelled over time.

We examined our salary data to see if this levelling effect had occurred, and we gave particular attention to the larger arbitration states. We found that arbitration caused very little levelling to occur, at least on a statewide basis (i.e., the possibility remains that arbitration might have caused salaries in a specific cluster of cities within a state to have become more similar). In particular, we found that minimum salaries remained about equally dispersed over time. Maximum salaries have become somewhat less dispersed over time, but our

results suggest that arbitration may not have contributed much to this modest levelling: we found that among 11 arbitration states, maximum salaries became less dispersed in four states, remained equally dispersed in two states, and became more dispersed in five states. It appears, then, that there are some unmeasured state characteristics which have more impact than arbitration on the dispersion of police salaries within states.

Implications. An examination of our arbitration states shows that most of them are located in what has come to be known as the Frost Belt. These are the states that are the net losers in the migration of people, jobs, and concomitant economic growth to the Sun Belt. In turn, this migration suggests a relative erosion of the tax base in Frost Belt states, which implies that cities in those states should be hard-pressed to maintain the same relative salary levels that they experienced in the early 1970s. However, our disaggregated cross section results show little or no diminution of these state-specific salary levels by 1981, and our time series results show that salaries have increased faster in arbitration cities than in others. These results suggest that arbitration's greatest benefit for police officers may be the protection it provides against management attempts to hold down the rate of increase in salaries.

Having concluded that arbitration matters, we also emphasize that "market" factors appear to matter more. For example, a Southern location systematically and relentlessly exerts very strong downward pressure on salaries. Along this same geographical dimension, police in Western states are paid very well simply by virtue of their location, and police

in cities outside of metropolitan areas are paid noticeably less than police in metropolitan areas. Similarly, larger cities, cities with higher crime rates, and city manager cities pay more than smaller cities, low crime rate cities, and cities headed by mayors. Rather unsurprisingly, wealthier cities and cities which have high manufacturing wages pay more than poorer cities and those with low manufacturing wages. In other words, collective bargaining and interest arbitration appear to have independent and positive effects on police salaries, but there are a host of other factors which also influence these salaries, and many of these other factors may be even more important than police labor relations arrangements.

Research methods. Finally, the analyses in this chapter have shown that it is inappropriate to test arbitration's impacts by simply comparing arbitrated and negotiated outcomes in the same state. Consequently, we hope that this report, even if it does nothing else, will eliminate statements of the following type: "During the past year in this arbitration state, arbitrated wages increased an average of 6.7 percent while negotiated wages increased 6.8 percent; therefore, arbitration had no effect on wages during this past year."

#### Chapter IV: Fringe Benefits and Total Compensation

Using the same analytical model that we used to analyze salaries, we also analyzed the impacts of collective bargaining and interest arbitration on fringe benefits paid to sworn police officers and on total compensation paid to police department employees. Fringes are defined as city contributions to retirement and insurance plans, while total compensation

includes salaries, fringes, and any other monetary payments made to employees.

Our results indicate that police collective bargaining has much larger and stronger associations with fringes than with salary or total compensation, which suggests that police unions may be systematically influencing cities to contribute larger amounts of money to fringes than these cities would contribute otherwise. Our results indicate that fringe benefit expenditures are one-fifth to one-third (20-33 percent) higher in bargaining cities than in nonbargaining cities, ceteris paribus. Because 80-90 percent of our fringe benefit measure is composed of retirement contributions (with the balance consisting of various insurance contributions), our results strongly suggest that unionized police officers prefer that a larger share of their total compensation package be devoted to retirement and insurance benefits than would occur in the absence of police unions. This finding is very consistent with the results of studies which have investigated the impacts of private sector unions (Freeman, 1981) and firefighter unions (Ichniowski, 1980) on the wage and fringe components of the total compensation package. In turn, our results are quite consistent with the "median voter" explanation of union behavior, namely, that police unions emphasize the compensation preferences of the more senior (or median) police officers more than would occur in a nonunion situation with its individual bargaining.

Second, our analyses show that collective bargaining increases the total cost of employing a police officer. During the 1971-80 years, bargaining cities paid 8-12 percent more in total compensation per police department employee than nonbargaining cities paid, ceteris paribus.

This difference disappeared in our 1981 data, which may indicate that (a) unionized cities no longer pay more due to unionism by itself, or (b) such a large proportion of all police departments have become unionized that spillover effects have made accurate union-nonunion comparisons rather difficult, or (c) that the small sample size of total compensation information for 1981 somehow affected the results. Considering that the bargaining coefficients in the 1981 maximum salary analyses in Chapter III also were not significant, item (a) seems to be the most likely explanation.

Third, our analyses of arbitration's availability contain results which will please both arbitration proponents and opponents. Our aggregated data show that the availability of arbitration had a substantial positive impact on fringe benefits (i.e., 20-30 percent) during the 1974-79 years but had little or no effect on fringes either before or after those years, and that the availability of arbitration had an 8-12 percent positive impact on total compensation during the 1974-81 period. However, our disaggregated fringe-by-fringe and state-by-state analyses showed that arbitration seemed to be unequivocally associated with higher fringe benefits only in New York and Wisconsin cities and possibly in Michigan cities, and that arbitration seemed to be unequivocally associated with larger total compensation packages only in New York cities and possibly in Michigan cities. In other words, our fringe-by-fringe and state-by-state results strongly suggest that in most states arbitration has had little effect on the general levels of fringe benefits and total compensation or on specific fringe benefits, and this conclusion is very similar to the conclusion suggested by the state-by-

state analyses of salaries in Chapter III.

Fourth, our analyses of the use of arbitration (defined as using the arbitration procedure to receive an award on any issue) indicate that neither the current use nor the prior use of arbitration has had systematic and consistent association with the level of total fringe benefits, specific fringes, or the level of total compensation. These nonimpacts are very similar to the nonimpact of the use of arbitration on salaries discovered in Chapter III.

Fifth, our contract analyses showed that there was a much greater prevalence of fringe benefits and pay supplement provisions in police contracts in arbitration states compared to nonarbitration states, especially during the 1975-79 years. This result is consistent with the results of the aggregated dollar analyses of fringe benefits and total compensation, and it also is consistent with the results of our complete contract index analyses presented in Chapter V. However, comparisons between our dollar analyses and contract analyses should be made carefully, for our contract index may not yield much useful information about the actual dollar cost of various retirement and insurance contract provisions.

Sixth, our focus in this chapter on bargaining, arbitration availability, and arbitration use should not obscure the fact that, as with salaries, police fringes and total compensation are influenced by a wide variety of "market" variables. Our results show, for example, that fringe benefit and total compensation levels are significantly higher, during most years of the 1971-81 period, in larger cities, in wealthier cities, in cities with higher reported crime rates, in cities with a city

manager form of government, and in cities located in North Central states. In particular, our analyses showed that there are some strong regional effects on total compensation and especially on fringe benefits: North Central cities tended to pay the highest fringes and total compensation, and Southern cities generally paid the lowest. Combined with our salary results from Chapter III, our findings suggest that small nonunion police departments in the South pay the lowest salaries, fringes, and total compensation in the nation.

Finally, we saw that police salaries and fringes are positively correlated all across the country. However, this correlation is much stronger among cities which do not have access to arbitration than it is among those who do.

#### Chapter V: Contracts

In this chapter we present the results of our police contract analyses. We developed a contract scoring index containing 130 provisions which might be found in police contracts. The options which each provision could take (such as open shop, maintenance of membership, agency shop, and union shop on the compulsory membership provision) were ranked on a favorableness to the union scale. We then scored our collected contracts with our contract index. Contracts which received higher scores were judged to be more favorable to the union than contracts which received lower scores. Not only did we calculate a score for the overall contract, we also calculated separate scores for each of several subindices included in our total index. These indices include fringe benefits, pay supplements (these two subindices were analyzed in Chapter

IV), working conditions, individual security, union security, equity, and intrusion into managerial prerogatives. As this list implies, these subindex scores indicate how favorable or unfavorable (to the union) are the particular types of provisions included in police contracts.

In Chapters III and IV arbitration's impact upon salaries and fringe benefits depended, at least in part, on the research methods used to make the assessments. No such ambiguity exists in this chapter, however, for arbitration is associated with higher contract scores no matter what research methods are used.

For instance, our descriptive statistics show that total contract scores in arbitration states regularly average more than 100 points higher than contract scores in other states. Our cross section analyses show that, after controlling for the influences of other factors, total contract scores are 18-40 percent higher in arbitration states than elsewhere among our larger sample, and our time series analysis shows that the scores are about 80 percent higher in arbitration states among our smaller sample of cities. Similarly, grievance procedures in police contracts are much more fully developed in arbitration states than elsewhere, and, in particular, grievance arbitration almost always exists in police contracts where interest arbitration exists but is much spottier elsewhere. Further, most of our subindex scores are larger in arbitration states than in other states, and our intrusion into managerial prerogatives subindex is much stronger in arbitration states than elsewhere. As a result, police unions in arbitration states are in a much stronger position to challenge or appeal managerial decisions than police unions in other states.

An especially interesting finding is that police unions in cities where arbitration is available do not need to trade off high salaries to get good contracts (or vice versa) as police unions in other cities sometimes appear to do. In other words, police unions in arbitration cities apparently are able to obtain both favorable contracts and adequate salaries to a much greater extent than police unions in other cities.

Our arbitration use analyses confirm the results obtained in the two preceding chapters: the actual use of arbitration in any particular year does not produce better (to the union) contracts than are produced via the negotiation process. In fact, some of our arbitration use results suggest that over time the actual users of arbitration tend to be those unions with less favorable contracts who apparently are using arbitration to catch up to the provisions obtained by their peers in other cities.

After having emphasized how much more favorable to the unions these contracts are in arbitration states, it is important to note that the favorableness of police contracts is increasing at a faster rate in nonarbitration states than where arbitration is available. If the 1976-81 trends we identified in our time series analysis continue into the future, our results imply that eventually police contracts in all states will be equally favorable to the unions. However, given the huge absolute advantage that presently favors contracts in arbitration states, "eventually" is many years away.

In sum, police contracts in cities where arbitration is available are much more favorable to the unions than where arbitration is absent.

Due to the limited size of our sample of contracts, we have been unable to do the kind of "before" and "after" analyses we performed on salaries and fringe benefits. As a result, it is possible that the favorable contracts in arbitration states might have occurred in any case (i.e., even if arbitration had never come along). We believe this occurrence is unlikely, however, for the two most likely explanations for such a result -- the increasing age of the bargaining relationship and the increasing favorableness of bargaining legislation -- were much less useful in explaining why high contract scores existed than was the availability of arbitration.

Finally, we have emphasized that the more fully developed contracts in arbitration cities have enabled police unions in those cities to mount stronger challenges to police management than unions in other cities ostensibly can do. We also emphasize, though, that we have neither collected nor presented any data about the day-to-day interactions between police unions and police managers. Consequently, the analyses and results in this chapter cannot be used as evidence that police unions in arbitration states actually have challenged, obstructed, or interfered with police management's ability to manage any more than have police unions in other cities. However, we have assessed the impacts of unionization and arbitration on police employment (of both sworn officers and civilians), total police costs, crime rates, and clearance rates. We review these assessments next.

#### Chapter VI: Productivity

Given all the problems associated with measuring police productivity

and the (sometimes) conflicting results which we have obtained, we should emphasize that our conclusions in this chapter are very tentative. However, we did discover some consistent patterns which in turn suggest some tentative conclusions.

We found that both bargaining and arbitration are typically associated with increased costs in operating a police department. This result is consistent with our earlier findings of their impacts on salaries and fringe benefits. We also found that, after controlling for other influences, bargaining departments employ fewer sworn police officers and more civilian employees than nonunion departments, but that bargaining departments in arbitration states do exactly the opposite: they employ more officers and fewer civilians. Although these two effects partially offset each other in bargaining cities in arbitration states, the net effect shows that arbitration cities employ more police employees than nonarbitration cities, ceteris paribus. If we assume that the total amount of work to be performed in a department either stays the same or increases over time (i.e., does not decline), our results imply that police bargaining is associated with more productive officers and less productive civilian employees but that arbitration is associated with less productive officers and more productive civilians.

In addition, we found that, after controlling for other influences, crime rates for rape, robbery, assault, burglary, and auto theft frequently are lower in bargaining cities than in nonbargaining cities, but that the presence of arbitration seems to negate this bargaining influence. Further, we found that bargaining and arbitration exerted no consistent influence on clearance rates (i.e., crimes cleared by arrests). Conse-

quently, when we measure police productivity with crime rates we find that bargaining is associated with increased productivity (i.e., lower crime rates) but that arbitration is associated with decreased productivity (i.e., higher crime rates), and when we measure police productivity with clearance rates we find that neither bargaining nor arbitration has any consistent effect.

These results suggest that management reacts to the increasing costs brought on by bargaining by substituting less expensive civilians (and possibly capital goods such as cars and other equipment) for more expensive sworn officers. This substitution leads to increased productivity for sworn officers, either because there are fewer of them to perform the same amount of work or because they are assigned in a more effective manner (i.e., to street jobs rather than desk jobs). However, for some reason managements in cities in arbitration states do not make these substitutions. We cannot be certain if arbitration prevents these substitutions or if arbitration is an unwitting proxy for some unmeasured state or city characteristics. If arbitration in fact does have this direct impact, it may occur because arbitration enables police unions in arbitration states to resist trading off higher salaries and benefits for more efficient staffing practices as police unions in nonarbitration states may have done.

Similarly, we are unsure why our measurements of bargaining and arbitration's associations with crime rates differ from each other and why these associations vary so much from year to year. Bargaining by itself is associated with substantially fewer rapes, robberies, assaults, burglaries, and auto thefts, but these decreased crime rates

are not observed when bargaining is combined with arbitration. On the one hand, bargaining may contribute to a more effective police response against crime (or at least against certain types of crimes), while arbitration may cancel this effect. On the other hand, bargaining by itself somehow may be associated with lower crime rates for reasons beyond those examined in this research, while arbitration similarly may not have such an association.

Finally, it is important to note that our research focus on "productivity" has been a very limited one. We have not examined actual work practices (deployment, patrolling, response times, arrest and arrest processing methods, report writing, breaks, and so on), and we have not examined any data representing the non-law enforcement work (traffic control, order maintenance, social services, etc.) which constitutes the heavy majority of the average police officer's work time. When this narrow focus is combined with the data interpretation warnings offered earlier, the conclusions we have reached in this chapter are tentative indeed.

#### Chapter VII: Discussion, Conclusions, and Recommendations

Constitutional form vs. practical application. The judicial survey presented earlier indicates that state supreme courts have rather decisively answered in the affirmative the threshold issue of whether or not state arbitration laws are compatible with the American form of government. Accordingly, we believe that normative assessments of arbitration should be made on the basis of how arbitration works in practice. In other words, conclusions about the costs and benefits of

arbitration are more usefully based on how the procedure affects public employees, public managers, and the public than on how the form of arbitration fits with legal abstractions.

Costs and benefits. Using earlier research and the results of this research, we can identify three sets of tangible (i.e., measurable) benefits which arbitration has had and two sets of costs it has imposed.

Examining benefits first, the available evidence indicates that arbitration has reduced the number of police strikes which otherwise would occur. Second, it has increased the practice of collective bargaining by giving police officers a very strong incentive to bargain (i.e., only police who bargain collectively are eligible to use the arbitration process). Our data show that among the cities whose bargaining status we have identified, almost all the police in arbitration states are unionized but only about half of the cities in other states have police unions. Third, our results show that arbitration has done a good job of guarding the employment interests of police officers. Police salaries, fringe benefits, and contract provisions are positively associated with the presence of an arbitration statute (although arbitration may not have caused these favorable outcomes). These positive associations indicate that police officers have a stronger voice in police department affairs where arbitration exists than where it does not.

In contrast, arbitration imposes two sets of tangible costs. As shown in previous research, the first cost is its tendency in some jurisdictions to weaken the incentives to negotiate (though, in general, collective bargaining remains a very viable process in the presence of arbitration). The second -- which is the focus of our study -- is its

apparent impact on the costs of delivering police services. Arbitration is positively associated with higher salaries, higher fringe benefits, increased police employment, and increased police department total expenses. As a result, arbitration is clearly associated with increases in the monetary costs of delivering a given bundle of police services to the community. It is the policymakers' task to decide if arbitration's benefits outweigh its costs.

In addition, police contracts in arbitration states contain much more favorable language (to the unions) than do contracts in other states. We collected no police operations data, though, so we cannot translate these contract provisions into a precise impact upon management's ability to manage the police department on a day-to-day basis. However, we did find that bargaining in nonarbitration states is associated with lower reported rates of rape, robbery, assault, burglary, and auto theft, but that this impact is not evident in arbitration states.

Do these significant associations indicate that bargaining somehow causes police managers to deliver police services in a more effective manner but that arbitration somehow inhibits this managerial response to bargaining? Alternatively, do bargaining and arbitration somehow serve as unwitting proxies for some unmeasured city characteristics which actually influence crimes? We speculate in this chapter about how bargaining and arbitration could have opposing impacts on the reported crime rates via their opposing impacts on the effectiveness of police service delivery, and these speculations are based upon the changes in the sworn officer/civilian employment mix identified in Chapter VI and the favorable contract language identified in Chapter V. However, we

have been unable to directly examine the actual mechanisms or processes connecting bargaining, arbitration, police management decisions, police work practices, and the reporting of crimes. Accordingly, at this point it is much safer to conclude that bargaining and arbitration are somehow associated with unmeasured city characteristics which exert the actual influences upon crime rates.

Recommendations for future research. Methodologically, we believe that our research has demonstrated (a) the value of performing longitudinal analyses of union impacts and (b) the value of performing aggregated and disaggregated analyses across and within different jurisdictions. As a result, we hope that future researchers will also be able to analyze other public sector union impacts in a similar manner.

Substantively, our research indicates the need to examine arbitration's impacts within particular states on a more complete and intensive basis than we have been able to do. These kinds of analyses are necessary to conclusively determine if arbitration is a monolithic process which has the same or similar impacts everywhere or is a variable process which has different impacts in different jurisdictions.

Our research also offers a very intriguing set of topics for future investigation: the actual connections among bargaining, arbitration, contract language, the sworn officer/civilian employment mix, the deployment and assignment of police employees, the street-level delivery of police services, crime rates, and clearance rates. Our results suggest that some connections may exist among these things, but our data are insufficient to precisely specify why these connections exist. We hope that future research will be more illuminating.

## CHAPTER II

### ARBITRATION AWARDS

In our opening chapter we described how we collected our data and we offered a summary of our main findings, and in the following chapters we present the details of our analyses and results. In this chapter we describe various characteristics of the interest arbitration process and awards, and we place particular emphasis on the issues taken to arbitration.

#### THE AWARDS SAMPLE

We collected 343 usable arbitration awards applying to the patrol officers' bargaining unit. These awards covered 525 award-years (i.e., an award with a duration of two years covers two award-years), so the "typical" award in our sample covered one and a half years. These 343 awards were in effect at various points in time during the 1970-83 period, with the vast majority (90 percent) effective during the 1975-82 years. We collected these awards from cities in 16 different states, though as we shall see, ten of these states accounted for almost all the awards. We were unable to calculate precisely how many police awards were issued in these states during the 1975-82 years, so we have no way of knowing what percentage of the total we have obtained. However, we believe this multi-state sample of awards is the largest such sample ever collected, in part because more police bargaining units are covered by arbitration laws than the bargaining units of any other occupational group. If we assume that these 343 awards are reasonably representative of all awards, our findings allow us to offer some useful conclusions about various characteristics of the public sector interest arbitration process.

## THE ARBITRATION PROCEDURES

Type of decision mechanism. One widely used criterion for differentiating among arbitration procedures is the amount of discretion arbitrators are allowed when fashioning their rulings. Conventional arbitration allows arbitrators the widest discretion, for such a procedure places no official limits on how an arbitrator rules, and the practical limits are established only by the parties' positions at arbitration. Final offer arbitration with package selection gives arbitrators the least discretion, for such a procedure requires that the arbitrator select - as a single package - either the employer's or the union's offer, without alteration, on all the disputed issues (i.e., the arbitrator will make only one selection decision to settle the dispute no matter how many issues are on the agenda). In between is final offer arbitration with issue selection. Such a procedure requires the arbitrator to select either the union's or employer's final offer, but these selection decisions are to be made separately on each disputed issue. In addition, a few states have hybrid or mixed procedures: Michigan, for example, requires final offer by issue arbitration on economic issues and conventional arbitration on noneconomic issues, while New Jersey specifies final offer by package arbitration on economic issues and final offer by issue arbitration on noneconomic issues - except that the parties may agree to use conventional arbitration. Further, Iowa specifies final offer by issue arbitration and also specifies that arbitrators may select the fact-finder's recommendation on each issue rather than the employer's or union's final offer.

Table II-1 shows that most of the awards in our sample were issued under conventional arbitration procedures: 60 percent of the awards

TABLE II-1  
TYPES OF ARBITRATION PROCEDURES

Type of Decision Mechanism:	<u>Conventional</u>	<u>Final Offer By Issue</u>	<u>Final Offer By Package</u>
Used on all issues in award	207	52	37
Used on some issues in award	15	45	27
(These numbers will not total 343 because of the double counting involved in hybrid awards.)			
Type of Panel:			
Single Arbitrator	136		
Tripartite Panel	207		

involved conventional arbitration, 15 percent were final offer by issue awards, and 11 percent were final offer by package awards. The remaining 14 percent involved a mixture of decision mechanisms. These data indicate that interest arbitrators in police disputes have considerable discretion to shape their awards to fit the circumstances of each case. Expressed another way, relatively few police unions and municipal employers negotiate with the truly all-or-nothing sword of final offer by package arbitration hanging over their heads.

Another criterion for differentiating among arbitration procedures is whether the award is issued by a single neutral arbitrator or a tripartite panel (union representative, employer representative, and neutral chairperson). As Table II-1 shows, 60 percent of our awards were issued by tripartite panels. We were unable to tell from examining these awards how many of these panels existed because the law required them and how many of these panels were used (instead of the single arbitrator format) because the parties preferred the tripartite arrangement. In any case, the tripartite format is much more common in interest arbitration than in grievance arbitration.

Number of arbitrators. For years the conventional wisdom in the labor relations community has said that the grievance arbitration process has been dominated by a relatively small number of "mainline" arbitrators who handle most of the cases (Sinicropi, 1982). We have no data to test the accuracy of this assertion regarding grievance disputes, but we did record the names of the arbitrators (either single arbitrators or the chairpersons of the tripartite panels) in our awards sample to see if this same phenomenon was occurring in interest disputes. With a

very few exceptions, we found that it was not.

Our 343 awards were issued by 208 different individuals: 143 persons wrote one award, 32 people wrote two awards, and 15 individuals wrote three awards. In other words, the modal arbitrator in this sample wrote a single award. At the other extreme, one arbitrator issued 15 awards, another issued 13, a third person issued nine, a fourth issued seven, and two arbitrators each issued six awards. Nobody else issued more than five awards. This distribution of arbitration cases across a large number of persons has occurred for many reasons, including the preferences of unions, employers, and state arbitration administrative agencies to use arbitrators who are residents of the same state in which the arbitration case is located. Whatever the reasons, our data show that, with few exceptions, a small number of mainline arbitrators does not appear to be dominating the police interest arbitration process.

#### THE DISPUTED ISSUES

As the data in this section will show, the typical interest arbitration case involves a large number of disputed issues, primarily over money, in which the unions propose most of the changes. However, our data suggest that arbitrators generally are reluctant to write new issues into the parties' contracts.

Number of issues. Unlike grievance arbitration, our data show that interest arbitration cases involve large numbers of disputed issues. As the top part of Table II-2 shows, the number of issues handled in our 343 awards ranged from one to 57, with a median of 10 issues and a mean

of 13 issues per award. We saw above that most of the awards are either conventional or final offer by issue awards, and thus the arbitrators in these cases were required to make numerous rulings. (They did so in awards which ranged in length from one to 159 pages, with a median length of 13 pages and a mean length of 18 pages.) The large number of issues per award, and in particular the fact that more than one-fifth of the awards involved more than 20 issues, strongly suggest that some of the unions and employers in this sample have used arbitration as much more than a dispute settlement procedure of last resort. Instead, it appears that some of the parties are practicing a "let's take this to arbitration and see what happens" approach to the process.

Although our study was not designed to examine the effects of arbitration on the collective bargaining process, we were able to conduct a rough test of the impact of the type of arbitral decision mechanism upon the number of issues taken to arbitration. In particular, we were able to examine the conventional wisdom that final offer arbitration, with the greater risks it poses for those parties who fail to agree in negotiations, will induce the parties to settle more issues during negotiations than will conventional arbitration (Feuille, 1975a). If this conventional wisdom is valid, there should be fewer issues per award in final offer arbitration states than in conventional arbitration states, and further, there should be fewer issues per award under final offer arbitration with package selection than under issue selection.

In the bottom half of Table II-2 we have grouped the arbitration awards on a state-by-state basis, and we have calculated the mean or average number of issues per award. These data provide moderate support

TABLE II-2  
NUMBER OF ISSUES PER AWARD

## All Awards:

Number of Issues Per Award	Number (Percent) of Awards	Other Characteristics
1-5 issues	86 (25.1%)	Range: 1-57 issues Median: 9.7 issues Mean: 13.0 issues Mode: 6 issues (26 awards)
6-10	98 (28.5)	
11-15	51 (14.9)	
16-20	31 (9.1)	
21 or more	77 (22.4)	

## State-By-State Distribution:

State and Type of Arbitration Procedure <sup>a</sup>	Mean Number of Issues Per Award	Standard Deviation	Number of Awards <sup>b</sup>
Rhode Island (CON)	21.5	12.8	21
Minnesota (FOAI)	17.2	9.0	44
Connecticut (FOAI)	15.9	8.0	12
Pennsylvania (CON)	15.2	8.7	44
New York (CON)	13.7	13.4	47
Michigan (FOAI+CON)	11.8	7.9	33
Massachusetts (FOAP)	10.9	11.2	19
New Jersey (FOAP+FOAI)	10.5	8.5	66
Iowa (FOAI)	8.9	6.9	20
Washington (CON)	8.2	4.8	6
Oregon (CON)	3.5	3.0	4
Wisconsin (FOAP) <sup>c</sup>	2.5	1.6	19

## Notes:

<sup>a</sup>CON = conventional arbitration, FOAI = final offer arbitration with issue selection, FOAP = final offer arbitration with package selection. These are the types of arbitral decision mechanisms specified in the state statutes, but many statutes allow the parties to mutually agree to use another type of procedure.

<sup>b</sup>The state-by-state distribution excludes four awards, each from Alaska, California, Nebraska, and Montana.

<sup>c</sup>The Wisconsin results exclude four Milwaukee police awards issued under a separate conventional arbitration statute for that city, and these four Milwaukee awards averaged 28 issues per award.

for the idea that final offer arbitration with package selection will encourage the parties to settle more issues in negotiation and thereby bring fewer issues to arbitration than will the other two kinds of arbitration. In particular, Wisconsin appears to be the most visible final offer success story to date, for the average number of issues per award in that state is well below the average in other states. However, these results provide almost no support for the notion that final offer arbitration with issue selection will encourage the parties to hold down the number of issues they place before the arbitrators. In addition, the very wide variation in the state averages, and the rather large standard deviations within each state, suggest that other factors are more important than the type of arbitration procedure as determinants of the number of arbitrated issues.

Categories or types of issues. As mentioned in Chapter I and as will be explained in more detail in Chapter V, we developed a contract scoring index which we used to evaluate or score the contracts and awards we collected. This index contained 130 separate items (exclusive of salaries) which our research showed could exist in police contracts and awards. In addition, we grouped all of these 130 items into one of six subindex categories: working conditions (such as scheduling and equipment issues), individual security (examples include promotion, transfer, and layoff provisions), union security (this includes membership requirements, dues checkoff, time for union business, and so on), equity (all grievance procedure and disciplinary provisions belong here), fringe benefits (all kinds of insurances, leaves, vacation, etc.), and pay supplements (this category includes all pay items, such as overtime pay

and clothing allowances, over and above basic salaries). A complete listing of these items and their subindex groupings can be seen in Appendix I-3. In addition, we recorded (in dollars) as many as eight different annual salaries (patrol officer minimum, patrol officer maximum, corporal, sergeant, lieutenant, captain, dispatcher, and parking enforcers).

In these 343 awards arbitrators decided 4,444 separate issues (covering a total of 6,833 issue-years). We were able to score 3,586 (or 81 percent) of these issues with our scoring index (i.e., the remaining 19 percent of the disputed issues were not included in our index, so we were unable to score them).

Table II-3 indicates the frequency with which various issues occurred in these awards. Using the 3,586 issues evaluated with our scoring index, the first half of the table shows how frequently the various types of issues were placed on the arbitral agenda, and the second half of the table lists the ten most frequently arbitrated specific issues. Taken together, these data indicate that economic issues are much more frequent in our awards than are noneconomic issues. If we consider that fringe benefits, pay supplements, and salaries are economic issues and the rest are noneconomic, Table II-3 shows that more than three-fourths of the arbitral rulings which we were able to score involved economic issues (this conclusion might change somewhat if our scoring index were lengthy enough to evaluate 100 percent of the items in these awards). Further, all of the ten most arbitrated specific issues are economic in nature: three are salary issues, three involve pay supplements, and the other four involve various fringe benefits. In addition, Table II-3

TABLE II-3  
TYPES OF ISSUES IN DISPUTE

Type of Issue	Percent of Awards With At Least One Issue in Category	Number of Arbitrated Issues in Category
Economic:		2,823
Salaries	91.0%	716
Pay Supplements	75.5	978
Fringe Benefits	72.6	1129
Noneconomic:		763
Union Security	46.1	248
Working Conditions	34.4	165
Individual Security	33.2	179
Equity	22.7	171
Ten Most Arbitrated Specific Issues	Number (Percent) of Awards	Number (Percent) of Contracts With This Issue <sup>a</sup>
Patrol Officer Maximum Salary	311 (90.7%)	819 (90.7%)
Patrol Officer Minimum Salary	151 (44.0)	800 (88.6)
Longevity Pay	129 (37.5)	761 (84.3)
Annual Clothing Allowance	127 (37.0)	795 (88.0)
Maximum Amount of Vacation	98 (28.7)	801 (88.7)
Additional Insurance Protection	92 (26.8)	415 (46.0)
Number of Holidays	91 (26.6)	874 (96.8)
Shift Differential Pay	90 (26.2)	365 (40.4)
Sergeant Salary	88 (25.7)	510 (56.5)
Dental Insurance (Employee)	81 (23.6)	326 (36.1)

<sup>a</sup>Calculated from the 903 contracts we collected from arbitration states.

also shows that these ten most arbitrated issues frequently appear in police contracts in arbitration states.

These findings are reinforced by our investigation of the ability to pay question. Municipal officials frequently object to interest arbitration because they claim that arbitrators often give short shrift to municipal fiscal pressures, and most arbitration statutes require arbitrators to use the employer's ability to pay as one of the criteria involved in fashioning their awards. We evaluated each award to determine, from the arbitrator's language, if the employer's ability to pay was in question in each dispute. We found that the arbitrators explicitly indicated that the employer's ability to pay was involved in 198 cases, that arbitrators said nothing about ability to pay in 127 instances, and in 18 awards we were unable to determine from the arbitrator's language whether or not ability to pay was involved. These results show that a clear majority of these 343 arbitration cases involved the employer's ability to pay, which in turn suggests a key reason why many of these disputes went to arbitration. In addition, ability to pay may be even more important than these numbers indicate, for the absence of this criterion from 127 awards may mean only that some of these arbitrators chose not to discuss this criterion even though it may have been introduced at the hearing by the employer and/or the union.

Law enforcement issues. Many of the 130 issues in our scoring index have direct law enforcement connections or implications: court appearance pay and false arrest insurance can affect officers' incentives to make arrests, residency requirements reflect political judgments about the relationships between officers and the citizens with whom they interact

and also reflect operational judgments about the speed with which officers can respond to emergency calls to report for work, and so on.

In Table II-4 we have listed 15 specific law enforcement issues contained in our scoring index. All of these issues are unique (or almost unique) to the police service, and thus they would rarely be found in the contracts of other occupational groups. Table II-4 also contains the number of times that arbitrators ruled on these issues. These data confirm the findings just discussed by showing that law enforcement issues are rarely placed before arbitrators. In addition, the three most frequently appearing issues in Table II-4 are economic issues which can have a direct impact on the material well-being of police officers. In other words, these results show that arbitrators very rarely have a direct impact upon such things as a police department's weapons policy or internal investigations policy.

Personnel allocation issues. However, management's ability to assign or deploy officers to particular shifts, activities, or beats also may have a direct impact on a police department's effectiveness in delivering law enforcement services. We are unable to establish a direct link between arbitration awards and law enforcement effectiveness, but we are able to examine how often arbitrators rule on personnel allocation issues. As the figures in the bottom half of Table 4 indicate, these kinds of issues also are infrequently taken to arbitration. Further, 180 of the 301 arbitral rulings on these 14 issues involved requests for extra pay for particular assignments rather than direct restrictions on assignment practices. In other words, our results indicate that, just as management has almost no reason to fear that

TABLE II-4  
LAW ENFORCEMENT ISSUES AT ARBITRATION

Issue	Number (Percent) of Awards Containing This Issue	Number (Percent) of Contracts Containing This Issue <sup>a</sup>
Court Appearance Pay	42 (12.2%)	696 (77.1%)
False Arrest Insurance	33 ( 9.7)	370 (41.0)
Court Appearance Pay Rate	27 ( 7.8)	728 (80.6)
Special Equipment	24 ( 7.0)	279 (30.9)
Residency Requirement	20 ( 5.8)	170 (18.8)
Officer Bill of Rights	15 ( 4.4)	353 (39.1)
Officer Rights in Citizen Complaints	9 ( 2.6)	396 (43.9)
Off Duty Armament	9 ( 2.6)	51 ( 5.6)
Firearm Qualification	4 ( 1.2)	125 (13.8)
Armament in Squad Cars	2 ( 0.6)	27 ( 3.0)
Type of Ammunition Used	2 ( 0.6)	84 ( 9.3)
Type of Sidearm Carried	1 ( 0.3)	102 (11.3)
Firearm Review Board	0	7 ( 0.8)
Use of Arms/Deadly Force	0	9 ( 1.0)
Use of Less than Deadly Force	0	9 ( 1.0)

PERSONNEL ALLOCATION ISSUES AT ARBITRATION

Issue	Number (Percent) of Awards Containing This Issue	Number (Percent) of Contracts Containing This Issue <sup>a</sup>
Special Assignment Pay	50 (14.6%)	380 (42.1%)
Pay For Out of Title Work	44 (12.8)	480 (53.2)
Vacancies/Promotion Procedure	30 ( 8.7)	467 (51.7)
Overtime Pay	29 ( 8.5)	851 (94.2)
Staffing Provision	24 ( 7.0)	150 (16.6)
Standby Pay	22 ( 6.4)	222 (24.6)
Call-in Pay	21 ( 6.1)	740 (81.9)
Shift Assignment	18 ( 5.2)	472 (52.3)
Voluntary Transfer	15 ( 4.4)	319 (35.3)
Choice of Overtime Pay or Time Off	14 ( 4.1)	171 (18.9)
Involuntary Transfer	13 ( 3.8)	267 (29.6)
Shift Exchange	11 ( 3.2)	278 (30.8)
Subcontracting	7 ( 2.1)	327 (36.2)
Job Posting and Bidding	3 ( 0.9)	224 (24.8)

<sup>a</sup>Calculated from the 903 contracts we collected from arbitration states.

arbitrators will attempt to write law enforcement policies for police departments, so management has little reason to worry that arbitrators will tell them how to deploy their officers (though arbitrators may tell management how much particular deployment practices will cost).

In sum, the information in Tables II-3 and II-4 indicates that it is disputes over money rather than management's ability to manage which are the primary grist for the police interest arbitration mill.

Who proposes changes? Except in rare cases, interest arbitration involves disputes over the negotiation of existing contracts. The arbitral conventional wisdom says that in these disputes it is the unions who propose changes in the contracts and it is the employers who propose to continue the status quo. We examined this conventional wisdom by recording which party proposed a change from the status quo on each arbitrated issue.

Table II-5 indicates that this view is applicable to nonsalary issues but not to salaries. The figures in the table indicate that unions propose most of the changes in the nonsalary portions of the contract but that both parties propose changes in salary issues. These results are not particularly surprising, for managements have little incentive to place nonsalary items in the contract while the unions see such items as protection for their members. In contrast, both sides have a strong incentive to influence the size of salary increases, for the perennial question in this era of the annual pay raise is not "if" but "how much?" Further, we believe these change-proposing tendencies would be even more pronounced if we could accurately classify the issues in the "unable to determine" category. This designation means that we

TABLE II-5  
THE PARTIES PROPOSING CHANGES IN  
ISSUES AT ARBITRATION

<u>Party Proposing Changes</u>	<u>Number (Percent) of Nonsalary Issues</u>	<u>Number (Percent) of Salary Issues</u>
Union Only	1,725 (60.1%)	94 (13.1%)
Management Only	271 (9.4)	1 (0.0)
Both Parties	229 (8.0)	455 (63.5)
Unable to Determine*	646 (22.5)	166 (23.2)
	2,871 issues	716 issues

\*For these 812 issues the information in the awards did not identify who proposed changes.

were unable to determine from the language in the award precisely who proposed changes in the contract.

Arbitrator responses. How do arbitrators respond to these change proposals? In particular, how do arbitrators respond to proposals for improvements in existing contractual items and the insertion of new items into the contract? The conventional wisdom has portrayed interest arbitration as a conservative rather than innovative process, with arbitrators being generally reluctant to rewrite substantial portions of the parties' contracts.

We have an excellent opportunity to test the accuracy of this assessment. We collected several years' worth of police awards and contracts from numerous cities, and by examining these documents plus the language in the awards, we were able to determine when arbitrators placed new nonsalary issues in the parties' contracts (for those issues in our scoring index). In addition, we were able to determine how often arbitrators ordered improvements in existing contractual items. In this context, "improvement" means changing an item so that it is noticeably more favorable to the union than formerly. For example, a change from a 40-hour workweek to a 38-hour workweek qualifies as an "improvement" in the length of workweek item in our scoring index (see Appendix I-3), but rewriting the existing workweek language - even in a manner sought by the union - would not qualify as an improvement as long as the workweek remained at 40 hours. In other words, our scoring index is constructed in such a way that it records only clear and unambiguous changes in existing items and excludes minor rewriting of existing language.

Table II-6 indicates that interest arbitration indeed is a conservative rather than innovative process. Arbitrators do not appear eager to add new items to the contract, as only 400 of their 2,871 rulings on nonsalary issues (or 13.9 percent) resulted in the addition of new issues. (It would be informative to know how many new issue requests arbitrators received, but we were unable to collect this information.) Similarly, arbitrators do not appear eager to award "improvements" in nonsalary items to the unions, at least as we have defined that term. We counted a total of 603 improvements out of the 2,871 rulings (21.0 percent), and that figure includes the new items just discussed. These results suggest that unions should not look to arbitrators for major revisions in the nonsalary portions of their contracts.

This conclusion is subject to several caveats. First, 19 percent of the arbitrated nonsalary issues were not scored on our index, and many of these may have involved new items or improvements in existing items. Second, our stringent definition of "improvements" undoubtedly has omitted numerous changes sought by the unions and which the employers and unions would agree have been to the unions' advantage. Third, the data in this chapter do not permit us to say anything about the availability effect that arbitration may have had (i.e., the union might seek and the employer might agree to new items during negotiations simply because both sides are aware of arbitration's availability). As we shall see in Chapter V, police contracts in arbitration states earn much higher scores on our scoring index than do police contracts in other states, and some part of this difference may be attributable to the availability (rather than use) of arbitration. Finally, the data in Table II-6 say

TABLE II-6  
CHANGES IN NONSALARY ISSUES ORDERED  
BY ARBITRATORS

New Issues Added by Arbitrators:		Improvements (Including New Issues) Ordered by Arbitrators:	
Number of New Issues Per Award	Number (Percent) of Awards	Number of Improvements Per Award	Number (Percent) of Awards
None	179 (52.2%)	None	131 (38.2%)
One	80 (23.3)	One	78 (22.7)
Two	30 ( 8.7)	Two	40 (11.7)
Three	27 ( 7.9)	Three	43 (12.5)
Four	9 ( 2.6)	Four	19 ( 5.5)
Five	5 ( 1.5)	Five	11 ( 3.2)
Six or More	13 ( 3.8)	Six or More	21 ( 4.7)
	343		343

Range:	0-23 new issues	0-23 improvements
Median:	0.5 new issues	1.0 improvements
Mean:	1.2 new issues	1.8 improvements
Mode:	0 new issues	0 improvements
Total:	400 new issues	603 improvements

nothing about how arbitrators have resolved salary issues.

Even with these caveats, however, we believe that our Table II-6 results indicate that interest arbitrators are generally reluctant to order wholesale changes in nonsalary sections of police contracts. Less than one-seventh of their nonsalary rulings inserted new issues into these contracts, and about four-fifths of these rulings resulted in no significant improvements in these nonsalary items. With a few exceptions, then, our data imply that interest arbitrators define their role in a conservative rather than innovative manner.

#### SUMMARY AND CONCLUSIONS

The data in this chapter have told us several things about the "typical" police interest arbitration case and resulting award. As long as observers realize that there are many, many exceptions to the "typical" case, this information provides an improved understanding of how the police interest arbitration process functions.

First, these results show that interest arbitrators usually have considerable discretion to fashion the rulings they deem appropriate. More than four-fifths of the awards occur under conventional or final offer by issue arbitration procedures, and thus the all-or-nothing quality of final offer by package arbitration is present only a small part of the time. In the exercise of this discretion, three-fifths of the awards were produced by neutral arbitrators with the assistance of the advocate arbitrator members of the tripartite panels, and only two-fifths of the awards were written by neutrals working in a single arbitrator capacity.

This heavy reliance upon tripartite panels is one of the key procedural differences between the interest and grievance arbitration processes. Our results also suggest that the exercise of this discretion may not lead to recurring work as an interest arbitrator, for more than two-thirds of the neutral arbitrators in this sample produced only a single award. Of course, these same arbitrators may have rendered other interest arbitration awards which we did not collect, and a few individuals in this sample were used over and over again. However, our data strongly suggest that nobody has full-time work as an interest arbitrator.

Second, the number of issues decided in each award indicates that the typical interest arbitration case is more complex than the typical grievance arbitration case. Half of our awards involved fewer than ten issues, but half involved more, and some awards involved many more. Further, our state-by-state distribution of awards suggests that final offer arbitration has been only moderately successful in reducing the number of issues per arbitration case. Final offer arbitration with package selection is associated with a lower than average number of issues per award, but final offer arbitration with issue selection is not. Our state-by-state results imply that factors other than the type of arbitration procedure are the key determinants of the number of issues taken to arbitration in police negotiating disputes.

Third, the number of issues decided in these awards indicates that some unions and employers are burdening the interest arbitration process with more issues than the process was designed to handle. There is no such thing as an optimal number of issues per arbitration case. However, above all else, compulsory interest arbitration is a process designed to

settle negotiating disputes without strikes. This purpose clearly implies that the issues taken to arbitration should be those which - if not settled - would lead to a union decision to call a strike or an employer decision to take a strike. However, observers properly may be skeptical that 20, 30, 40, or 57 strike issues actually existed in some of the arbitration cases examined here. Expressed another way, our results imply that some police unions and employers view interest arbitration as a very low-risk method for seeking favorable contract terms, and thus the procedure plays host to a larger number of issues than would a high-risk procedure. Further, this conclusion is supported by the Wisconsin results. The average number of issues per award in that state is well below the national average, and final offer by package arbitration as practiced there is a truly high-risk, all-or-nothing procedure (final offers, once certified, cannot be individually modified; the arbitrator first attempts to mediate a settlement; and there are no factfinder recommendations in which to seek refuge). In other words, our results suggest that conventional arbitration and final offer by issue arbitration may be susceptible to a sort of "issue inflation" by some users of these procedures.

Fourth, most of the issues taken to arbitration are economic. Specifically, 78 percent of the issues we evaluated with our scoring index were economic issues involving salaries, pay supplements, or fringe benefits. Further, all of the ten most arbitrated specific issues in these 343 awards were economic issues, and arbitrators specifically indicated in 198 awards that the employer's ability to pay was in question. In addition, law enforcement-type issues are rarely taken to arbitration.

Fifth, the unions proposed most of the changes in the nonsalary provisions taken to arbitration, but both employers and unions proposed changes in the salary items.

Finally, arbitrators appear reluctant to engage in the wholesale rewriting of police contracts. Only 400 of the 2,871 arbitral rulings on nonsalary issues included in our scoring index resulted in the insertion of new issues into the parties' contracts. Further, only 603 of these 2,871 rulings resulted in significant improvements for the police unions who participated in these cases (though our definition of "improvements" is a stringent one which probably far exceeds what unions and employers consider to be contractual improvements). As a result, our data suggest that interest arbitrators conceive of themselves as conservative adjusters of police contracts rather than as innovative molders of police union-management relationships.

It is important to keep in mind that the data in this chapter describe only those police negotiating disputes which ended in an arbitration award. In order to properly examine the impacts that interest arbitration may have had on police employment conditions, though, we must investigate the impact of both the availability and the use of arbitration. It is to these impact investigations that we now proceed.

### CHAPTER III

#### SALARIES

The primary connection between an employer and an employee is pay for services rendered (or what economists call the wage-effort bargain). We are unable to measure services or effort particularly well (though we will partially do so in Chapter VI), but we can measure pay with great precision and accuracy. Accordingly, in this chapter we examine how collective bargaining, the availability of interest arbitration, and the use of arbitration have affected police salaries throughout the United States.

#### COLLECTIVE BARGAINING

By the late 1970s police officers in most medium and large cities collectively bargained their wages with their municipal employers. As Table III-1 shows, during the 1971-81 period the number of cities which bargained with police unions steadily expanded while the number of nonbargaining cities steadily declined.

Table III-1 presents a superficially persuasive reason why police officers would prefer collective bargaining: police salaries are noticeably higher in bargaining cities. By 1981 the average annual top step patrol officer salary in bargaining cities was more than \$2,200 higher than in nonbargaining cities.

Similarly, the figures in Table III-2 indicate why police officers and their unions might prefer to have access to arbitration: the average maximum patrol officer salary is generally higher in states with arbitration laws. For example, eleven of the thirteen arbitration

states in this list are in the top half of the table, and four arbitration states are listed in the first five places. Obviously, arbitration does not guarantee high salaries, nor is arbitration necessary to receive high pay. Nevertheless, arbitration appears to have a comforting (to the unions) presence in several high salary states.

However, the salary figures in Tables III-1 and III-2 are incomplete. Our portrayal of these figures in this form implies that only collective bargaining and arbitration affect police salaries, and such an implication is erroneous. In fact, previous research (Bartel and Lewin, 1981; Ehrenberg and Goldstein, 1975; Hall and Vanderporten, 1977; Lewin and Keith, 1976; Schmenner, 1973; and Victor, 1980) has shown that police pay responds to a variety of factors which have nothing directly to do with bargaining. As a result, we must perform a series of multivariate analyses to see if bargaining or arbitration makes any difference after controlling for these other factors.

#### Model

Our multivariate model is based upon previous research. Drawing upon these earlier findings, we have included nine city characteristics variables and three regional dummy variables to control for many of the forces which affect police pay. For example, we know that municipal employee salaries generally are positively correlated with city size, so we have included a population variable, and we expect that it will have a positive association with police pay. In addition, we have included several variables which may reflect a city's "tastes," or demand, for

TABLE III-1  
AVERAGE POLICE SALARIES 1971-81

Year	Nonbargaining Cities		Bargaining Cities	
	MINSAL	MAXSAL	MINSAL	MAXSAL
1971	\$ 7,277 (480)	\$ 8,586 (477)	\$ 8,459 (256)	\$10,029 (254)
1972	7,639 (412)	9,116 (410)	9,044 (283)	10,750 (282)
1973	7,967 (358)	9,577 (354)	9,421 (318)	11,235 (318)
1974	8,516 (331)	10,280 (326)	9,898 (334)	11,932 (333)
1975	9,062 (304)	11,099 (296)	10,652 (394)	12,849 (392)
1976	9,558 (284)	11,742 (281)	11,402 (446)	13,823 (444)
1977	10,023 (255)	12,298 (255)	12,084 (465)	14,703 (463)
1978	10,605 (257)	13,223 (255)	12,808 (472)	15,667 (472)
1979	11,461 (243)	14,257 (241)	13,404 (448)	16,446 (447)
1980	12,334 (239)	15,481 (237)	14,472 (467)	17,853 (466)
1981	13,427 (226)	16,771 (226)	15,434 (440)	18,991 (440)

Samples sizes are in parentheses.

MINSAL is the starting annual patrol officer salary as of January 2 of each year.

MAXSAL is the maximum annual patrol officer salary as of January 2 of each year.

TABLE III-2  
1981 MAXIMUM PATROL OFFICER SALARY AVERAGES BY STATE

State	Average	Standard Deviation
(A) Washington (13)	\$23,195	\$1,684
California (113)	22,268	1,712
(A) Minnesota (20)	22,042	2,170
(A) Oregon (3)	22,019	2,330
(A) Michigan (32)	21,377	1,821
Illinois (48)	21,099	2,103
Arizona (8)	20,695	1,023
Nevada (4)	20,504	2,472
Colorado (14)	20,417	1,315
Utah (4)	19,935	2,815
(A) Wisconsin (14)	19,602	1,708
(A) New Jersey (27)	19,468	1,553
Florida (37)	18,547	2,337
(A) New York (17)	18,539	3,553
Ohio (37)	18,188	1,904
North Dakota (3)	18,056	635
(A) Pennsylvania (20)	17,789	1,271
Idaho (5)	17,473	1,322
(A) Iowa (13)	17,021	1,572
(A) Connecticut (15)	16,930	1,356
(A) Nebraska (5)	16,923	2,164
Texas (41)	16,704	2,838
Missouri (12)	16,588	2,905
(A) Massachusetts (24)	16,410	1,316
Kansas (10)	16,284	2,569
New Mexico (4)	16,014	3,530
Montana (4)	15,735	1,180
Virginia (16)	15,734	1,701
Maryland (5)	15,276	2,700
North Carolina (16)	15,194	2,052
West Virginia (4)	15,188	1,246
Tennessee (7)	15,100	2,449
(A) Rhode Island (4)	15,001	399
Oklahoma (11)	14,875	1,783
Alabama (11)	14,866	1,175
Indiana (13)	14,355	1,967
Georgia (8)	14,285	1,626
Louisiana (8)	14,005	3,747
Kentucky (6)	13,541	2,079
South Carolina (8)	13,339	1,783
Mississippi (4)	13,070	1,654
Arkansas (6)	12,902	2,260
All cities	18,682	3,468

(A) indicates arbitration state; sample sizes are in parentheses; a state needed to report at least three salaries to be included.

police services: the crime rate (as reported by the FBI in its annual Index Crime Report); density (population per square mile); the wealth of the city's inhabitants (measured by median per capita income of the city's residents and by the median value of residential housing in the city); and percentage of city residents who are black. We expect all five of these variables to be positively associated with police pay, for all of these variables reflect either the need for more police officers (which in turn puts upward pressure on pay rates) or the city's ability to pay for police services.

We also have included a form of city government variable, namely, a dummy variable which measures the presence of the city manager form of government. We are unsure, however, how the city manager form of government should influence police pay: on the one hand, appointed city managers may be more able than elected officials to resist police union political pressures for higher salaries; on the other hand, however, city managers may value a "high quality" (and hence high paid) police force more than elected officials do. Consequently, we make no prediction about how police salaries might be influenced by the form of city government.

In addition, we have included a measure of the non-metropolitan cities in our sample. These "independent cities" (as we will call them) are those cities located outside the Standard Metropolitan Statistical Areas (SMSAs) which dot the country. In other words, these are the small and medium size cities which are not central cities or suburbs in the metropolitan areas. Because salaries generally are higher in urban than in rural areas, we expect location as an independent city to be

negatively associated with police salaries.

Further, we have included an opportunity wage variable which measures an alternative wage available to police officers and to persons considering police careers. Because police pay must be responsive to the pay rates available in non-police jobs (or else cities would be unable to recruit police officers), we expect that this opportunity wage measure (which is the 1972 average manufacturing wage in the community) will be positively associated with police salaries.

Finally, we have included regional variables to measure a city's location in the Northeast, North Central, West, or South census regions. Using North Central as the reference category, we expect a city's location in the South will be associated with lower police salaries, but we are unsure how police salaries in the Northeast and West will compare with police pay in the North Central region.

At the same time that we measure the impacts of these control variables on police pay, we also will include a bargaining variable to separate those cities which practice police collective bargaining from those which do not. Because previous researchers have found that collective bargaining exerts upward pressure on police pay, we expect that bargaining will be positively associated with police wages.

Accordingly, we will test the usefulness of this analytical model with an ordinary least squares (OLS) multiple regression equation of the following form:

$$\begin{aligned} \ln\text{MINSAL} &= B_0 + B_1\ln\text{POP} + B_2\ln\text{CRATE} + B_3\text{DENSITY} + B_4\ln\text{PCI} \\ \text{and} &+ B_5\ln\text{HOUSE} + B_6\text{PBLACK} + B_7\text{CMGR} + B_8\text{ICITY} \\ \ln\text{MAXSAL} &+ B_9\text{OPPWAGE} + B_{10}\text{NEAST} + B_{11}\text{SOUTH} + B_{12}\text{WEST} + B_{13}\text{CBA} + e \end{aligned}$$

where

- $\ln\text{MINSAL}$  = log of minimum patrol officer annual salary in each year
- $\ln\text{MAXSAL}$  = log of maximum patrol officer annual salary in each year
- $\ln\text{POP}$  = log of the city's population in each year
- $\ln\text{CRATE}$  = log of the city's crime rate in each year as measured by the number of "index" crimes reported to the FBI (murder, manslaughter, rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft; arson was added to the index in 1979 but is not included in our data)
- $\text{DENSITY}$  = density of the city in each year (number of inhabitants per square mile)
- $\ln\text{PCI}$  = log of the per capita income of city residents in each year
- $\ln\text{HOUSE}$  = log of the median value of residential housing in each city in each year
- $\text{PBLACK}$  = percentage of city residents who are black in each year
- $\text{CMGR}$  = form of government dummy variable = 1 for city manager form and 0 for all other forms
- $\text{ICITY}$  = a dummy variable = 1 for each city located outside an SMSA and 0 for each SMSA city
- $\text{OPPWAGE}$  = the average manufacturing wage in the city in 1972
- $\text{NEAST}$  = regional dummy variables for Northeast, South, and West  
 $\text{SOUTH}$  (North Central is the reference category)  
 $\text{WEST}$
- $\text{CBA}$  = presence of a written police collective bargaining agreement in each year
- $e$  = error term

As discussed above, we hypothesize that  $B_1, B_2, B_3, B_4, B_5, B_6, B_9,$  and  $B_{13}$  will be positive,  $B_8$  and  $B_{11}$  will be negative, and  $B_7, B_{10},$  and  $B_{12}$  will be indeterminate.

Our model is exogenous; that is, it assumes that collective bargaining influences salaries rather than the reverse. Alternatively, we could have used an endogenous model which is based on the assumption that

police bargaining and police salaries may have been jointly determined, and thus salary levels may have influenced the existence of collective bargaining as well as vice versa. Studies of private sector unionism which have modeled unionization as endogenously determined have generally found smaller union impacts than when unionism is assumed to be exogenous (though sometimes larger impacts emerge). However, Freeman and Medoff (1981) have shown that the results from some of these endogenous analyses have been rather implausible and also extremely sensitive to the specification of the analytical model. The one previous study of police salaries and collective bargaining which used both exogenous and endogenous analyses found that the endogenous measurements produced larger estimates of the police union impact than the exogenous measurements (Bartel and Lewin, 1981).

We believe that an exogenous model is much more appropriate in the law enforcement case. In the private sector unions appear to choose particular targets to organize, presumably to defend their primary domains (particular industries or crafts). They appear to make these choices based on the characteristics of various targets, and one relevant characteristic is the wage or salary levels of the employees involved (e.g., the airline unions representing blue collar ground employees frequently have attempted to organize white collar ground employees -- who already receive high pay; similarly, unions representing grocery clerks concentrate their organizing efforts on the high-wage chain stores rather than the low-wage mom-and-pop stores). In contrast, police unionism is a highly localized phenomenon (Juris and Feuille, 1973), which implies that officers in individual departments are the

main impetus for organization. These officers may or may not decide to unionize, but there is no evidence to indicate that police unions choose among cities for organizing targets. In addition, we believe that police officers in low-wage, medium-wage, and high-wage cities should have similar incentives to unionize and bargain: police in high-wage cities will want to protect their favored status, while police in medium- and low-wage cities will want to improve their situation.

Later in this chapter we will also treat arbitration as exogenously determined. Our primary justification for doing so is that arbitration's availability is determined at the state level while police salaries are determined at the city level. Thus, arbitration's availability should be treated as part of the external environment (i.e., as exogenous) in analyses of bargaining between individual police organizations and individual cities.

Finally, most of our analyses use annual salary levels rather than year-to-year salary changes. We realize that each year police salaries are incrementally rather than zero-based adjusted (i.e., salaries are increased by some amount over the prior year's salary rather than determined from zero). However, our data are reported as salary levels rather than as changes, and we minimize the risk of error by using them as such (we have used the minimum and maximum patrol officer salaries reported by U.S. cities each year to the International City Management Association). Similarly, police officers are paid an annual salary rather than a salary change, and thus salary levels rather than salary changes represent a more accurate portrayal of a city's cost of employing a police officer and of the standard of living a police officer may

enjoy. Perhaps most important, we have reasonably good data representing the long run factors (such as city size, location, private sector wage levels), but we have very little data representing the short run forces (such as city budget changes, personalities, negotiating tactics) which shape year-to-year changes in police salaries. Our longitudinal analyses later in this chapter will examine the annual rate of salary increase, but the cross-sectional analyses throughout most of this chapter are based on salary levels.

#### Results

Table III-3 presents the percentage impact that police collective bargaining had on minimum and maximum patrol officer salaries during the 1971-81 period. These figures show that police salaries were 4-10 percent higher in cities where the police bargained collectively with city hall than where they did not; clearly, it paid to unionize. These figures also show that the monetary payoff from bargaining increased during most of this period, reached its peak in 1978, and declined somewhat during the 1979-81 period.

The complete results of these analyses are found in Appendices III-1 and III-2 in the back of this report. The figures in these appendices show that police pay indeed is strongly influenced by a variety of factors which have nothing directly to do with bargaining. For instance, these coefficients show that police salaries are consistently higher in larger rather than smaller cities, in more dense rather than less dense cities, in wealthier rather than poorer cities, and in city manager rather than mayoral cities. Compared to police pay in North Central

cities, salaries are lower in Southern cities and higher in Western cities. In addition, cities located outside of metropolitan areas pay lower salaries than cities within SMSAs.

In contrast, there are some city characteristics which were associated with police pay levels during the early and middle years of our time period but no longer exerted any influence by the end of our period. For example, police salaries were positively and strongly associated with manufacturing wages through 1976, but since then the strength of this association has faded into insignificance. Similarly, police pay was positively associated with the reported crime rate through 1976, but since then there has been no systematic association between police salaries and reported crimes. Further, maximum police pay was positively associated with the proportion of city residents who are black, but this relationship disappeared after 1978-79.

Each of the particular findings reported in Table III-3 and in Appendices III-1 and III-2 are subject, of course, to the ceteris paribus constraint, or "all other things being equal." This means, for example, that the impact of collective bargaining on maximum patrol officer salaries in 1981 was 3.5 percent after controlling for the influences of the other variables included in the equation.

These results are highly plausible for three reasons. First, they are consistent with the results obtained in Bartel and Lewin's (1981) and Victor's (1980) studies of police pay. Second, most of the variables in our model are statistically significant, and the overall equations are highly significant (see the significance levels reported in Appendices III-1 and III-2). This pattern of results suggests that we have correctly

TABLE III-3  
 PERCENTAGE IMPACT OF BARGAINING ON  
 POLICE SALARIES<sup>a</sup>  
 (not controlling for arbitration)

Year	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>N</u>
1971	5.33*** (5.06)	5.17*** (4.90)	603
1972	6.90*** (6.02)	5.93*** (5.18)	551
1973	6.82*** (6.22)	5.77*** (5.29)	551
1974	5.53*** (5.08)	5.42*** (4.82)	552
1975	7.22*** (6.51)	6.37*** (5.80)	595
1976	8.64*** (7.42)	6.78*** (5.97)	646
1977	10.33*** (8.88)	9.66*** (8.39)	716
1978	10.54*** (8.74)	8.96*** (7.98)	725
1979	6.62*** (5.65)	5.57*** (4.87)	688
1980	7.02*** (5.41)	4.66*** (3.64)	616
1981	5.92** (4.40)	3.50*** (2.70)	573

<sup>a</sup>The percentage impacts are calculated using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

N = sample size

t-statistics are in parentheses.

\*\*\*Significant at the .01 level;

\*\*Significant at the .05 level;

\*Significant at the .10 level (two-tailed tests).

specified many of the factors which influence police pay levels. Third, our sample size is quite large; as a result, our findings should accurately portray the influences on police pay in most American cities.

So, when we divide American cities into those with union and nonunion police departments, and when we control for many of the "market" forces which affect police pay, we see that police unionization for the purpose of collective bargaining yielded financial gains for police officers during the 1971-81 period.

#### THE AVAILABILITY OF ARBITRATION

Having just touted our unionization results, we now retreat from them in order to include arbitration in our analyses. Specifically, the results in Table III-3 are valid only if we pretend that police interest arbitration does not exist. Such pretense is unrealistic, of course, for we know that by 1981 police interest arbitration was available to several hundred cities in fifteen states. However, we did not include an arbitration variable in the model specified earlier. As a result, any police salary impact due to arbitration would not be identified as such. Instead, if any such impact existed it would be picked up by the collective bargaining variable. If this happened, the bargaining impact results reported in Table III-3 would be misleading, for these results would not be due solely to collective bargaining by itself but partly to the presence of a police interest arbitration law (in addition to bargaining).

## Model

Is there any reason to expect that the availability of interest arbitration will have any effect on police salaries (and on other police bargaining outcomes)? A quick and easy "yes" answer is based on the fact that police unions would not lobby vigorously for such statutes (and municipal managements would not lobby vigorously against them) unless arbitration worked to the unions' advantage. A second quick and easy "yes" answer is based on the empirical results of two prior studies which found that the availability of interest arbitration was associated with higher pay for police officers and firefighters (Delaney and Feuille, 1983; Olson, 1980).

These two quick answers tend to beg the question, however, of why arbitration might provide police unions with more favorable bargaining outcomes than they could obtain otherwise. A fuller understanding of how arbitration affects bargaining power depends upon how arbitration affects union and employer costs of disagreement with each other's demands.

Police officers cannot legally strike. As a result, a police union has no readily available mechanism to manipulate management's costs of disagreeing with (i.e., saying "no" to) employee demands. Consequently, management is in a position to implement its own demands up to the point where police officers will respond with resignations or an illegal strike (i.e., the employer can unilaterally determine the terms of employment, but the union cannot). In other words, the employer can insist on its own favorable terms (up to the limits of the "market") because the union has no feasible way to make such employer insistence

costly. Further, the availability of mediation and factfinding do not really change this scenario, for employers can ignore the non-binding recommendations of mediators and factfinders. Consequently, when the police have no right to strike, the balance of bargaining power tends to favor management.

In addition to striking, police unions may be able to forge political alliances with elected officials or otherwise acquire sufficient political influence that they can persuade the municipal employer to accede to union demands. However, union political influence may be elusive and ineffective when faced with a stringent city budget constraint. For example, even mayors and city council members elected with police union support may not be able to meet union bargaining demands when city expenses already exceed city revenues. Further, a favorable (to the union) city administration may be voted out of office and replaced by a less responsive administration. In other words, police unions may have more power than simply the ability to conduct a strike, but union political influence may be too uncertain to alter the balance of bargaining power in the union's favor.

The availability of compulsory interest arbitration changes this distribution of bargaining power by eliminating the employer's ability to insist on its own terms and say "no" to union demands. When arbitration is available, a police union may respond to such an employer posture by invoking the arbitration procedure. If arbitration is used, the arbitrator must be responsive to the employer and union demands, for three reasons. First, state arbitration laws invariably require arbitrators to apply several evaluative criteria -- such as the rate of inflation, comparability,

ability to pay, and bargaining history — to the disputed issues when making their decisions. These decision criteria requirements permit employers and unions to submit evidence supporting their positions on these issues and ensure that arbitrators consider the evidence from both sides. Second, the adjudicator in any adversarial proceeding must provide a full and fair hearing to both sides. Third, both sides have the ability to influence the availability of future work opportunities for the arbitrator by informing other unions and employers of the arbitrator's perceived fairness and competence. If we assume that arbitrators want to continue working as arbitrators, these three characteristics of the arbitration procedure will cause arbitrators to issue "fair" awards which are responsive to the demands and concerns put forth by both sides. In practice, this means that an arbitrator usually will not award the employer as favorable a set of outcomes as the employer could have imposed in the absence of arbitration. It is also true that the arbitrator usually will not award the union its most preferred terms, either. However, in the absence of arbitration, the union is not in a position to impose its preferred terms while the employer is. Consequently, arbitration increases the cost to management of saying "no" to union demands.

This reasoning suggests that arbitration increases union bargaining power only when unions declare impasse, invoke the arbitration procedure, put their demands in front of an arbitrator, and receive an award which is more favorable to the union than the terms offered by the employer during negotiations. This scenario happens from time to time, especially as unions and employers "test" each other. However, our Table I-2

results in the first chapter and previous research have shown that most unions and employers do not use arbitration when it is available; instead, they negotiate their own agreements most of the time even though they could have received arbitration awards (Anderson and Kochan, 1977; Feuille, 1975, 1977; Gallagher and Pegnetter, 1979; Kochan and Baderschneider, 1978; Lipsky and Barocci, 1978; Stern, et. al., 1975; Wheeler, 1975). In turn, the fact that most unions and employers negotiate rather than arbitrate does not mean that arbitration has no impact upon the terms of these negotiated agreements, for neither the unions nor the employers will agree to negotiated terms which are less favorable than the terms they could expect to obtain via arbitration. In addition, we must assume that employers and unions can use the negotiation and arbitration experiences in their states to anticipate what will happen if they use arbitration (this assumption simply says that employers and unions have positively sloped learning curves). As a result, unions and employers will be able to modify their negotiating positions to anticipate what they believe would happen if they actually placed their situation in the hands of an arbitrator.

If we assume that employers and unions are rational (i.e., they seek favorable terms) and risk-averse or risk-neutral (i.e., they are cautious enough to prefer a particular outcome with certainty rather than gamble on a more favorable outcome which may or may not occur), this anticipatory knowledge means that the employer will offer more concessions to the union when arbitration is available than when it is not. Both the employer and the union can anticipate what an arbitrator is likely to award, and the rational and risk-neutral employer will offer concessions

to the union which are equal (or almost equal) in value to the expected value of the arbitral award in order to save the arbitration transaction costs (time, money) and avoid the risk of an award which is more costly than anticipated. Similarly, no rational and risk-neutral union will negotiate an agreement with terms which are significantly less advantageous than what the union expects it could obtain from an arbitrator. In other words, the availability of arbitration will alter union and employer negotiating behavior in a direction which will yield unions more favorable outcomes than when arbitration is unavailable.

#### Results: Cross Section

As a result of the above reasoning, we included an arbitration availability (ARB) variable in our salary determination model. This is a dummy variable with a value of one for each city which is located in an arbitration state and zero for each city located in a state with no police arbitration law. The inclusion of this ARB variable will enable us to identify salary differences among police forces which do not bargain, which bargain without access to arbitration, and which bargain with access to arbitration. If the availability of arbitration contributes to union bargaining power as hypothesized, the ARB coefficients will be significantly positive. It is important to note, however, that this analysis does not measure the impact of actually using the arbitration procedure.

Bargaining vs. arbitration. Table III-4 shows the percentage impacts that bargaining and the availability of arbitration had on minimum and maximum patrol officer salaries in each year of the 1971-81

period. These figures show that arbitration had a strong and positive impact on salaries throughout most of this period, especially during the 1977-81 years. These impacts were in the 4-9 percent range during these final five years of our measurement period, and these arbitration impacts grew larger during this recent time period while the bargaining impacts became smaller. In fact, during the 1980-81 years arbitration had a six percent impact on entry salaries and a nine percent impact on top step salaries, while bargaining by itself had a five percent impact on entry salaries and almost no impact on maximum pay. Thus, these data show that the existence of a compulsory arbitration statute enables police unions to obtain higher wages than they would negotiate otherwise.

In addition, the Table III-4 results also show that the Table III-3 results are somewhat misleading. Specifically, the CBA figures in Table III-4 are smaller than the CBA figures in Table III-3 (by 1-2 percentage points in each year). In other words, the Table III-4 results demonstrate that the Table III-3 results were pulled upward by the availability of arbitration in many of the cities in the sample, and thus Table III-3 overstates the salary impact of bargaining by itself. In turn, the findings in these two tables emphasize the importance of measuring the impact of both bargaining and arbitration in public sector wage impact research.

Perhaps the most interesting Table III-4 result is that bargaining and arbitration seem to have somewhat different effects over time on entry and top step salaries. For example, bargaining's impact peaked during 1977-78, and by 1981 bargaining no longer had an impact on maximum salaries but still contributed about five percentage points on entry salaries (compared

**CONTINUED**

**1 OF 5**

TABLE III-4  
 PERCENTAGE IMPACTS OF BARGAINING AND  
 ARBITRATION'S AVAILABILITY ON POLICE SALARIES<sup>a</sup>

Year	lnMINSAL		N	lnMAXSAL	
	CBA	ARB		CBA	ARB
1971	5.19*** (4.78)	1.36 (0.79)	603	4.75*** (4.42)	3.08* (1.80)
1972	6.32*** (5.30)	2.78* (1.66)	551	4.84*** (4.11)	5.36*** (3.18)
1973	6.13*** (5.26)	2.23 (1.64)	551	4.31** (3.77)	4.85*** (3.56)
1974	4.62*** (3.99)	2.87** (2.12)	552	3.98*** (3.35)	4.62*** (3.28)
1975	6.45*** (5.59)	2.86** (2.11)	595	5.65*** (4.93)	2.73** (2.02)
1976	7.93*** (6.61)	2.89** (2.15)	646	5.90*** (5.06)	3.64*** (2.76)
1977	9.17*** (7.49)	3.81*** (2.66)	716	7.76*** (6.48)	6.43*** (4.51)
1978	8.83*** (7.10)	5.75*** (4.25)	725	7.44*** (6.42)	5.15*** (4.07)
1979	5.16*** (4.27)	5.24*** (3.93)	688	4.34*** (3.67)	4.39*** (3.37)
1980	5.54*** (4.14)	6.09*** (3.54)	616	2.51* (1.93)	9.21*** (5.35)
1981	4.78*** (3.50)	5.92*** (3.44)	573	1.77 (1.37)	9.39*** (5.59)

<sup>a</sup>The percentage impacts reported were calculated from the OLS coefficients using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

to nonunion departments). In contrast, by 1980-81 arbitration had a comparatively hefty nine percent impact on maximum salaries and a six percent impact on minimum rates, and these were arbitration's largest impacts during the entire eleven year period.

The pattern of results in Table III-4 strongly suggests that arbitration helps police unions resist municipal fiscal pressures much better than bargaining by itself does. As seen in the table, bargaining's coefficients peaked during 1977-78 and declined considerably thereafter, while arbitration's coefficients increased. The 1978-81 period also saw the emergence of fiscal pressures on local governments caused by "taxpayer revolts" and the national economic recession. Table III-4 suggests that by 1981 police unions which bargained without access to arbitration were not able to resist these fiscal pressures very well. In contrast, by 1981 arbitration enabled police unions to obtain a substantial salary advantage over their peers who did not have access to arbitration. In other words, these results suggest that arbitration's greatest attraction for police unions may be its ability to help them resist employer demands for "less" during periods of fiscal restraint.

Our supplementary data in Appendices III-3 and III-4 show that the dollar magnitude of arbitration's impact has become substantial in recent years. For example, arbitration's 3.6 percent impact on 1976 maximum salaries translated into an annual dollar increase of \$525. By 1981, arbitration boosted top step annual pay by \$1,674 (or 9.4 percent) compared to cities in nonarbitration states. This is a hefty pay advantage, and it must appear especially attractive to police officers during a period of tight city finances.

Caveats. There are four caveats to keep in mind when interpreting the Table III-4 results. First, these coefficients are the result of cross sectional analyses of salary levels, which means that each year is examined in isolation from all other years. Consequently, Table III-4 enables us to say little about the cumulative impacts over time of bargaining and arbitration (though we will perform such longitudinal analyses later).

Second, our analytical model is an exogenous one; that is, we have assumed that bargaining and arbitration are external or environmental parameters which influence police wages in the same manner that the other external variables in our model do. In other words, we have assumed that bargaining, arbitration, and police wages are not jointly determined by the other variables in our model. Obviously, discarding this assumption might yield results different from those reported here.

Third, we have presented and discussed our findings as if all cities in arbitration states have unionized police departments. This condition is almost completely accurate, for we saw in Chapter II that there are very few nonunion police departments in arbitration states. However, our Table III-4 analyses treat all cities in arbitration states, union and nonunion alike, as belonging in the ARB category. Because this is slightly misleading (for nonunion police do not have access to the arbitration procedure), we included a bargaining-arbitration interaction term (CBA·ARB) in an alternate set of equations. This term measures only the unionized cities in arbitration states (the few nonunion cities in arbitration states are sorted into the overall nonunion category). These CBA·ARB coefficients (reported in Appendices III-3 and III-4) are almost the same as the ARB coefficients reported in

Table III-4 (in recent years the interaction terms are about one percent smaller than the ARB coefficients). This finding indicates that arbitration's availability tends to push up wages in nonunion police departments at least as much as in unionized departments (but only in those states where arbitration exists, of course), which in turn confirms the results reported in Table III-4.

Fourth, our focus on the bargaining and arbitration coefficients in Table III-4 should not obscure the fact that, as noted earlier, there are several "market" forces which affect police pay but which have nothing directly to do with bargaining or arbitration. As shown in Appendices III-3 and III-4, police pay is higher in larger cities, in high density cities, in wealthy cities, in Western cities, and in cities with a city manager form of government. In contrast, police pay is lower in nonmetropolitan cities (e.g., small and medium size cities located outside the large urban areas). Further, some of these market forces had strong effects on police pay. For example, in 1981 location outside a metropolitan area was associated with a decrease in minimum and maximum salaries of \$1,117 and \$1,242, respectively, compared to cities located in SMSAs; a Southern location was associated with entry and top step salaries which were \$1,580 and \$1,814 lower than cities in North Central states; and cities with a city manager form of government paid entry and top step salaries which were \$497 and \$1,232 higher than cities with mayor or commission forms of government. For comparison purposes, the availability of arbitration in 1981 was associated with minimum and maximum salaries which were \$890 and \$1,674 higher than where arbitration was not available. So, arbitration's strong influence

on police pay should not hide the fact that many other factors also strongly affect police salaries.

Age of the bargaining relationship. As noted above, our cross sectional analyses contain no direct measure of the length of time that the police have bargained with city hall; instead, in each year all the unionized cities are treated the same regardless of how long police collective bargaining has existed in each city. However, it may be possible that police unions have negotiated more favorable wage rates as they accumulate increased opportunities to do so. Alternatively, it may be possible that police unions have an initial "shock effect" on management which then disappears over time as management becomes more adept at bargaining. Consequently, we have included a variable (CBAGE) which measures the length or age (in years) of the bargaining relationship.

The data used to construct this variable were supplied by the respondents to our questionnaire survey, who indicated the first year that each city signed a written agreement with a police union. In these analyses, we have assumed that police collective bargaining has existed continuously in each city since the year it began. Further, these analyses use only the unionized portion of our sample (i.e., all nonunion cities are excluded) because of the computational difficulty which would exist if both the CBA and CBAGE terms were included in the same equation. Because both of these terms measure bargaining status, they overlap considerably, and thus their simultaneous inclusion would provide confusing results. Consequently, the results reported next are derived from the unionized cities.

Table III-5 presents the CBAGE and ARB coefficients on minimum and

maximum salaries for each year during the 1971-81 period. These coefficients were calculated from the unionized cities using the same model presented earlier in this chapter; the only change was the substitution of the CBAGE term (i.e., length of bargaining relationship in years) for the CBA term (i.e., bargaining or nonbargaining status). The Table III-5 results show that arbitration's impact on both minimum and maximum salaries is almost the same as reported in Table III-4; the ARB coefficients in the two tables are usually within one percentage point of each other. This result is encouraging, for it indicates that arbitration's observed impact on police wages is not highly sensitive to alternative specifications of our model.

The Table III-5 results also show that the age of the bargaining relationship had almost no impact during the 1971-76 period but has had a positive impact on police pay during the 1977-81 period. The coefficients in the table indicate that police pay in unionized cities is about three-tenths to four-tenths of a percentage point higher for each year that police collective bargaining has existed, controlling for other influences. For example, in 1981 maximum salaries were 3.7 percent higher in unionized cities which had bargained for ten years compared to unionized cities which had just started bargaining in 1981, after controlling for the influence of the other factors in our model. In other words, bargaining seems to have a greater impact on police pay when it has been in place for several years than when it is new.

Length of arbitration's availability. The fact that the length of the bargaining relationship is positively correlated with salaries suggests that the length of time that arbitration has been available in

TABLE III-5  
 PERCENTAGE IMPACTS OF BARGAINING AGE AND  
 ARBITRATION'S AVAILABILITY ON POLICE SALARIES<sup>a</sup>

Year	lnMINSAL		N	lnMAXSAL	
	CBAGE	ARB		CBAGE	ARB
1971	-0.05 (0.24)	1.21 (0.62)	223	-0.25 (1.43)	3.14* (1.95)
1972	0.04 (0.20)	3.78** (2.01)	244	-0.45** (2.31)	6.43*** (3.66)
1973	0.01 (0.08)	1.19 (0.73)	271	0.13 (0.72)	2.84* (1.85)
1974	0.15 (0.82)	5.47*** (3.53)	283	-0.06 (0.34)	6.22*** (4.14)
1975	0.03 (0.16)	3.87** (2.57)	341	-0.08 (0.49)	3.16** (2.14)
1976	0.20 (1.16)	3.79** (2.45)	396	0.23 (1.43)	3.61** (2.45)
1977	0.41*** (2.66)	4.22*** (2.63)	462	0.18 (1.30)	7.57*** (5.14)
1978	0.38** (2.42)	5.17*** (3.54)	471	0.33** (2.28)	4.43*** (3.33)
1979	0.34** (2.35)	4.92*** (3.36)	447	0.24* (1.73)	3.52*** (2.58)
1980	0.38** (2.40)	6.31*** (3.32)	417	0.29* (1.91)	8.62*** (4.64)
1981	0.47*** (3.17)	6.40*** (3.56)	387	0.37*** (2.74)	9.31*** (5.49)

<sup>a</sup>The percentage impacts reported were calculated from the OLS coefficients using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

a particular state also may have an effect on police pay. Up to this point we have used only a crude measure of arbitration's availability, namely, the presence or absence of an arbitration statute (ARB). This measure says nothing about how long an arbitration law has existed, with the result that in our 1978 figures, for example, New Jersey and Michigan cities are treated alike even though New Jersey had had arbitration for one year and Michigan had had arbitration for nine years.

We remedy this problem in two ways. First, we will replace the arbitration availability term (ARB) with a length of arbitration availability term (ARBAGE). ARBAGE is a continuous variable which measures the number of years that an arbitration statute has existed in each arbitration state. Second, in an alternative equation we will replace the ARB term with four dummy variables representing four different lengths of time that arbitration has existed: A12 for cities covered by an arbitration law for only one or two years; A35 for cities covered by an arbitration law for three, four, or five years; A68 for cities covered by arbitration for six, seven, or eight years; and A9+ for cities covered by arbitration for nine or more years. Because 1968 was the first year any police arbitration statute existed, some of these dummy variables will not become operative until the more recent years of our measurement period. In addition, these length of time variables have been included in the estimation equations along with the other independent variables specified in the analytical model at the beginning of this chapter.

Table III-6 presents the results from these two alternative specifications of the length of arbitration's availability. Both specifications indicate that there are substantial dollar payoffs for police officers

TABLE III-6  
 PERCENTAGE IMPACTS OF BARGAINING AND  
 LENGTH OF ARBITRATION'S AVAILABILITY ON POLICE SALARIES<sup>a</sup>  
 (all cities sample)

YEAR	N	InMINSAL								InMAXSAL						
		Equation 1		Equation 2						Equation 1		Equation 2				
		CBA	ARBAGE	CBA	A12	A35	A68	A9+	CBA	ARBAGE	CBA	A12	A35	A68	A9+	
1971	603	5.27*** (4.87)	0.34 (0.50)	5.04*** (4.64)	3.56 (1.52)	-1.17 (0.47)	—	—	4.84*** (4.52)	1.09 (1.60)	4.68*** (4.33)	4.28* (1.84)	1.67 (0.67)	—	—	
1972	551	6.52*** (5.58)	0.77 (1.44)	6.34*** (5.31)	2.19 (0.76)	2.99 (1.59)	—	—	5.15*** (4.45)	1.65*** (3.07)	4.93*** (4.17)	2.81 (0.97)	6.30*** (3.31)	—	—	
1973	551	6.35*** (5.67)	0.69* (1.77)	6.22*** (5.32)	1.06 (0.62)	3.47** (1.97)	—	—	4.89*** (4.43)	1.32*** (3.40)	4.43*** (3.86)	3.18* (1.86)	6.61*** (3.74)	—	—	
1974	552	5.26*** (4.69)	0.32 (0.92)	4.45*** (3.84)	2.90* (1.88)	5.24*** (2.27)	-2.22 (0.75)	—	4.75*** (4.12)	0.82** (2.29)	3.74*** (3.17)	3.77** (2.37)	10.07*** (4.17)	-1.85 (0.61)	—	
1975	595	6.77*** (5.99)	0.52* (1.79)	6.35*** (5.48)	2.10 (1.30)	5.09* (1.84)	3.02 (1.53)	—	5.86*** (5.23)	0.61** (2.10)	5.64*** (4.90)	1.98 (1.23)	2.97 (1.09)	3.94** (1.98)	—	
1976	646	7.99*** (6.75)	0.60** (2.40)	7.77*** (6.46)	0.43 (0.22)	4.17** (2.33)	3.38* (1.69)	—	5.83*** (5.09)	0.90*** (3.68)	5.56*** (4.80)	-1.93 (1.00)	6.50*** (3.68)	5.02** (2.56)	—	
1977	638	8.05*** (6.64)	0.86*** (3.95)	9.38*** (6.92)	-4.08* (1.87)	2.20 (1.34)	6.95*** (2.96)	4.59 (1.37)	7.28*** (5.96)	1.08*** (4.85)	7.23*** (5.81)	1.26 (0.55)	4.52*** (2.68)	10.48*** (4.33)	9.04*** (2.62)	
1978	646	8.45*** (6.47)	0.99*** (4.75)	8.76*** (6.58)	-2.94 (1.17)	3.43* (1.78)	7.83*** (2.62)	8.17*** (3.66)	6.75*** (5.58)	1.16*** (5.97)	6.87*** (5.55)	2.09 (0.86)	5.31*** (2.92)	4.84* (1.76)	11.94*** (5.62)	
1979	617	4.29*** (3.53)	0.77*** (4.32)	4.23*** (3.42)	3.75 (1.15)	1.22 (0.50)	7.39*** (3.85)	6.77*** (3.03)	2.73** (2.28)	0.95*** (5.38)	2.71** (2.27)	9.83*** (3.00)	-0.39 (0.17)	8.69*** (4.62)	9.99*** (4.52)	
1980	616	5.38*** (4.08)	0.79*** (4.50)	5.56*** (4.20)	—	-0.70 (0.29)	6.75*** (3.66)	7.67*** (3.30)	2.55** (1.99)	1.04*** (5.98)	2.49* (1.94)	—	1.91 (0.79)	9.50*** (5.17)	12.01*** (5.15)	
1981	573	4.69*** (3.47)	0.67*** (4.03)	4.74*** (3.49)	—	3.05 (0.12)	6.00*** (2.94)	7.69*** (3.56)	1.92 (1.50)	0.89*** (5.53)	1.72 (1.33)	—	9.08*** (3.49)	8.46*** (4.25)	10.50*** (4.98)	

A12 refers to cities covered by arbitration for less than 3 years.  
 A35 refers to cities covered by arbitration for at least 3 but less than 6 years.  
 A68 refers to cities covered by arbitration for at least 6 but less than 9 years.  
 A9+ refers to cities covered by arbitration for 9 years or more.

<sup>a</sup>The percentage impacts reported were calculated from the OLS coefficients using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

the longer that arbitration laws exist. For example, the ARBAGE coefficients indicate that during the 1977-81 period minimum salaries increased about three-fourths of one percent and maximum salaries increased about one percent for each additional year that an arbitration statute had been on the books. Similarly, the A12, A35, A68, and A9+ coefficients show that there are very strong salary effects associated with greater arbitration longevity. The strong and highly significant coefficients on the A68 and A9+ terms in the more recent years indicate that arbitration has its greatest salary impact many years after an arbitration law is passed. For instance, during 1979-81 maximum salaries in cities in A9+ states were 10-12 percent higher than in cities located in nonarbitration states, ceteris paribus, and maximum salaries in A68 states were about nine percent higher. Further, the coefficients in the minimum salary equations exhibited a similar pattern but at a somewhat lower level of magnitude.

Because only bargaining cities have access to arbitration, we re-analyzed our data for a smaller sample composed only of those cities which bargain with police. In this analysis we used the same arbitration categories (ARBAGE, A12, A35, etc.) as in the all cities sample. We also included the length of the bargaining relationship variable (CBAGE) described earlier to insure that our arbitration variables did not accidentally pick up any salary impacts due to bargaining longevity.

The results from our bargaining cities sample are reported in Table III-7. A comparison of the coefficients reported in Tables III-6 and III-7 shows that the results are highly similar across the two samples. Both tables indicate that arbitration longevity is strongly associated

TABLE III-7  
 PERCENTAGE IMPACTS OF BARGAINING AND  
 LENGTH OF ARBITRATION'S AVAILABILITY ON POLICE SALARIES<sup>a</sup>  
 (bargaining cities sample)

YEAR	N	lnMINSAL								lnMAXSAL							
		Equation 1		Equation 2						Equation 1		Equation 2					
		CBAGE	ARBAGE	CBAGE	A12	A35	A68	A9+	CBAGE	ARBAGE	CBAGE	A12	A35	A68	A9+		
1971	223	-0.06 (0.28)	0.15 (0.20)	-0.05 (0.25)	5.85** (2.03)	-2.83 (1.06)	—	—	-0.26 (1.45)	0.97 (1.56)	-0.25 (1.44)	6.46*** (2.68)	0.30 (0.14)	—	—		
1972	244	0.05 (0.22)	1.24** (2.14)	0.05 (0.22)	-0.35 (0.22)	5.25** (2.53)	—	—	-0.45** (2.28)	2.17* * (4.05)	-0.45** (2.31)	-0.26 (0.09)	8.86*** (4.60)	—	—		
1973	271	0.01 (0.04)	0.65 (1.52)	-0.02 (0.10)	-1.22 (0.61)	3.37* (1.72)	—	—	0.11 (0.61)	1.09*** (2.76)	0.08 (0.48)	-0.36 (0.19)	5.69*** (3.13)	—	—		
1974	283	0.13 (0.72)	0.77** (2.00)	0.07 (0.40)	5.26*** (3.16)	11.66*** (4.15)	-0.62 (0.21)	—	-0.08 (0.46)	1.09*** (2.93)	-0.27 (1.04)	5.32*** (3.40)	16.65*** (6.15)	-1.23 (0.44)	—		
1975	341	0.04 (0.25)	0.78** (2.45)	0.01 (0.05)	2.59 (1.52)	6.33** (2.22)	5.38** (2.42)	—	-0.08 (0.46)	0.75** (2.40)	-0.10 (0.58)	2.29 (1.37)	2.80 (1.02)	5.58** (2.56)	—		
1976	396	0.15 (0.87)	0.79*** (2.79)	0.15 (0.89)	1.69 (0.77)	4.41** (2.25)	5.54** (2.39)	—	0.14 (0.89)	1.00*** (3.69)	0.15 (0.93)	-1.78 (0.86)	6.11*** (3.20)	6.02*** (2.80)	—		
1977	412	0.25 (1.59)	1.06*** (4.40)	0.22 (1.35)	-2.35 (0.95)	3.80** (2.08)	9.06*** (3.54)	6.82* (1.90)	0.04 (0.25)	1.32*** (5.94)	0.05 (0.33)	2.66 (1.17)	6.48*** (3.82)	12.98*** (5.42)	11.67*** (3.45)		
1978	418	0.29* (1.78)	1.02*** (4.49)	0.23 (1.33)	-1.30 (0.48)	3.23 (1.61)	8.34*** (2.71)	8.86*** (3.67)	0.25* (1.71)	1.20*** (5.87)	0.29* (1.89)	3.97 (1.59)	5.50*** (3.00)	5.05* (1.85)	12.77*** (5.75)		
1979	405	0.23 (1.56)	0.88*** (4.49)	0.31** (2.01)	7.23** (2.01)	3.97 (1.48)	8.62*** (4.17)	8.85*** (3.64)	0.14 (0.97)	1.03*** (5.52)	0.24* (1.69)	12.23*** (3.61)	1.46 (0.60)	9.30*** (4.86)	11.36*** (5.01)		
1980	417	0.28* (1.75)	0.80*** (4.16)	0.25 (1.58)	—	1.60 (0.06)	6.93*** (3.46)	8.34*** (3.33)	0.18 (1.14)	0.95*** (5.11)	0.16 (1.01)	—	2.11 (0.84)	9.03*** (4.62)	11.52*** (4.69)		
1981	387	0.40*** (2.69)	0.73*** (4.15)	0.36*** (2.30)	—	1.91 (0.71)	6.08*** (2.92)	8.61*** (3.78)	0.29** (2.10)	0.87*** (5.32)	0.38*** (2.65)	—	10.27*** (3.96)	8.30*** (4.25)	10.12*** (4.75)		

A12 refers to cities covered by arbitration for less than 3 years.

A35 refers to cities covered by arbitration for at least 3 but less than 6 years.

A68 refers to cities covered by arbitration for at least 6 but less than 9 years.

A9+ refers to cities covered by arbitration for 9 years or more.

<sup>a</sup>The percentage impacts reported were calculated from the OLS coefficients using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

with higher police salaries, especially during the 1979-81 period. In other words, the results from these two tables suggest that arbitration availability per se may be a less important influence on police salaries than the length of time that arbitration is available. In addition, these results indicate that arbitration has a cumulative impact on salaries over time rather than a one-time "shock effect" shortly after an arbitration law is passed, and this finding corroborates Olson's (1980) finding of the impact of arbitration's availability on firefighter wages during the 1972-77 period.

State-specific effects. The fact that some of the coefficients in Tables III-6 and III-7 change substantially in size from year to year, especially in the A12 and A35 columns, indicates that there may be some results being reported in these two tables which are specific or unique to particular states. For example, in both tables the A35 maximum salary coefficient for 1978 reports a positive and significant five percent impact, but the A35 coefficients for 1979 are insignificant, and Iowa was the only state to move into the A35 category in 1979.

More generally, arbitration laws came into existence almost continuously during the 1968-77 period, as seen in the following list of arbitration states and the year each law went into effect:

Alaska (1972)	New Jersey (1977)
Connecticut (1975)	New York (1974)
District of Columbia (1980)	Oregon (1973)
Iowa (1976)	Pennsylvania (1968)
Massachusetts (1974)	Rhode Island (1968)
Michigan (1969)	Washington (1973)
Minnesota (1973)	Wisconsin (1972)
Nebraska (1969)	

As a result, there is a substantial amount of switching each year across the arbitration longevity categories, which in turn makes it possible that some of the Table III-6 and III-7 coefficients are being driven by the results from particular states. Further, these state-specific effects may reflect the existence of arbitration or they may reflect some unmeasured state characteristics which have nothing to do with arbitration. For example, police salaries in Iowa may always have been low compared to police salaries in other states; alternatively, police salaries in some arbitration states may always have been higher than elsewhere.

To test more fully for any state-specific effects, we re-estimated arbitration's impact in our all cities sample. However, instead of using categorical dummy variables (ARBAGE, A12, etc.), we used state-specific dummy variables representing each arbitration state. To insure that these state coefficients were not unduly influenced by the results from one or two cities in a state, we established a requirement that each arbitration state needed to have at least ten cities' worth of information in each year to be included in the analysis. As a result, we excluded Alaska, District of Columbia, Nebraska, and Oregon from this particular analysis because of insufficient sample size in each of those jurisdictions.

The results of our state-specific analyses are reported in Table III-8 for maximum salaries in the all cities sample. The coefficients in this table enable us to see how arbitration's impact varies across states and over time within the same state. In addition, for eight states (all but Michigan, Pennsylvania, and Rhode Island) we can compare

police salaries before and after arbitration laws were passed. Keep in mind that in this table we are comparing maximum police salaries in these eleven arbitration states to maximum salaries in states which do not have arbitration, controlling for bargaining (CBA) and the other independent variables in our salary determination model.

The coefficients in Table III-8 indicate that the availability of arbitration may not have as much of an impact on police salary levels as suggested by our preceding aggregate analyses. This conclusion seems especially appropriate for the eight states for which we have before and after arbitration data. If we look at the coefficients above and below the arbitration line drawn through Table III-8, we see that cities in Minnesota, New York, and New Jersey consistently paid higher police salaries both before and after the availability of arbitration (compared to cities in nonarbitration states), and we see that cities in Wisconsin, Massachusetts, and Connecticut have paid police salaries which over time are not significantly different from salaries in nonarbitration states. In addition, police salaries in Iowa compared even less favorably to salaries elsewhere after that state implemented its arbitration law.

Cities in Michigan and Pennsylvania paid higher than average salaries throughout our time period, but because arbitration took effect in both states prior to 1971 we cannot make any before and after comparisons. In addition, arbitration preceded our time period in Rhode Island, and salary levels in that state have been similar to salaries in nonarbitration states.

Washington is the only state in which the availability of arbitration may have unequivocally pushed up salaries. For a few years before and

TABLE III-8  
 PERCENTAGE STATE-SPECIFIC EFFECTS OF  
 ARBITRATION'S AVAILABILITY<sup>a</sup>  
 (all cities sample; maximum salary only)

YEAR	N	CA	MI	PA	RI	WI	MN	WA	NY	MA	CT	IA	NJ
1971	600	3.66*** (3.28)	7.49*** (2.93)	11.62** (2.13)	1.09 (0.18)	7.76** (2.74)	13.59*** (4.55)	1.26 (0.38)	8.01 (1.51)	7.51 (1.48)	1.45 (0.28)	-5.17 (0.17)	10.72** (1.99)
1972	551	4.19*** (3.57)	11.75*** (4.31)	27.71*** (4.25)	14.30* (1.73)	3.89 (1.31)	10.48*** (3.63)	-3.02 (0.84)	28.54*** (4.41)	22.99*** (3.72)	15.21** (2.49)	-4.02 (1.25)	27.39*** (4.25)
1973	551	3.28*** (2.86)	13.66*** (5.43)	17.52*** (2.93)	7.38 (1.05)	3.02 (1.13)	10.30*** (3.72)	-2.68 (0.78)	20.33*** (3.38)	13.83** (2.40)	8.23 (1.44)	-3.23 (1.09)	19.34*** (3.28)
1974	552	4.04*** (3.38)	11.37*** (4.44)	17.88*** (2.89)	3.24 (0.50)	-1.12 (0.38)	5.71** (1.97)	-0.45 (0.12)	27.33*** (4.39)	16.63*** (2.85)	7.90 (1.42)	-1.41 (0.44)	21.92*** (3.75)
1975	595	5.30*** (4.61)	7.84*** (2.85)	12.76* (1.93)	-2.97 (0.43)	-1.17 (0.41)	5.79** (1.97)	1.94 (0.60)	15.36** (2.36)	2.80 (0.48)	-0.01 (0.00)	-3.95 (1.25)	9.52 (1.56)
1976	646	5.62*** (4.97)	12.65*** (4.89)	9.33 (1.52)	-5.46 (0.85)	-0.10 (0.04)	11.05*** (4.05)	12.56** (2.12)	5.72 (1.04)	-1.27 (0.23)	-7.45** (2.24)	11.34** (2.00)	Before Arbitration
1977	638	7.72*** (6.46)	11.94*** (4.77)	20.74*** (3.49)	-1.44 (0.20)	-3.59 (1.22)	11.18*** (3.88)	13.20** (2.35)	7.73 (1.50)	2.14 (0.41)	-7.58** (2.18)	17.33*** (3.11)	After Arbitration
1978	646	7.41*** (6.21)	11.84*** (4.66)	32.18*** (4.86)	7.38 (1.03)	-0.33 (0.12)	9.21*** (3.17)	7.08** (2.05)	24.38*** (3.89)	13.30*** (2.24)	6.50 (1.10)	-7.46** (2.21)	19.89*** (3.24)
1979	617	3.19*** (2.74)	13.83*** (5.53)	18.76*** (2.59)	-0.57 (0.08)	3.50 (1.15)	9.59*** (3.38)	12.14*** (3.32)	15.65** (2.23)	5.54 (0.84)	0.57 (0.09)	-7.47** (2.38)	17.09** (2.47)
1980	616	3.17** (2.57)	15.99*** (5.76)	22.25* (1.76)	0.01 (0.00)	3.02 (1.03)	13.78*** (4.52)	13.44*** (3.54)	20.57 (1.63)	9.06 (0.77)	2.92 (0.25)	-11.45*** (2.90)	16.56 (1.36)
1981	573	2.22* (1.79)	11.85*** (4.10)	23.43** (2.50)	1.96 (0.20)	5.05 (1.57)	11.10*** (3.61)	18.18*** (4.93)	15.33* (1.71)	6.14 (0.73)	5.32 (0.63)	-5.20 (1.45)	20.90** (2.34)

<sup>a</sup>The percentage impacts reported were calculated from the OLS coefficients using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

after the passage of arbitration in 1973, Washington cities paid salaries comparable to those paid in nonarbitration states. However, during 1978-81 salaries in Washington cities climbed substantially above salaries paid elsewhere, reaching an 18 percent differential by 1981. Washington is the only state in which such a clear before and after effect occurred.

Taken together, the coefficients in Table III-8 suggest that, because arbitration emerged in states which already paid relatively high salaries (Minnesota, New York, New Jersey, and possibly Michigan and Pennsylvania), and because these high salary states contain a disproportionate number of the cities in our sample, much of arbitration's salary impact reported earlier may not be attributable to arbitration. Instead, some unmeasured state-specific effects may have caused salaries in these particular states to be higher than average, and our arbitration term is simply reporting these unmeasured effects.

Having said that, it is important to note that arbitration does not exist only in high salary states. Table III-8 shows that cities in Rhode Island, Wisconsin, Connecticut, and Iowa pay police salaries comparable to (or, in Iowa's case, lower than) salaries paid in non-arbitration states.

When all is said and done, however, the disaggregated state-specific results in Table III-8 cast doubt on the usefulness of the aggregate results reported in the preceding four tables. Specifically, arbitration is associated with comparatively high salary levels in several states, but it appears that these high salary levels may be due to some unmeasured state characteristics, or to some unmeasured characteristics of cities

in those states, rather than to arbitration. If this is the case, arbitration may be reporting but not causing higher salaries.

Results: Time Series Pooling of Data

Our aggregate year-by-year cross section analyses showed that both collective bargaining and the availability of arbitration were positively associated with police salary levels during most of the 1971-81 period. Our disaggregated cross section analyses confirmed these results for bargaining, but the before-and-after portion of our disaggregated analyses indicated that several states which paid high salaries after arbitration also paid high salaries before arbitration became available. Consequently, our cross section analyses show that arbitration and police salary levels are positively associated but that arbitration may have done less than is frequently believed to cause higher salaries.

Another approach to the analysis of these data is to pool all the cross-sectional data together for the entire time period. The advantage of this approach is that it allows us to obtain statistics which summarize the average impact of bargaining and arbitration over the course of this eleven year period. More precise estimates of individual variable effects are also possible since the pooling of the data provides much larger sample sizes than are available for estimating single year coefficients.

The disadvantage of using a pooled approach is that individual city characteristics which are not captured in the model and unique yearly effects which influence all observations in each year will tend to lead to conditions which violate some of the basic assumptions of ordinary

least squares (OLS) regression analysis. In practice this means that the estimated OLS coefficients may be inconsistent (i.e., they will not tend toward the true population value as the sample size gets larger) and the estimated standard errors of these coefficients will tend to be underestimated (therefore t-statistics will be too large). Fortunately, there are methods which are available to correct for these problems when data are pooled.

The general term for a number of possible approaches to these problems is termed "generalized least squares" or GLS. The approach which we have used is the so-called "variance-components" model. We have assumed that year-specific effects can be captured using individual year dummy variables (with 1971 as the reference year), and we have estimated a two component variance model. For the remainder of this chapter we will refer to this approach as GLS.

Analysis of our data revealed that we had complete (i.e., eleven years of) salary information for only 220 cities. We also discovered that we had 6588 observations from a total of 915 cities during the 1971-81 period (i.e., about 600 cities per year). In other words, our panel of city-specific data contains missing observations in each year. This required a slight modification of the usual GLS procedure to account for missing observations (see Carliner, 1982).

Results. The OLS and GLS percentage coefficients for CBA and ARB in both the minimum and maximum salary time series equations are presented in Table III-9 (the complete time series results are presented in Appendix III-6). These coefficients indicate that both minimum and maximum salaries are positively associated with bargaining and arbitration's

TABLE III-9

1971-81 TIME SERIES ANALYSIS OF ARBITRATION'S AVAILABILITY<sup>a</sup>

	OLS		GLS	
	<u>CBA</u> <sup>b</sup>	<u>ARB</u> <sup>c</sup>	<u>CBA</u> <sup>b</sup>	<u>ARB</u> <sup>c</sup>
lnMINSAL	6.41*** (17.57)	2.53*** (5.96)	5.09*** (13.48)	1.73*** (4.04)
lnMAXSAL	5.03*** (13.97)	4.01*** (9.44)	1.89*** (5.04)	1.41*** (3.34)

<sup>a</sup>These percentage coefficients were calculated with the formula  $\% = e^B - 1$ , where B is the log coefficient.

<sup>b</sup>Of the 6588 observations, 3797 (57.64 percent) were in CBA cities.

<sup>c</sup>Of the 6588 observations, 1616 (24.53 percent) were in ARB cities.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level, (two-tailed test).

availability. In the OLS results, minimum salary levels in bargaining cities are an average of 6.41 percent higher each year than in cities that do not bargain, *ceteris paribus*, and minimum salaries are an average of 2.53 percent higher each year in arbitration cities than in cities where arbitration is not available. Similarly, maximum salaries are an average of 5.03 percent higher and 4.01 percent higher each year in bargaining and arbitration cities respectively after controlling for other influences.

The GLS results exhibit the same pattern as the OLS results but at lower levels of magnitude. For example, minimum salaries are an average of 5.09 percent higher and 1.73 percent higher each year in bargaining and arbitration cities respectively after controlling for the influences of other factors. Similarly, maximum salaries are an average of 1.89 percent higher and 1.41 percent higher each year in bargaining and arbitration cities respectively, *ceteris paribus*. We believe that these GLS results are preferable for the reasons stated above.

These pooled results are consistent with both the aggregate and disaggregate cross section results in Tables III-4 and III-8. First, both sets of results indicate that bargaining has had a stronger influence on minimum salaries than on maximum salaries. Second, both sets of results indicate that bargaining has had a somewhat stronger influence on entry and top step salaries than has arbitration. Third, in both sets of results the CBA and ARB coefficients are positive and significant. Fourth, the rather modest ARB coefficients in the GLS analysis (i.e., less than two percent) are consistent with the state-specific coefficients in the Table III-8 cross section analysis which indicated that in most

arbitration states arbitration is not the cause of higher salaries.

In sum, our OLS results indicate that bargaining and arbitration have positive associations with police salaries. Our GLS results also indicate significantly positive associations between bargaining and arbitration and our two salary measures, but at noticeably smaller levels of magnitude. In other words, our GLS results, when combined with our disaggregate OLS cross section results, indicate that arbitration's causal influence on higher police salaries may be rather modest overall and even nonexistent in several arbitration states.

Other influences. As noted, the complete pooled results are presented in Appendix III-6. These coefficients are consistent with the results obtained for our control variables in the year-by-year cross section analyses. For example, in the GLS analyses maximum salaries were positively and significantly associated with city size, city residents' income, city manufacturing wage rates, the city manager form of government, city crime rates, and city location in Western states (compared to North Central cities). Similarly, salaries were negatively and significantly lower in cities outside of metropolitan areas and in Southern cities. In fact, Southern location was one of the very strongest influences on salaries in our GLS time series analysis; maximum salaries in Southern cities averaged 15 percent below those in North Central cities during the 1971-81 years after controlling for other factors.

#### THE USE OF ARBITRATION

##### Model

In the preceding section we saw that the existence of an arbitration law in a state is associated with higher police salaries. In addition, we saw that the longer an arbitration law remains in effect, the greater the impact that arbitration's availability seems to have on police pay. However, we cannot yet interpret these results with great precision, for the analyses in the preceding section do not allow us to differentiate between those cities which have and have not used the arbitration process to determine police salaries. Although we know that in any given year a minority of cities actually have their police salaries set by arbitrators, it is possible that these arbitration awards could exert a disproportionate influence on average police pay levels in arbitration states. As a result, it is necessary to simultaneously measure the availability of an arbitration procedure and the use of an arbitration procedure in order to determine how these two phenomena have affected police pay.

In addition, our discussion of the impact of arbitration's availability in the preceding section may have overemphasized the effect of the existence of an arbitration statute and concomitantly underemphasized the importance of actually using the arbitration procedure. One possible interpretation of our previous discussion might have been that police unions need never use arbitration to obtain higher salaries; instead, they only need an arbitration law on the books in their respective states. This interpretation is misleading, for an arbitration procedure which is never used would not cause employers to offer the higher salaries identified in the preceding section. Further, we know that a

certain amount of arbitration use exists in arbitration states every year, and thus there is no such thing as arbitration availability completely divorced from arbitration use. Similarly, employers and unions in arbitration states need at least a few bargaining units to use arbitration in order to be able to determine how arbitration changes the negotiating environment. Consequently, we need to simultaneously measure arbitration's availability and use to insure that the availability effects identified earlier are not somehow masking a use effect.

In the analyses which follow, we have used the same salary determination model we introduced earlier in this chapter, except that we have included an arbitration availability variable (ARB) and an arbitration use variable (ARBUSE). Both of these are dummy variables; ARB has a value of one for each city which is located in an arbitration state and zero for each city located in a state with no police arbitration law, and ARBUSE has a value of one for each city which received an arbitration award covering a particular year and zero for each city which did not use arbitration in that year. As a result, for each year in our time period the ARB and ARBUSE variables will accurately differentiate between those cities which had arbitration available but did not use it and those cities which had arbitration available and did use it. In turn, the combined ARB and ARBUSE coefficients will enable us to determine the separate impacts that arbitration's availability and arbitration's use have had on police salaries.

In order to be certain of our results, we measure the ARB and ARBUSE impacts in several different ways. First, we measure these two impacts across all the cities in the sample (for which we have the necessary data). Second, because only bargaining cities can use arbitration, we

measure these two impacts among only the cities in our sample which bargain with police. Third, we attempt to capture the effects of prior arbitration use by measuring ARB and ARBUSE with a prior arbitration use variable also included in the measurement equation. This approach will enable us to determine if the current use and prior use of arbitration have different effects.

Following Farber and Katz (1979), we hypothesize that the availability of arbitration (ARB) will continue to have a positive and significant impact on police pay but that the use of arbitration (ARBUSE) will not have a statistically significant impact in either direction. We make this prediction because the availability of arbitration determines the environment within which cities and police unions negotiate. As a result, the availability of arbitration will affect the terms of negotiated agreements as much as the terms of arbitration awards, for police unions obviously will not agree to negotiated terms which are less favorable than what they expect to obtain from arbitrators' awards. Consequently, there should be no significant differences between negotiated and arbitrated police salaries in those states with arbitration laws. However, for the reasons explained in the preceding section, the availability of arbitration in certain states will cause the average police salaries in those states to be higher than the average salaries in other states, *ceteris paribus*.

Finally, our 1971-74 arbitration use results should be interpreted cautiously. We obtained these data from our questionnaire survey form (see Appendix II-1), and our inspection of these responses indicates that respondents may have been less diligent (or had less perfect

memories) when reporting their use of arbitration during 1971-74 than during 1975-81. There seems to be no noticeable discontinuities in our results between 1971-74 and 1975-81, but nevertheless we have more confidence in our arbitration use data during the latter seven years than during the first four years.

#### Results

All cities. The first half of Table III-10 presents the results for the all cities sample (i.e., all the cities for which we collected the necessary data). The results in this part of the table confirm the results presented earlier in Table III-4: bargaining by itself (CBA) has a positive impact on police pay, though this impact has faded in recent years (especially on maximum salaries); and the availability of arbitration (ARB) continues to have a strong and positive impact on police pay even after controlling for those cities which actually used arbitration. For example, during 1979-81 ARB had a 5-6 percent impact on entry salaries and a 7-9 percent impact on top step salaries, while ARBUSE had no impact.

Expressed another way, the nonsignificant ARBUSE coefficients indicate that there is almost no statistically significant difference in any year between police salaries in arbitration states which are negotiated and those which are arbitrated. In fact, of the 22 ARBUSE coefficients (one for minimum pay and another for maximum pay in each of 11 years), 13 are negative, 9 are positive, and only two are statistically significant at our weakest significance level (0.10). In sum, (1) the mixture of positive and negative nonsignificant ARBUSE coefficients indicates that

TABLE III-10

PERCENTAGE IMPACTS OF BARGAINING,  
ARBITRATION AVAILABILITY, AND ARBITRATION USE ON POLICE SALARIES<sup>a</sup>

YEAR	N	All Cities						Bargaining Cities Only						
		lnMINSAL			lnMAXSAL			lnMINSAL			lnMAXSAL			
		CBA	ARB	ARBUSE	CBA	ARB	ARBUSE	CBA	ARB	ARBUSE	CBA	ARB	ARBUSE	
1971	586	5.34*** (4.81)	1.54 (0.78)	1.57 (0.38)	4.85*** (4.39)	3.33* (1.67)	0.45 (0.11)	210	-0.12 (0.54)	0.22 (0.10)	4.71 (1.15)	-0.30 (1.63)	2.74 (1.41)	3.36 (0.97)
1972	538	6.37*** (5.31)	3.47* (1.90)	-4.24 (0.92)	4.94*** (4.14)	5.55*** (3.02)	-2.27 (0.48)	232	0.007 (0.03)	4.75** (2.27)	-5.92 (1.31)	-0.48** (2.39)	6.83** (3.47)	-3.92 (0.92)
1973	535	5.76*** (4.92)	3.56** (2.55)	-3.15 (0.86)	3.98*** (3.45)	5.65*** (3.99)	-2.08 (0.56)	256	0.04 (0.22)	2.84* (1.70)	-3.92 (1.10)	0.16 (0.91)	4.08*** (2.63)	-1.88 (0.54)
1974	536	4.29*** (3.72)	3.56** (2.53)	1.31 (0.43)	3.87*** (3.26)	5.34*** (3.59)	-3.63 (1.17)	268	0.10 (0.60)	5.55*** (3.62)	3.15 (1.08)	-0.10 (0.55)	6.50*** (4.22)	-2.86 (1.00)
1975	578	5.87*** (5.05)	4.29*** (3.02)	-0.79 (0.32)	5.42*** (4.64)	3.51** (2.46)	-4.40* (1.77)	326	0.06 (0.37)	4.92*** (3.24)	0.16 (0.06)	-0.08 (0.47)	3.56** (2.31)	-3.82 (1.58)
1976	628	7.36*** (6.06)	3.42** (2.40)	1.48 (0.65)	5.65*** (4.74)	3.97*** (2.81)	-1.88 (0.84)	380	0.17 (0.99)	4.04** (2.54)	1.61 (0.72)	0.27 (1.61)	3.56** (2.34)	-1.98 (0.92)
1977	611	8.57*** (6.51)	3.01* (2.95)	-1.69 (0.12)	7.36*** (5.57)	5.13*** (3.50)	-0.85 (0.06)	385	0.39** (1.52)	4.97*** (2.76)	-2.37 (0.12)	0.18 (1.84)	6.93*** (3.19)	-1.19 (0.06)
1978	621	8.83*** (6.51)	5.04*** (2.95)	0.25 (0.12)	7.10*** (5.57)	5.70*** (3.50)	-0.12 (0.06)	393	0.26 (1.52)	4.99*** (2.76)	0.24 (0.12)	0.29* (1.84)	5.38*** (3.19)	0.11 (0.06)
1979	596	3.57*** (2.92)	6.00*** (3.86)	3.59* (1.74)	2.34* (1.89)	7.12*** (4.48)	0.11 (0.05)	384	0.27* (1.81)	7.58*** (4.44)	3.12 (1.50)	0.29 (1.33)	7.98*** (4.76)	-0.34 (0.17)
1980	592	5.45*** (4.04)	5.11*** (2.94)	2.12 (0.91)	2.50* (1.87)	8.16*** (4.62)	0.43 (0.29)	393	0.31* (1.95)	4.90*** (2.62)	2.10 (0.91)	0.25 (1.53)	7.18*** (3.81)	0.45 (0.20)
1981	553	4.58*** (3.33)	5.99*** (3.37)	-2.76 (1.22)	1.60 (1.21)	9.05*** (5.18)	-0.99 (0.45)	367	0.43*** (2.86)	6.24*** (3.42)	-2.81 (1.27)	0.34** (2.37)	8.76*** (5.06)	-1.35 (0.65)

<sup>a</sup>The percentage impacts reported were calculated from the OLS coefficients using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

there are no consistent differences between negotiated and arbitrated entry and top step patrol officer salaries, and (2) the fact that the ARB coefficients retain their robustness in the presence of the ARBUSE coefficients confirms that it is the availability rather than the use of arbitration which exerts the upward pressure on police pay.

Methodologically, it is important to note that our ARBUSE variable measures those cities which received an arbitration award on any issue for a particular year. We used this inclusive measure because most arbitration awards involve salaries and because we wanted to be sure our ARBUSE variable captured any indirect effect on salaries of using arbitration on nonsalary issues (e.g., a police union might use all its bargaining chips to negotiate to a conclusion on salaries but then go to arbitration on other issues). As a result, our inclusive definition of ARBUSE should ensure that we have not missed some of ARBUSE's impact by defining the term too narrowly.

Bargaining cities. Because only unionized police forces may use arbitration, we re-analyzed our data for a smaller sample composed of cities which bargain with the police. In this analysis we used the same arbitration availability (ARB) and arbitration use (ARBUSE) variables as in the all cities sample. We also included the length of the bargaining relationship variable (CBAGE) described earlier to ensure that our ARB and ARBUSE coefficients did not accidentally pick up any salary impacts due to bargaining longevity.

The second half of Table III-10 presents the results of our bargaining cities sample. These results are highly similar to the all cities results and confirm that it is the availability rather than the use of

arbitration which is associated with higher police salaries: in both samples the ARB coefficients are about equally robust (and retain the robustness identified earlier in Table III-4), while the ARBUSE coefficients are both positive and negative but almost never significant. In addition, the inclusion of the ARBUSE term in the bargaining cities sample substantially reduces the strength of the CBAGE coefficients compared to their magnitude in Table III-5. In recent years the CBAGE coefficients in Table III-10 are uniformly positive, but most of them have become nonsignificant with the addition of the ARBUSE term.

The Table III-10 results provide strong and convincing support for the Farber and Katz (1979) hypothesis that the availability of arbitration alters the negotiating environment in a manner which affects negotiated and arbitrated outcomes equally. The multi-year pattern of nonsignificant ARBUSE coefficients across a sample of about 600 cities, including almost 400 bargaining cities, allows us to offer this conclusion with considerable confidence.

Current use vs. prior use. The analyses presented in Table III-10 examined only the current use of arbitration, i.e., were salaries in a particular year affected by the use of arbitration in that same year? As a result, the Table III-10 results do not permit us to say anything about how the use of arbitration in years prior to, say, 1977 might have affected 1977 salaries.

To address this issue we constructed two variables. The first is what we call prior arbitration (PARB), and it is a dummy variable with a value of one for each city which used arbitration on any issue in any year prior to the year in question and a value of zero for each city

which had never used arbitration prior to the year in question. As can be seen from this definition, over time more and more cities will fall into this PARB category, for once a city has used arbitration it will always have a PARB value of one. As a result, over time the arbitration availability (ARB) and prior arbitration (PARB) variables will increasingly overlap. To cope with this problem, we constructed a past years of arbitration use variable (PASTUSE), which is a continuous variable which measures the number of past years in which a city was covered by arbitration awards. Specifically, PASTUSE measures years of arbitral awards coverage rather than number of arbitration awards; thus a city with a three-year award will be scored the same as another city which received three one-year awards covering those three years. PASTUSE is a much more precise measure of previous arbitration use than PARB, for the PASTUSE term allows us to identify the salary impact, if any, of each additional year of previous arbitral use. In contrast, the PARB dummy variable cannot differentiate among different amounts of previous use. Of course, the PARB and PASTUSE terms will be used in separate equations.

To ensure that the PARB and PASTUSE terms do not measure and report any other arbitration effects, we have included an arbitration longevity term (ARBAGE) and a current arbitration use term (ARBUSE) in our regression equation. The ARBAGE term is a continuous variable which measures the number of years that an arbitration statute has existed, and we included it to ensure that the PARB and PASTUSE terms did not pick up and report any effects due to the longevity of arbitration's availability. We included the ARBUSE term to be able to differentiate any current use and prior use effects. In addition, we included all the other independent

variables identified in our salary determination model earlier in this chapter.

Table III-11 presents the results of the all cities sample, and Table III-12 presents the results for our bargaining cities sample (which reflects the fact that only bargaining cities can use arbitration). The first half of each table reports the effects on minimum salaries, and the second half of each table reports the effects on maximum salaries. Because the results across the two samples are so consistent, we will examine the two tables together.

The coefficients in both tables show that the length of time that arbitration has been available has a much greater impact on police salaries than does either current use or previous use. This conclusion is most apparent for maximum salaries in both samples: the PARB and PASTUSE coefficients are never statistically significant, and the ARBUSE coefficients are rarely significant. When the ARBUSE coefficients are significant (five of 44 times), they are negative, which means that in those years the cities which used arbitration paid salaries below the salaries paid elsewhere. Most apparent, though, are the positive and highly significant ARBAGE coefficients for each year after 1971. These ARBAGE coefficients indicate that each additional year of arbitration's availability is associated with an approximate one percent increase in maximum police salaries, *ceteris paribus*. When combined with the current use and previous use coefficients, the results in the two tables indicate that the actual use of arbitration has almost no impact on maximum salaries compared to the impact associated with the length of arbitration's availability.

TABLE III-11  
 PERCENTAGE IMPACTS OF ARBITRATION AVAILABILITY,  
 CURRENT USE, AND PRIOR USE ON POLICE SALARIES<sup>a</sup>  
 (all cities sample)

YEAR	N	InMINSAL						InMAXSAL					
		Equation 1			Equation 2			Equation 1			Equation 2		
		ARBAGE	ARBUSE	PAPB	ARBAGE	ARBUSE	PASTUSE	ARBAGE	ARBUSE	PAPB	ARBAGE	ARBUSE	PASTUSE
1971	586	0.23 (0.29)	0.71 (0.14)	3.30 (0.53)	0.20 (0.26)	0.28 (0.06)	4.39 (0.90)	1.17 (1.47)	1.71 (0.35)	-2.28 (0.37)	1.15 (2.44)	1.23 (0.25)	-0.93 (0.93)
1972	538	1.06 (1.57)	-5.50 (1.03)	1.07 (0.25)	0.96 (1.51)	-6.19 (1.15)	1.22 (0.60)	1.85*** (2.86)	-2.99 (0.56)	-1.09 (9.26)	1.94*** (3.05)	-1.94 (0.36)	-1.62 (0.80)
1973	535	1.23*** (2.67)	-2.31 (0.56)	-4.19 (0.98)	1.21*** (2.68)	-2.13 (0.51)	-2.49 (1.02)	1.68*** (3.62)	-2.27 (0.55)	-1.60 (0.38)	1.74*** (3.82)	-1.49 (0.36)	-2.02 (0.82)
1974	536	0.59 (1.46)	2.19 (0.71)	-3.39 (0.84)	0.60 (1.53)	2.40 (0.78)	-2.16 (1.13)	1.27*** (3.00)	-3.40 (1.05)	-2.64 (0.63)	1.28*** (3.10)	-3.24 (1.00)	-1.66 (0.83)
1975	578	0.63* (1.79)	-0.99 (0.37)	2.05 (0.71)	0.67* (1.92)	-0.93 (0.35)	0.71 (0.48)	0.91*** (2.59)	-5.28* (1.95)	-1.73 (0.06)	0.88** (2.52)	-5.42** (1.99)	0.32 (0.21)
1976	628	0.51 (1.59)	0.74 (0.31)	2.60 (1.13)	0.62* (1.95)	0.73 (0.30)	0.49 (0.46)	1.13*** (3.54)	-3.74 (1.58)	0.21 (0.09)	1.26*** (3.94)	-3.44 (1.44)	-0.71 (0.68)
1977	611	0.70** (2.46)	-4.18** (1.99)	4.61** (2.18)	0.84*** (2.97)	-3.63* (1.75)	1.17 (1.33)	0.99*** (3.35)	-2.00 (0.93)	1.27 (0.59)	1.13*** (3.87)	-1.67 (0.79)	-0.31 (0.34)
1978	621	0.97*** (3.73)	-0.80 (0.39)	2.26 (1.08)	0.92*** (3.62)	-0.97 (0.47)	1.31* (1.67)	1.12*** (4.58)	-0.70 (0.36)	0.77 (0.39)	1.14*** (4.76)	-0.60 (0.31)	0.18 (0.24)
1979	596	0.56*** (2.63)	3.37 (1.55)	2.44 (1.37)	0.61*** (2.88)	3.89* (1.84)	0.63 (0.99)	0.83*** (3.80)	0.40 (0.18)	1.47 (0.82)	0.92*** (4.26)	1.09 (0.51)	0.00 (0.01)
1980	592	0.48** (2.26)	0.75 (0.33)	5.39*** (3.01)	0.52** (2.49)	-0.18 (0.08)	1.72*** (2.77)	0.92*** (4.32)	-0.23 (0.10)	1.81 (1.02)	0.90*** (4.29)	-0.79 (0.33)	0.82 (1.31)
1981	553	0.58*** (2.74)	-2.86 (1.28)	2.63 (1.36)	0.55*** (2.71)	-3.34 (1.48)	1.15** (2.00)	0.80*** (3.85)	-0.59 (0.27)	1.78 (0.95)	0.89*** (4.47)	-0.61 (0.28)	0.11 (0.21)

<sup>a</sup>The percentage impacts reported were calculated from the OLS coefficients using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

TABLE III-12  
 PERCENTAGE IMPACTS OF ARBITRATION AVAILABILITY,  
 CURRENT USE, AND PRIOR USE ON POLICE SALARIES<sup>a</sup>  
 (bargaining cities sample)

YEAR	N	lnMINSAL						lnMAXSAL					
		Equation 1			Equation 2			Equation 1			Equation 2		
		ARBAGE	ARBUSE	PARB	ARBAGE	ARBUSE	PASTUSE	ARBAGE	ARBUSE	PARB	ARBAGE	ARBUSE	PASTUSE
1971	210	0.52 (0.85)	3.54 (0.73)	4.94 (0.85)	-0.53 (0.59)	2.70 (0.56)	5.27 (1.15)	0.75 (0.98)	4.84 (1.18)	-1.91 (0.39)	0.74 (0.98)	4.92 (1.19)	-1.60 (0.42)
1972	232	1.53** (2.13)	-8.87* (1.67)	2.49 (0.60)	1.46** (2.09)	-9.77* (1.83)	2.08 (1.08)	2.37*** (3.53)	-7.15 (1.46)	1.76 (0.46)	2.55*** (3.88)	-5.87 (1.20)	-0.60 (0.33)
1973	256	1.09** (2.14)	-2.99 (0.74)	-4.97 (1.16)	1.04** (2.09)	-2.98 (0.74)	-2.63 (1.09)	1.33*** (2.79)	-2.31 (0.61)	-1.41 (0.36)	1.37*** (2.95)	-1.81 (0.48)	-1.54 (0.69)
1974	268	1.01** (2.29)	3.95 (1.30)	-5.04 (1.36)	1.01** (2.34)	4.24 (1.39)	-2.75 (1.46)	1.57*** (3.57)	-2.83 (0.94)	-3.35 (0.85)	1.56*** (3.84)	-2.64 (0.88)	-1.79 (0.96)
1975	326	0.70* (1.89)	-0.07 (0.03)	2.26 (0.80)	0.76** (2.09)	0.13 (0.05)	0.43 (0.29)	0.97*** (2.62)	-4.72* (1.80)	-0.30 (0.11)	0.95*** (2.58)	-4.78* (1.82)	0.03 (0.02)
1976	380	0.55 (1.59)	0.94 (0.40)	1.10 (1.50)	0.69** (2.01)	0.92 (0.39)	0.61 (0.58)	1.12*** (3.37)	-3.90* (1.72)	1.01 (0.47)	1.27*** (3.86)	-3.60 (1.57)	-0.65 (0.64)
1977	385	0.92*** (3.05)	-4.37** (2.08)	4.81** (2.27)	1.10*** (3.63)	-3.74* (1.79)	0.97 (1.10)	1.27*** (4.43)	-2.09 (1.06)	1.29 (0.65)	1.44*** (5.05)	-1.74 (0.90)	0.49 (0.59)
1978	393	0.99*** (3.71)	-0.84 (0.42)	2.53 (1.25)	0.97*** (3.68)	-0.90 (0.46)	1.20 (1.56)	1.14*** (4.60)	-0.58 (0.31)	0.96 (0.51)	1.19** (4.87)	-0.36 (0.20)	0.03 (0.04)
1979	384	0.65*** (2.86)	3.05 (1.39)	2.88 (1.61)	0.72*** (3.19)	3.81* (1.79)	0.63 (0.97)	0.89*** (4.01)	0.04 (0.02)	1.75 (1.00)	0.99*** (4.47)	0.79 (0.39)	0.06 (0.09)
1980	393	0.44** (1.97)	0.81 (0.36)	5.93*** (3.38)	0.52** (2.35)	0.00 (0.00)	1.66*** (2.66)	0.80*** (3.60)	-0.17 (0.07)	2.13 (1.22)	0.80*** (3.63)	0.65 (0.28)	0.80 (1.28)
1981	367	0.58** (2.64)	-2.91 (1.33)	3.26* (1.72)	0.58*** (2.79)	-3.40 (1.54)	1.16** (2.05)	0.75*** (3.62)	-1.03 (0.50)	2.22 (1.24)	0.85*** (4.28)	-1.09 (0.53)	0.22 (0.42)

<sup>a</sup>The percentage impacts reported were calculated from the OLS coefficients using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

The minimum salary coefficients in the two tables show a similar pattern: the ARBAGE coefficients are usually positive and statistically significant, the current use (ARBUSE) coefficients are rarely significant (and when the ARBUSE term is significant it is usually negative), and the previous use coefficients (PARB and PASTUSE) are usually insignificant. In other words, the length of arbitration's availability has a much stronger effect upon minimum salaries than does either current use or prior use: the ARBAGE coefficients in the two tables indicate that each additional year of arbitration's availability is associated with an increase in minimum salaries between one-half and one percent, ceteris paribus.

However, the prior use of arbitration appears to have exerted somewhat more influence on entry than on top step salaries during the 1977-81 period. During those years some of the PARB and PASTUSE coefficients are positive and significant, which indicate that minimum salaries have been affected by using arbitration in prior years. For instance, the 1981 results in both tables indicate that entry salaries are a bit more than one percent higher for each additional year that arbitration was used during the years prior to 1981. Taken together, these two tables present some of the strongest evidence in this report that arbitration's availability has a stronger influence on police salaries than does arbitration's use. Neither minimum nor maximum salaries seem to be influenced by current use, maximum salaries are not influenced by previous use, and minimum salaries are only modestly associated with previous use. In contrast, both minimum and maximum salaries are positively and consistently associated with each additional year that

arbitration is available.

The results in Tables III-11 and III-12 also confirm the results in Tables III-6 and III-7. The earlier tables showed that arbitration longevity and police salaries are positively associated, and Tables III-11 and III-12 show the same thing. All four of these tables suggest that arbitration's impact on police salaries grows rather than shrinks over time. In other words, arbitration's impact on salaries may not be apparent in the years immediately following the passage of an arbitration law but may become increasingly apparent as the law acquires increased vintage. Olson (1980) found a similar result regarding the impact of arbitration's availability on firefighter salaries during the 1972-77 period, and the similarity between his results and ours enhances the plausibility of our findings.

#### THE LEVELLING EFFECT

There has been considerable speculation over the years that arbitration would cause the salaries in a particular state to become more similar over time, or "regress to the mean" (Stern, et. al., 1975). This regression to the mean, or "levelling effect" seemed likely because arbitration was a process by which salaries could be artificially manipulated in a manner which would overcome some of the dispersion caused by market forces. This manipulation would occur because of the emphasis that unions, managements, and arbitrators give to the principle of pay comparability when presenting their salary arguments and salary awards. Further, because each city in a state tends to use the other

cities in that state for comparison purposes, over time these mutual comparisons would cause salaries in a state to become more similar. In addition, comparability is usually a less contentious salary adjustment rationale than either inflation or ability to pay. Accordingly, if police salaries in arbitration states actually are adjusted as just described, we should find that the salary dispersion in arbitration states has become smaller over time when compared to the dispersion in nonarbitration states.

One straightforward method for measuring this dispersion over time is the computation of coefficients of variation for each state for each year. These coefficients are a measure of dispersion around the average salary in each state. They are computed by dividing the state standard deviation of salaries for a particular year by the state average salary for that year (see the data in Table III-2). As a result, the coefficient of variation is a better measure of dispersion than the standard deviation, for it controls for differences in average salaries across states (e.g., a standard deviation of \$2,000 around a state average salary of \$25,000 indicates a much narrower dispersion than does an identical \$2,000 standard deviation around a state average salary of \$10,000).

Table III-13 presents the coefficients of variation for minimum and maximum salaries for eleven arbitration states plus an average figure for the nonarbitration states. Each state needed salary data from at least eight cities to be included in this analysis. The coefficients in the rows marked "varies" are those for either 1971 or the last year prior to the implementation of arbitration, as noted in the parentheses, and these are our "before" coefficients. Our "after" coefficients were

TABLE III-13  
COEFFICIENTS OF VARIATION

<u>YEAR</u>	<u>Nonarbitration States</u>	<u>MI</u>	<u>PA</u>	<u>RI</u>	<u>WI</u>	<u>MN</u>	<u>WA</u>	<u>NY</u>	<u>MA</u>	<u>CT</u>	<u>IA</u>	<u>NJ</u>
<u>MINIMUM SALARIES</u>												
Varies	.1038 (1971)	.1260 (1971)	.1234 (1971)	.0900 (1971)	.0854 (1971)	.0944 (1972)	.0956 (1972)	.1195 (1973)	.0982 (1973)	.0976 (1974)	.0652 (1975)	.1362 (1976)
1981	.1011	.1239	.1552	.1139	.1186	.1052	.0885	.1456	.0885	.1022	.0850	.1367
<u>MAXIMUM SALARIES</u>												
Varies	.1262 (1971)	.1077 (1971)	.0869 (1971)	.0729 (1971)	.0745 (1971)	.0804 (1972)	.1008 (1972)	.1538 (1973)	.0747 (1973)	.0807 (1974)	.0751 (1975)	.0763 (1976)
1981	.1344	.0852	.0715	.0266	.0871	.0984	.0726	.1916	.0802	.0801	.0924	.0798

C.V. =  $\frac{S.D.}{\bar{X}}$  (i.e., the coefficient of variation is computed for each state by computing each state's standard deviation and then dividing that standard deviation by the average salary in that state). Each state needed salary data from at least eight cities to be included in this analysis.

calculated for 1981 in all states.

These coefficients are easy to interpret. Large coefficients (e.g., maximum salaries in New York) indicate wide dispersions in salaries within the state while small coefficients (e.g., maximum salaries in Rhode Island) indicate narrow dispersions in salaries across cities in the state. As this implies, the smaller the coefficient of variation the less the dispersion of salaries.

Looking at minimum salaries, we see that the dispersion in nonarbitration states essentially remained unchanged over time. In the arbitration states, only Washington and Massachusetts experienced a non-trivial reduction in dispersion, and in neither case was the reduction very large. In all the other arbitration states, the dispersion of minimum salaries remained unchanged (Michigan, Connecticut, New Jersey), increased moderately (Minnesota), or increased substantially (Pennsylvania, Rhode Island, Wisconsin, New York, Iowa). In addition, the 1981 minimum salary coefficients in arbitration states were larger than the nonarbitration state average in eight of eleven instances. Clearly, there has been no levelling effect on minimum salaries in arbitration states during the 1971-81 period.

Maximum salaries tell a similar though somewhat confusing story. In 1981, the coefficients in all arbitration states except New York were noticeably smaller than the average coefficient across the nonarbitration states. However, the same pattern existed in earlier years, so it is not clear that any overall trend occurred. It is true that the maximum salary dispersion narrowed in Michigan, Pennsylvania, Rhode Island, and Washington. However, it remained unchanged in Connecticut and New

Jersey, and increased in Wisconsin, Minnesota, New York, Massachusetts, and Iowa. As a result, it appears that arbitration may have contributed to a narrowing of maximum salary dispersion in a few states, but there certainly is no uniform trend in this direction across arbitration states.

When we combine the minimum and maximum salary results, we are tempted to conclude that no overall levelling effect exists, though there may be a few state-specific exceptions. However, we believe a more refined analysis is necessary before drawing a final conclusion. We performed such an analysis by taking the 467 cities for which we had 1971 and 1981 salary data, computing the percentage difference between each city's salary and the statewide average salary in both 1971 and 1981, and then regressing the 1981 salary percentage difference on the 1971 salary percentage difference plus various bargaining and arbitration terms. More specifically, we performed OLS multiple regression analyses of the following form:

$$\begin{aligned} \text{MAXDIFF81} &= B_0 + B_1 \text{MAXDIFF71} + B_2 \text{CBAYRS} + \\ & B_3 \text{ARBYRS} + B_4 \text{MAXDIFF71} \cdot \text{CBAYRS} \\ & + B_5 \text{MAXDIFF71} \cdot \text{ARBYRS} + e \end{aligned}$$

where

$$\begin{aligned} \text{MAXDIFF81} &= \text{The percentage difference between a city's} \\ & \text{maximum patrol officer salary and the statewide} \\ & \text{average patrol officer maximum salary in 1981,} \\ & \text{computed as} \\ & \frac{1981 \text{ CITY MAXSAL} - 1981 \text{ STATE AVERAGE MAXSAL}}{1981 \text{ STATE AVERAGE MAXSAL}} \end{aligned}$$

MAXDIFF71	=	The percentage difference between a city's maximum patrol officer salary and the statewide average patrol officer maximum salary in 1971, computed in the same manner
CBAYRS	=	number of years of bargaining in each city between 1971 and 1981
ARBYRS	=	number of years a city was covered by an arbitration statute between 1971 and 1981
MAXDIFF71·CBAYRS	=	an interaction term which combines a city's position in the state salary distribution in 1971 with the number of years the city has bargained between 1971 and 1981
MAXDIFF71·ARBYRS	=	an interaction term which combines a city's position in the state salary distribution in 1971 with the number of years the city was covered by an arbitration statute between 1971 and 1981

We also performed the same analysis for minimum salaries.

This type of analysis regresses the 1981 salary dispersion in each state on the 1971 salary dispersion in that same state, for each city's salary position is measured as the percentage difference from the average salary in that state. The inclusion of the bargaining and arbitration terms allows us to see if either bargaining or arbitration are associated with any changes in the dispersion of salaries between 1971 and 1981. In particular, if bargaining has contributed to a reduced or narrowed salary dispersion, one or both of the bargaining terms will have a negative and significant coefficient. The same holds true for arbitration. Expressed another way, insignificant coefficients on the bargaining and arbitration term will mean that neither bargaining nor arbitration has contributed to a statistically significant change in the statewide dispersion of police salaries between 1971 and 1981.

Our levelling effect regression results are presented in Table

III-14, and they tell the following story. First, the salaries in these 467 cities became less dispersed (or more levelled) in general between 1971 and 1981. Specifically, in 1981 there was only 71.21 percent of the 1971 dispersion of minimum salaries and only 77.44 percent of the 1971 dispersion of maximum salaries. (A coefficient of 1.00 (i.e., 100 percent) on the 1971 dispersion term would mean that in 1981 salaries were as far away from the state average salary as they were in 1971; a coefficient larger than 1.00 would mean that 1981 salaries were farther away from the state average salary in 1981 than they were in 1971; and a coefficient less than 1.00 means that in 1981 salaries were less dispersed than in 1971). At the same time, however, it is also true that by far the largest influence (in our model) on the 1981 salary dispersion was the 1971 salary dispersion.

Second, the dispersion of minimum salaries was not affected by bargaining or arbitration. Third, arbitration did not affect maximum salary dispersion. The bargaining term by itself was not significant, but the salary plus bargaining interaction term (MAXDIFF71·CBAYRS) was significantly negative. This coefficient means that for each year that a city bargained with the police between 1971 and 1981, the maximum patrol officer salary in that city became 2.81 percentage points closer to the statewide average salary in that state, compared to nonunion cities.

In practice, this means that over time unionized cities below the state average were pulled up toward the average, and unionized cities above the average were pulled down toward the average, at a slightly faster rate than occurred among nonunion cities. However, arbitration

TABLE III-14  
1981 CHANGES IN SALARY DISPERSION

	<u>MINSALDIFF81</u>	<u>MAXSALDIFF81</u>
Intercept	.0067 (1.01)	.0029 (0.52)
MIN or MAX $\rightarrow$ SALDIFF71	.7121*** (12.04)	.7744*** (18.27)
CBAYRS	-.0002 (0.18)	-.000001 (0.001)
MIN or MAX $\rightarrow$ SALDIFF71·CBAYRS	-.0175 (1.50)	-.0281*** (3.22)
ARBYRS	-.0004 (0.28)	.0004 (0.41)
MIN or MAX $\rightarrow$ SALDIFF71·ARBYRS	-.0138 (1.07)	.0138 (1.26)
F	42.9***	94.2***
R <sup>2</sup>	.310	.500
N	467	467

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level,

\*\*Significant at the .05 level,

\*Significant at the .10 level (two-tailed tests).

contributed nothing to this narrowing trend, at least on an aggregate basis across this sample of 467 cities. In addition, before anyone concludes that bargaining has had a dampening effect on pay increases, it is necessary to keep in mind that this Table III-14 analysis focusses only on the dispersion of salaries within states and says nothing at all about how salary levels differ between union and nonunion cities. In addition, our aggregate analysis does not allow us to pinpoint any dispersion changes in particular states.

Taken together, our levelling effect analyses suggest four conclusions. First, the overall dispersion of minimum and maximum salaries did not change very much between 1971 and 1981. Second, neither bargaining nor arbitration appeared to have an impact on the dispersion of minimum salaries. Third, arbitration had no overall effect on the dispersion of maximum salaries, but bargaining contributed to a modest levelling effect during the 1971-81 years. Fourth, maximum salaries have become more similar in a few arbitration states and more dispersed in others, which suggests that there are some unmeasured state characteristics which have strong influences on the dispersion of salaries in particular states.

To follow up on this last point, compare the maximum salary coefficients of variation (in Table III-13) for New York and Rhode Island. Over time the Rhode Island coefficient shrank while the New York coefficient grew. When we compare Rhode Island's geographical and economic compactness with New York's geographical and economic diversity, these two changes make sense. The Rhode Island data suggest that negotiators and arbitrators in that state peg the salaries in each Rhode Island city to

all the other cities in the state. In contrast, the New York data suggest that there are salary contours across groups of cities in that state which have become more (rather than less) divergent over time. Expressed another way, it means that negotiators and arbitrators in upstate cities do not peg their salaries to those paid in the downstate cities clustered around New York City, and vice versa. In other words, there may be clusters of cities within arbitration states whose police salaries have become more similar over time, but salaries across these city clusters may have become even more diverse over the years. Consequently, the levelling effect may be influenced more by these kinds of unmeasured state characteristics than by the presence of an arbitration statute.

#### LOCAL ARBITRATION

So far we have treated only those unionized cities covered by state arbitration laws as "arbitration cities." However, as shown in Table I-3, our survey responses identified 17 cities which by 1981 were covered by a local interest arbitration procedure (usually a city ordinance or a city charter provision). The vast majority of these cities are located in California and Ohio, for both states have experienced considerable police bargaining but have had only modest state regulation of this bargaining (the Meyers-Milius-Brown Act in California is a meet-and-confer statute with no mandatory impasse procedures, and until 1983 Ohio had no state law which directly regulated police bargaining). In our analyses up to this point we have treated these cities as unionized

but without access to arbitration. We did this because arbitration's availability via a state law makes arbitration part of the external negotiating environment of the cities in that state (i.e., arbitration is exogenous to each city), but when arbitration is available via a local procedure it is part of that city's internal characteristics (i.e., arbitration is an endogenous part of that city's union-management relationship). In practical terms, this difference means that a city has no control over the existence of a state arbitration law but has full control over a local arbitration arrangement (i.e., an arbitration procedure installed at the local level can be removed at the local level). As a result of this treatment, our earlier analyses may have produced results which are inaccurate.

To test this possibility we re-analyzed our 1980 and 1981 aggregate salary data in two ways. First, we sorted the local arbitration cities into the overall arbitration category (i.e., we sorted both local and state arbitration cities into the ARB category). Second, we sorted the local arbitration cities into their own separate category by including a local arbitration (LOCARB) dummy variable in the various estimating equations (i.e., cities with a local arbitration procedure had a value of one, all other cities had a value of zero).

The results of these analyses are presented in Table III-15. In this table the "local arbitration not considered" results, are the comparison figures similar to our earlier analyses where we excluded the local arbitration cities from the state arbitration groups; "local and state arbitration together" are the results from grouping the local and state arbitration cities together; and "local and state arbitration

separately" are the results from treating the local and state arbitration cities as two separate arbitration samples apart from the other cities. These Table III-15 results suggest that local arbitration arrangements are not as helpful to police unions as are state laws. In particular, the LOCARB coefficients are never statistically significant (though they are positive), which indicates that, in general, salary levels in local arbitration cities are no different from the salary levels in non-arbitration cities. However, salary levels in state arbitration cities remain just as high in this analysis as they were earlier.

These results should be interpreted cautiously, primarily because the number of local arbitration cities is so small (there were 16 such cities in 1980 and 17 in 1981, versus a sample of state arbitration cities more than 20 times larger). Further, a more precise analysis would compare the salaries in these local arbitration cities to the salaries of other non-arbitration cities only in those same states, and this precision might yield results which differ from the aggregate analyses performed here. Even with these caveats, though, our local arbitration analyses indicate that any salary advantages which might have accrued to police officers via local arbitration arrangements are too small for our statistical analyses to detect. In turn, this finding suggests that police unions should continue to lobby for state arbitration laws rather than local arbitration ordinances.

TABLE III-15  
PERCENTAGE IMPACTS OF LOCAL ARBITRATION  
ON POLICE SALARIES<sup>a</sup>  
(all cities sample)

YEAR	lnMINSAL				lnMAXSAL			
	N	CBA	ARB	LOCARB	N	CBA	ARB	LOCARB
LOCAL ARBITRATION NOT CONSIDERED								
1980	595	4.79*** (3.71)	2.85 (1.57)	—	592	2.13 (1.63)	7.49*** (3.95)	—
1981	560	4.02*** (3.05)	3.66* (1.95)	—	554	1.46 (1.13)	8.48*** (4.47)	—
LOCAL AND STATE ARBITRATION TOGETHER								
1980	595	4.79*** (3.68)	2.38 (1.36)	—	592	2.11 (1.60)	6.43*** (3.52)	—
1981	560	3.83*** (2.89)	3.93** (2.22)	—	554	1.24 (0.95)	7.95*** (4.41)	—
LOCAL AND STATE ARBITRATION SEPARATELY								
1980	595	4.82*** (3.70)	2.81 (1.54)	0.68 (0.16)	592	2.16 (1.64)	7.45*** (3.89)	0.68 (0.16)
1981	560	3.83*** (2.88)	3.90** (2.07)	4.06 (1.04)	554	1.30 (1.00)	8.70*** (4.55)	3.59 (0.90)

<sup>a</sup>The percentage impacts reported were calculated from the OLS coefficients using the formula: % impact =  $e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level;  
\*Significant at the .10 level (two-tailed tests).

## SUMMARY AND CONCLUSIONS

This chapter is overly long, partly because of the wealth of our salary information and our concomitant desire to manipulate it, but primarily to demonstrate that the calculated impacts of collective bargaining and interest arbitration upon police employment conditions can be heavily influenced by the research methods used to perform the analyses. For example, we saw that collective bargaining always appears to have a positive effect on salaries (in the 4-10 percent range), but the magnitude of this effect declines (by 1-2 percent) when one or more arbitration variables are added. Our results indicate, then, that any union wage impact study performed upon public sector occupations or jurisdictions covered by arbitration must control for the influence of arbitration; failure to do so means that any impact attributed to collective bargaining by itself might be overstated.

Similarly, the impact of arbitration's availability upon police salaries differs depending upon the degree of aggregation of the available data and upon whether cross section or time series analyses are used. When cross section analyses are performed upon our national sample of cities, arbitration has a significant and substantial impact upon salaries which became noticeably larger during the 1980-81 years. The aggregate impacts during these most recent years were six percent on entry salaries and nine percent on maximum salaries. However, when we begin to disaggregate the data with our arbitration longevity dummy variables (A12, A35, etc.) we see that the arbitration coefficients can change dramatically in magnitude from one year to the next. And when we disaggregate further with our state-specific analyses, we see that

arbitration had a clear upward impact on salaries only in Washington and possibly in Michigan and Pennsylvania. Most of the states which were high salaried states after arbitration was implemented also had high salaries before arbitration came along. Some arbitration states exhibited no salary advantage after arbitration's arrival, and one state (Iowa) actually demonstrated a salary disadvantage after arbitration arrived. These state-specific results indicate that arbitration frequently is associated with comparatively high police salaries but rarely causes these high salaries. As a result, arbitration may help protect high police salaries from being eroded, but it is unlikely to enable police officers in any state to leapfrog ahead of their peers in nonarbitration states on the police salary scale.

Similarly, our time series results are generally consistent with our cross section results, but the magnitude of our time series coefficients depends upon whether these coefficients were produced with ordinary least squares (OLS) or generalized least squares (GLS) multiple regression analysis. In both time series analyses the CBA and ARB coefficients are positive and significant, but the size of these coefficients is noticeably smaller in the GLS analyses. In fact, our time series results indicate that over time bargaining has had a stronger influence on salaries than has arbitration. Similarly, the arbitration effect in both time series analyses was smaller than the arbitration effect in the aggregate cross section analyses.

Along the same lines, the levelling effect results also depend partly upon method. The coefficients of variation suggest a levelling effect on maximum salaries in only a few arbitration states, while the

regression analyses indicate that arbitration has had no effect on salary dispersion across an aggregate sample. The regression analyses also suggest that bargaining by itself may contribute more to the levelling of maximum salaries than arbitration. Both the coefficient of variation and regression analyses indicate that neither bargaining nor arbitration has affected minimum salary dispersion. When compared with the maximum salary results, this suggests that negotiators and arbitrators may have handled minimum and maximum salaries a bit differently in their decision processes. Further, our state-specific analyses of the levelling effect indicate that unmeasured state characteristics have had much more influence upon the distribution of salaries in most states than arbitration has. Taken together, our levelling effect results indicate that there is little, if any, generic statewide levelling effect upon police salaries contributed by arbitration.

In contrast to this diversity of particular findings when assessing arbitration's availability, our analyses of the use of arbitration are quite consistent. No matter how we assess arbitration use -- current use in the year being examined or past use in prior years, holding constant arbitration's availability or arbitration's longevity, looking at minimum salaries or maximum salaries, or investigating the entire sample of cities or only the bargaining sample -- our analyses show that the actual use of arbitration is not systematically associated with salary levels. In other words, after controlling for the presence of arbitration (and other city and regional characteristics), police officers who have their salaries set by arbitrators experience no long run net advantage compared to police who negotiate their salaries at the

bargaining table. In short, our salary analyses strongly confirm the prediction (Farber and Katz, 1979) that the presence of arbitration may change the negotiating environment, but the actual use of arbitration will not lead to higher salaries than those negotiated in the same state.

However, this conclusion does not mean that there is no connection between arbitration's availability, arbitration's use, and salaries. Obviously, for arbitration to have any impact on anything it must be used from time to time. In that sense, it is very similar to the strike threat and strike use system. Most unions in most negotiations do not go on strike; instead, they use the threat of a strike to obtain their goals. However, some strikes must occur some of the time for the strike threat to have any credence. Similarly, in each police negotiating round in each arbitration state, some police unions need to use the arbitration procedure so that it will retain whatever impact it has on the negotiation process.

After having looked at police bargaining and police interest arbitration from several different angles, we conclude that: (a) bargaining was associated with a 4-10 percent increase in police salaries during the 1971-81 period, but this bargaining effect declined by 1-2 percentage points when arbitration was included in the measurement effort; (b) arbitration contributes some modest upward pressure on police salaries over and above the effect of bargaining by itself. This pressure ranges from a 1.4 percent salary advantage in our GLS time series analysis to a 3-9 percent salary advantage in our aggregate cross section analyses. More importantly, however, we also conclude that the strength of arbitration's

upward pressure on salaries appears to vary from state to state, and although arbitration is associated with comparatively high salaries in some states and comparatively moderate salaries in other states, arbitration very rarely appears to be the cause of these different salary levels. Consequently, arbitration's impact on salaries may be less uniform and more diverse than previously believed.

An examination of our arbitration states shows that most of them are located in what has come to be known as the Frost Belt (Massachusetts, Connecticut, Rhode Island, New York, New Jersey, Pennsylvania, Michigan, Wisconsin, Minnesota, and Iowa). These are the states that are the net losers in the migration of people, jobs, and concomitant economic growth to the Sun Belt states in the Southeast, Southwest, and West Coast. In turn, this migration suggests a relative erosion of the tax base in Frost Belt states, which implies that cities in those states should be hard-pressed to maintain the same relative police salary levels that they experienced in the early 1970s. However, our disaggregated cross section results show little or no diminution of these state-specific relative salary levels by 1981. These results suggest that arbitration's strongest salary impact may be the protection and maintenance of existing relative salary advantages rather than the creation of new advantages. Expressed another way, our results suggest that arbitration's greatest salary benefit for police officers may be the protection it provides against management attempts to hold down the rate of increase in salaries. However, this tentative conclusion needs to be assessed with more intensive and city-specific ability to pay data than we have been able

to obtain.\*

Having concluded that bargaining and arbitration matter, we also emphasize that "market" factors appear to matter more. For example, a Southern location systematically and relentlessly exerts very strong downward pressure on salaries; our results imply that if Southern police bargained and were protected by arbitration, they still would receive comparatively modest salaries. Along this geographical dimension, police in Western states are paid very well simply by virtue of their location, and police in cities outside of metropolitan areas are paid noticeably less than police in metropolitan areas. Similarly, larger cities, cities with higher crime rates, and city manager cities pay more than smaller cities, low crime rate cities, and cities headed by mayors. Rather unsurprisingly, wealthier cities (measured by per capita income and the price of residential housing) and cities which have high manufacturing wages pay more than poorer cities and those with low manufacturing wages. In other words, collective bargaining and interest arbitration appear to have independent and upward effects on police salaries, but there are a host of other factors which also influence these salaries, and some of these other factors are even more important than police labor relations arrangements.

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\*We did analyze our 1980 and 1981 maximum salaries with the inclusion of a TAXPOP variable which measured the annual amount of taxes received by a city on a per capita basis (i.e., total tax revenues divided by city population). The TAXPOP coefficients never even approached statistical significance, which means either that police salaries are not systematically associated with this measure of ability to pay or else that TAXPOP is not a good measure of ability to pay.

#### CHAPTER IV

#### FRINGE BENEFITS AND TOTAL COMPENSATION

In the preceding chapter we saw that bargaining and arbitration are associated with higher police salaries. However, a substantial share of the total cost of employing a police officer is devoted to nonsalary items: pensions, different kinds of insurance, paid time off, and a wide variety of pay supplements (uniform allowance, court appearance pay, standby pay, and so on). Accordingly, in this chapter we examine the impact of collective bargaining, the availability of arbitration, and the use of arbitration on police fringe benefits and total compensation.

#### MODEL

We will use the same analytical model we used in Chapter III to analyze salaries. This model worked well in our salary analyses and because the same factors which influenced salaries also should influence fringes, our model seems equally appropriate for the analysis of fringe benefits.

We will use three measures of fringe benefits as dependent variables in this chapter. First, we will use a dollar measure of the fringe benefits per police officer (FRNGCOP) paid in each city during each year of the 1971-81 period. We computed this measure by dividing each city's annual police "fringe benefits" aggregate figure by the number of police officers in that city. We obtained these data from the International City Management Association, who collected them via annual reports filed by U.S. cities. In this analysis, "fringe benefits" is defined (by the ICMA) as city contributions to retirement systems and to health, hospital,

disability, and life insurance.

Because this is a relatively narrow definition of fringe benefits, we also used as our second dependent variable a dollar measure of the total compensation per police department employee (TOTCOMP) paid in each city during each year of the 1971-81 period. We computed this measure by adding the total salaries and wages paid in a year to all police department employees (not just to sworn officers) with the fringe benefits paid to sworn officers and with the fringe benefits paid to other police department employees to produce a total personnel expenditure annual figure. We then divided this total personnel expenditure figure by the total number of police department employees (sworn and civilian) in that year to produce our TOTCOMP figure.

The FRNGCOP and TOTCOMP coefficients will enable us to assess the impact of bargaining and arbitration on the nonsalary portion of police compensation. We hypothesize that these bargaining and arbitration coefficients will be positive and significant. In addition, comparison of these coefficients with the salary coefficients obtained in the previous chapter will enable us to determine if police unions have had stronger or weaker impacts on fringes than on salaries. Because private sector unions (Freeman, 1981) and firefighter unions (Ichniowski, 1980) have pushed up fringes more than wages, we expect that police unions may have had stronger percentage impact on fringes than on salaries.

Our third dependent variable will not be a dollar measure, but instead a contract index measure of the "fringe benefits" and "pay supplements" provisions in police contracts. As explained more fully in the next chapter, we evaluated and scored all the police contracts

we collected with a contract scoring index which is designed to measure various contract provisions on a favorableness to the union scale. This contract index is divided into six subindices designed to group together the contract provisions of a roughly similar type (e.g., working conditions provisions, union security provisions, and so on). Two of these subindices are "fringe benefits" and "pay supplements."

The fringe benefits subindex includes 36 items which measure the extent to which police contracts provide retirement, insurance, paid time off (vacations, holidays, various leaves), and related benefits. The pay supplements subindex includes 27 items which measure the extent to which police contracts contain provisions which require pay over and above the officer's regular salary: court appearance pay, overtime pay, call-in pay, standby pay, educational incentive pay, severance pay, longevity pay, roll call pay, shift differential pay, special assignment pay, uniform allowance, and so on. The complete subindices can be found in Appendix II-3. These contract subindex analyses can only be performed for 1975-81, for these are the only years for which we collected police contracts.

These contract subindices do not tell us how much money a city spent on police fringes and pay supplements, but they do tell us about the extent to which police contracts contain these kinds of provisions and how favorable these provisions are for the unions and their members. As a result, these subindex coefficients should be associated with the bargaining and arbitration variables in a manner similar to the FRNGCOP and TOTCOMP coefficients.

Accordingly, we will test the usefulness of our analytical model on

police fringes with ordinary least squares (OLS) multiple regression equations of the following form:

$$\begin{aligned} \ln\text{FRNGCOP} &= B_0 + B_1\ln\text{POP} + B_2\ln\text{CRATE} \\ \text{or} &+ B_3\text{DENSITY} + B_4\ln\text{PCI} + B_5\ln\text{HOUSE} \\ \ln\text{TOTCOMP} &+ B_6\text{PBLACK} + B_7\text{CMGR} + B_8\text{ICITY} \\ \text{or} &+ B_9\text{OPFWAGE} + B_{10}\text{NEAST} \\ \ln\text{SUBI} &+ B_{11}\text{SOUTH} + B_{12}\text{WEST} + B_{13}\text{CBA} \\ &+ B_{14}\text{ARB} + B_{15}\text{ARBUSE} \\ &+ B_{16}\text{PASTUSE} + e \end{aligned}$$

where

$\ln\text{FRNGCOP}$  = log of the dollar amount spent on fringe benefits (retirement and insurance contributions) per police officer in each year,

$\ln\text{TOTCOMP}$  = log of the total dollar amount (pay, pay supplements, and fringes) spent on each police department employee in each year,

$\ln\text{SUBI}$  = log of the fringe benefits and pay supplements subindex scores in each year,

and all the other terms are the same as identified in Chapter III. As noted, our FRNGCOP and TOTCOMP analyses will span the 1971-81 years, while our SUBI analyses will be limited to the 1975-81 period.

In addition, our model will be expanded in our subindex equations to include a bargaining law index variable (BARGLIND). In any analysis of contract provisions, it is necessary to control for the influence of state bargaining legislation upon the scope of bargaining. In particular, increasingly favorable (to the unions) bargaining legislation may increase the unions' abilities to negotiate favorable fringe benefit and pay supplement items into their contracts, for these statutes place a

stronger and wider duty to bargain upon employers than would exist otherwise. As a result, we constructed the BARGLIND variable and included it in our subindex equation to ensure that arbitration is not credited with the influence of bargaining legislation upon contract terms. (To save space and avoid repetition, a complete rationale for and description of the BARGLIND variable is not presented until the "Model" section of the next chapter).

Finally, we will not perform any time series analyses with our fringe benefit and total compensation data. As can be seen in subsequent tables, relatively few cities supplied this information in 1973 and 1981. In turn, these missing data seriously reduce the usefulness of time series analyses.

## RESULTS

Table IV-1 shows the annual dollar amount of benefits per sworn police officer and total compensation per police department employee paid in bargaining (CBA) and nonbargaining (NonCBA) cities during the 1971-81 years. This table shows that the total compensation paid per employee rose about 115 percent during our eleven year period in both bargaining and nonbargaining cities, but that fringe benefits rose 227 percent in bargaining cities and 191 percent in nonbargaining cities during this same time period. Because cities pay anywhere from five to fifteen times as much each year in retirement contributions as they pay in insurance premiums, Table IV-1 primarily reflects the huge increase in police pension costs which has afflicted American cities in the past dozen or so years. The figures in Table IV-1 also show that fringes and

TABLE IV-1

POLICE OFFICER FRINGE BENEFITS AND POLICE DEPARTMENT  
TOTAL COMPENSATION

YEAR	Fringe Benefits Paid Per Sworn Police Officer		Total Compensation Paid Per Police Department Employee	
	CBA	NonCBA	CBA	NonCBA
1971	\$1,722 (236)	\$1,221 (449)	\$11,559 (255)	\$9,255 (477)
1972	2,000 (272)	1,331 (381)	12,621 (284)	9,990 (411)
1973	2,251 (165)	1,472 (174)	13,901 (107)	10,374 (119)
1974	2,522 (314)	1,554 (311)	14,311 (331)	10,958 (324)
1975	2,741 (365)	1,729 (286)	15,183 (390)	11,722 (300)
1976	3,165 (414)	1,946 (265)	16,943 (442)	13,090 (277)
1977	3,696 (440)	2,179 (245)	18,521 (462)	14,116 (253)
1978	4,189 (446)	2,498 (249)	19,838 (463)	14,952 (258)
1979	4,651 (422)	2,921 (233)	21,454 (442)	16,358 (241)
1980	5,027 (438)	3,141 (230)	23,381 (461)	17,538 (235)
1981	5,625 (256)	3,552 (118)	24,891 (224)	19,892 (105)

Number of cities in parentheses.

total compensation are much higher in bargaining than in nonbargaining cities. In 1981, total compensation was 25 percent higher in bargaining cities, and fringes were 58 percent higher. While the percentage gap in total compensation remained stable during this period (it was also 25 percent in 1971), the percentage gap in fringe benefits widened steadily from the 41 percent existing in 1971.

For comparison purposes, the Table III-1 figures (from the preceding chapter) showed that during 1971-81 maximum salaries increased 89 percent in bargaining cities and 95 percent in nonbargaining cities. When these salary data are compared with the fringe data, it is obvious that an increasingly larger share of the police total compensation package has been allocated to fringes since 1971.

Table IV-2 presents the means and standard deviations of 1971 and 1980 fringe benefits per sworn officer on a state-by-state basis. This table indicates that police fringes varied dramatically across states at both ends of the past decade. This table also indicates that by 1980 the availability of arbitration was associated with states whose cities pay higher rather than lower fringes: ten arbitration states are in the top half of the table, and only three are in the bottom half. However, neither Table IV-1 nor IV-2 indicate what roles bargaining and arbitration have played in determining the levels of fringe benefits. It is to these analyses that we now turn.

## Fringe Benefits (Total)

Table IV-3 presents the percent coefficients associated with the fringe benefits and the CBA, ARB, ARBUSE, and PASTUSE variables in our

TABLE IV-2

AVERAGE FRINGE BENEFITS PER POLICE OFFICER  
BY STATE IN 1971 AND 1980<sup>a</sup>

1971			1980		
State	X	(S.D.)	State	X	(S.D.)
(A) Michigan (41)	\$2,361	(\$1,411)	(A) New York (17)	\$8,794	(\$3,250)
Wisconsin (18)	2,019	(618)	(A) Wisconsin (19)	6,901	(996)
New York (21)	1,989	(1,143)	(A) Michigan (29)	6,393	(2,501)
Ohio (40)	1,845	(552)	California (114)	6,198	(2,688)
California (121)	1,746	(849)	(A) Oregon (5)	5,973	(3,675)
Iowa (16)	1,609	(956)	(A) Connecticut (21)	5,767	(2,548)
Minnesota (20)	1,581	(787)	Illinois (41)	5,544	(2,618)
Maryland (5)	1,458	(1,439)	(A) Minnesota (22)	5,108	(1,644)
New Jersey (37)	1,447	(943)	Idaho (4)	5,026	(2,932)
Utah (5)	1,446	(413)	(A) Iowa (12)	4,842	(1,653)
Kentucky (6)	1,406	(412)	Kansas (8)	4,557	(1,160)
Illinois (50)	1,384	(1,011)	Ohio (39)	4,462	(1,483)
Connecticut (25)	1,372	(999)	(A) Rhode Island (6)	3,962	(1,043)
(A) Rhode Island (9)	1,263	(687)	Maryland (6)	3,884	(2,265)
Indiana (20)	1,232	(1,292)	Tennessee (5)	3,866	(1,846)
Washington (18)	1,199	(546)	(A) New Jersey (26)	3,857	(1,775)
Kansas (10)	1,174	(756)	(A) Washington (11)	3,779	(2,157)
South Carolina (8)	1,167	(359)	Kentucky (6)	3,671	(2,107)
Oregon (6)	1,154	(556)	Florida (39)	3,538	(1,812)
Arizona (8)	1,023	(292)	Arizona (8)	3,452	(819)
(A) Nebraska (5)	923	(393)	Alabama (9)	3,303	(1,273)
North Dakota (4)	912	(141)	Colorado (13)	3,220	(1,106)
Missouri (16)	896	(504)	(A) Nebraska (5)	3,077	(820)
West Virginia (5)	885	(516)	Virginia (17)	3,019	(1,864)
Florida (36)	883	(518)	Missouri (10)	2,953	(905)
Tennessee (8)	860	(549)	Louisiana (8)	2,902	(1,336)
New Mexico (5)	814	(396)	Indiana (9)	2,852	(2,140)
Idaho (4)	813	(173)	Nevada (4)	2,800	(755)
Texas (48)	812	(1,072)	Mississippi (4)	2,674	(1,392)
Virginia (18)	777	(410)	(A) Pennsylvania (19)	2,404	(1,324)
Colorado (14)	776	(318)	North Carolina (16)	2,370	(979)
North Carolina (19)	768	(430)	Georgia (8)	2,308	(635)
Alabama (14)	752	(404)	Texas (39)	2,295	(816)
Louisiana (7)	714	(542)	West Virginia (4)	2,282	(914)
Georgia (10)	682	(625)	South Carolina (8)	2,095	(1,045)
(A) Pennsylvania (23)	628	(363)	Oklahoma (12)	2,012	(824)
Massachusetts (42)	589	(1,119)	(A) Massachusetts (18)	1,739	(1,563)
Oklahoma (15)	535	(195)	Arkansas (7)	592	(501)
Mississippi (8)	529	(669)			
Arkansas (6)	369	(477)			

(A) indicates arbitration state; numbers in parentheses are sample sizes in each state; each state needed at least four cities to be included.

<sup>a</sup>Fringes are defined as city contributions to all retirement systems plus city contributions for health, hospital, disability, and life insurance.

analytical model (the results for all the variables in the model are presented in Appendix IV-1 for the year 1980).

Collective bargaining. The pattern of CBA coefficients in the first half of Table IV-3 indicates that, after controlling for the influence of other factors, bargaining is positively and significantly associated with larger fringe benefits. If we omit the 1973 results because of the inordinately small sample size, we see that bargaining's association with fringes ranged from 18 percent to 33 percent during the years in question. A comparison of these CBA coefficients with the CBA coefficients on salaries in Table III-10 (in the preceding chapter) shows that bargaining's influence on fringes is much larger than on salaries. Depending on the year used for comparison purposes, the fringe coefficients are four to eight times larger than the salary coefficients. These CBA coefficients in Table IV-3 are strong evidence that police unions influence cities to emphasize fringes and hence allocate many more dollars to fringe contributions than these cities would otherwise.

Availability of arbitration. The presence of an arbitration statute has a less consistent association with fringes than bargaining. Arbitration had no significant association with fringes during the early and late years in our time period, but had a strongly positive association during the middle six years (1974-79). During those six years the ARB coefficients ranged between 20 and 30 percent and were statistically significant. These coefficients indicated that cities in arbitration states paid significantly higher fringe benefits to their officers than did cities in other states, *ceteris paribus*. However, this statistical pattern did not exist in 1980 and 1981 (even though the arbitration coefficients are

TABLE IV-3

PERCENTAGE IMPACTS OF BARGAINING, ARBITRATION AVAILABILITY,  
AND ARBITRATION USE ON FRINGE BENEFITS AND TOTAL COMPENSATION<sup>a</sup>

YEAR	N	Fringe Benefits <sup>b</sup> Paid Per Sworn Police Officer (All Ranks)				Total Compensation <sup>c</sup> Paid Per Police Department Employee			
		CBA	ARB	ARBUSE	PASTUSE	CBA	ARB	ARBUSE	PASTUSE
1971	584	25.86*** (3.00)	-24.63* (1.72)	59.94 (1.43)	-68.10* (1.72)	12.05*** (5.59)	2.91 (0.85)	-1.67 (0.19)	-9.44 (1.13)
1972	510	30.68*** (3.15)	-5.97 (0.47)	15.02 (0.37)	-18.86 (1.25)	11.09*** (6.14)	5.98** (2.31)	0.43 (0.06)	-4.61 (1.61)
1973	181	60.69*** (3.41)	-12.16 (0.69)	17.89 (0.27)	23.47 (0.55)	14.04*** (3.48)	2.23 (0.49)	-2.74 (0.16)	4.20 (0.39)
1974	508	32.52*** (3.71)	23.53** (2.22)	14.15 (0.61)	-1.15 (0.09)	9.23*** (4.32)	11.03*** (4.09)	0.51 (0.09)	1.48 (0.45)
1975	545	27.09*** (2.99)	29.74** (2.53)	-66.00*** (2.60)	17.93* (1.68)	9.53*** (3.70)	11.57*** (3.47)	5.26 (0.86)	1.76 (0.58)
1976	586	23.79*** (3.46)	22.62*** (2.70)	26.57* (1.80)	-10.04* (1.83)	8.33*** (4.61)	7.88*** (3.57)	-0.60 (0.16)	0.53 (0.36)
1977	585	18.93** (2.52)	30.29*** (2.91)	-0.01 (0.01)	-0.29 (0.07)	10.79*** (5.38)	7.77*** (2.97)	2.00 (0.60)	-0.62 (0.50)
1978	598	33.66*** (4.83)	20.65** (2.37)	3.02 (0.30)	2.86 (0.82)	11.96*** (6.10)	11.69*** (4.54)	-3.12 (1.00)	1.04 (0.97)
1979	571	24.28*** (3.17)	20.79** (2.12)	-3.05 (0.25)	4.94 (1.45)	8.85*** (4.36)	9.11*** (3.45)	-1.84 (0.53)	1.42 (1.49)
1980	565	31.32*** (3.84)	14.37 (1.39)	4.66 (0.34)	1.85 (0.56)	8.94*** (4.07)	9.18*** (3.06)	-3.84 (0.96)	2.44** (2.50)
1981	277	17.88** (2.12)	13.18 (1.23)	-13.27 (0.89)	4.16 (1.26)	3.21 (1.44)	8.69*** (2.93)	-6.70 (1.64)	0.88 (0.96)

<sup>a</sup>The percentage impacts were calculated from the OLS log coefficients with the formula  $\% = e^B - 1$ , where B is the estimated log coefficient.

<sup>b</sup>Defined as city contributions to retirement systems and to health, hospital, disability, and life insurance.

<sup>c</sup>Defined as total salaries, pay supplements, and fringe benefits.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

large and positive for those years). As a result, it appears that arbitration's availability is no longer associated with higher fringes. When comparing the ARB coefficients here with those in Table III-10 (in the preceding chapter), it appears that the influence of arbitration on fringes waned in recent years as it grew stronger on salaries.

Arbitration use. The use of arbitration appears to have no systematic relationship with the dollar amount of fringe benefits. The ARBUSE and PASTUSE coefficients are almost never statistically significant, and they are never significant in the same direction two years in a row. As a result, the associations which do exist between the level of fringe benefits and the current or prior use of arbitration seem to be one-time relationships rather than a multi-year pattern in a particular direction. In turn, we can conclude from these ARBUSE and PASTUSE coefficients that police unions do not obtain higher fringe benefits via the actual use of arbitration than they do via negotiations. In other words, just as the use of arbitration has had no significant impact on police salaries (see Chapter III), it similarly has had no significant impact on police fringes.

Other influences. Just as many of the "market" variables in our model significantly influenced police salaries, so do many of them influence police fringes. In particular, in most years fringes are higher in larger rather than smaller cities, in more densely populated rather than less dense cities, in cities with wealthier rather than poorer housing, and in cities with a city manager rather than a mayoral form of government. In addition, fringes also are positively associated with city crime rates. In the earlier years of our time period (i.e.,

up through 1977), fringes are positively associated with the level of manufacturing wages in each city, but this association disappeared after 1977 (which is not surprising considering that our manufacturing wage term (OPPWAGE) consists of 1972 wages).

Most of these associations are in the same direction as those reported for salaries in the previous chapter. In addition, there are some regional fringe patterns which are similar to and different from the regional salary patterns identified in Chapter III. Compared with cities in North Central states, cities in the South and Northeast paid significantly lower fringes, ceteris paribus. In addition, Western cities also paid significantly lower fringes than North Central cities during the 1971-73 years and in 1978, but in recent years this difference faded into nonsignificance. Our fringe analyses show that North Central cities paid the largest fringes, ceteris paribus, in the country. Considering that retirement contributions comprise 80-90 percent of the ICMA-defined fringe benefit variable, our data show that North Central cities have the highest police pension costs.

By far the most powerful explanatory variable in our model was Southern location. The SOUTH coefficients in our analyses ranged from -31 percent to -81 percent during our eleven year period, they averaged -53 percent, and they were always highly significant (see Appendix IV-1 for one year's example). In fact, our SOUTH results were consistently the largest coefficients reported in our calculations during our time period. These results show rather dramatically that, after controlling for other influences, Southern cities pay much lower police fringes than cities in North Central states. Expressed another way, our data show

that Southern cities have much lower police pension costs than North Central cities.

Overall, our multiple regression fringe benefit equation was highly significant in each year of our time period. However, our analytical model explained less of the variation in police fringes than in salaries. We saw in the previous chapter that our model explained 55-70 percent of salaries; in this chapter it explained 25-33 percent of the variation in fringes. In other words, the variables which explain most of the variation in police salaries do less well in explaining the variation in cities' police retirement contributions.

Caveat. Finally, we realize that our fringe benefit term is not limited strictly to bargaining unit personnel but instead includes city contributions to retirement and insurance plans for all ranks of sworn police officers. As a result, it is possible that this variable could be biased upward by unusually generous retirement and insurance arrangements for superior officers. However, the heavy majority of officers in any unionized department are in the primary bargaining unit. Perhaps more important, city retirement and insurance contributions for police management personnel tend to be adjusted in response to retirement and insurance adjustments made with the union for bargaining unit personnel, rather than vice versa. As a result, we believe that our fringe benefit variable accurately measures how police fringes respond to different labor relations influences.

#### Fringe Benefits (Separate)

As noted earlier, our measure of fringe benefits includes city contributions to all applicable retirement systems (federal (i.e., Social Security), state, local) and to four different kinds of insurance coverage (health, hospital, disability, life). In the previous section, we analyzed the relationships that bargaining and arbitration have with this overall fringe benefits measure. However, this overall analysis tells us little about how bargaining or arbitration might be associated with specific fringe benefits. Accordingly, we have decomposed our fringe benefit measure into three parts in order to perform a more precise analysis. In particular, we have examined bargaining and arbitration's relationships with (1) city contributions per police officer to all retirement systems, (2) city contributions per police officer to state and local retirement systems (i.e., city contributions to Social Security will be excluded), and (3) total city contributions per police officer for the four kinds of insurance.

Bargaining. The separate retirement and insurance results are presented in Table IV-4. These regression coefficients, calculated using our standard analytical model, indicate that bargaining has a consistently strong and positive association with both retirement measures and with insurance. In all three parts of the table the CBA coefficients routinely are large (or very large) and highly significant. These CBA coefficients are more consistently significant in the retirement equations than in the insurance equation, but in all three equations the CBA coefficients are always positive and differ only in magnitude. Consequently, these results clearly indicate that the practice of police

TABLE IV-4  
 PERCENTAGE IMPACTS OF BARGAINING, ARBITRATION AVAILABILITY,  
 AND ARBITRATION USE ON SEPARATE FRINGE BENEFITS<sup>a</sup>

YEAR	N	ALL RETIREMENT				STATE AND LOCAL RETIREMENT <sup>b</sup>				INSURANCE <sup>c</sup>			
		CBA	ARB	ARBUSE	PASTUSE	CBA	ARB	ARBUSE	PASTUSE	CBA	ARB	ARBUSE	PASTUSE
1971	510	15.11*** (2.07)	-48.84*** (3.34)	73.05* (1.93)	-98.95*** (2.64)	15.14* (1.87)	-52.82*** (3.15)	79.27* (1.85)	-89.50** (2.21)	26.66*** (3.08)	21.62 (1.47)	70.95* (1.68)	-82.12** (2.05)
1972	478	27.16*** (3.31)	-31.25** (2.42)	89.82 (1.58)	-39.95* (1.89)	32.22*** (3.47)	-45.28*** (2.99)	78.91 (1.30)	-29.10 (1.30)	24.42*** (2.64)	54.79*** (3.50)	-8.43 (0.23)	-1.25 (0.10)
1973	309	21.28* (1.84)	-34.19** (2.25)	131.66* (1.93)	-38.89 (1.52)	33.58** (2.52)	-52.41*** (2.97)	100.91 (1.45)	-29.15 (1.07)	11.16 (1.12)	30.88** (2.25)	-3.42 (0.09)	18.80 (0.82)
1974	469	26.71*** (3.40)	8.26 (0.89)	3.69 (0.19)	1.30 (0.12)	35.74*** (4.03)	-4.47 (0.45)	3.84 (0.18)	3.32 (0.27)	11.06 (1.7)	31.05** (2.54)	-4.62 (0.19)	9.60 (0.48)
1975	504	26.55*** (3.47)	21.75** (2.19)	-7.77 (0.40)	0.43 (0.05)	40.83*** (4.83)	2.78 (0.29)	-8.32 (0.41)	-3.72 (0.42)	15.10* (1.84)	26.36** (2.29)	-22.46 (1.09)	17.45 (1.60)
1976	554	25.99*** (4.21)	19.27** (2.54)	25.62** (1.96)	-18.73*** (3.71)	34.06*** (4.69)	12.98 (1.54)	13.35 (0.94)	-15.44*** (2.74)	6.80 (0.94)	22.21** (2.24)	-1.73 (0.11)	8.27 (1.31)
1977	560	22.56*** (3.05)	24.71** (2.45)	10.69 (0.87)	-8.63* (1.89)	32.00*** (3.75)	9.50 (0.92)	9.76 (0.73)	-8.72* (1.74)	11.94* (1.68)	5.72 (0.62)	3.98 (0.34)	6.73 (1.47)
1978	574	35.78*** (5.05)	6.22 (0.72)	3.56 (0.35)	-0.93 (0.27)	56.38*** (6.11)	-12.72 (1.20)	9.52 (0.76)	-1.68 (0.40)	9.88 (1.38)	18.03* (1.76)	-19.89 (1.60)	10.39** (2.51)
1979	541	23.97*** (3.13)	12.18 (1.21)	-2.90 (0.23)	1.80 (0.52)	39.60*** (4.37)	0.07 (0.01)	-6.16 (0.43)	3.00 (0.79)	10.55 (1.36)	10.82 (1.02)	-3.92 (0.29)	9.04** (2.37)
1980	542	28.38*** (3.39)	6.84 (0.63)	-11.26 (0.76)	0.68 (0.20)	45.59*** (4.57)	-8.37 (0.69)	-20.56 (1.20)	1.27 (0.34)	17.43** (2.30)	10.83 (1.04)	17.19 (1.19)	0.59 (0.18)
1981	333	14.96 (1.49)	26.83* (1.91)	-3.50 (0.22)	0.06 (0.02)	28.21** (2.53)	13.11 (0.94)	-16.44 (0.90)	-0.97 (0.24)	47.23*** (95.61)	4.08 (0.43)	-5.19 (0.43)	3.14 (1.14)

<sup>a</sup>The percentage impacts were calculated from the OLS log coefficients with the formula  $\% = e^B - 1$ , where B is the estimated log coefficient.

<sup>b</sup>State and local retirement contributions are city expenditures per sworn police officer to all state and local retirement systems applicable to police officers (i.e., city and employee contributions to the federal Social Security system are excluded).

<sup>c</sup>Insurance contributions are city expenditures per sworn police officer for health, hospital, disability, and life insurance.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\* Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

collective bargaining is associated with larger retirement and insurance expenditures per officer than would exist otherwise, which in turn is consistent with the "median voter" explanation of union behavior (Freeman, 1981), namely, that police unions emphasize the fringe benefit preferences of the more senior (or median) police officers more than would occur in a nonunion situation.

Arbitration availability. The availability of arbitration appears to exert much less influence upon retirement and insurance expenditures than bargaining. Of the 33 ARB coefficients in Table IV-4, only 16 are statistically significant, and six of these 16 coefficients are negative. In particular, during 1971-73 the six retirement ARB coefficients are strongly negative but not thereafter, and this pattern probably is a result of (a) the fact that Pennsylvania (which has very low retirement expenditures) is in the ARB category during those years and (b) the fact that New York (which has very high retirement expenditures) joined the ARB category in 1974.

Arbitration's availability also seems to have had different associations with different fringes over time. For instance, during 1972-76 the insurance ARB coefficients were large and strongly positive, but not thereafter (with one exception). Similarly, during 1975-77 and 1981 the all retirement ARB coefficients were large and positive, but during 1978-80 they were insignificant. Further, the state and local retirement ARB coefficients were never statistically significant after 1973. These two columns of retirement ARB coefficients indicate that the availability of arbitration and a city's contributions to the federal Social Security system are positively correlated (which we confirmed in a separate and

unreported analysis), but that the availability of arbitration by itself has not influenced the level of city contributions to state and local police retirement systems (i.e., cities in New York, Wisconsin, Michigan, and several other arbitration states may have high police retirement expenditures, but these high expenditures are not the result of the existence of arbitration laws). These ARB results will be confirmed in a state-by-state examination of fringe benefit expenditures later in this chapter.

Arbitration use. The current use and prior use of arbitration seem to have no systematic relationship with retirement and insurance expenditures per police officer. The ARBUSE and PASTUSE coefficients are rarely significant (e.g., only 15 of these 66 coefficients reach statistical significance), and the significant coefficients establish no consistent pattern in either direction. These arbitration use results confirm the conclusion offered a few pages ago, namely, that police unions do not obtain higher fringe benefits via the actual use of arbitration than they do via negotiations.

#### Total Compensation

The second half of Table IV-3 presents the percent coefficients associated with total compensation and the CBA, ARB, ARBUSE, and PASTUSE variables in our analytical model. As noted earlier, our TOTCOMP measure includes expenditures for all police department employees, and as a result it is not limited to expenditures for bargaining unit personnel (for police bargaining units typically are limited to sworn officers). However, 81-87 percent of police employees in various years

are sworn officers, and median sworn officer pay typically is much higher than median civilian employee pay in most departments (most civilian employees are clericals). In addition, pay adjustments for police civilians usually follow the pay adjustments for sworn officers rather than vice versa. Consequently, we believe that our TOTCOMP variable accurately measures the annual total cost of employing a police officer, and the inclusion of police civilian employee costs in this variable tend to bias it downward and hence understate any influences that bargaining or arbitration might have on total compensation.

Collective bargaining. The CBA coefficients in the second half of Table IV-3 show that the presence of unionism in a police department is associated with significantly higher total compensation costs during the 1971-80 years. If we disregard the 1973 results due to the very small sample size for that year, we see that unionism had an 8-12 percent influence on TOTCOMP during this ten year period. When we compare these coefficients with the salary CBA coefficients in Table III-10, we see that the TOTCOMP coefficients are several points larger in each year than the salary coefficients are for that same year. This comparison provides additional evidence that police unions are associated with larger influences on the nonsalary portion of total compensation than on salaries.

This comparison of salary and total compensation coefficients also shows that police bargaining no longer has a statistically significant association with maximum salaries or total compensation in 1981.

Availability of arbitration. The presence of an arbitration statute is associated with significantly higher total compensation consistently throughout the 1974-81 period. During these years arbitration was

associated with an approximate 8-12 percent increase in total compensation per employee, and these increases are larger than the ARB increases associated with salaries reported in Table III-10. These coefficient comparisons indicate that, as with bargaining, arbitration has had a greater influence on the nonsalary portions of the total compensation package than on salaries.

Use of arbitration. The actual use of arbitration exhibited no significant association with TOTCOMP. Of the 22 ARBUSE and PASTUSE coefficients in the second half of Table IV-3, ten are negative, twelve are positive, and only one is statistically significant. During the 1978-81 years, the current users of arbitration paid slightly smaller compensation packages than did other cities, but these differences were not significant. Just as the actual use of arbitration is not associated with salaries or fringes, it is not associated with total compensation.

Other influences. As with salaries and fringes, several "market" variables influenced total compensation (see Appendix IV-2). Specifically, in most years total compensation is higher in larger rather than smaller cities, in wealthier rather than poorer cities (as measured by per capita income and the median value of residential housing), in city manager rather than mayoral cities, in cities with higher rather than lower manufacturing wages, in more densely populated rather than less dense cities, and in cities with higher rather than lower crime rates. TOTCOMP is significantly lower in cities outside of metropolitan areas than in metro areas. Also, cities in Southern states pay significantly lower total compensation than cities in North Central states, but this difference is much smaller (i.e., in the -15 to -20 percent range) than

that reported for fringe benefits (i.e., -31 to -81 percent).

Overall, our TOTCOMP equation was highly significant in each year of our time period, and in most years a heavy majority of the independent variables in the equation were significant. Our analytical model explained about 60 percent of the variation in TOTCOMP, which is commensurate with the explanatory power of the salary equations in the previous chapter and about twice as powerful as the fringe benefit equations.

#### State-Specific Effects

Following the state-specific analysis of salaries performed in Chapter III (see Table III-8), we recalculated the ARB coefficients in the fringe benefit and total compensation analyses on a state-by-state basis for each of the arbitration states which reported data for at least eight cities (i.e., Michigan, Pennsylvania, Rhode Island, Wisconsin, Minnesota, Washington, New York, Massachusetts, Connecticut, Iowa, New Jersey). These results are presented in Tables IV-5 and IV-6.

Fringe benefits. The state-by-state ARB coefficients in Table IV-5 suggest that the availability of arbitration has varied substantially across states and, with few exceptions, has had little effect on the level of police fringe benefits. Our results show that police fringe levels vary dramatically across states in each year, and this variation seems about as large at the end of our time period as at the beginning.

Among the three arbitration states for which we have no "before" data (Michigan, Pennsylvania, and Rhode Island), the availability of arbitration appears to have had no consistent long-run effect on the

level of fringes paid in Pennsylvania and Rhode Island cities. Michigan cities pay much higher than average fringes, but they might have been doing so before arbitration was implemented. Certainly, the high level of Michigan fringes has shown no signs of moderating in recent years under the arbitration statute.

Among the eight arbitration states for which we have "before" and "after" data (Wisconsin, Minnesota, Washington, New York, Massachusetts, Connecticut, Iowa, and New Jersey), only Wisconsin and New York show any consistent before and after differences. Cities in both states paid comparatively high fringes before arbitration came along, but the Wisconsin and New York coefficients became much larger after arbitration took effect in those states. In these two states, then, the level of fringe benefits shows a positive and significant association with the availability of arbitration.

However, the results for the other six states in the before and after category show no consistent long-term changes associated with the emergence and availability of arbitration. When these six states are combined with Rhode Island and Pennsylvania, we see that cities in eight of the eleven arbitration states in Table IV-5 exhibit no consistent relationship between the availability of arbitration and the level of police fringe benefits. In turn, these results strongly imply that police fringes are influenced much more by unmeasured state characteristics than by arbitration's availability.

In addition, it is important to note that collective bargaining has a very strong and positive association with fringes. Further, the CBA coefficients in Table IV-5 are very similar to the CBA coefficients in

TABLE IV-5

PERCENTAGE STATE-SPECIFIC EFFECTS OF ARBITRATION'S  
AVAILABILITY ON FRINGE BENEFITS<sup>a, b</sup>

YEAR	N	CA	MI	PA	RI	WI	MN	WA	NY	MA	CT	IA	NJ	
1971	564	14.88* (1.77)	34.12* (1.71)	-77.82 (1.62)	62.11 (1.17)	51.26** (2.19)	23.61 (1.07)	-29.95 (1.14)	91.02* (1.82)	-6.56 (0.18)	92.78* (1.82)	35.78 (1.44)	51.04 (1.15)	
1972	523	22.70** (2.42)	42.68* (1.93)	-213.46*** (2.80)	-35.19 (0.55)	32.45 (1.35)	17.01 (0.83)	11.23 (0.43)	19.47 (0.44)	-72.03 (1.34)	8.89 (0.21)	90.88*** (2.90)	-41.89 (0.85)	
1973	185	41.06** (2.52)	37.81 (1.46)	-247.95** (2.31)	-70.88 (0.72)	15.62 (0.42)	63.85 (1.43)	-87.09* (1.68)	82.72 (1.22)	-235.85** (1.98)	-23.54 (0.40)	233.11* (1.94)	-44.22 (0.68)	
1974	524	29.59*** (3.45)	61.61*** (3.11)	-36.93 (0.86)	15.03 (0.34)	95.80*** (3.52)	54.76** (2.39)	-66.66** (2.14)	220.91*** (3.31)	-6.94 (0.19)	42.82 (1.04)	102.59*** (3.41)	42.42 (1.04)	
1975	560	25.26*** (3.13)	53.16** (2.53)	-36.67 (0.79)	16.36 (0.35)	105.83*** (3.89)	40.33* (1.91)	-56.44** (2.18)	186.51** (2.72)	-526.20*** (4.91)	17.62 (0.43)	79.71*** (2.87)	-59.68 (1.21)	
1976	603	19.60*** (3.11)	42.48*** (2.91)	-63.46 (1.62)	-15.52 (0.45)	101.96*** (5.00)	35.68** (2.21)	-46.54** (2.18)	95.66** (2.35)	-197.07*** (3.87)	2.81 (0.10)	62.94*** (2.85)	-18.61 (0.61)	Before Arbitration
1977	609	19.76*** (2.76)	26.20* (1.75)	-26.95 (0.79)	34.39 (0.75)	111.68*** (4.61)	19.12 (1.14)	-4.00 (0.21)	106.91** (2.43)	-130.90*** (2.87)	13.91 (0.44)	35.97 (1.56)	9.41 (0.31)	After Arbitration
1978	619	33.80*** (5.07)	28.70** (2.10)	-37.25 (1.11)	31.72 (0.77)	85.52*** (4.44)	13.48 (0.92)	-15.00 (0.85)	87.70** (2.27)	-109.89** (2.56)	22.94 (0.73)	40.28** (1.96)	-21.69 (0.70)	
1979	589	27.05*** (3.69)	25.71* (1.71)	-26.01 (0.62)	31.85 (0.63)	87.27*** (3.72)	22.69 (1.31)	-30.67 (1.37)	155.28** (2.54)	-94.47* (1.80)	38.68 (0.89)	26.58 (1.38)	-2.78 (0.08)	
1980	583	32.27*** (4.15)	35.43** (2.07)	-39.54 (0.53)	23.75 (0.33)	75.07*** (3.51)	6.80 (0.41)	-29.52 (1.27)	128.26 (1.31)	-101.31 (1.21)	41.88 (0.56)	22.96 (1.00)	-30.33 (0.43)	
1981	286	16.79** (2.11)	45.43** (2.49)	-33.27 (1.25)	40.79 (1.00)	88.27*** (4.16)	18.06 (1.01)	56.10** (2.45)	123.78*** (2.86)	121.11*** (2.77)	68.74*** (2.85)	46.64** (2.01)	-6.40 (0.39)	

<sup>a</sup>The percentage impacts were calculated from the OLS log coefficients with the formula  $\% = e^B - 1$ , where B is the estimated log coefficient.  
<sup>b</sup>Fringe benefits are defined as city contributions to retirement systems and to health, hospital, disability, and life insurance.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

TABLE IV-6

PERCENTAGE STATE-SPECIFIC EFFECTS OF ARBITRATION'S  
AVAILABILITY ON TOTAL COMPENSATION<sup>a,b</sup>

YEAR	N	CA	MI	PA	RI	WI	MN	VA	NY	MA	CT	IA	NJ	
1971	564	8.62*** (4.01)	20.42*** (4.12)	-12.22 (1.24)	-3.35 (0.30)	11.09** (2.12)	13.83** (2.48)	-5.04 (0.08)	8.59 (0.88)	9.11 (0.92)	10.26 (1.03)	7.76 (1.34)	-7.98 (0.81)	
1972	523	9.29*** (5.08)	17.55*** (4.24)	-6.94 (0.79)	-9.54 (0.81)	10.57** (2.33)	11.86*** (2.84)	4.06 (0.77)	5.28 (0.62)	2.41 (0.28)	8.74 (1.00)	4.43 (0.94)	2.88 (0.33)	
1973	185	12.28*** (3.01)	14.37** (2.16)	-0.18 (0.01)	-9.77 (0.45)	0.28 (0.03)	8.34 (0.82)	-4.41 (0.41)	29.11* (1.83)	5.09 (0.29)	2.82 (0.19)	24.60 (1.25)	9.40 (0.59)	
1974	524	8.70*** (4.08)	21.47*** (4.63)	-2.93 (0.29)	4.12 (0.37)	15.45*** (2.76)	14.87*** (2.78)	5.37 (0.81)	39.96*** (3.51)	26.10** (2.41)	7.66 (0.79)	7.13 (1.22)	17.55* (1.75)	
1975	560	8.78*** (3.48)	21.76*** (3.48)	4.36 (0.32)	-0.31 (0.02)	15.23** (2.27)	13.61** (2.14)	6.08 (0.85)	36.12** (2.37)	8.46 (0.65)	-8.43 (0.63)	-14.82** (2.01)	-7.58 (0.56)	
1976	603	8.72*** (4.94)	15.70*** (4.07)	14.99 (1.56)	-1.09 (0.12)	8.84** (2.05)	9.36** (2.20)	4.41 (0.84)	39.32*** (3.96)	22.35** (2.44)	12.33 (1.37)	0.35 (0.07)	18.87** (2.10)	Before Arbitration
1977	609	11.54*** (5.89)	5.24 (1.35)	11.90 (1.31)	16.30 (1.35)	15.42*** (3.11)	9.33** (2.05)	9.81* (1.74)	29.50*** (3.05)	14.08 (1.59)	9.70 (1.10)	-9.40 (1.61)	11.54 (1.31)	After Arbitration
1978	619	12.78*** (6.69)	16.28*** (4.01)	13.35 (1.40)	14.49 (1.21)	13.15*** (2.84)	9.23** (2.06)	9.93* (1.84)	35.51*** (3.49)	17.30* (1.76)	7.08 (0.77)	-2.33 (0.42)	5.83 (0.64)	
1979	589	10.66*** (5.41)	18.93*** (4.49)	9.85 (0.87)	9.91 (0.74)	13.38*** (2.58)	9.16* (1.94)	5.15 (0.89)	48.74*** (3.74)	15.74 (1.37)	12.27 (1.09)	-2.28 (0.46)	13.07 (1.17)	
1980	583	11.08*** (4.75)	16.44*** (3.16)	16.31 (0.73)	18.09 (0.78)	8.72 (1.59)	19.29*** (3.34)	6.23 (0.90)	54.19** (2.09)	29.28 (1.25)	28.15 (1.21)	-9.82 (1.38)	15.71 (0.71)	
1981	286	3.48 (1.60)	23.45*** (4.81)	-11.45 (1.62)	-9.60 (0.92)	13.22*** (2.80)	10.80** (2.15)	3.12 (0.58)	23.12** (2.54)	2.58 (0.30)	5.90 (1.07)	5.65 (0.99)	9.77** (2.00)	

<sup>a</sup>The percentage impacts were calculated from the OLS log coefficients with the formula  $\% = e^B - 1$ , where B is the estimated log coefficient.

<sup>b</sup>Total compensation is defined as total salaries, pay supplements, and fringe benefits.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

the first half of Table IV-3 and in Table IV-4. As a result, our analysis indicates that collective bargaining has a more consistent, widespread, and much stronger association with police fringes than does the availability of arbitration.

Total compensation. We repeated our state-specific analysis using total compensation per police employee as our dependent variable, and these results are presented in Table IV-6. Although the coefficients vary substantially across states, they generally indicate that the availability of arbitration has had little or no effect on the amount of the total compensation package paid to police department employees.

Among our eleven states, only cities in Michigan, Wisconsin, Minnesota, and New York consistently paid significantly higher total compensation than cities elsewhere. However, Wisconsin and Minnesota cities paid significantly more before arbitration became available, and the coefficients in those states remained large but did not become consistently larger after arbitration was implemented. Michigan cities also paid significantly higher total compensation packages since 1971, but our data do not go back far enough to permit a before and after assessment in that state.

The Table IV-6 results indicate that New York cities are the only cities whose total compensation packages became significantly larger as a result of the emergence and availability of arbitration. New York adopted an arbitration law in 1974, and the 1974-81 New York coefficients are much larger and much more highly significant than the 1971-73 coefficients in that state.

The CBA coefficients in Table IV-6 indicate that collective bargaining has a more consistent and stronger effect on the size of the total

compensation package than arbitration's availability. The CBA coefficients are in the 8-12 percent range in each year of the 1971-80 period, and become nonsignificant only in 1981. This pattern of results is very similar to the pattern displayed by the CBA coefficients in the second half of Table IV-3. In addition, the fact that the CBA coefficients in Table IV-6 are larger than the CBA coefficients in the Chapter III salary analyses indicate that bargaining has a stronger impact on nonsalary monetary items than it does on salaries themselves.

Salaries, fringes, and total compensation. A comparison of the salary results in Tables III-4, III-5, and III-8 with the fringe benefit results in Tables IV-3, IV-4, and IV-5 and with the total compensation results in Tables IV-3 and IV-6 indicates two very important findings. Methodologically, our results show that the availability of arbitration generally has a strongly positive and significant impact on salaries, fringes, and total compensation when the data are aggregated and analyzed on a national basis. However, our disaggregated, fringe-by-fringe, state-by-state, and before and after results indicate that arbitration's availability has little or no consistent effect on these monetary items in most states. For example, our salary analyses indicated that arbitration was clearly associated with higher salaries only in Washington; our fringe benefit analyses indicated that arbitration was clearly associated with larger fringe benefits only in New York and Wisconsin; and our total compensation analyses indicated that arbitration was clearly associated with larger compensation packages only in New York. In addition, arbitration may have contributed to higher salaries in Pennsylvania and to higher salaries, fringes, and total compensation in

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Michigan, but our data for these two states do not go back far enough to permit any before and after analyses and conclusions. In other words, arbitration's impact on police monetary employment conditions appears to depend upon whether the data are analyzed in an aggregated or disaggregated manner.

Substantively, our findings indicate that arbitration's impact varies considerably across states. As noted above, cities in only a few states paid significantly higher salaries, fringes, or total compensation after arbitration's arrival than they did beforehand. In most arbitration states, the salary, fringe, and compensation patterns which existed before arbitration's arrival seemed to continue after arbitration became available. In turn, the modest impact of arbitration in most states indicates that over time collective bargaining may have done more to push up the components of police pay than arbitration has done. In addition, the variation in salary, fringe, and compensation levels across states indicates that police pay is heavily influenced by unmeasured state characteristics which have little or nothing to do with bargaining or arbitration.

Fringes controlling for compensation. So far we have examined labor relations influences on fringes without controlling for total compensation. However, if police fringes are positively correlated with police salaries, and bargaining or arbitration has a positive impact on salaries, then the results in this chapter may be simply the by-products of union influences on salaries. To test for this possibility, we re-estimated our Table IV-3 equations with the inclusion of a total compensation per police department employee (sworn plus civilian) variable ( $\ln\text{TOTCOMP}$ ).

Because total compensation includes fringes we have a simultaneity bias due to the presence of fringes on both sides of the equation. To correct for this problem,  $\ln\text{TOTCOMP}$  first will be regressed on the log of salaries per police department employee and the other variables in our model, and then the instrumented or predicted value of  $\ln\text{TOTCOMP}$  will be used in place of the actual value of  $\ln\text{TOTCOMP}$  in the  $\ln\text{FRNGCOP}$  equation. (This instrumental variables technique has been borrowed from Freeman (1981).)

These results are presented in Table IV-7. As expected, there is a very strong association between police fringes and  $\text{TOTCOMP}$ . However, the general pattern of results in Table IV-7 is very similar to the pattern in Table IV-3: the CBA coefficients are positive and significant in almost each year, though the CBA coefficients in Table IV-7 are somewhat smaller than those in Table IV-3; the ARE coefficients are a bit less influential in Table IV-7 than in Table IV-3; and the ARBUSE and PASTUSE coefficients are never consistently significant. Because salaries comprise most of the  $\text{TOTCOMP}$  term, Table IV-7 indicates that there is a positive relationship between police fringes and salaries. In addition, Table IV-7 confirms the results in Tables IV-3, IV-4, and IV-5: controlling for levels of total compensation, collective bargaining has a much stronger association with police fringes than either the availability or the use of arbitration.

#### Fringe Benefit and Pay Supplement Subindex Scores

Table IV-8 shows that fringe benefit and pay supplement provisions are more numerous and more favorable to the unions in arbitration states

TABLE IV-7

PERCENTAGE IMPACTS OF BARGAINING, ARBITRATION  
AVAILABILITY, AND ARBITRATION USE ON TOTAL FRINGE  
BENEFITS, CONTROLLING FOR TOTAL COMPENSATION<sup>a</sup>

YEAR	N	Fringe Benefits <sup>b</sup> Paid Per Sworn Police Officer (All Ranks)				
		CBA	ARB	ARBUSE	PASTUSE	TOTCOMP
1971	548	16.21* (1.92)	-33.08** (2.13)	67.85 (1.60)	-55.94 (1.49)	104.42*** (4.22)
1972	510	19.71** (2.05)	-13.68 (0.99)	16.39 (0.41)	-14.26 (0.97)	125.99*** (3.47)
1973	181	44.11*** (2.59)	-22.95 (1.21)	5.29 (0.09)	28.52 (0.66)	165.49*** (3.18)
1974	508	21.02** (2.53)	14.41 (1.39)	13.84 (0.61)	-3.74 (0.31)	159.43*** (5.18)
1975	545	23.32*** (2.61)	22.79* (1.92)	-68.34*** (2.68)	16.98 (1.60)	49.93*** (2.73)
1976	586	19.41*** (2.84)	21.03** (2.44)	26.39* (1.80)	-10.68* (1.96)	54.98*** (2.61)
1977	585	9.19 (1.27)	23.94** (2.36)	-2.26 (0.19)	-0.21 (0.05)	131.08*** (5.11)
1978	598	20.32*** (3.10)	7.52 (0.90)	4.91 (0.50)	2.28 (0.68)	140.51*** (6.20)
1979	571	16.43** (2.24)	8.78 (0.91)	-0.92 (0.08)	3.87 (1.15)	138.81*** (5.24)
1980	565	25.82*** (3.25)	8.59 (0.82)	4.33 (0.32)	0.64 (0.20)	56.49*** (2.87)
1981	277	16.04* (1.93)	21.45* (1.79)	-11.14 (0.76)	2.64 (0.79)	21.13 (0.75)

<sup>a</sup>The percentage impacts were calculated from the OLS log coefficients with the formula  $\% = e^B - 1$ , where B is the estimated log coefficient.

<sup>b</sup>Defined as city contributions to retirement systems and to health, hospital, disability, and life insurance.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

than in other states. While the percentage differences in subindex scores in the two types of states declined over time, the absolute difference remained rather large throughout the entire time period. However, the relatively large standard deviations indicate that there is a great deal of variation in these portions of police contracts in both arbitration and nonarbitration states.

Table IV-9 shows how these contract subindex scores are associated with the availability and use of arbitration during the 1975-81 period. When these two subindices are analyzed separately most of the coefficients are insignificant. When they are joined in a combined "monetary provisions" subindex, the availability of arbitration (ARB) is consistently associated with significantly higher contract subindex scores. These ARB coefficients are smaller during the 1978-81 years than during 1975-77, and thus the contract score advantage associated with arbitration seems to be declining. In contrast, the ARBUSE and PASTUSE coefficients exhibit no more consistent connections with the combined monetary subindex than with the two separate subindices. The ARBUSE term is negative and barely significant in 1975; after that it exhibits no pattern at all. The PASTUSE term is always negative and is significant at the beginning and end of our time period; these results indicate that prior users of arbitration may have been trying to catch up to the contract provisions obtained by their peers in other cities.

An important explanatory variable is the bargaining law index term (BARGLIND). The BARGLIND coefficients are positive and highly significant in every year except 1977. These coefficients mean that in 1980, for instance, the combined monetary subindex score improved by 3.39 percent

TABLE IV-8  
AVERAGE FRINGE AND PAY SUPPLEMENT SUBINDEX SCORES

YEAR	N	FRINGE BENEFITS		PAY SUPPLEMENTS	
		X	S.D.	X	S.D.
1975	205	86.4	45.0	66.4	37.1
	Arb 99	103.3	37.3	83.5	32.3
	NonArb 106	70.7	46.1	50.4	34.2
1976	312	86.5	44.6	65.9	37.4
	Arb 131	108.3	37.6	87.6	32.1
	NonArb 181	70.7	42.6	50.3	33.0
1977	367	92.9	45.6	71.2	36.1
	Arb 196	112.0	40.0	88.0	29.8
	NonArb 171	70.9	41.7	52.0	33.2
1978	406	100.4	44.5	77.8	33.3
	Arb 219	117.0	40.5	92.1	26.9
	NonArb 187	81.0	41.1	61.0	32.3
1979	413	105.7	45.0	80.6	33.6
	Arb 220	121.8	40.7	94.9	26.0
	NonArb 193	87.3	42.8	64.3	33.9
1980	436	112.1	45.0	84.1	32.5
	Arb 235	124.9	42.4	96.1	26.8
	NonArb 201	97.2	43.4	70.1	33.0
1981	461	116.8	43.6	88.0	31.1
	Arb 242	128.7	41.1	100.2	27.2
	NonArb 219	103.7	42.5	74.6	29.6

for each advancement from one bargaining law category to the next on our six category index (see our description of the index in Chapter V). Because the reference category for this variable is states with no police bargaining legislation, and because most of the states which have bargaining laws are clustered in the top two categories in our index, the BARGLIND coefficients in Table IV-9 indicate that having a highly favorable (to the unions) bargaining law is associated with much larger subindex scores compared to the subindex scores in states with no bargaining law. Further, these significant BARGLIND coefficients allow us to have increased confidence in our ARB, ARBUSE, and PASTUSE results, for the inclusion of the BARGLIND variable in our equations ensures that the arbitration terms do not pick up and report the influence of favorable bargaining legislation (which exists in all of our arbitration states).

In sum, our analysis of the monetary provisions in police contracts generally confirms our aggregate analysis of the dollar amounts spent on fringe benefits and total compensation: the availability of arbitration is associated with higher amounts of these dependent variables, while the current and past use of arbitration has no consistent association with any of these measures.

Finally, after comparing our dollar analyses (Tables IV-1, 2, 3, 4, 5, 6, and 7) with our subindex analyses (Tables IV-8 and 9), we believe that the analyses of the actual dollars paid by cities for police fringes and compensation provide much more useful information than the analyses of contract subindex scores. We express this preference because the contract scoring index may give the same score to contract provisions which have widely different funding requirements. For

TABLE IV-9

PERCENTAGE IMPACTS OF ARBITRATION AVAILABILITY,  
CURRENT USE, AND PAST USE ON FRINGE AND SUPPLEMENT SUBINDEX SCORES<sup>a</sup>

YEAR	N	FRINGE BENEFITS			PAY SUPPLEMENTS			COMBINED			
		ARB	ARBUSE	PASTUSE	ARB	ARBUSE	PASTUSE	ARB	ARBUSE	PASTUSE	BARGLIND
1975	194	30.13** (2.19)	-25.78 (1.50)	-18.08** (2.17)	138.50** (2.05)	-49.62 (0.69)	-41.65 (1.28)	47.37*** (3.22)	-31.48* (1.65)	-20.38** (2.41)	6.29*** (3.85)
1976	291	27.67** (2.33)	-1.53 (0.12)	-12.47* (1.93)	93.67 (1.40)	-2.66 (0.05)	-14.07 (0.48)	44.59*** (3.46)	-5.31 (0.43)	-11.66* (1.80)	4.26*** (3.09)
1977	347	65.98*** (3.87)	-0.13 (0.01)	-5.39 (1.23)	168.08* (1.78)	-4.58 (0.10)	1.52 (0.08)	78.28*** (4.51)	3.44 (0.35)	-5.45 (1.29)	1.10 (0.77)
1978	381	22.40* (1.83)	10.30 (1.24)	-3.51 (1.13)	15.29 (0.43)	13.36 (0.53)	-0.97 (0.11)	29.11** (2.53)	7.20 (0.96)	-2.69 (0.96)	3.31*** (2.98)
1979	386	17.93* (1.68)	9.77 (1.11)	-1.04 (0.45)	16.56 (0.57)	9.68 (0.40)	-1.48 (0.23)	22.32** (2.15)	6.63 (0.85)	-1.87 (0.83)	3.61*** (3.47)
1980	398	10.41 (1.04)	3.08 (0.38)	0.00 (0.00)	1.49 (0.07)	1.47 (0.08)	3.66 (0.80)	7.37 (0.85)	0.94 (0.13)	-0.22 (0.12)	3.39*** (3.59)
1981	422	9.94 (1.07)	15.33** (1.97)	-3.70** (2.06)	3.42 (0.22)	2.87 (0.22)	0.03 (0.01)	14.45* (1.75)	6.54 (1.05)	-3.27** (2.14)	2.11** (2.41)

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<sup>a</sup>The percentage impacts were calculated from the OLS log coefficients with the formula  $\% = e^B - 1$ , where B is the estimated log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

example, consider two police noncontributory pension plans, one of which requires a \$3000 annual contribution by the city for each officer and the other requiring a \$6000 contribution. The second plan is twice as expensive as the first, but on our contract scoring index both plans would receive ten points because both are noncontributory. Similarly, insurance plans can vary widely in cost, but these differences cannot be captured in our scoring index. As a result, we recommend that dollar cost data rather than our contract subindices be used to assess the impact of bargaining and arbitration upon the monetary terms of police employment relations.

#### Salaries and Fringes

Other research has shown that wages and fringes in the private sector are positively correlated; thus, high-wage companies also tend to pay high fringes, and vice versa (Freeman, 1981). To see if the same phenomenon exists in the police industry, we correlated the maximum patrol officer salary in each city with that city's fringe benefit expenditures per police officer. The results of this correlational analysis are presented in Table IV-10.

The correlation coefficients in Table IV-10 tell us the extent to which cities with high salaries also pay high fringes and cities with low salaries also pay low fringes. These coefficients indicate that in fact police salaries and fringes are positively correlated in a very significant manner. In other words, American cities exhibit a "rich get richer" tendency: cities which pay high police salaries also provide

TABLE IV-10

ZERO-ORDER CORRELATIONS BETWEEN MAXIMUM PATROL OFFICER  
SALARY AND FRINGE BENEFITS PER POLICE OFFICER

YEAR	All Cities	Cities in Nonarbitration States	Cities in Arbitration States
1971	.422*** (746)	.422*** (673)	.445*** (73)
1972	.469*** (706)	.448*** (622)	.596*** (84)
1973	.425*** (369)	.442*** (289)	.367*** (89)
1974	.368*** (661)	.430*** (509)	.244*** (152)
1975	.402*** (686)	.471*** (506)	.313*** (180)
1976	.456*** (714)	.543*** (515)	.304*** (199)
1977	.489*** (706)	.547*** (479)	.307*** (227)
1978	.453*** (722)	.544*** (497)	.252*** (225)
1979	.451*** (681)	.511*** (468)	.275*** (213)
1980	.447*** (693)	.525*** (469)	.286*** (224)
1981	.408*** (390)	.549*** (268)	.154* (122)

Sample sizes in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level;  
\*Significant at the .10 level (two-tailed tests).

generous fringes, and cities with low salaries provide modest fringes.

What is more interesting is the comparison between cities in arbitration and nonarbitration states. By comparing the correlation coefficients in the second and third columns of the table, we see that the positive salary-fringe association is much tighter outside of arbitration states than within them. What this pattern means is that cities in nonarbitration states almost always pay similar level of salaries or fringes (i.e., both the salary and the fringes will be high, medium, or low), while cities in arbitration states are somewhat less likely to do so. Although salaries and fringes are positively correlated in arbitration cities, the strength of this correlation is weaker than in other states. In other words, there is a greater tendency for cities in arbitration states to balance relatively high wages with relatively low fringes (and vice versa) than exists in cities which do not have access to arbitration.

## SUMMARY AND CONCLUSIONS

First, our analyses show that collective bargaining has had a very large and strongly positive association with police fringe benefits during the entire 1971-81 period. The CBA percentage coefficients in the fringe analyses are much larger than in the salary and total compensation analyses, which suggests that police unions may be systematically influencing cities to contribute larger amounts of money to fringes than these cities would contribute otherwise. Our results indicate that

fringe benefit expenditures are one-fifth to one-third (20-33 percent) higher in unionized cities than in nonunion cities, ceteris paribus. Because 80-90 percent of our fringe benefit measure is composed of retirement contributions (with the balance consisting of various insurance contributions), our results strongly suggest that unionized police officers prefer that a larger share of their total compensation package be devoted to retirement and insurance benefits than would occur in the absence of police unions. This finding is very consistent with the results of studies which have investigated the impacts of private sector unions (Freeman, 1981) and firefighter unions (Ichniowski, 1980) on the wage and fringe components of the total compensation package. In turn, our results are quite consistent with the "median voter" explanation of union behavior, namely, that police unions emphasize the compensation preferences of the more senior (or median) police officers more than would occur in a nonunion situation with its individual bargaining.

Second, our analyses show that collective bargaining increases the total cost of employing a police officer. During the 1971-80 years, unionized cities paid 8-12 percent more in total compensation per police department employee than nonunion cities paid, ceteris paribus. This difference disappeared in our 1981 data, which may indicate that (a) unionized cities no longer pay more due to unionism by itself, or (b) such a large proportion of all police departments have become unionized that spillover effects have made accurate union-nonunion comparisons rather difficult, or (c) the small sample size for 1981 somehow affected the results. Considering that the CBA coefficients in the 1981 maximum salary analyses in Chapter III also were not significant, item (a) seems

to be the most likely explanation.

Third, our analyses of arbitration's availability contain results which will please both arbitration proponents and opponents. Our aggregated data show that the availability of arbitration had a substantial positive impact on fringe benefits (i.e., 20-30 percent) during the 1974-79 years but had little or no effect on fringes either before or after those years, and that the availability of arbitration had an 8-12 percent positive impact on total compensation during the 1974-81 period. However, our disaggregated fringe-by-fringe and state-by-state analyses showed that arbitration seemed to be unequivocally associated with higher fringe benefits only in New York and Wisconsin cities and possibly in Michigan cities, and that arbitration seemed to be unequivocally associated with larger total compensation packages only in New York cities and possibly in Michigan cities. In other words, our fringe-by-fringe and state-by-state results strongly suggest that in most states arbitration has had little effect on the general levels of fringe benefits and total compensation or on specific fringe benefits, and this conclusion is very similar to the conclusion suggested by the state-by-state analyses of salaries in the preceding chapter.

Fourth, our analyses of the use of arbitration (again, defined as using the arbitration procedure to receive an award on any issue) indicate that neither the current use nor the prior use of arbitration has had any systematic and consistent association with the level of total fringe benefits, the level of specific fringes, or the level of total compensation. These nonimpacts are very similar to the nonimpact of the use of arbitration on salaries discovered in the preceding chapter.

Fifth, our contract subindex analyses showed that there was a much greater prevalence of fringe benefits and pay supplement provisions in police contracts in arbitration states compared to nonarbitration states, especially during the 1975-79 years. This result is consistent with the results of the aggregated analyses of fringe benefits and total compensation, and it also is consistent with the results of our complete contract index analyses presented in the next chapter. However, comparisons between our dollar analyses and contract score analyses should be made carefully, for our contract index may not yield much useful information about the actual dollar cost of various retirement and insurance contract provisions.

Sixth, our focus in this chapter on bargaining, arbitration availability, and arbitration use should not obscure the fact that, as with salaries, police fringes and total compensation are influenced by a wide variety of "market" variables. Our results show, for example, that fringe benefits and total compensation levels are significantly higher, during most years of the 1971-81 period, in larger cities, in wealthier cities, in cities with higher reported crime rates, in cities with a city manager form of government, and in cities located in North Central states. In particular, our analyses show that there are some strong regional effects on total compensation and especially on fringe benefits: North Central cities tended to pay the highest fringes and total compensation, and Southern cities generally paid the lowest. Combined with our salary results from the preceding chapter, our findings suggest that small nonunion police departments in the South pay the lowest salaries, fringes, and total compensation in the nation.

Finally, we saw that police salaries and fringes are positively correlated all across the country. However, this correlation is much stronger among cities which do not have access to arbitration than it is among those who do.

CHAPTER V

CONTRACTS

In the two preceding chapters we saw that arbitration is positively associated with higher police salaries and fringe benefits. However, cities and police unions negotiate and arbitrate over much more than pay and fringes. As a result, in this chapter we will examine how arbitration has affected the shape and substance of the police collective bargaining agreements, or contracts, negotiated around the country.

MODEL

Evaluation of Contracts

For decades union-management contracts languished as under-researched sources of information about collective bargaining. Most researchers, when measuring union impacts on management, followed one of two approaches: they focussed only on wages, which could be quantitatively measured, and essentially ignored all the other items in union contracts; or, they qualitatively examined selected nonwage items in a case study fashion. Both approaches provided very useful information, but neither method permitted precise analyses and comparisons of contractual employment terms across large numbers of union-management relationships.

This deficiency was remedied during the 1970s by the work of Tom Kochan and his colleagues, who developed a method to analyze the substance and determinants of union-management contracts across large numbers of bargaining units with almost the same precision as exists in union wage analyses (for example, see Kochan and Wheeler, 1975; Kochan and Block, 1977; Kochan, et. al., 1979). This method is relatively straightforward

and involves three key steps: (1) the collection of the desired union contracts; (2) the development of a contract scoring index, which is an instrument for evaluating the items in union contracts on a favorableness-to-the-union scale or index; and (3) the actual scoring of the collected contracts with the contract scoring index.

The centerpiece of this analytical method is the contract scoring index. These indices can be developed in a variety of shapes and can be tailored to fit the union-management circumstances of the occupation or industry being studied. For example, contract indices have been developed to evaluate firefighter contracts (Kochan and Wheeler, 1975), manufacturing contracts (Kochan and Block, 1977), local government contracts (Gerhart, 1976), and teacher contracts (Delaney, 1983b). The three essential features of any index are that it (a) lists a wide variety of contract items, (b) ranks the possible forms or options within each item on a favorableness-to-the-union scale, and (c) assigns point values to the various options of each item (usually, options which are more favorable to the union have higher point values than options which are less favorable). After the contract index has been developed, the contract scorers search through each contract and assign point values to the various contractual items based on the values listed in the index.

The actual contract scoring index which we developed and used is reproduced in Appendix I-3. Our index included 130 items, each of which was scored on a scale of zero points for the option least favorable to the union, ten points for the option most favorable to the union, and intermediate points for intermediate options. As can be seen by inspecting the index in Appendix I-3, some items were scored on a straightforward

no provision/some provision basis (i.e., either zero points or ten points) while other items had several options. Using our scoring index, a police contract which contained all 130 items specified in our index would have received a total of 1300 points if each item received the maximum of ten points. In fact, our highest contract score was 783.4 points.

An inspection of our index reveals that we weighted all the contract provisions equally (for no item can receive more than ten points). This means, for instance, that grievance arbitration and the union's right to free bulletin board are equally important in our scoring scheme. No police labor relations practitioner or observer would seriously argue that in fact these two provisions are equally important to police unions, or that all the items in our index are equally valuable items for police unions to negotiate into their contracts, and we wholeheartedly agree with this view. However, we have no reliable information about how American police unions evaluate the importance or worth of all these items relative to each other, nor do we have the resources to obtain such information. Further, it is quite likely that different police unions in different cities and states would evaluate these items differently. As a result, we had no choice but to assign an equal weight to each provision, for any differential weighting scheme we could devise would be hopelessly arbitrary and misleading.

Because of the large number of provisions in our total index, our equal weighting scheme should provide an accurate portrayal of contracts which are more and less favorable to the unions in our sample. However, this accuracy may be reduced somewhat for one or more of the several

subindices (working conditions, individual security, union security, equity, fringe benefits, pay supplements and intrusion into managerial prerogatives) because of the relatively small number of items in each subindex. As a result, we believe that our results from the total contract analyses are more reliable than the results from our subindex analyses.

There are three features of our contract index evaluation methodology which should be noted. First, our equal weighting scheme creates a bias in favor of contracts which contain large numbers of provisions, even if these provisions are not especially favorable to the unions. Consequently, it is theoretically possible that a police union could negotiate a short contract with a few items (such as staffing, shift assignment, transfer, layoff, maintenance of standards, limited management rights, and grievance arbitration provisions) which give the union an exceptionally strong voice in departmental operations, and yet the contract could receive a low score. We do not believe that this theoretical possibility is very likely to occur, however, because previous research (Feuille, Hendricks, Kahn, 1981) has indicated that contract scores are positively correlated with other measures of union power.

Second, our contract index may not measure and score every single provision in each contract we have collected. If union and management representatives have negotiated items which are not included in our index, these items will not be scored. As a result, our index may omit some important contractual provisions from subsequent analyses. Third, our index is not designed to pick up and record all changes in contract language. Instead, the index is designed to identify only those

changes which are clearly more favorable or less favorable to the union.

Even with these three features, we believe our contract scoring index is extremely well suited for this research. It contains many more items than earlier indices (for example, one index used to score firefighter contracts contained 54 items (Kochan and Wheeler, 1975)), so it should enable us to perform comprehensive analyses of police contracts. In addition, it has been tailored to fit the specific circumstances of police collective bargaining, with the result that many of the items in the index (e.g., type of sidearm carried, armament in squad cars, firearms qualification provision, etc.) are unique to the police industry (and hence would be unlikely to be included in a more general scoring index designed to score contracts across numerous occupations.)

All of the contracts and awards we collected were scored by one of three contract scorers during the July 1982 - May 1983 period. To ensure consistency, the scorers underwent a rigorous three week training program in the use of the index. By the end of that period all three scorers evaluated contract items in an almost perfectly consistent manner, with one exception. The exception was maintenance of standards clauses, which were all scored by one scorer. In addition, all the contracts and awards from within a single state were scored by the same scorer to ensure that any state-specific bargaining patterns were evaluated consistently.

#### Determinants

In our contract analyses we will use an analytical model which is

similar to the models we used in our analyses of salaries and fringes. Because salaries, fringes, and other contract items are bargained simultaneously at the negotiation table or addressed simultaneously in an arbitration award, the factors which influenced pay and fringes also might have influenced other contractual provisions. Consequently, we will include all the city and region characteristics identified earlier (population, density, crime rate, per capita income, median value of housing, percent black, form of government, nonmetropolitan location, average manufacturing wage, and region of the country). Because in private sector contracts wage and nonwage contract terms are positively correlated (Kochan, 1980; Feuille, Hendricks, and Kahn, 1981), we expect that these variables will be positively or negatively associated with contract scores in the same manner that these variables were positively or negatively associated with salaries in Chapter III.

The analyses in this chapter necessarily must be limited to the bargaining cities sample, for there are no nonunion contracts in nonbargaining cities which we can use as a control group. As a result, we cannot use our bargaining variable (CBA) to identify the impact of bargaining on contract scores. Instead, we will use our length of the bargaining relationship term (CBAGE) to measure the effect that bargaining longevity has on contract terms. Because a police union should be able to negotiate a larger number of more favorable terms into a contract as it has increased opportunities to do so, we expect that CBAGE will be positively associated with contract scores.

In our contract score determination model, it is imperative to control for the influence of state bargaining legislation which affects

police. In general, police unions whose bargaining rights are statutorily guaranteed should be able to negotiate more favorable contracts than police unions in states which provide no statutory bargaining protection. This disparity should occur because bargaining legislation usually places an obligation on management to negotiate over all the subjects which exist within the scope of bargaining specified in the legislation, whereas management has no corresponding statutory obligation to bargain over the same range of subjects in states without bargaining legislation.

In addition, we know that different state bargaining statutes may have different impacts on both the range of subjects over which management is obligated to bargain and the depth or intensity of management's bargaining obligation. In particular, employers in "meet and confer" law states have a legal obligation to discuss employment terms with their unions, while employers in "duty to bargain" states have a legal obligation to negotiate employment terms to a conclusion with their unions. The extent of the employers' legal obligation is weaker in meet and confer states than in duty to bargain states (though the practical difference may be small), but employers under either type of law have a more substantial bargaining obligation than when no law exists.

In addition, state legislatures have regulated the scope of bargaining in three ways. First, some states have specified a "laundry list" of subjects over which a bargaining obligation exists, which means that employers have no legal obligation to bargain over the subjects not included in the list. Second, some states have said that the scope of bargaining includes "wages, hours, and other terms and conditions of employment" and then simultaneously listed specific exclusions from this

scope, such as pension provisions, civil service requirements, and so forth. Third, other states have established the potentially widest scope of bargaining by specifying it to include "wages, hours, and other terms and conditions of employment" without any statutory exclusions (i.e., as done in the National Labor Relations Act).

As a result, we have constructed a bargaining law index variable (BARGLIND) to capture the influence of the different scope of bargaining requirements in police bargaining legislation. This index term has six categories, arranged in ascending order of increasingly stronger scope requirements:

1. Meet and confer states with laundry list scope;
2. Meet and confer states with specific limitations on the scope;
3. Meet and confer states with no specific limitations on the scope;
4. Duty to bargain states with laundry list scope;
5. Duty to bargain states with specific limitations on the scope;
6. Duty to bargain states with no specific limitations on the scope.

The inclusion of this bargaining law index will enable us to measure the impact that different types of bargaining legislation have on the shape of police contracts. In turn, this term should ensure that the arbitration variables do not pick up and report any effects due to the statutory protection of bargaining. Because the reference category for the BARGLIND variable is states with no bargaining legislation, we expect the BARGLIND term to be positively related to the favorableness (to the union) of police contracts. Such positive BARGLIND coefficients would mean that police unions are able to negotiate more favorable

contracts as the statutory scope of bargaining becomes stronger.

Finally, we will include the same arbitration availability (ARB), current arbitration use (ARBUSE), and prior arbitration use (PASTUSE) variables identified in the two previous chapters to measure whatever effects arbitration might have had on police contracts. Following the pattern of results identified in the two preceding chapters, we expect the ARB coefficients to be positive and significant, and the ARBUSE and PASTUSE coefficients to have little or no impact.

Accordingly, we will test the usefulness of this analytical model with ordinary least squares (OLS) multiple regression equations of the following form:

$$\begin{aligned} \lnTCI &= B_0 + B_1 \lnPOP + B_2 \lnCRATE + B_3 \text{ DENSITY} \\ \text{or} &+ B_4 \lnPCI + B_5 \lnHOUSE + B_6 \text{ PBLACK} \\ \lnSUBI &+ B_7 \text{ CMGR} + B_8 \text{ ICITY} + B_9 \text{ OPPWAGE} \\ &+ B_{10} \text{ NEAST} + B_{11} \text{ SOUTH} + B_{12} \text{ WEST} \\ &+ B_{13} \text{ CBAGE} + B_{14} \text{ BARGLIND} + B_{15} \text{ ARB} \\ &+ B_{16} \text{ ARBUSE} + B_{17} \text{ PASTUSE} + e \end{aligned}$$

where

$\lnTCI$  = log of the total contract index score in each year  
 $\lnSUBI$  = log of the various contract subindex scores in each year  
 CBAGE = age of the bargaining relationship in years  
 BARGLIND = an index variable which categorizes police bargaining legislation into:

1. Meet and confer state with "laundry list" scope (District of Columbia 1975-79),
2. Meet and confer states with specific limitations on the scope (California, Kansas),
3. Meet and confer states with no specific limitations on the scope (Oklahoma),
4. Duty to bargain states with "laundry list" scope (Iowa, Nevada),
5. Duty to bargain states with specific limitations on the scope (Alaska, Connecticut, Delaware, Florida, Hawaii, Minnesota, New Hampshire, New York, Vermont),
6. Duty to bargain states with no specific limitations on the scope (District of Columbia 1980-81, Maine, Massachusetts, Michigan, Montana, Nebraska, New Jersey, Oregon, Pennsylvania, Rhode Island, South Dakota, Washington, Wisconsin),

and all the other terms are the same as identified in earlier chapters. Our analyses will be limited to the years 1975-81, for these are the years for which we collected contracts and awards.

#### RESULTS

Table V-1 presents the total contract scores and several subindex scores for the 1975-81 period. All of these contracts cover the bargaining units which contain patrol officers (plus, in some cities, one or more superior officer ranks); there are no superior officer-only contracts in this sample. This listing of averages (or means) and standard deviations shows that during this period the average contract improved about 93 points (or 25 percent) on our favorableness-to-the-union scoring scale. However, the standard deviation remained rather large. These data indicate that over time police unions have steadily bargained more favorable contracts but that these contracts still vary a great deal in

their favorableness to the union. For instance, the 1981 coefficient of variation of these contract scores is 27.9 percent while the 1981 coefficient of variation of maximum salaries is 18.1 percent (as discussed in Chapter III, the coefficient of variation is a measure of dispersion which is calculated by dividing the standard deviation by the mean). In other words, in 1981 contract scores varied much more than maximum salaries.

The scores in Table 1 also show that the index and subindex scores in contracts from arbitration states are generally much larger than in contracts from states without arbitration. However, this advantage in favor of arbitration has steadily declined over time: in 1975, contracts from arbitration states scored 47 percent higher (136 points) on our total index than the contracts from other states, and by 1981 this difference had declined to 28 percent (or 110 points). In other words, over time contracts in both types of states steadily became more favorable to the unions, and the rate of increase was faster in nonarbitration states. As a result, it appears that over time police contracts are slowly becoming more similar across all states.

Our subindex scores also demonstrated some interesting patterns (keep in mind that because we analyzed the fringe benefits and pay supplements subindices in the preceding chapter we will not re-analyze them here). In particular, the working conditions, individual security, equity, and management prerogatives subindices increased in strength during our time period in both arbitration and nonarbitration states, though the large standard deviations indicate that some contracts scored quite well on these subindices while others scored quite poorly. In

contrast, the average union security subindex score remained almost unchanged during our time period.

The figures in Table V-2 confirm that average contract scores are higher in arbitration states than in other states. This table presents the average total contract scores and standard deviations across the cities in those 22 states from which we had collected four or more contracts. The availability of arbitration may not guarantee the inclusion of a large number of favorable (to the union) contract provisions, but it certainly seems to help. As can be seen, nine of the first eleven states had arbitration laws. Together, Tables V-1 and V-2 strongly suggest that arbitration has an effect on the favorableness of union contracts, and we now turn to our multivariate analyses in order to isolate arbitration's impact.

#### Total Contract Scores

Table V-3 presents the results of our analyses of the influence of bargaining age, bargaining law, arbitration availability, and arbitration use on total contract scores (the complete results of this analysis for 1981 are presented in Appendix V-1). The first half of the table presents the percent coefficients associated with our five variables of interest, and the second half of the table presents the contract index points associated with these percentages.

Length of the bargaining relationship. We expected that the CBAGE coefficients would be significantly positive, for police unions should be able to bargain more favorable contracts as they acquire increased

TABLE V-1  
 AVERAGE CONTRACT SCORES  
 IN ARBITRATION AND NONARBITRATION STATES

YEAR	N	Total Index		Working Conditions		Individual Security		Union Security		Equity		Management Prerogatives	
		$\bar{X}$	S.D.	$\bar{X}$	S.D.	$\bar{X}$	S.D.	$\bar{X}$	S.D.	$\bar{X}$	S.D.	$\bar{X}$	S.D.
1975	205	356.0	147.5	31.5	17.2	20.2	21.7	81.9	18.7	69.6	46.3	64.1	38.2
Arb	99	426.6	119.3	38.8	16.7	27.8	23.0	85.8	20.8	87.5	38.1	80.3	35.9
Nonarb	106	290.0	141.0	24.7	14.6	13.1	17.7	78.3	15.7	52.9	47.2	48.9	33.9
1976	312	356.0	147.7	31.8	17.6	20.6	20.7	81.5	18.5	69.6	47.8	63.9	38.2
Arb	131	442.3	116.3	40.2	16.7	29.1	17.9	85.5	21.1	91.7	36.6	83.2	34.6
Nonarb	181	293.6	136.3	25.8	15.6	14.5	21.4	78.6	15.8	53.7	48.6	49.9	34.5
1977	367	378.4	147.1	34.1	17.9	22.9	21.0	82.0	19.2	75.4	47.8	69.3	38.6
Arb	196	453.2	111.0	41.1	17.0	29.4	20.7	86.6	21.0	96.0	34.4	86.7	33.0
Nonarb	171	292.6	136.3	26.0	15.4	15.4	18.9	76.6	15.3	51.7	50.1	49.2	34.6
1978	406	404.5	138.1	36.2	17.9	25.6	21.1	83.0	19.5	81.5	46.0	74.9	37.2
Arb	219	469.8	104.8	41.9	16.7	31.5	20.6	87.7	20.8	99.5	32.0	89.5	30.9
Nonarb	187	328.1	133.2	29.4	16.8	18.8	19.5	77.5	16.3	60.3	50.8	57.8	36.8
1979	413	418.8	136.8	37.1	17.6	26.8	20.9	83.5	20.0	85.1	44.9	77.6	37.0
Arb	220	481.1	104.7	42.5	16.5	31.5	20.4	88.3	21.5	102.0	32.1	90.9	30.7
Nonarb	193	347.8	134.8	30.8	16.8	21.5	20.3	77.9	26.7	65.9	49.5	62.4	37.8
1980	436	434.9	133.3	38.2	17.1	29.0	20.9	83.5	20.0	88.0	44.6	80.4	36.3
Arb	235	487.4	109.6	42.7	16.3	32.6	20.6	88.2	21.7	103.0	33.3	91.6	31.8
Nonarb	201	373.5	132.5	32.9	16.7	24.8	20.5	78.1	16.1	70.4	49.4	67.3	37.0
1981	461	449.2	125.4	39.2	16.9	30.5	20.3	83.0	20.7	91.8	42.3	83.2	34.4
Arb	242	501.5	101.0	43.5	16.2	33.8	20.1	88.5	22.1	106.8	29.7	94.5	29.5
Nonarb	219	391.5	124.5	34.5	16.5	26.7	20.0	76.8	17.1	75.2	47.6	70.7	35.2

1981 Total Index Range: 124.8 to 783.4 points.

TABLE V-2  
1981 AVERAGE TOTAL CONTRACT SCORES BY STATE

<u>State</u>	<u>Average</u>	<u>Standard Deviation</u>
(A) Michigan (26)	633.3	68.4
(A) Connecticut (16)	568.1	93.1
(A) Oregon (6)	547.7	58.4
Nevada (4)	500.0	136.9
(A) Iowa (15)	498.6	80.4
(A) New Jersey (35)	495.8	90.1
Montana (5)	493.8	42.6
(A) Rhode Island (7)	491.7	51.4
(A) New York (32)	490.9	77.8
(A) Massachusetts (42)	487.8	73.3
(A) Minnesota (18)	479.9	52.6
Florida (23)	475.2	81.4
(A) Washington (9)	472.6	58.0
Ohio (22)	459.6	136.1
Texas (5)	459.0	95.0
Oklahoma (6)	441.3	60.5
(A) Wisconsin (14)	429.0	94.9
Illinois (26)	425.5	102.4
(A) Pennsylvania (19)	403.7	133.4
Colorado (4)	395.0	142.5
Indiana (9)	353.7	126.4
California (90)	322.5	109.9

(A) indicates arbitration state; numbers in parentheses are cities for which we have 1981 contracts; each state needed at least four contracts to be included.

opportunities to do so. However, in the presence of the other variables in our model CBAGE contributed very little to contract strength. In 1975, for example, each additional year of bargaining added 4.8 points to the typical contract, but by 1981 each additional year added less than one point (and this amount was not statistically significant). The CBAGE coefficients might well be strongly positive if some of the other variables were removed, for our Table V-1 descriptive statistics showed that as time passes police contracts have become clearly more favorable to the unions. Be that as it may, our Table V-3 results show that a lengthier bargaining relationship by itself does not contribute significantly to the favorableness of police union contracts.

Bargaining legislation. In contrast, the BARGLIND coefficients are almost uniformly significant and positive. These coefficients mean that in 1980, for example, each incremental advancement from one bargaining law category to the next in our bargaining law index was associated with an approximate two percent (or eight point) improvement in the strength of police contracts. In other words, as bargaining legislation places upon employers a stronger duty to bargain obligation involving a wider scope of negotiations, police unions are able to bargain more favorable contracts. Because our bargaining law index has six categories and the reference (or excluded) category is "no law," our 1980 results imply that police unions in the states with the strongest bargaining laws are able to bargain contracts which are several percentage points more favorable than the contracts negotiated by police unions in states with no bargaining legislation, ceteris paribus.

We recognize that there are some potential problems interpreting the

TABLE V-3  
 IMPACTS OF ARBITRATION AVAILABILITY,  
 CURRENT USE, AND PRIOR USE ON CONTRACT INDEX SCORES

YEAR	N	PERCENTS <sup>a</sup>					INDEX POINTS				
		CBAGE	BARGLIND	ARB	AREUSE	PASTUSE	CBAGE	BARGLIND	ARB	AREUSE	PASTUSE
1975	194	0.95 (1.16)	4.32*** (3.78)	38.96*** (3.78)	-20.99* (1.71)	-17.83*** (3.18)	4.8** (2.01)	12.7*** (3.86)	91.9*** (3.59)	-52.0 (1.59)	-51.0*** (3.36)
1976	291	0.92 (1.39)	2.93*** (3.11)	33.83*** (3.87)	-5.11 (0.63)	-12.04*** (2.88)	4.3** (2.22)	8.6*** (3.14)	92.2*** (4.14)	-10.3 (0.44)	-36.9*** (3.16)
1977	347	0.84* (1.73)	0.61 (0.53)	61.27*** (4.51)	5.15 (0.76)	-7.98*** (2.82)	3.5** (2.29)	2.7 (0.74)	150.6*** (4.53)	28.5 (1.37)	-26.5*** (3.09)
1978	381	0.76* (1.70)	3.36*** (3.52)	23.40** (2.31)	6.40 (1.17)	-5.61** (2.71)	3.3** (2.23)	12.2*** (3.91)	67.8** (2.25)	22.0 (1.25)	-17.1** (2.55)
1979	386	0.82* (1.89)	2.90*** (3.22)	23.55** (2.47)	2.03 (0.35)	-2.61 (1.62)	3.2** (2.13)	10.7*** (3.45)	73.7** (2.48)	6.3 (0.32)	-8.4 (1.50)
1980	398	0.28 (0.71)	2.13** (2.36)	17.34* (1.88)	3.11 (0.56)	-1.52 (1.01)	1.4 (0.99)	8.2*** (2.58)	59.1** (1.96)	8.8 (0.45)	-5.1 (0.97)
1981	422	0.02 (0.06)	1.68** (1.17)	18.95** (2.39)	5.18 (1.08)	-2.56** (2.21)	0.6 (0.46)	7.4** (2.56)	64.5** (2.35)	18.1 (1.02)	-9.2** (2.14)
			<u>BARGLAW</u>				<u>BARGLAW</u>				
1981	422	0.00 (0.02)	-6.50 (1.10)	40.14*** (5.78)	5.26 (1.09)	-2.20* (1.89)	0.48 (0.37)	-16.2 (0.75)	129.7*** (5.87)	18.4 (1.03)	-7.9* (1.82)

<sup>a</sup>The percentage coefficients are calculated with the formula  $\% = e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level,

\*\*Significant at the .05 level,

\*Significant at the .10 level (two-tailed tests).

results associated with our BARGLIND variable. For example, the use of such an index variable requires the assumption that the difference between any two legislative categories in the index is the same as the difference between any other two legislative categories (i.e., that the differences across all the categories are equal). In fact, it is possible that most of the results reported in the BARGLIND coefficients reflect a difference between categories four and five or five and six (for it is categories five and six in which most states fall). If this is the case, our bargaining law index variable may be unnecessarily complicated and might even be obscuring the influence of bargaining legislation in general.

To test for the possibility that some kind of bargaining legislation is more important than the differences across various types of bargaining laws, we re-estimated our 1981 regression after replacing the BARGLIND variable with a BARGLAW dummy variable which had a value of one for each city in a state with any kind of bargaining legislation and zero for each city in a state with no police bargaining legislation.

The BARGLAW results are reported in the last row of Table V-3, and they are troubling. The BARGLAW coefficient is negative though insignificant, which means that police unions in states with protective bargaining legislation bargain either the same or less favorable contracts than police unions in states without such statutory protection. Not only does this result contradict the BARGLIND results, it is not very believable. It suggests that police unions are no better off when they acquire a legally guaranteed right to bargain, and hence it suggests that police union lobbying for bargaining legislation has been a waste

of time. Neither of these implications makes much sense.

In addition, the use of the BARGLAW variable more than doubles the magnitude of the arbitration availability (ARB) term (from 19 percent to 40 percent). Because almost all the arbitration states ranked high on our bargaining law index (i.e., almost all the arbitration states were in the two top index categories), and because the BARGLAW dummy variable cannot differentiate among bargaining statutes with differing degrees of strength, the inclusion of BARGLAW in the equation seems to transfer some of the influence of particularly favorable (to the union) bargaining legislation to the arbitration availability term.

Consequently, the BARGLAW term does not appear to have done an effective job of measuring the influence of different types of bargaining legislation. Further, BARGLAW appears to assign a stronger influence to arbitration's availability than is warranted. As a result, we believe that the results generated with the BARGLIND variable provide more accurate measures of the influence of bargaining legislation and of arbitration on the favorableness of police contracts than does the use of the BARGLAW term.

Methodologically, the contrast between our BARGLIND and BARGLAW results provide a cautionary note. These different results clearly show that our findings in this chapter are sensitive to the specification of our analytical model, and alternative specifications might yield results different than those presented here. Consequently, our findings should be interpreted with this methodological warning in mind.

Arbitration availability. The ARB term is strongly positive in each year of our time period, and the ARB coefficients indicate that police

contracts in arbitration states are 17 to 61 percent more favorable to the unions than police contracts in nonarbitration states during the various years, after controlling for the influences of other factors. Further, the ARB coefficients are much stronger than the bargaining age, bargaining law index, and arbitration use coefficients. Hence, the Table V-3 results indicate that arbitration's presence contributes much more to police contract strength than the longevity of the bargaining relationship or the actual use of arbitration.

Arbitration use. In six of the seven years the current arbitration use (ARBUSE) coefficients are not significantly different from zero, which means that the police unions which arbitrate (on any issue) in a given year do not have more favorable contracts than those who negotiate. However, the previous arbitral use (PASTUSE) coefficients are always negative and are significantly negative in five of the seven years. This means that in a given year police unions which have actually used arbitration in prior years have less favorable contracts than those who negotiated their own contracts during those previous years. In turn, the consistently negative PASTUSE coefficients plus the negative ARBUSE coefficients for 1975-76 indicate that some police unions with comparatively less favorable contracts apparently have used arbitration in an attempt to catch up to their more favorably situated peers in other cities.

Other influences. The results for the complete model in 1981 are presented in Appendix V-1. These coefficients reveal that several other factors have strong associations with police contract strength: police unions in larger cities have more favorable contracts than in smaller cities, police unions in poorer cities have more favorable

contracts than in wealthier cities (which is the exact opposite of our salary result), police unions in cities with high manufacturing wages have much more favorable contracts than in cities with low manufacturing wages, and police unions in cities in Western states have substantially less favorable contracts than police unions located in North Central cities. These patterns also apply during the 1975-80 period. In addition, police unions in cities with higher crime rates had more favorable contracts during the 1975-78 and 1980 years.

Our total contract estimation equations are highly significant for each year, and about half of the variables in our model are also significant in each year. As a result, our model seems to be correctly specified. In addition, our model explains 30 percent to 48 percent of the variation in contract scores in various years. Interestingly, the amount of explained variation declines over time. Further, our analytical model explains less of the variation in contract scores than it explains of the variation in patrol officer salaries, but this is to be expected given the artificial nature of our contract scoring index.

In sum, police contract scores are positively associated with supportive bargaining legislation and the availability of arbitration, and are positively and negatively associated with a variety of "market" factors.

#### Subindex Scores

As noted in the preceding chapter, we have divided our total contract index into several subindices. In the preceding chapter we examined the

impact of arbitration on the monetary subindices (fringe benefits and pay supplements); in this chapter we examine the impact of arbitration on the nonmonetary subindices of working conditions, individual security, union security, equity, and intrusion into management prerogatives. However, before we present and discuss our results it is necessary to describe the purpose of and content of these subindices.

Our contract subindices are designed to measure the different kinds of provisions which are included in police contracts. Following the categorizations developed by Kochan and Block (1977), we grouped all of our total contract index items into six categories: the fringe benefits and pay supplements categories examined in Chapter IV, and the four nonmonetary categories of working conditions, individual security, union security and equity. As can be seen in Appendix II-3, the working conditions (WC) subindex includes 18 scheduling, equipment, and other items which affect the actual on-the-job conditions of work. Many of these WC items are police-specific, such as the eight different provisions regarding firearms and the use of force.

The individual security (IS) subindex includes 11 items which affect the individual officer's ability to transfer, select shifts or vacations, or be protected from layoffs. Not surprisingly, a majority of the IS items involve the use of seniority to make personnel decisions.

The union security (US) subindex contains 19 items which affect the institutional strength or security of the union and union representatives in the workplace. These items include recognition, dues checkoff, time and space for union business, and no strike clauses. It is worth noting that the degree of compulsory union support or membership (i.e., open

shop, maintenance of membership, agency shop, or union shop) is only one item in this subindex. In addition, the US subindex also includes a management rights clause (which, of course, is reverse scored).

The equity (EQ) subindex contains 19 items which affect the ability of the individual officers and the union to be protected from adverse managerial actions and to seek redress from such actions. Eight of these items involve grievance procedures, and the remainder deal with such topics as performance appraisal, internal investigations, discipline, and rule change procedures.

These four subindices plus the subindices examined in the previous chapter together are both exclusive and exhaustive: each item in the total contract index appears in only one subindex, and these six subindices contain all the items in the total index. We recognize that the specification of particular items in one subindex or another is somewhat arbitrary, for some items arguably could be placed in different subindices (for example, the grievance procedure items could be placed in the IS or US subindices almost as well as in the EQ subindex). However, we believe that, on balance, each of the items in each subindex is directly related to the theme expressed in the title of that subindex. As a result, our subindices provide us with a method to determine if arbitration enables unions to obtain particular types of contract provisions.

We also constructed an intrusion into management prerogatives (MPI) subindex. This subindex contains 31 items which directly limit management's discretion or flexibility to do such things as assign officers to jobs, require unscheduled work, change personnel practices, impose discipline, and regulate the issuance and use of weapons. The MPI is a cross-over

or hybrid subindex, in that all of its items also exist in one or another of the six subindices described earlier. However, we believe that the MPI provides a better measure of the extent to which police contract language directly constrains management's unilateral freedom to manage than is provided by the other six subindices.

We recognize that all of the items in any union contract pose some direct or indirect constraint upon management, for the employer is obligated to honor all contractual provisions, and presumably few of these provisions would exist in a nonunion workplace. We believe that our total contract index is an excellent device for measuring all of the various kinds of items which police unions might negotiate, but we also believe that our MPI subindex provides a better measure of the extent to which police contracts directly constrain management's discretion to allocate and discipline employees. As with the other six subindices, higher MPI scores indicate contract language which is increasingly favorable to the unions.

Results. The results of our subindex analyses are presented in Table V-4. The coefficients in this table represent the percentage difference in the various subindex scores under the ARB, ARBUSE, and PASTUSE conditions compared to the subindex scores of police contracts in nonarbitration states, controlling for the influence of the other variables in our model.

The Table V-4 results must be interpreted with great caution, for the relatively small number of items in each subindex means that relatively minor differences between contracts in arbitration and nonarbitration states can generate unusually large percentage coefficients in our

regression analyses. For example, our results indicate that in 1975 the EQ subindex in arbitration state contracts was 1925 percent(!) greater than in contracts negotiated in nonarbitration states, ceteris paribus. Coefficients of this magnitude are difficult to interpret; in this instance, it means that 1975 police contracts in nonarbitration states had far fewer or weaker equity provisions in them than did contracts in arbitration states.

Similarly, some of these very large coefficients are not statistically significant. For example, in 1976 the IS subindices in arbitration state contracts were 268 percent larger than the IS subindices in contracts negotiated in other states, but this huge percentage difference was not statistically significant. This huge but nonsignificant difference means that there is so much variation in the strength of the IS subindex across our sample of contracts that we can draw no statistical conclusion for 1976. There are many large but nonsignificant percentage coefficients in Table V-4, and they must be interpreted carefully.

With these warnings in mind, Table V-4 supports a variety of conclusions. Most noticeably, location in an arbitration state apparently made a difference on three of the four subindices during the 1975-77 period, but during the 1978-81 period this difference disappeared. By 1980, arbitration's availability no longer is associated with any statistically significant differences in the strength of the four primary subindices. In addition, the use of arbitration in any given year (ARBUSE) seems to make no significant difference with any of our subindices. However, the use of arbitration in previous years (PASTUSE) seems to be associated with significantly weaker WC and EQ subindices during the first half of

our time period but not during the second half. This finding implies that during the earlier part of our period the unions who actually used arbitration did so (at least in part) to catch up with the WC and EQ terms negotiated by their peers in other cities, and by 1980-81 the previous users of arbitration had indeed caught up.

Union security. As we saw in Table V-1 and in the preceding chapter, five of six primary subindices consistently grew in strength during our 1975-81 period. The single exception to this trend occurred with the union security subindex, for its average score in 1981 was almost identical to its average score in 1975.

The results in Table V-4 are consistent with this earlier finding, for the US coefficients appear impervious to the availability and use of arbitration throughout the 1975-81 period. In particular, the US coefficients are much smaller than the coefficients for the other subindices. When combined with the Table V-1 results, these findings imply that police unions obtain a degree of institutional security not long after bargaining is established which appears to remain relatively fixed for several years thereafter. There undoubtedly are individual exceptions to this conclusion, but in general the union security subindex is the only subindex whose average score does not improve over time -- irrespective of arbitration's presence or absence.

Management prerogatives. The contrast between the coefficients for our four primary subindices and our hybrid management prerogatives subindex is striking. The ARB coefficients in the MPI equations are positive and statistically significant in each year; these results indicate, for instance, that during the 1978-81 years police contracts

TABLE V-4

PERCENTAGE IMPACTS OF ARBITRATION AVAILABILITY,  
CURRENT USE, AND PRIOR USE ON SUBINDEX SCORES<sup>a</sup>

YEAR	N	WORKING CONDITIONS			INDIVIDUAL SECURITY			UNION SECURITY			EQUITY			MANAGEMENT PREROGATIVES		
		ARB	AREUSE	PASTUSE	ARB	AREUSE	PASTUSE	ARB	AREUSE	PASTUSE	ARB	AREUSE	PASTUSE	ARB	AREUSE	PASTUSE
1975	194	46.81*** (3.05)	-20.31 (1.14)	-25.03*** (2.99)	866.30** (2.31)	-48.94 (0.32)	-26.91 (0.41)	7.00 (1.33)	-8.04 (1.18)	-5.41* (1.74)	1925.70*** (2.88)	-428.14 (1.24)	-648.80*** (3.26)	71.00*** (3.35)	-31.05 (1.32)	-34.43*** (3.12)
1976	291	37.07*** (2.80)	-10.58 (0.08)	-16.51*** (2.59)	268.47 (1.52)	13.04 (0.14)	-35.65 (0.68)	2.74 (0.58)	-0.06 (0.13)	-3.27 (1.33)	404.25* (1.93)	-41.55 (0.39)	-302.65*** (3.17)	52.41*** (3.15)	-8.75 (0.59)	-25.33*** (3.22)
1977	347	36.97** (1.96)	0.03 (0.00)	-8.53** (1.98)	866.10* (1.86)	0.16 (0.00)	-12.01 (0.36)	6.91 (0.94)	14.61*** (3.08)	-4.72** (2.53)	815.13** (1.85)	8.32 (0.11)	-122.60*** (2.59)	89.82*** (3.45)	3.64 (90.31)	-15.45*** (3.00)
1978	381	11.16 (0.70)	-1.74 (0.20)	-4.89 (1.43)	163.48 (0.95)	32.14 (0.47)	-27.65 (1.07)	7.27 (1.07)	3.23 (0.83)	-1.09 (0.74)	227.00 (1.27)	22.01 (0.37)	-72.77*** (2.64)	33.74* (1.74)	10.35 (2.02)	-13.02*** (3.31)
1979	386	3.38 (0.24)	1.90 (0.20)	-2.63 (1.00)	129.70 (0.91)	-38.61 (0.53)	-3.29 (0.19)	9.68 (1.48)	-2.59 (0.61)	-0.02 (0.02)	277.50* (1.62)	10.38 (0.18)	-33.42* (1.88)	34.64* (1.86)	-0.69 (0.06)	-5.95* (1.93)
1980	398	-8.63 (0.64)	-1.42 (0.17)	-1.56 (0.68)	175.94 (1.21)	4.82 (0.09)	-3.16 (0.21)	5.60 (0.87)	-1.03 (0.25)	0.13 (0.12)	142.69 (1.09)	54.16 (0.84)	-20.15 (1.29)	33.94* (1.89)	9.19 (0.88)	-4.54* (1.65)
1981	422	4.54 (0.37)	8.08 (1.01)	-3.66* (1.91)	158.08 (1.34)	-38.79 (0.72)	1.23 (0.11)	3.55 (0.56)	3.36 (0.83)	0.00 (0.00)	44.41 (0.55)	23.21 (0.48)	-11.57 (1.04)	33.03** (2.13)	8.97 (1.00)	-4.47** (2.08)

<sup>a</sup>The percentage coefficients are calculated with the formula  $\% = e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level,

\*\*Significant at the .05 level,

\*Significant at the .10 level (two-tailed tests).

in arbitration states consistently scored about 33-34 percent higher on the MPI subindex than police contracts in other states, even after controlling for the influence of other factors. The ARBUSE coefficients are never significant (which matches the pattern established with the other subindices), but the PASTUSE coefficients are negative and significant in each year. The negative PASTUSE coefficients mean that police unions which actually went to arbitration prior to the year in question had contracts with weaker MPI provisions than police unions which had never used arbitration (in both arbitration and nonarbitration states); presumably, the unions which did use arbitration might have been trying to catch up to the MPI provisions obtained by their peers in other cities.

The Table V-4 results do not show that arbitration causes police contracts to contain more provisions which directly limit managerial discretion, for it is possible that police contracts in arbitration states would have scored high on the MPI subindex even if these states never adopted arbitration. Similarly, the absence of any pre-1975 police contracts in our data set has prevented us from doing the kind of "before and after" analysis we did with salaries and fringes. However, the table shows that arbitration is unequivocally associated with higher MPI scores (which reflect a greater number of MPI provisions, MPI provisions which are more rather than less favorable to the union, or both). Further, although the MPI differences between arbitration and nonarbitration states are smaller in 1981 than in 1975, the arbitration advantage shows no sign of disappearing (i.e., it is as large in 1981 as it was in 1978).

Managers naturally are opposed to contract terms which directly limit their ability to allocate, direct, assign, and discipline employees, and the high MPI scores in arbitration state contracts offer a partial explanation for managerial opposition to arbitration laws. To the extent that these more restrictive contract provisions are associated with the presence of a statutory arbitration procedure, it is natural to attribute causality of the former to the latter.

We note that we have not collected any data about the actual use or application of these managerial prerogative restrictions in U.S. police departments. Consequently, there is nothing in this chapter which says or implies that these MPI provisions prevent management from assigning the right officer to the right job, or from deploying the necessary number of officers during the high-crime hours, or from disciplining errant officers, and so on. In other words, we have not collected any data about the actual operation of personnel practices in police departments. As a result, our contract analyses do not permit us (or anybody else) to conclude that arbitration leads directly to contract provisions which handcuff police management.

However, we have collected several years of crime rate and clearance rate data, and in the next chapter we will examine how these output measures are affected by unionization and arbitration.

Other influences. The other explanatory factors identified earlier in the total contract analyses also operate in a similar fashion in the subindex analyses: subindex scores are higher in larger cities than in smaller cities, are higher in poorer cities than in wealthier cities, are higher in cities with higher rather than lower manufacturing wages, and

are lower in Western cities than elsewhere. As with our total contract equations, our subindex equations are highly significant for each year.

In addition, as would be expected, our analysis explains less of the variation in subindex scores than in the total contract scores. Our model explains from 10 percent to 40 percent of the variation in particular subindices in particular years, and, as with the total index, the amount of explained variation declines rather steadily during our 1975-81 period. This relatively modest explanatory power is not surprising considering both the artificial nature of our subindices and the fact that there is a "restricted range" associated with each subindex.

#### Grievance Procedures and Grievance Arbitration

A central feature of American collective bargaining agreements in the private sector is the almost universal inclusion of a grievance procedure culminating in binding arbitration. The widespread existence of grievance arbitration in private sector contracts reflects the quid pro quo whereby unions have relinquished their right to strike over contract interpretation disputes in return for the employers' promise to arbitrate such disputes.

However, grievance arbitration is not nearly so widespread in the police industry. For instance, in 429 contracts in effect in 1981, only 364 had any type of grievance procedure, and only 298 had a grievance procedure which culminated in grievance arbitration. This pattern is not surprising; presumably it reflects the fact that municipal employers have little incentive to agree to grievance arbitration (for police

unions have no right to strike to offer as a quid pro quo) and the fact that police officers usually are covered by a civil service appeals procedure which is independent of any negotiated grievance procedures.

Because of this pattern, we used our analytical model, somewhat modified, to estimate the existence of a grievance procedure, the existence of grievance arbitration, and the strength of the grievance procedure from the union's point of view (as measured by our grievance procedure (GPI) subindex score). We deleted our bargaining law index (BARGLIND) variable and substituted three other bargaining law dummy variables: BARGLAW to measure the presence or absence of any kind of bargaining law; GPLAW to measure the presence or absence of a provision in the bargaining law requiring the negotiation of a grievance procedure or explicitly listing a grievance procedure in the scope of bargaining; and GALAW to measure the presence or absence of a provision in the bargaining law which requires grievance arbitration. Because some states have grievance-specific requirements in their bargaining statutes, these three dummy variables are more appropriate than our BARGLIND variable used earlier. To keep the number of independent variables down to a reasonable number, we also deleted the ICITY, OPPWAGE, DENSITY, HOUSE, and PBLACK variables used earlier. In addition, the very limited number of items in our grievance procedure subindex caused us to delete the arbitration use terms and focus primarily on location in an arbitration state.

Consequently, we used an analytical model of the following form:

$$\begin{aligned}
 (1) \text{ PROBGP} &= B_0 + B_1 \ln\text{POP} + B_2 \ln\text{PCI} \\
 (2) \text{ PROBGARB} &= + B_3 \ln\text{CRATE} + B_4 \text{CMGR} + B_5 \text{SOUTH} \\
 (3) \text{ GPI} &= + B_6 \text{NEAST} + B_7 \text{WEST} + B_8 \text{CBAGE} \\
 &+ B_9 \text{BARGLAW} + B_{10} \text{GPLAW} + B_{11} \text{GALAW} \\
 &+ B_{12} \text{ARB} + e
 \end{aligned}$$

where

- PROBGP = the presence of a contractual grievance procedure (grievance procedure in contract = 1; no procedure = 0)
- PROBGARB = the presence of a grievance procedure culminating in arbitration (binding arbitration is final step = 1; any other final step = 0)
- GPI = the strength of the grievance procedure from the union point of view
- CBAGE = age of bargaining relationship (in years)
- BARGLAW = a dummy variable = 1 in states that have police bargaining laws which do not explicitly mention grievance procedures (except that in equation (2) GPLAW is also included here)
- GPLAW = (in equations (1) and (3) only) a dummy variable = 1 if the police bargaining law requires the negotiation of a grievance procedure or explicitly lists a grievance procedure in the scope of bargaining
- GALAW = (in equation (2) only) a dummy variable = 1 if the police bargaining law requires grievance arbitration

and the other variables are as defined earlier.

Because equations (1) and (2) have dichotomous dependent variables (the presence or absence of a grievance procedure and grievance arbitration), ordinary least squares (OLS) multiple regression estimation is inappropriate. Consequently, equations (1) and (2) are probit estimations; derivatives of the probit coefficients measure the approximate percentage impact

that each independent variable has on the probability that the "average" police contract contains a grievance procedure or grievance arbitration, respectively. In contrast, equation (3) is an OLS multiple regression estimation of the standard form, and these coefficients measure the approximate impact that each independent variable has on the strength (i.e., number of GPI points) of the grievance procedures which do exist.

Our estimations for 1975 and 1981 are reported in Table V-5 (space constraints prevent us from reporting the results for each year of the 1975-81 period). Each equation is strongly significant (this was true for each equation in all intervening years), though equation (3) explains only a small portion of the total variation in the strength of these grievance procedures.

Only a few environmental influences are consistently statistically significant over time. City size is positively associated with one or more of the dependent variables in both 1975 and 1981 (and in each intervening year as well). A Western location is negatively and strongly associated with all three dependent variables in both 1975 and 1981 (and in each intervening year as well) compared to a North Central location. This is the only consistent and strong regional effect. Rather unsurprisingly, the presence of a bargaining law which requires that police contracts contain grievance arbitration is positively and strongly associated with the presence of grievance arbitration in 1975 and 1981 (and in each intervening year).

Most of the remaining variables are never significant or else their early significance disappears over time. For instance, the age of the bargaining relationship makes no difference in 1975 or 1981 (or in any

TABLE V-5  
INFLUENCES ON POLICE GRIEVANCE PROCEDURES

	1975			1981		
	PROBIT <sup>a</sup>		OLS	PROBIT <sup>a</sup>		OLS
	PROBGP	PROBGARB	GPI	PROBGP	PROBGARB	GPI
Intercept	-.216 (0.12)	3.14 (1.57)	23.35 (0.40)	-.459 (0.45)	-1.44 (1.18)	-25.03 (0.81)
lnCRATE	.423*** (3.57)	-.176 (1.41)	-2.27 (0.69)	.061 (0.97)	.009 (0.13)	2.13 (1.20)
lnPOP	-.059 (1.20)	.128** (2.04)	4.33*** (2.69)	.068** (2.13)	.012 (0.40)	1.30* (1.65)
lnPCI	.278 (1.35)	-.613*** (2.63)	-4.14 (0.62)	.019 (0.17)	.173 (1.25)	7.81** (2.25)
CBAGE	-.007 (0.57)	-.005 (0.39)	0.29 (0.87)	-.004 (0.74)	-.009* (1.67)	-0.24 (1.61)
NEAST	-.130 (0.91)	-.198 (1.49)	-2.06 (0.66)	-.076 (0.73)	-.297* (1.86)	-2.20 (1.21)
SOUTH	-.253 (1.28)	-.213 (0.98)	-0.48 (0.07)	.206* (1.86)	.076 (0.72)	3.79 (1.24)
WEST	-.628*** (4.48)	-.291** (1.96)	-9.51** (2.33)	-.138* (1.68)	-.152 (1.55)	-6.54*** (2.70)
CMGR	-.221** (2.28)	.089 (0.90)	7.24*** (2.63)	-.029 (0.49)	-.049 (0.82)	1.14 (0.78)
GPLAW	.168 (1.08)	---	4.83 (0.95)	-.120 (1.14)	---	-4.04 (1.36)
GALAW	---	.528** (2.32)	---	---	.280** (1.97)	---
BARGLAW	.218 (1.43)	.390** (2.34)	-0.74 (0.14)	-.090 (1.14)	.059 (0.66)	-4.86* (1.82)
ARB	.364*** (3.36)	.153 (1.37)	6.12* (1.69)	.426*** (4.13)	.616*** (4.02)	13.50*** (5.17)
-2 x log-likelihood ratio	80.1**	36.4**	---	117.9**	117.2**	---
F	---	---	2.5**	---	---	8.7**
R <sup>2</sup>	---	---	.11	---	---	.19
N	197	138	138	429	364	364

Absolute (asymptotic) t-statistics in parentheses for the OLS (probit) equations.

<sup>a</sup>The probit coefficients presented are the derivatives calculated at the mean of the dependent variables.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level;  
\*Significant at the .10 level (two-tailed tests).

intervening year); the city manager form of government ceased having an impact after 1977; and the presence of an ordinary bargaining law or a bargaining law which requires a grievance procedure no longer (i.e., by 1981) makes a difference. Similarly, the crime rate and the wealth of a city have no consistent impact over time.

The strongest environmental influence is the presence of an interest arbitration statute covering police. These ARB coefficients are strongly positive on all three dependent variables in almost every year during the 1975-81 period. By 1981 the presence of such a statute, controlling for other influences, was associated with a 43 percent increase in the probability that a police contract contained a grievance procedure, with a 62 percent increase in the probability that such a procedure culminated in grievance arbitration, and with an increase of 13.5 points in the strength of the procedure (i.e., one full standard deviation).

Table V-6 demonstrates more visibly the difference that location in an interest arbitration state can make to police unions and managements. These results show that by 1981 police contracts in interest arbitration states almost always contained a grievance procedure culminating in arbitration. However, police contracts in other states are much less likely to have a grievance procedure or grievance arbitration. For example, police contracts in interest arbitration states are more than twice as likely (42.8 percent vs. 94.6 percent) to contain grievance arbitration as police contracts negotiated elsewhere.

We were unable to determine if our interest arbitration variable is measuring some "pure" impact of interest arbitration's availability or is acting as a proxy for some unmeasured police bargaining characteristics

TABLE V-6  
INTEREST ARBITRATION AND GRIEVANCE PROCEDURES

<u>Year</u>	<u>Number of Contracts</u>	<u>Number (Percent) of Contracts With Grievance Procedure</u>	<u>Number (Percent) of Procedures With Grievance Arbitration</u>	<u>Average GPI Score</u>
<u>Nonarbitration States</u>				
1975	101	53 (52.5%)	30 (56.6%)	46.8
1976	171	88 (51.5)	53 (60.2)	48.0
1977	166	80 (48.2)	48 (60.0)	47.9
1978	181	102 (56.4)	63 (61.8)	48.2
1979	187	116 (62.0)	70 (60.3)	47.6
1980	192	129 (67.2)	78 (60.5)	47.5
1981	208	148 (71.2)	89 (60.1)	47.2 <sup>a</sup>
<u>Arbitration States</u>				
1975	96	85 (88.5%)	75 (88.2%)	52.9
1976	124	115 (92.7)	100 (86.9)	53.6
1977	187	176 (94.1)	154 (87.5)	53.6
1978	208	201 (96.6)	178 (88.6)	53.3
1979	207	200 (96.6)	183 (91.5)	54.3
1980	213	204 (95.8)	193 (94.6)	55.5
1981	221	216 (97.7)	209 (96.8)	56.6 <sup>a</sup>

<sup>a</sup>In 1981, the standard deviation of the GPI score in nonarbitration states was 15.7 points while in arbitration states it was 9.8 points.

in these interest arbitration states. However, we can say that the existence of a state interest arbitration statute is a convenient shorthand indicator of the almost universal existence of grievance procedures and grievance arbitration in the police contracts in that state. When this pattern is combined with our earlier findings that police unions in interest arbitration states have more favorable contracts generally, our analyses indicate that police officers and their unions in arbitration states are in a much stronger position to challenge or appeal managerial decisions than police unions in other states.

#### Time Series Results

We saw in Chapter III that our OLS cross section analyses perform each year's observations independently of all the other years and use all of the data available in each year. Consequently, the OLS cross section results may not provide an especially accurate portrait of how bargaining or arbitration has affected outcomes in the same group of cities over time.

Accordingly, we have analyzed total contract scores in the same group of cities over a multi-year period using generalized least squares (GLS) pooled cross section time series analysis. We discovered 118 cities for which we had contract scores in each year of the 1976-81 period, and these are the cities we used in this time series analysis. This is a small portion of all the cities for which we had some contract data (especially during the more recent years), but the need for complete contract information in each year substantially reduced the size of our time series sample.

TABLE V-7  
1976-81 TIME SERIES ANALYSIS OF ARBITRATION'S AVAILABILITY ON TOTAL CONTRACT SCORES (PERCENT)<sup>a</sup>

<u>N</u>	<u>ARB</u>	<u>ARBTREND</u>	<u>TREND</u>	<u>CBAGE</u>	<u>BARGLIND</u>
708 <sup>b</sup> (118 x 6)	79.71*** (6.20)	-6.35*** (9.11)	7.71*** (5.58)	0.70 (0.64)	1.51 (1.53)

<sup>a</sup>These percentage coefficients were calculated using the formula  $\% = e^B - 1$ , where B is the GLS log coefficient.

<sup>b</sup>38.6 percent of these observations occurred in arbitration cities.

Absolute value of t-statistics in parentheses.

\*\*\*Significant at the .01 level;

\*\*Significant at the .05 level;

\*Significant at the .10 level (two-tailed tests).

Our time series results are presented in Table V-7, and they confirm our cross section analyses. Our ARB coefficient is huge and significant; it means that each year contracts in arbitration cities scored an average of 79.7 percent higher on our contract scoring index than the contracts negotiated in nonarbitration cities, ceteris paribus. This is a much larger percentage than the cross section coefficients presented in Table V-3 for the ARB variable; the discrepancy suggests that there may be a substantial difference between the 118 cities in our time series analysis and the 300-400 cities included in our earlier cross section analysis.

The two time trend variables present some particularly interesting results. The TREND coefficient indicates that each year the contracts in all 118 cities improved an average of 7.71 percent on our scoring index. However, the ARBTREND coefficient indicates that each year the contracts in the arbitration cities lost an average of 6.35 percent of their advantage over the contracts in the nonarbitration cities. Expressed another way, these two variables indicate that while all contracts became more favorable to the unions over time, the contracts in nonarbitration cities improved at a faster rate than contracts in arbitration cities. This result is hardly surprising when we consider how much better (for the unions) the contracts are in arbitration cities.

In our time series analysis, we used the analytical model identified in the early part of this chapter, except that we deleted the ARBUSE and PASTUSE terms and substituted the CBAGE and two time trend variables in their place. We have included in Table V-7 the results on two of our

control variables, CBAGE and BARGLIND. These two coefficients are positive (as expected) but insignificant, which means that the age of the bargaining relationship and a favorable (to the unions) bargaining law contributed almost nothing to improved contract scores in this group of 118 cities after controlling for arbitration and the general upward trend in contract scores (and the other influences in our model).

These time series results are generally consistent with our descriptive and cross section results. The positive time trend coefficient is consistent with the descriptive data in Table V-1; the negative ARBTREND coefficient is consistent with the Table V-1 figures which show that contract scores are improving at a faster rate outside of arbitration states than in them; the huge ARB coefficient differs not in direction but only in magnitude from the results portrayed in Tables V-1 and V-3; and the age of the bargaining relationship (CBAGE) term contributes little in both the cross section and time series results. The only apparent inconsistency is the contribution of the bargaining law index variable, which was positive and significant in the larger cross section samples but was positive and insignificant in the smaller time series sample. It is possible that BARGLIND's fade into insignificance in the time series analysis occurred primarily because of the much smaller sample size.

#### Contract Scores and Salaries

Correlations. In Chapter IV we correlated salaries and fringe benefits to see if they were differentially associated in arbitration

and nonarbitration cities. Here we do a similar correlation of salaries and total contract scores to see how they are associated where arbitration is and is not available.

Our correlation results are contained in Table V-8. When all the cities (for which data are available) are grouped together, there is always a negative correlation between maximum patrol officer salary and total contract score, though in four of the seven years this association is insignificant. However, when the cities are grouped into arbitration and nonarbitration categories, the correlation coefficients differ dramatically. In cities located in nonarbitration states, there is a negative and significant correlation between salaries and contract scores in each year, while at the same time there is a positive and significant correlation in arbitration cities (except for 1981).

These results indicate that salaries and contract provisions may be substituted or traded off for each other where arbitration is not available, and thus in nonarbitration states we can find high salaries coexisting with relatively unfavorable (to the unions) contracts, and vice versa. However, the positive correlation coefficients for arbitration cities indicate that such substitutions are rarely made; instead, high salaries tend to coexist with high contract scores (and vice versa).

The results in Table V-8 are consistent with previous investigations of the relationship between wage and nonwage bargaining outcomes in both the public and private sectors. In a study of Canadian municipalities, Anderson (1979) found a negative but insignificant correlation ( $r = -.02$ ) between wages and contract scores (for contracts covering all groups of city workers, including police officers). Our results in the first

TABLE V-8

ZERO-ORDER CORRELATIONS BETWEEN MAXIMUM PATROL OFFICER SALARY AND TOTAL CONTRACT INDEX SCORE

<u>Year</u>	<u>All Cities</u>	<u>Cities in Nonarbitration States</u>	<u>Cities in Arbitration States</u>
1975	-.022 (143)	-.207* (73)	.247** (70)
1976	-.146** (234)	-.463*** (137)	.314*** (97)
1977	-.163*** (252)	-.364*** (132)	.186** (120)
1978	-.096 (282)	-.274*** (148)	.252*** (134)
1979	-.030 (278)	-.149* (146)	.194** (132)
1980	-.039 (297)	-.187** (149)	.244*** (148)
1981	-.173*** (282)	-.303*** (157)	.145 (125)

Sample sizes are in parentheses.

\*\*\*Significant at the .01 level;

\*\*Significant at the .05 level;

\*Significant at the .10 level (two-tailed test).

column of Table V-8 are roughly consistent with his, for we also found negative but weak correlations in our sample.

In two private sector studies (Feuille, Hendricks, and Kahn, 1981; and Kochan, 1980), researchers found strongly positive and significant correlations ( $r=.51$  and  $.59$ , respectively) between wage rates and contract scores across numerous private sector industries. These two studies suggest that private sector unions use whatever strike-based bargaining power they have to bargain consistent levels of wages and contracts: unions with ample bargaining power have bargained high wages and good contracts, while weak unions have negotiated modest wages and modest contracts.

Our results in the second and third columns of Table V-8 suggest that arbitration may provide police unions with a source of power which is similar to the power that the right to strike gives to many private sector unions. The consistently positive coefficients in arbitration cities suggest that police unions in arbitration states do not need to trade off good contracts to get high pay or high pay to get good contracts. Instead, they can use (or threaten to use) the arbitration procedure to get both.

We do not want to overinterpret these results, for we have seen in this and earlier chapters that the presence of arbitration does not guarantee high wages, good fringes, and favorable contracts. Nevertheless, the contrast between the correlation coefficients in arbitration and nonarbitration states is striking and may be one of the most important findings of our research. This Table V-8 comparison suggests that arbitration enables police unions to simultaneously obtain a range

of favorable wage and nonwage terms for their members that police unions in nonarbitration states simply cannot match. In turn, Table V-8 may offer a very important reason why police unions have lobbied so vigorously for arbitration laws and why municipal management has worked against such statutes.

Tradeoffs. However, the Table V-8 correlation coefficients do not tell us if arbitration is directly associated with the positive correlations between contract scores and salaries or if these Table V-8 results occur for some other reasons. Accordingly, we have borrowed an analytical technique used elsewhere to isolate arbitration's influence.

Following an analysis of the relationships between contract scores and wage rates in the private manufacturing sector (Feuille, Hendricks, and Kahn, 1981), we have selected those cities for which we have 1975-81 contract score and salary data (plus the necessary independent variable information). We then analyzed the relationships between contract scores and maximum patrol officer salaries in these cities with three OLS multiple regression equations. The dependent variable in the first equation was  $\ln\text{TRADEOFF}$ , which was measured as the log of the total contract score in that city minus the log of the maximum patrol officer salary in that city ( $\ln\text{TCI} - \ln\text{MAXSAL}$ ). The ARB regression coefficients in this first equation will tell us if arbitration "tilts" the employment package toward better contracts or higher salaries. Because such a tilt, or tradeoff, could occur as a result of an increase in one variable or a decrease in the other, the second and third equations will measure arbitration's influence separately on the log of total contract scores ( $\ln\text{TCI}$ ) and on the log of maximum patrol officer salaries ( $\ln\text{MAXSAL}$ ).

TABLE V-9

CONTRACT SCORE-SALARY TRADEOFFS<sup>a</sup>

YEAR	N	lnTCI - lnMAXSAL			lnTCI			lnMAXSAL		
		ARB	CBAGE	BARGLIND	ARB	CBAGE	BARGLIND	ARB	CBAGE	BARGLIND
1975	134	35.43** (2.56)	1.67* (1.68)	3.46** (2.31)	40.86*** (2.98)	1.61* (1.66)	3.86*** (2.65)	4.02 (1.28)	-0.07 (0.26)	0.38 (1.00)
1976	222	40.10*** (3.60)	1.46* (1.67)	0.99 (0.86)	43.19*** (3.88)	1.59* (1.83)	1.67 (1.46)	2.20 (0.86)	0.12 (0.51)	0.67** (2.16)
1977	241	60.87*** (3.70)	1.62** (2.24)	-0.18 (0.13)	71.16*** (4.31)	1.72** (2.46)	0.20 (0.15)	6.40* (1.73)	0.10 (0.51)	0.39 (0.97)
1978	269	26.10** (2.05)	0.44 (0.68)	2.13* (1.76)	27.98** (2.26)	0.70 (1.10)	2.75** (2.34)	1.49 (0.44)	0.26 (1.34)	0.60* (1.70)
1979	267	24.89** (2.03)	0.04 (0.08)	0.82 (0.72)	28.94** (2.36)	0.40 (0.70)	1.60 (1.42)	3.24 (0.94)	0.35** (1.99)	0.77** (2.19)
1980	276	36.19*** (2.62)	-0.25 (0.42)	-0.80 (0.64)	30.93** (2.34)	0.02 (0.03)	0.55 (0.46)	-4.02 (1.04)	0.26 (1.38)	1.35*** (3.39)
1981	261	10.88 (1.03)	-0.81 (1.55)	0.93 (0.86)	15.07 (1.47)	-0.59 (1.19)	1.71* (1.67)	3.78 (1.17)	0.22 (1.36)	0.77** (2.28)

<sup>a</sup>The percentage coefficients are calculated with the formula  $\% = e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics are in parentheses.

\*\*\*Significant at the .01 level, \*\*Significant at the .05 level, \*Significant at the .10 level (two-tailed tests).

All three equations will include all the independent variables specified at the beginning of this chapter.

The ARB results of this tradeoff analysis are presented in Table V-9. In addition, this table also presents the regression coefficients for two other potential explanations for these tradeoffs: the length of the bargaining relationship (CBAGE), and the strength of police bargaining legislation (BARGLIND). The ARB coefficients in the tradeoff equation ( $\ln\text{TCI} - \ln\text{MAXSAL}$ ) are large, positive, and significant, which means that (among this sample) cities in arbitration states have police employment packages which are tilted toward better (to the unions) contracts than occurs in cities in nonarbitration states. The pattern of large, positive, and significant ARB coefficients in the contract score equation ( $\ln\text{TCI}$ ) and insignificant coefficients in the salary equation ( $\ln\text{MAXSAL}$ ) indicates that this tilt occurs because of higher contract scores rather than lower salaries in arbitration cities. In fact, Table V-9 indicates that among these cities contract scores are much higher where arbitration is available but that arbitration's availability has no relationship with salaries. Interestingly, the 1981 ARB coefficients in the tradeoff and contract score equations are not significant (though they remain positive and reasonably large).

The 1975-77 CBAGE coefficients are positive and significant in the tradeoff and contract score equations, which means that during those years each additional year of bargaining added a bit more than 1.5 percentage points to each contract, ceteris paribus. However, the length of the bargaining relationship by itself added nothing to 1978-81 contract scores. In addition, the BARGLIND coefficients are only

occasionally significant in the first two equations but are consistently positive and significant in the salary equation, which indicates that cities in states with more supportive police bargaining legislation pay higher salaries than cities in states with less supportive legislation (or no legislation at all). However, neither the CBAGE nor the BARGLIND coefficients were as helpful in explaining the relationships between contract scores and salaries as were the ARB coefficients.

Similarly, the current use (ARBUSE) and prior use (PASTUSE) of arbitration had little influence on these relationships. Of 21 ARBUSE coefficients (three equations times seven years), none were statistically significant. Of 21 PASTUSE coefficients, 13 were not significant, seven were negative and significant (in various  $\ln\text{TRADEOFF}$  and  $\ln\text{TCI}$  equations), and one was positive and significant (in the  $\ln\text{MAXSAL}$  equation). These PASTUSE coefficients indicate that in some years some police unions have used arbitration to catch up to the contract provisions obtained by their peers in other cities.

Taken together, our ARB, ARBUSE, and PASTUSE coefficients in Table V-9 confirm our earlier findings, namely, that it is the availability rather than the use of arbitration which influences the outcomes of police union-management bargaining relationships. In addition, these Table V-9 coefficients confirm the earlier findings from this chapter and the findings from the disaggregated analyses in Chapter III, namely, that arbitration's availability has a strong association with better (to the unions) contract scores but has little or no association with maximum patrol officer salaries. Further, these Table V-9 coefficients confirm that police unions in arbitration cities do not need to accept

lower salaries in order to obtain better contracts (or vice versa) as police unions in nonarbitration cities might need to do.

#### SUMMARY AND CONCLUSIONS

In our two preceding chapters we saw that arbitration's impact upon salaries and fringe benefits depends, at least in part, on the research methods used to make the assessments. No such ambiguity exists in this chapter, however, for arbitration is associated with higher contract scores no matter what research methods are used.

For instance, our descriptive statistics show that total contract scores in arbitration states regularly average more than 100 points higher than contract scores in other states. Our cross section analyses show that, after controlling for the influences of other factors, total contract scores are 18-40 percent higher in arbitration states than elsewhere among our larger sample, and our time series analysis shows that these scores are about 80 percent higher in arbitration states among our smaller sample of cities. Similarly, grievance procedures in police contracts are much more fully developed in arbitration states than elsewhere, and in particular grievance arbitration almost always exists in police contracts where interest arbitration exists but is much spottier elsewhere. Further, most of our subindex scores are larger in arbitration states than in other states, and our intrusion into managerial prerogatives (MPI) subindex is much stronger in arbitration states than elsewhere. As a result, police unions in arbitration states are in a

much stronger position to challenge or appeal managerial decisions than police unions in other states.

An especially interesting finding is that police unions in cities where arbitration is available do not need to trade off high salaries to get good contracts (or vice versa) as police unions in other cities appear to do. In other words, police unions in arbitration cities apparently are able to obtain both favorable contracts and adequate salaries to a much greater extent than police unions in other cities.

Our arbitration use analyses confirm the results obtained in the two preceding chapters: the actual use of arbitration in any particular year does not produce better (to the union) contracts than are produced via the negotiation process. In fact, some of our arbitration use results, especially the PASTUSE coefficients, suggest that over time the actual users of arbitration tend to be those unions with less favorable contracts who apparently are using arbitration to catch up to the provisions obtained by their peers in other cities.

After having emphasized how much more favorable to the unions these contracts are in arbitration states, it is important to note that the favorableness of police contracts is increasing at a faster rate in nonarbitration states than where arbitration is available. If the 1976-81 trends we identified in our time series analysis continue into the future, our results imply that eventually police contracts in all states will be equally favorable to the unions. However, given the huge absolute advantage that presently favors contracts in arbitration states, "eventually" is many years away.

In sum, police contracts in cities where arbitration is available

are much more favorable to the unions than where arbitration is absent. Due to the limited size of our sample of contracts, we have been unable to do the kind of "before" and "after" analyses we performed on salaries and fringe benefits. As a result, it is possible that the favorable contracts in arbitration states might have occurred in any case (i.e., even if arbitration had never come along). We believe this occurrence is unlikely, however, for the two most likely explanations for such a result -- the increasing age of the bargaining relationship and the increasing favorableness of bargaining legislation -- were much less useful in explaining why high contract scores existed than was the availability of arbitration.

Finally, we have emphasized that the more fully developed contracts in arbitration cities have enabled police unions in those cities to mount stronger challenges to police management than unions in other cities ostensibly can do. We also emphasize, though, that we have neither collected nor presented any data about the day-to-day interactions between police unions and police managers. In addition, police bargaining and arbitration may have forced management to specify and protect managerial prerogatives in a clearer and less ambiguous manner than might have occurred under civil service regulations (i.e., union pressure might have forced management to act in a more managerial fashion than might have occurred otherwise). Consequently, our analyses and results in this chapter cannot be used as evidence that police unions in arbitration states actually have restricted, obstructed, or interfered with police management's ability to manage any more than have police unions in other cities. However, we have assessed the impacts of unionization and

arbitration on police employment (of both sworn officers and civilians), total police costs, crime rates, and clearance rates. It is to those assessments that we now turn.

## CHAPTER VI PRODUCTIVITY

In previous chapters we have investigated the possibility that interest arbitration has increased police unions' abilities to bargain for more favorable employment conditions. There appears to be some evidence in favor of this proposition when employment conditions are measured by salary levels, fringe levels, or contract language. In turn, these changes in contract provisions can be expected to elicit some long run responses from both police officers and management. For example, higher salaries typically are associated with lower turnover. If more experienced officers are more efficient, we might anticipate a positive association between officer productivity and arbitration since arbitration might indirectly reduce turnover. On the other hand, arbitration might yield more restrictive work rules than would have occurred in its absence. These work rules could reduce management's ability to effectively manage officers. Thus, arbitration could be associated with reduced officer productivity.

In this chapter we study the indirect influence of arbitration on "policing." The direct influence occurs when arbitration alters contract provisions (wages, fringes, work rules, grievance procedures, etc.). The indirect influence occurs when these contract provisions yield officer and managerial responses which in turn affect officer productivity. While this arbitration-productivity connection is a totally unexplored area, its roots lie in two fairly extensive literatures. First, the impact of unions on productivity has drawn considerable attention in the private sector. This impact is analytically similar to an arbitration impact. Second, police productivity has been the subject of a number of

studies, although none of these studies has focussed (to be consistent) on a union or arbitration impact. We therefore begin this chapter with a short review of the previous approaches in these two areas.

After our review of these literatures, we present our empirical model. We then provide our empirical results and conclude with a discussion of the implications of our findings.

#### UNIONS AND PRODUCTIVITY

There has been a long debate about the impact of unions on productivity which continues to this day. Brown and Medoff (1978) provide one of the better discussions of the opposing influences which generate this controversy. The analytical model which they use is quite simple. Unions are seen to influence working conditions through the negotiation of particular contract provisions. These contract provisions in turn lead to worker and managerial responses which generate productivity changes. The controversy arises not so much from the direct impact of unions on contract provisions but rather from different evaluations of the net effect of these provisions on management and workers and ultimately on productivity. To understand this controversy, it is worthwhile to enumerate some of the potential indirect effects of unions on productivity through contract provisions.

Brown and Medoff identify several potentially positive influences of unions on productivity. First, unions can reduce turnover by providing better working conditions. Reductions in turnover can improve productivity by increasing investment in training and reducing costs associated with

hiring and training of new workers. Second, unions can positively influence the workers' perceptions of their jobs. Besides improving material rewards, unions can negotiate grievance and seniority provisions which potentially reduce unilateral decision making by management and can also reduce worker competition with each other. These changes might improve worker motivation and morale, which in turn might lead to higher productivity. Finally, unions might open a "channel of communication" between workers and management which yields work rule changes which improve productivity.

Brown and Medoff also point out some potentially negative impacts. First, work rules are often associated with reductions rather than increases in productivity. Examples include over-staffing requirements, loose production standards, and provisions which limit technological change. Second, provisions which reduce management ability to efficiently allocate, promote, or hire more qualified workers can also reduce productivity. Thus, seniority clauses and grievance provisions could have the opposite effects as those hypothesized above.

Since there is no theoretical reason for anticipating that one of these positive or negative influences dominates the others, the impact of unions on productivity is largely an empirical question. Studies carried out at Harvard during the past several years (e.g. Brown and Medoff, 1978 and Clark, 1980) tend to find that unions increase productivity in the private sector.

Studies of public sector unions provide mixed results. Perry (1979) implies a reduction in productivity among school teachers based on a qualitative assessment of contract provisions in nine school districts.

Hall and Carroll (1973), on the other hand, found increases in class size associated with unionization. They hypothesized a trade-off between wages and working conditions. Salkever (1982) attempted to disentangle the wage effects of unionization from non-wage effects on total costs in hospitals. He found that roughly two-thirds of cost increases associated with unions were attributable to non-wage impacts. He concluded that hospital administrators had been unable to react to unionization with measures to increase productivity. Three possible explanations were hypothesized: (1) incentives were weak due to third party payers; (2) substitution possibilities for union workers were limited; (3) hospital administrators lacked the information necessary to achieve productivity gains. Finally, Ehrenburg, Sherman, and Schwarz (1983) studied productivity in public libraries. Unlike other studies they found no effect of unions on wages. They were also unable to find any evidence of a union impact on productivity.

Arbitration can influence productivity in basically the same way hypothesized for collective bargaining. By increasing union power to bargain for better contract provisions, it can enhance any positive or negative influence on productivity. Previous empirical work on bargaining's impact, however, gives very little basis for anticipating the direction of this impact (if it exists at all) in law enforcement. Thus, arbitration's impact, like the bargaining impact, is an empirical question.

#### MEASUREMENT OBJECTIVES AND CONCEPTS

The analytical model we develop in the subsequent two sections of

this chapter may be difficult to follow and absorb by those observers unaccustomed to formal economic analysis. Accordingly, readers who do not wish to wade through a lengthy series of equations will find a qualitative summary of our model in this section, after which they may skip to the "Data Sources" section several pages hence. In contrast, readers who prefer equations are advised to skip this section and proceed directly to the "Police Productivity" section which follows.

In this chapter we have two objectives. First, we want to know if police collective bargaining and interest arbitration have an impact on the "inputs" necessary to deliver a given bundle of police services. Because police services are delivered in a very labor intensive manner, and because data on the nonlabor components of policing are rather scarce, we will examine bargaining and arbitration's impacts on the following two inputs: the number of sworn police officers, and the number of civilian police employees. In addition, we will examine bargaining and arbitration's impacts on total police department expenses. Second, we want to know if police bargaining and arbitration have an impact on the "outputs" of the police system. "Safety" is the ultimate output of any police department, but safety is not directly measurable. However, we can and will measure two safety-related outputs, reported crime rates (an inverse measure of output) and the ratios of crimes cleared by arrests (clearances, a direct measure of output).

In our input analyses we know that the number of officers and employees and the total expenses will vary tremendously across cities (e.g., a small low-crime city will rank much lower on all three input measures than will a large high-crime city). Consequently, for our

input analyses to be useful we must control for the different levels of police services provided. We do this by controlling for the number of reported index crimes (the eight major or index crimes included in the FBI's Uniform Crime Reports: murder, manslaughter, rape, robbery, assault, burglary, larceny, auto theft), the number of crimes cleared by arrests (clearances), and three other city characteristics (population, population density, percentage minority). In so doing, we are assuming that the crimes and clearances data accurately represent the level of police law enforcement services delivered and that the three city characteristics accurately proxy the level of other police services delivered (such as traffic control, order maintenance, social services, and so on). Finally, we know that the salary levels of police officers and civilian employees may affect the demand for them (i.e., the number that a city can afford to hire), so we must include these salaries in our input analyses. In sum, we believe that in any city the numbers of police officers and civilian employees and the total cost of police operations will be affected by the salary of police officers, the salary of police civilian employees, population, population density, percent minority, the amount of reported crime, the number of crimes cleared by arrests, police bargaining, and the availability of interest arbitration.

As we mentioned earlier, police bargaining and arbitration may have opposing effects on these three inputs. On the one hand, we saw in earlier chapters that police unions have negotiated and arbitrated increasingly favorable contracts. If these contracts restrict management's ability to deploy officers to meet the demands for police services, a city may feel compelled to hire more officers and/or civilians to

deliver a given bundle of police services. On the other hand, we also saw in earlier chapters that police unions have negotiated and arbitrated higher salaries and more fringe benefits for their members. This may cause cities to employ fewer officers, especially if they can substitute lower-priced civilians for higher-priced officers wherever possible in the production process (e.g., for paperwork processing, fingerprinting, crime analysis, and so on).

On the basis of previous research in labor economics (Brown and Medoff, 1978) and in police labor relations (Juris and Feuille, 1973), we expect that police bargaining will be associated with reduced demand for police officers and increased demand for police civilian employees. This reduction in officers and increase in civilians should occur primarily because of the substitution of low-cost labor for high-cost labor. Because arbitration also has been seen to exert upward pressure on police salaries, we expect that arbitration similarly will be associated with fewer officers and more civilians. However, we are unsure what net effects bargaining and arbitration will have on total expenses. Higher salaries will push up total expenses, but the substitution of low-cost labor for high-cost labor may reduce total expenses.

In our output analyses we know that crime and clearance rates also may vary tremendously across cities. Consequently, for our output analyses to be useful we must control for numerous city characteristics (see Appendices VI-4 and VI-5). We also must control for the number of police officers and police civilian employees, for crime and clearance rates certainly may be affected by the number of police employees available to "fight crime." Expressed another way, we need to control

for several city characteristics and for the number of police department employees in order to isolate any independent effects that bargaining and arbitration might have on crime and clearance rates.

Because there is so little previous research on this topic, we are unsure what to expect in our output analyses. It is difficult to establish causal connections among police bargaining, arbitration, and crime rates. It is theoretically possible that the higher salaries and potentially restrictive contract language negotiated and arbitrated by police unions have affected the occurrence of crimes, but it is not very plausible. Similarly, it is difficult to demonstrate how police bargaining and arbitration are causally related to clearance rates. For instance, higher salaries and greater contractual protections may lead to higher quality police officers, which in turn may lead to increased clearance rates. However, higher salaries also may lead to fewer officers, which may cause clearance rates to decline. As a result of these uncertainties, we make no predictions about how bargaining and arbitration might be related to crime and clearance rates.

As this discussion implies, we are analyzing police productivity in the same way that economists analyze productivity generally - by using a ratio of outputs (in this case, crimes and clearances) to inputs (in this case, the numbers of police officers and civilian employees). For example, if Factory A produces 1000 widgets per hour with 80 workers, and Factory B produces 1000 widgets per hour with 100 workers, Factory A and its workers clearly are more productive. Alternatively, if Factory A and Factory B each produce 1000 widgets per hour with the same number of workers, and then Factory A reduces its workforce but still manages

to produce 1000 widgets per hour, Factory A and its workers have become more productive than Factory B. Note that these productivity assessments use only ratios of outputs to inputs and do not involve measurements of technology, equipment, raw materials, work practices, or work habits in either factory. Similarly, in this chapter's police productivity analyses, we are substituting annual output measures of crimes and clearances for the number of widgets produced per hour, we are substituting annual input measures of the numbers of police officers and civilian employees for the number of workers employed per hour, and we are controlling for the levels of output (in the input analyses) and the levels of input (in the output analyses) rather than simply dividing inputs into outputs.

As a result, readers should note that our productivity analyses are limited to assessing the relationships between these specific police outputs and inputs across three types of cities: those with nonunion police departments, those with unionized police who bargain without access to arbitration, and those with unionized police who bargain with access to arbitration. We have no data regarding assignment and deployment practices or work patterns within police departments (i.e., we did not examine such things as the number of officers assigned to a car, the deployment of officers across shifts, the ratio of detectives to uniformed officers, the percentages of officers patrolling the streets versus patrolling station house desks, the amount of time necessary to complete arrest and report-writing procedures, and so on). Similarly, we have not assessed how fast, smart, eager, aggressive, diligent, or efficient individual officers are in the performance of their duties. Consequently,

the organizational production practices and individual work habits which are featured in popular discussions of "productivity" form no part of the assessment of police productivity presented in this chapter.

Instead, our focus is strictly on ratios of outputs to inputs (and vice versa) rather than on particular work practices (i.e., our measures of police productivity may be very different from police management's measures of productivity).

Finally, we are aware that our use of crimes and clearances in these productivity analyses treat police officers as "crime fighters." At the same time, we also are aware that there is no consensus in the police industry or in society generally about the appropriate role of police officers. Are they primarily crime fighters? Order maintainers? Providers of social services? Citizens may be very worried about crime, and media portrayals of police emphasize their law enforcement activities, yet only a small minority of the average patrol officer's work time is devoted to actual law enforcement tasks (e.g., responses to emergency calls of crimes in progress, actual investigation of crimes and apprehension of suspects, processing arrested suspects, and so on; Goldstein, 1977). In fact, the heavy majority of the average officer's work time is spent performing a variety of order maintenance and social services (e.g., traffic and parking control, providing directions, aiding destitute persons, intervening in domestic disturbances, random patrolling, and so forth). However, we have no data whatsoever about these aspects of police work. Accordingly, our productivity analyses of the police as crime fighters does not reflect any decision by us that law enforcement work is the most important function performed by police officers.

Instead, the use of crimes and clearances in these analyses reflect the fact that these data are readily available while non-law enforcement data are not.

#### POLICE PRODUCTIVITY

A great deal of the original interest in studying police productivity was generated by attempts to determine how crime might be deterred. These attempts naturally led to a focus on resource allocation problems (how much should society spend on police protection, corrections, or social programs to reduce crime?) which form the basis for standard economic analysis. Thus, much of the research on police productivity has been carried out within economic models of crime. These models range from models of individual decisions to commit crimes (see Becker, 1968) to models of the choice mechanism for public goods (see Borchering and Deacon, 1972 and Bergstrom and Goodman, 1973). Since we cannot possibly do justice to the entire field of research in this area, we will begin this section with a simplified model of crime and deterrence and focus on issues which we believe are important in measuring police productivity.

For simplicity define a social welfare function,  $U$ , over "safety",  $S$ , and other goods and services,  $G$ ,

$$SW = U(S,G) \quad (1)$$

The objective of the government is to maximize this function subject to constraints of resources available to produce  $S$  and  $G$  and subject to the "production functions" which translate these resources into the ultimate

outputs S and G. If "income", Y, is used as our measure of total resource availability in dollars, the first constraint becomes

$$Y = PG + WL \quad (2)$$

where P is a vector of prices for goods and services, W is a vector of prices for "safety producing" inputs and L is a vector of safety inputs. L could include police officers, judges, and capital in the law enforcement system as well as social programs designed to reduce crime. Ignoring the goods and services side of the picture, the second constraint is the production function which translates L into safety,

$$S = f(L). \quad (3)$$

Within this model it is clear that the optimal amount of resources which should be allocated to producing safety depends on the value of safety relative to other goods and the costs of achieving safety. If we want to solve the allocation problem of how many resources to allocate to the production of safety, we would need to know the social welfare function (1), the safety "production function" (3), and the resource constraint (2). Since the social welfare function is not observable, this task cannot be directly accomplished. However, it is possible to estimate how the demand for safety inputs (which can be derived from this system) varies with community characteristics (see, e.g., Phillips and Votey, 1977).

A simpler task is to restrict our attention to the safety subcomponent and within this component to law enforcement. We assume that society has solved the allocation problem of how many resources to allocate to generating safety and within this sector of how many resources to allocate to law enforcement as opposed to other methods of producing

safety (e.g. social programs, prisons, the criminal judicial system, etc.). The problem faced by police departments is therefore to maximize safety subject to a cost constraint and the law enforcement safety production function, e.g.

$$\text{Max } S = S(\text{OF}) \quad (4)$$

$$\text{subject to } \text{OF} = g(L) \quad (5)$$

$$\text{and } C = W \cdot L \quad (6)$$

where safety, S, is a declining function of the vector of offenses, OF, committed by category (e.g. murders, rapes, auto thefts, etc.), C is the cost of crime prevention, and the number of various types of offenses is a declining function of the law enforcement input vector L (e.g.,  $g/L < 0$ ).

A number of important modeling and measurement issues occur within this simple model. The first relates to the police objective function. Some authors have argued that police are primarily concerned with clearance rates for reported crimes as opposed to crime rates themselves. In its purest form this would require that society's welfare is a function of "revenge" rather than safety. However, safety can still be incorporated as long as the number of crimes, OF, is a function of clearance rates, CR. Thus,

$$\text{OF} = f(\text{CR}, X) \quad (7)$$

$$\text{CR} = h(L) \quad (8)$$

$$U = U(\text{CR}) \quad (9)$$

and police departments seek to maximize (9) subject to (8) and (6) and where X is a vector of exogenous factors influencing the number of crimes. There are numerous examples of studies implicitly following the

system implied by (4)-(6) which analyze crime rates (OF/population -- see, e.g., Ehrlich, 1972) while many others have used clearance rates (CR/OF -- see, e.g., Votey and Phillips, 1975). What's more, many of the clearance rate studies also model clearances as a function of crimes

$$CR = i(L, OF) \quad (10)$$

Some authors argue that it is harder to clear crimes when there are a large number of them. Some argue the reverse -- as the number of crimes increases it is easier to clear a higher percentage (Darrough & Heineke, 1979 label this the "fisheries" argument). On the surface, it is unclear whether either measure of police performance is totally accurate. Police spend a great deal of time in activities unrelated to solving crimes. Unfortunately, measures of these non-law enforcement activities are not typically available for a cross-section of cities. Because we are restricted to using FBI data on clearance and crime rates, it seems logical to model the police objective as a function of both, e.g.

$$\text{Max } U = U(CR, OF). \quad (11)$$

That is, police seek to reduce crime rates not only through increasing clearance rates, but also through other activities which directly reduce crime. In our measurement of police productivity, we therefore include both measures of clearances and crimes as "outputs" of the policing process.

A second issue in this model involves the specification of the vectors of the arguments in the police objective function and in the production function. Some studies have examined single components of these vectors, e.g. murder rates or murder conviction rates (see Phillips and Votey, 1981, ch. 11) or have estimated separate equations for

several crime categories (e.g. Ehrlich, 1972 or Mathieson and Passell, 1976). The problem with this approach is that it ignores the joint nature of the production process. An alternative strategy is to combine the diverse categories into a single value, for example the aggregate solution rate (e.g. Chapman, Hirsch and Sonenblum, 1975 or Ehrlich, 1973). This assumes that the value of solving a murder is the same as the value of solving an auto theft -- a highly unrealistic assumption. It also makes highly unrealistic assumptions about the production process. Some authors have attempted to finesse this problem either by using weights determined by the time police spend on the activity (Walzer, 1972) or determined by estimates of the loss involved with a particular crime (Darrough and Heineke, 1979).

The joint product problem is very difficult to handle when studies estimate police production functions (e.g. (5) or (8) above). Most of the literature on police productivity falls into this category. Thus separate equations by crime category or single aggregate equations are estimated. While both approaches are probably incorrect, the separate equation approach in our opinion seems preferable to a single measure of output such as an aggregate crime or solution rate. Aggregate statistics tend to be dominated by a few categories (e.g., auto thefts) which probably do not reflect their true weights in society's trade-off function.

Under fairly mild regularity conditions, McFadden (1970) has shown that a multiproduct cost function can be used to describe the technological structure of the firm in the same way as the production function. This duality between cost and production functions is very useful because

multiple outputs are easily handled in the cost function context. Let

$$C = C(Y,W) \quad (12)$$

be the dual cost function defined over the vector of  $Y$  outputs (crimes and clearances) and the vector  $W$  of input prices. If police are assumed to minimize cost for a given output vector, we can apply Shepard's (1953) Lemma to give cost minimizing factor demands as a function of the partial derivatives of the cost function with respect to input prices:

$$L_i = c / W_i. \quad (13)$$

In empirical work, the system of equations defined by (12) and (13) is estimated with (13) usually defined in "share equation" form (either shares of individual inputs or outputs in total cost). Unfortunately, data available on police departments provide good information on wages and employment but not on capital or depreciation (capital outlays during a given year are available, but this is not the same thing.) As a consequence, both production function work and cost function estimates have assumed a single input, labor. Share equations for inputs cannot be estimated, since they are unity by definition. Darrough and Heineke (1979) were able to solve this problem by using "prices" for outputs. They assigned prices to clearance rates based on the average value stolen for four property crimes and lumped crimes against the person in a fifth category. By assuming that property crimes and other crimes were "separable" in the production function, they could estimate (12) and four share equations corresponding to (13). This technique rests on the assumption that the "prices" for outputs can be accurately captured in this way.

A final issue within this model is the data used to measure police

performance. There are a number of possible measures of this performance, including arrest ratios, clearance rates, complaint rates, conviction rates, etc. Since clearance rates and crime rates (complaint rates) are the most widely available, they are almost universally used in productivity studies. The problems involved with these two statistics are widely known. Unintentioned problems occur, for example, because far fewer offenses are reported than actually committed. Problems also occur due to the time frame of reporting. Offenses cleared by arrest in a given year do not necessarily correspond to offenses committed within that year. Thus clearance ratios may be overstated in some years and understated in others. Intentional problems also occur due to built-in incentives for police departments to "fudge" the data. "Crimes" which are easily solved may be reported even when the "crime" is marginal; alternatively, crimes with no likely solution may be ignored.

The bias in using these crime statistics is difficult to determine. Phillips and Votey (1981, ch. 7) conclude that useful information can be obtained from these series. We have nothing to add to their analysis. In addition, we have no reason to believe that reporting errors vary systematically with either arbitration or unionization, which are the major foci of this work. The reader should bear in mind, however, that measurement errors will tend to lead to inefficient results at a minimum and possible bias.

#### EMPIRICAL MODEL

Following previous literature on police performance, we take two

approaches to the measurement of the impact of bargaining and arbitration on productivity. First, we use the cost function approach to estimate factor demand equations and total cost equations. Second, we estimate "production function - like" equations for individual crime rate and clearance rate categories.

We assume that police departments act to minimize cost for a vector of outputs  $Y$  and this vector reflects the trade-offs which the community dictates. Specifically, we hypothesize a cost function of the following form:

$$C = W^\alpha L^{\alpha_1} Y^P e^{\phi ARB + BCBA} \quad (14)$$

where  $ARB$  is a dummy variable for arbitration,  $CBA$  is a dummy variable for unionization, and other variables have been defined above. Taking logs we have

$$\ln C = \alpha \ln W + \alpha_1 \ln L + \sum P \ln Y + \phi ARB + BCBA \quad (15)$$

Applying the log version of Shepard's Lemma yields input demand equations

$$\ln L_i = \alpha_i \ln W_i + \sum \alpha_j \ln W_j + \sum_j \alpha_j \ln L_j + \sum P \ln Y + \phi ARB + BCBA \quad j \neq i \quad (16)$$

Affixing a classical disturbance term to (15) and (16) yields the form for initial estimation equations. Since we have no data on capital or its price, we use only two inputs: number of police officers and number of civilian personnel in the department.

As we noted above, the vector  $Y$  of outputs is assumed to include both clearances and crimes (an inverse measure of output). We include these measures for the eight major FBI crime categories. Finally, we proxy all other police services with four variables: population, percentage

minority, per capita income, and population density. We assume that these three capture differences in the levels of these services across departments. Our final estimation equations are therefore the following:

$$\ln C = \alpha_1^1 \ln W_1 + \alpha_2^1 \ln W_2 + \alpha_1^1 \ln L_1 + \alpha_2^1 \ln L_2 + \sum_{j=1}^{16} P_j^1 \ln Y_j + \phi^1 ARB + B^1 CBA + \sum_{k=1}^4 \eta_k^1 \ln X_k + e \quad (17)$$

$$\ln L_1 = \alpha_1^2 \ln W_1 + \alpha_2^2 \ln W_2 + \alpha_2^2 \ln L_2 + \sum_{j=1}^{16} P_j^2 \ln Y_j + \phi^2 ARB + B^2 CBA + \sum_{k=1}^4 \eta_k^2 \ln X_k + u \quad (18)$$

$$\ln L_2 = \alpha_1^3 \ln W_1 + \alpha_2^3 \ln W_2 + \alpha_1^3 \ln L_1 + \sum_{j=1}^{16} P_j^3 \ln Y_j + \phi^3 ARB + B^3 CBA + \sum_{k=1}^4 \eta_k^3 \ln X_k + v \quad (19)$$

where

- $C$  = total cost of police operations
- $L_1$  = number of police officers
- $L_2$  = number of civilians
- $W_1$  = salary of police officers
- $W_2$  = salary of civilians
- $Y_j$  = number of clearances for  $j=1$  to 8 and number of crimes for  $j=9$  to 16 for the eight FBI major crime categories (murder, manslaughter, rape, robbery, assault, burglary, larceny, auto theft)
- $X_1$  = population
- $X_2$  = percent minority
- $X_3$  = population density (population divided by square miles)
- $X_4$  = per capita income
- $e, u, v$  = error terms

Since we found previously that bargaining and arbitration may increase the wage levels of police, we also estimate equation (18) excluding the wage in an attempt to measure the total impact of these two variables.

It is possible that either bargaining or arbitration alters the mix of civilians and police officers and changes expenditures for a given level of service, but that basic levels of output remain unchanged. To investigate this possibility we also estimate separate equations for each "output" for the eight clearance and eight crime rates. These equations take the following form:

$$\ln Y_i = \alpha_{1,i}^4 \ln L_1 + \alpha_{2,i}^4 \ln L_2 + \sum_{j=1}^{14} \alpha_{i,j}^4 \ln X_j + \phi_i^4 \text{ARB} + B_i^4 \text{CBA} + e_i \quad i = 1 \text{ to } 16 \quad (20)$$

where  $X_j$  = vector of community characteristics including income, population, percent minority, population density, percent of population over 25 with a high school education, percent of population older than 65, and regional dummy variables,

$Y_i$  = clearance ratios for  $i = 1$  to 8 and crime rates for  $i = 9$  to 16.

We have included a number of additional community characteristics in this "production function" equation to measure factors which may influence police productivity. Following previous work, we have also standardized for population in this equation by measuring inputs and crime on a per capita basis and clearances as clearance rates.

#### DATA SOURCES

Data on policing "outputs" (reported crimes and crimes cleared by arrest) are available yearly from 1971 to 1980 from the FBI tapes. While finer detail is available for some of the eight major crime categories, we have chosen to use totals for each category (e.g. total

assaults as opposed to number of assaults with a gun, number of assaults with a knife, etc.). Arson, which was added as a major crime category in 1979, was not reported on the FBI tapes.

Some of the cities had no reported crimes in a category, no crimes cleared by arrest in a category, or they lacked both in a given year. Rather than exclude these cities from the input demand equations, we have set any zeros equal to a small number (.0001) in these regressions. We have followed the same procedure in the crime rate equations. However, clearance rates cannot be computed when there are no crimes in a category. We have therefore restricted these regressions to cities which had at least one crime in the given category. The sample sizes therefore differ considerably for these categories.

Data on policing "inputs" are available from two sources: the International City Management Association (ICMA) data and the Department of Justice's Expenditure and Employment data (hereafter EE). ICMA data are available for approximately 600-700 of the cities in our sample for 1971-1981. They provide information on total number of police, total number of civilians, minimum and maximum patrol officer salaries, total salaries and wages, total fringe benefits, total capital outlays, and other expenditures. We have used the maximum salary measure for police wages. However, no data are given on civilian wages. In the ICMA regressions, we proxy these civilian wages by the average manufacturing wage in the community. Since this is a crude measure at best, measures of demand elasticities for civilians should be treated with caution. Total cost was measured as the sum of salaries and wages, benefits, capital outlays, and all other expenditures.

**CONTINUED**

**3 OF 5**

EE data are available for 1971 to 1979 for approximately 1000 cities in our sample. Data are available for the number of full time employees, part time employees, and associated payrolls for October of each year. Data are also available for the full year concerning operation expenditures, capital outlays, and benefits. Unfortunately, "employees" includes both police officers and civilian personnel. Thus, workers both included and excluded from the bargaining unit are lumped together. This allows us to measure only a single input and a single wage for that input. We use only full time employees because no data are available to measure an equivalent part time salary. Full time average salary is used as our proxy for the wage. Since any bargaining arbitration impact will tend to be diluted by the inclusion of non-bargaining unit personnel, regressions using these data should be treated cautiously. We have reported results using these data primarily because of the increased sample sizes which are available. We have used these data only for the input equations. The ICMA data were used exclusively for the crime and clearance rate regressions. Total cost was measured as the sum of operating expenses, capital outlays and benefits in the EE regressions.

Beginning in 1978, the EE data report full and part time employees and payrolls separately for "sworn" and "nonsworn" personnel. "Sworn" personnel are the approximate equivalent of police officers in the ICMA data; "nonsworn" personnel are the approximate equivalent of civilians. These data are potentially "better" than either the ICMA data or the data for previous years. We therefore report regressions using these data separately from the remainder of the EE regressions. A few cities reported no non-sworn personnel. We have excluded them in this analysis.

Data on community characteristics (other than population) are measured at a single point in time (1970 or 1972) and were taken from the 1977 County and City Data Book.

#### RESULTS: INPUTS AND TOTAL COST

Empirical results for estimation of the total cost equation (17) and the input demand equations (18 and 19) are provided in Tables VI-1 and VI-2 for results based on the ICMA police data and in Tables VI-3 and VI-4 for the EE data. Results for the arbitration availability variable (ARB), the bargaining variable (CBA) and the wage variable (MAXSAL or AVGSAL) are reported in the tables with results for the remaining variables in the regressions reported in Appendix Tables VI-1, VI-2, and VI-3 for selected years.

#### Police Officers

Estimation results for the demand for police officers controlling for wage levels, number of civilians (or non-sworn personnel) and their wage levels, and levels of outputs (number of crimes and number of clearances by arrest) are given in Tables VI-1 and VI-3. Between 1971 and 1973, arbitration apparently had no significant impact on number of police. However, bargaining was associated with a larger number of police controlling for output and wages. This effect ranged from 6 to 10.6 percent in cities with bargaining but without arbitration. Since bargaining increases wages and increased wages reduce demand for police, we would anticipate that this bargaining impact would decrease when the

TABLE VI-1

PERCENTAGE IMPACTS OF BARGAINING AND ARBITRATION ON  
NUMBER OF POLICE OFFICERS: ICMA DATA 1971-80<sup>a</sup>

YEAR	N	Including Wage Measure			Excluding Wage Measure	
		ARB	CBA	MAXSAL	ARB	CBA
1971	646	-7.7 (1.32)	10.6*** (4.04)	-38.3*** (3.86)	-7.1 (1.47)	8.6*** (3.28)
1972	584	4.7 (1.05)	6.9** (2.48)	-41.8** (3.84)	3.5 (0.76)	5.0* (1.81)
1973	575	-2.9 (.74)	6.0** (2.00)	-30.9*** (2.83)	-2.8 (0.72)	4.2 (1.41)
1974	573	13.3*** (3.21)	4.2 (1.28)	-65.5*** (4.89)	13.7*** (3.20)	0.8 (0.06)
1975	616	14.0*** (3.97)	2.0 (0.74)	-44.1*** (4.24)	15.0*** (4.24)	-0.8 (0.33)
1976	663	16.4*** (5.24)	-1.6 (0.64)	-53.0*** (5.67)	16.5*** (5.10)	-5.1** (2.00)
1977	636	16.2*** (5.36)	-1.6 (0.59)	-49.3*** (5.28)	17.0*** (5.41)	-5.8** (2.15)
1978	654	16.1*** (5.32)	-3.6 (1.35)	-41.8*** (4.92)	17.0*** (5.61)	-7.6*** (2.92)
1979	625	24.7*** (7.37)	-8.0*** (2.85)	-30.9*** (3.28)	25.0*** (7.43)	-9.8*** (3.44)
1980	627	12.5*** (6.48)	-9.2*** (3.03)	-52.5*** (5.70)	21.1*** (6.37)	-11.5*** (4.04)

<sup>a</sup>The percentage coefficients are calculated with the formula  $\% = e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics in parentheses.

\*\*\*Significant at the .01 level,

\*\*Significant at the .05 level,

\*Significant at the .10 level (two-tailed tests).

TABLE VI-2

PERCENTAGE IMPACTS OF BARGAINING AND ARBITRATION ON  
TOTAL EXPENSE AND NUMBER OF CIVILIANS: ICMA DATA 1971-80<sup>a</sup>

YEAR	N	Total Expense		Number of Civilians		
		ARB	CBA	N	ARB	CBA
1971	600	-0.4 (0.06)	12.4*** (3.16)	646	-13.0 (1.04)	0.3 (0.05)
1972	544	-0.1 (0.02)	4.7 (1.18)	584	2.5 (0.25)	1.4 (0.23)
1973	197	1.9 (0.35)	7.7* (1.76)	574	-16.2* (1.81)	-83.3*** (9.77)
1974	517	11.4*** (3.48)	5.9*** (2.38)	573	-20.6** (2.37)	-4.4 (0.68)
1975	544	11.1*** (3.18)	3.4 (1.22)	616	-34.5*** (3.95)	-1.5 (0.24)
1976	596	7.4** (2.37)	4.9* (1.92)	663	-25.9*** (3.15)	4.5 (0.72)
1977	571	4.6** (2.25)	7.7*** (3.89)	636	-30.6*** (3.93)	16.9** (2.44)
1978	594	6.3** (2.10)	8.0*** (2.85)	654	-48.0*** (5.94)	17.9* (2.62)
1979	568	6.0** (2.52)	7.3*** (3.5)	625	-40.6*** (5.25)	17.1*** (2.87)
1980	619	8.3** (2.35)	5.0 (1.63)	627	-34.3*** (4.92)	10.0* (1.73)

<sup>a</sup>The percentage coefficients are calculated with the formula  $\% = e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics in parentheses.

\*\*\*Significant at the .01 level,

\*\*Significant at the .05 level,

\*Significant at the .10 level (two-tailed tests).

TABLE VI-3

PERCENTAGE IMPACTS OF BARGAINING AND ARBITRATION ON INPUTS AND EXPENSES: EE DATA 1978-79<sup>a</sup>

YEAR	N	Number FT Sworn Police			Number FT Sworn Police		Number FT Non-Sworn		Total Expenses	
		ARB	CBA	AVGSAL	ARB	CBA	ARB	CBA	ARB	CBA
1978	820	24.5*** (8.11)	-3.2 (1.41)	-43.6*** (8.83)	24.1*** (8.00)	-9.1*** (3.95)	-39.0*** (6.09)	7.9 (1.43)	-3.9* (1.81)	13.9*** (6.5)
1979	776	29.6*** (11.77)	-5.9** (2.59)	-49.0*** (7.13)	29.6*** (11.26)	-12.4*** (5.32)	-48.1*** (19.23)	7.9 (1.49)	-1.0 (0.53)	6.7*** (3.61)

<sup>a</sup>The percentage coefficients are calculated with the formula  $\% = e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics in parentheses.

\*\*\*Significant at the .01 level,

\*\*Significant at the .05 level,

\*Significant at the .10 level (two-tailed tests).

TABLE VI-4

PERCENTAGE IMPACTS OF BARGAINING AND ARBITRATION ON  
NUMBER OF FULL TIME EMPLOYEES AND TOTAL  
EXPENSES: EE DATA 1971-77<sup>a</sup>

YEAR	N	Number of FT Employees					Total Expenses	
		Including Wages		Excluding Wages			ARB	CBA
		ARB	CBA	AVGSAL	ARB	CBA		
1971	774	5.2 (1.06)	8.3*** (2.58)	-95.8*** (9.08)	3.6 (0.69)	3.4 (1.03)	2.5 (0.58)	7.4** (2.63)
1972	783	-4.2 (0.89)	4.6 (1.45)	-75.4*** (7.11)	-1.7 (0.36)	-0.2 (0.07)	4.7 (1.15)	4.4 (1.59)
1973	847	-0.1 (0.02)	0.7 (0.23)	-63.1*** (6.35)	-0.5 (0.12)	-4.1 (1.29)	7.3* (1.94)	0.9 (0.33)
1974	867	9.1*** (2.64)	-2.5 (0.93)	-70.1*** (7.48)	9.5** (2.68)	-7.7** (2.74)	7.0** (2.34)	0.1 (0.04)
1975	890	13.9*** (3.94)	-2.1 (0.75)	-55.0*** (6.08)	15.0*** (4.24)	-7.1** (2.46)	9.0*** (2.97)	-3.1 (1.20)
1976	898	9.8*** (2.82)	-2.3 (0.79)	-44.2*** (21.53)	10.5*** (3.03)	-6.4** (2.14)	4.9 (1.66)	-3.4 (1.27)
1977	871	14.9*** (3.97)	-2.3 (0.70)	-26.4*** (5.71)	14.7*** (3.81)	-5.6* (1.64)	12.2*** (3.71)	0.2 (0.07)

<sup>a</sup>The percentage coefficients are calculated with the formula  $\% = e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics in parentheses.

\*\*\*Significant at the .01 level,

\*\*Significant at the .05 level,

\*Significant at the .10 level (two-tailed tests).

regressions are re-estimated excluding the wage variable. This result occurs. The union estimated impact falls in each year and is no longer significant in 1973.

A different pattern emerges in 1974 and continues through 1980. During this time period arbitration is associated with a greater number of police officers. Moreover, the percentage estimates of this impact are quite large, ranging from 13.3 percent in 1974 to 24.7 percent in 1979 using the ICMA data (Table VI-1) or 29.6 percent using the EE data (Table VI-3). Like the impact of bargaining we would anticipate that the arbitration impact would fall when wage is excluded from the regression if arbitration is associated with higher salaries. This result does not occur. The impact of arbitration on number of police remains largely unchanged when the wage variable is excluded. The impact of bargaining also changes during this period. From 1976 through 1980 the impact of bargaining is negative (measured in the regression excluding wage level). This impact primarily reflects the decrease in demand for the police officers associated with bargaining's higher salary levels. In 1979 and 1980, unions are actually associated with significantly lower demand even after including the wage level. It is possible that this simply reflects higher union benefits not captured by the wage variable or higher productivity among unionized police not covered by arbitration.

These results suggest that bargaining is associated with higher productivity for police officers since a smaller number are required for given output levels controlling for salary level, and that arbitration is associated with lower productivity for police officers since a larger

number are required for given output levels controlling for salary level. This arbitration result cannot be explained by the exclusion of fringe benefits since arbitration is associated with higher benefit levels (see Chapter IV) and higher benefits should reduce the demand for police.

#### Civilians

We know that during the 1971-81 decade when police bargaining and arbitration increased rapidly, civilians as a percentage of all police department employees increased from an average of 13 percent in 1971 to 19 percent in 1981 among the 600-700 cities reporting these data to the ICMA. However, these descriptive statistics tell us little about how bargaining and arbitration directly might have affected the demand for police civilians and police officers.

Demands for civilians and police officers are related through two effects. First, factors which affect output will influence both demands through an "output effect." Thus, factors which increase police wages will tend to increase total costs and reduce output. They would then reduce demand for both police and civilians. Second, factors which increase the cost of one input will cause a substitution toward the other. Thus, if bargaining or arbitration increases police officer wages and does not increase civilian wages, we anticipate that police unions will increase the demand for civilians through the "substitution effect" to the extent that civilians can be substituted for officers in the production process.

When we control for output (i.e., crimes and clearances) in input demand equations, we eliminate any "output effect" which might indirectly

influence demand. Thus, factors which increase the demand for police should decrease the demand for civilians and vice versa (through the substitution effect). We therefore anticipate some off-setting decrease in demand for civilians due to arbitration and an increase in demand for civilians due to bargaining based on our police officer results.

Estimation results for the demand for civilians (non-sworn personnel) controlling for wage levels, number of police officers (sworn personnel) and their wage levels, and levels of "outputs" are given in Tables VI-2 and VI-3. These results support our expectations. Beginning in 1974, arbitration is associated with substantial reductions in civilian personnel. Estimates of these reductions range from 20.6 percent to 48 percent in the ICMA based regressions with similar results for 1978-79 in the EE based estimates. Results for bargaining are not quite as clear cut. Significant increases in the demand for civilians do not occur until 1977 in the ICMA data. What's more, the ICMA and EE data provide different results since the estimated union impact is smaller and insignificant in the EE data. However, the point estimates are still positive, indicating a probable substitution effect due to bargaining.

Taken with the results for police officers, the Tables VI-1, VI-2, and VI-3 results seem to indicate that both bargaining and arbitration shift the "mix" of police officer - civilian "inputs" which are used to achieve given output levels. Bargaining decreases the number of officers and increases the number of civilians. Arbitration has the opposite effect. Thus, bargaining is associated with an increase in officer productivity and decrease in civilian productivity with the reverse for

arbitration. The bargaining effect could be predicted on the basis of previously researched union impacts on wages and employment. The arbitration effect, however, cannot be so easily explained.

#### Total Costs and Total Employment

Estimates of the impact of bargaining and arbitration on total full time employment based on the EE data are provided in Table VI-4. While these data suffer from a number of problems (which were outlined above), they do serve as a check on the results in the previous two sections. The results are quite consistent with the previous findings. Arbitration is associated with increases in total employees from 1974 to 1977. Although arbitration decreases the number of civilians by a larger percentage than it increases the number of officers, the number of officers is much larger. Thus, the total effect is positive. Bargaining is estimated to have no significant effect on total employment when the average wage is included, and a significant negative effect when the average wage is excluded.

The impact of bargaining and arbitration on total costs is estimated controlling for levels of outputs and inputs and the prices of those inputs. Results for the total cost regressions are presented in Tables VI-2, 3, and 4. We have labeled these results "total expense" to emphasize the fact that measures of capital costs are not accurately represented. These results represent a unique experiment. While all the independent variables are treated as if they were exogenous, we have some evidence that all are influenced to some degree by bargaining and arbitration. Thus, these results answer the following question: Does

bargaining or arbitration increase or decrease total expenses above and beyond any influence which it might have on output levels, input levels, or wages? Since this really eliminates any impact through either productivity or wage rates, we expect that the results will primarily capture unmeasured costs associated with bargaining and arbitration. If such costs exist, they will probably be due to increased fringe benefits to the extent that benefits are a higher proportion of total labor costs under bargaining or arbitration. In the ICMA data, the results might also capture any bargaining or arbitration spillover to civilian wages since our measure of civilian wage levels (the average manufacturing wage) would not be expected to pick up this phenomenon.

These arguments suggest that the coefficients for bargaining and arbitration should be either positive or zero in these regressions. The results seem to confirm these expectations. Beginning in 1974, the ICMA regressions (Table VI-2) yield total expenses costs associated with both bargaining and arbitration. The increases range from three to eleven percent. The EE regressions give conflicting results. On the one hand, regressions using total full time employment and average wages (Table VI-4) yield positive significant coefficients for arbitration for 1973, 1974, 1975 and 1977 and no significant bargaining impact. On the other hand, regressions controlling for number of officers and civilians and their average wages separately (Table VI-3) yield positive significant coefficients for bargaining and negative or insignificant coefficients for arbitration for 1978 and 1979. On balance, we believe that these results provide tentative support for the hypothesis that both bargaining and arbitration increase total costs.

## RESULTS: OUTPUTS

The total effect of bargaining and arbitration on police department "performance" will be the sum of the two opposing impacts on police and civilian personnel. It is possible that these two impacts will offset each other. To consider this possibility, we have estimated output equations as specified in equation (20). As we have noted above, this estimation assumes perfect separability in the production process. Because the impact of this assumption on our estimates is unknown, these results must be viewed with considerable caution.

Empirical results for crime rates for the eight major crime categories are found in Table VI-5; clearance rate results are found in Table VI-6. These tables report the coefficients for the arbitration (ARB) and bargaining (CBA) variables while the full regression results (for 1980) are presented in a separate Appendix VI-4. We interpret crime rates as inverse measures of safety. Therefore, negative coefficients in Table VI-5 indicate increased productivity. Clearance rates are direct measures of police ability to "solve" crimes. Therefore, positive coefficients in Table VI-6 indicate increased productivity. As noted above, the clearance regressions used only the subsample of cities which actually had crimes reported in that category.

The results for crime rates (Table VI-5) provide a fairly consistent picture. Arbitration availability and bargaining are often significantly associated with crime rates. Where they are significant, bargaining is almost always associated with increased productivity (i.e., lower crime rates) while arbitration is associated with decreased productivity (i.e., higher crime rates). Like the results on input demands and

TABLE VI-5

BARGAINING, ARBITRATION, AND REPORTED CRIME RATES (PERCENTS<sup>a</sup>)

YEAR	N	Murder		Manslaughter		Rape		Robbery	
		ARB	CBA	ARB	CBA	ARB	CBA	ARB	CBA
1971	646	-6.3 (1.02)	-4.7 (0.40)	-1.6 (0.26)	-24.5* (1.86)	-2.0 (0.29)	20.8 (1.43)	2.8 (0.36)	20.9 (1.28)
1972	584	-16.8*** (2.72)	2.9 (0.31)	-20.8*** (2.78)	7.3 (0.63)	6.6 (0.99)	4.7 (0.42)	26.4*** (3.16)	-37.7*** (2.60)
1973	575	-19.1*** (2.69)	16.3* (1.72)	-4.7 (0.65)	-6.6 (0.67)	17.7** (2.33)	-23.5** (2.27)	23.9*** (2.82)	-18.5* (1.67)
1974	573	-.30 (0.05)	-3.5 (0.43)	-9.8 (1.37)	6.1 (0.69)	24.1*** (3.18)	-20.1*** (2.13)	-20.9*** (2.68)	-29.3*** (2.86)
1975	616	-6.3 (1.02)	.10 (0.01)	3.9 (0.56)	-15.3 (1.71)	26.9*** (3.45)	-21.9*** (2.33)	40.5*** (5.07)	-49.0*** (4.81)
1976	663	-7.1 (1.13)	-13.0* (1.72)	2.0 (0.32)	-1.0 (0.14)	38.0*** (4.88)	-39.7*** (4.34)	42.2*** (5.33)	-62.9*** (6.34)
1977	636	13.7** (2.00)	-17.7*** (2.33)	6.0 (0.88)	3.6 (0.49)	43.1*** (4.90)	-47.4*** (4.91)	51.4*** (6.01)	-80.0*** (7.84)
1978	654	9.1 (1.38)	-1.0 (0.14)	8.6 (1.17)	-2.7 (0.35)	29.8*** (3.90)	-54.5*** (5.88)	52.2*** (6.46)	-82.2*** (8.33)
1979	625	7.3 (1.08)	-24.9*** (2.88)	-5.1 (0.69)	8.0 (0.93)	35.8*** (4.43)	-58.3*** (5.88)	57.5*** (6.58)	-79.7*** (7.61)
1980	627	17.1** (2.47)	-24.2*** (3.10)	-6.9 (0.93)	10.3 (1.23)	32.6** (4.03)	-62.4*** (6.30)	65.2*** (7.28)	-81.3*** (7.83)

TABLE VI-5 (cont.)

BARGAINING, ARBITRATION, AND REPORTED CRIME RATES (PERCENTS<sup>a</sup>)

YEAR	N	Assault		Burglary		Larceny		Auto Theft	
		ARB	CBA	ARB	CBA	ARB	CBA	ARB	CBA
1971	646	-23.4** (2.47)	42.6** (2.15)	7.4 (1.54)	1.6 (0.18)	-.40 (0.09)	6.7 (0.77)	5.2 (0.91)	-4.9 (0.44)
1972	584	-1.7 (0.20)	-27.6* (1.71)	16.9*** (3.63)	-17.7** (2.30)	6.6 (1.56)	3.9 (0.56)	12.4** (2.05)	-35.7*** (3.24)
1973	575	-0.20 (0.02)	-33.9** (2.54)	15.3*** (3.30)	-5.9 (0.98)	6.3 (1.56)	7.9 (1.43)	18.4*** (2.91)	-11.1 (1.35)
1974	573	16.0* (1.90)	-38.0*** (3.25)	15.8*** (3.50)	-14.6** (2.56)	6.9* (1.68)	0 (0)	11.7** (1.98)	0.10 (0.01)
1975	616	16.0** (2.06)	-30.7*** (3.01)	23.1*** (5.20)	-24.4*** (4.36)	9.2** (2.44)	-8.3* (1.78)	19.5*** (3.18)	5.3 (.754)
1976	663	20.3*** (2.76)	-45.1*** (4.77)	25.9*** (5.61)	-31.0*** (5.63)	6.9** (2.03)	-5.2 (1.31)	26.2*** (4.40)	-6.9 (1.08)
1977	636	15.5** (2.06)	-35.5*** (4.00)	21.7** (3.92)	-26.1*** (4.30)	7.0 (1.55)	-9.3* (1.85)	19.0*** (3.35)	-5.7 (0.98)
1978	654	11.6 (1.55)	-23.4*** (2.66)	27.9*** (5.59)	-34.2*** (6.13)	4.6 (1.32)	-5.7 (1.49)	26.1*** (4.73)	-13.2** (2.30)
1979	625	17.0** (2.31)	-23.6*** (2.79)	24.0*** (4.89)	-32.1*** (5.56)	10.0** (2.57)	-6.2 (1.46)	25.7*** (4.87)	-12.8*** (2.31)
1980	627	11.4 (1.54)	-21.3** (2.51)	17.9*** (4.13)	-19.0*** (4.05)	5.3 (1.37)	-3.3 (0.76)	27.6*** (5.08)	-18.9*** (3.26)

<sup>a</sup>The percentage coefficients are calculated with the formula  $\% = e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics in parentheses.

\*\*\*Significant at the .01 level,

\*\*Significant at the .05 level,

\*Significant at the .10 level (two-tailed tests).

TABLE VI-6

BARGAINING, ARBITRATION, AND CLEARANCE RATES (PERCENTS)<sup>a</sup>

YEAR	Murder			Manslaughter			Rape			Robbery		
	ARB	CBA	N	ARB	CBA	N	ARB	CBA	N	ARB	CBA	N
1971	3.5 (0.15)	26.9 (0.57)	504	0.8 (0.02)	36.9 (0.46)	389	-48.1 (1.76)	33.5 (0.69)	583	-25.5* (1.72)	-26.6 (0.89)	646
1972	-16.4 (0.60)	-24.4 (0.51)	449	-40.4 (0.99)	-38.0 (0.56)	350	21.7 (0.93)	-21.7 (0.55)	546	-3.7* (0.24)	-33.6 (1.15)	582
1973	-9.5 (0.37)	-21.2 (0.55)	448	97.4* (1.94)	-13.0 (0.25)	341	-19.5 (0.79)	-41.8 (1.16)	545	-25.4 (1.52)	-72.6** (2.76)	576
1974	3.6 (0.13)	38.5 (0.91)	444	41.1 (1.07)	-86.8 (1.47)	344	68.7** (2.21)	-46.5 (1.28)	546	5.0 (0.73)	-15.6 (1.69)	572
1975	-50.4* (1.71)	-0.4 (0.01)	475	22.9 (0.65)	-168.1*** (2.56)	377	9.2 (0.41)	-36.9 (1.20)	584	-14.2 (1.29)	-3.1 (0.24)	616
1976	-15.0 (0.54)	-134.7*** (2.73)	501	2.1 (0.06)	-80.4 (1.37)	377	24.4 (1.05)	-122.3*** (3.32)	629	-3.6 (0.28)	-5.1 (0.35)	664
1977	-9.0 (0.29)	-9.9 (0.29)	494	40.9 (0.74)	25.2 (0.44)	279	-20.4 (0.84)	-6.1 (0.25)	604	32.7** (0.21)	-48.4*** (2.69)	637
1978	5.6 (0.24)	-5.4 (0.20)	498	-206.5* (1.80)	-242.1* (1.79)	125	4.7 (0.21)	2.8 (0.11)	634	-17.5* (1.71)	-16.7 (1.48)	655
1979	25.4 (0.92)	-136.1*** (2.97)	491	-37.6* (1.95)	55.4** (2.33)	112	14.5 (0.68)	-70.9** (2.41)	613	6.1 (0.45)	-29.4 (1.79)	628
1980	20.7 (0.76)	-18.5 (0.58)	492	-177.3 (1.17)	380.7* (1.70)	92	14.7 (0.67)	-26.2 (1.04)	604	-10.3 (0.85)	-1.7 (0.14)	627

TABLE VI-6 (cont.)  
 BARGAINING, ARBITRATION, AND CLEARANCE RATES (PERCENTS)<sup>a</sup>

YEAR	Assault			Burglary			Larceny			Auto Theft		
	ARB	CBA	N	ARB	CBA	N	ARB	CBA	N	ARB	CBA	N
1971	11.4 (1.64)	-23.4 (1.63)	638	1.6 (0.26)	-6.9 (0.57)	646	-46.8*** (5.33)	55.0*** (3.17)	646	-4.0 (0.44)	-7.1 (0.40)	646
1972	6.2 (0.82)	-20.7 (1.53)	583	-3.1 (0.55)	-4.5 (0.48)	586	-67.0*** (6.66)	101.4*** (5.51)	584	-24.7** (2.48)	-4.2 (0.28)	587
1973	.90 (0.16)	-26.1*** (3.09)	568	.80 (0.14)	-13.7* (1.71)	575	-18.7*** (3.00)	19.1** (2.30)	575	-8.9 (0.93)	-1.5 (0.12)	575
1974	.50 (0.12)	-4.6 (0.83)	572	.90 (0.17)	-5.1 (0.74)	573	-5.8 (1.05)	6.9 (0.97)	573	3.5 (0.43)	-1.9 (0.19)	573
1975	-2.7 (0.49)	-12.1* (1.68)	616	-7.8 (1.39)	-12.9* (1.83)	616	-10.9** (1.98)	7.0 (1.06)	616	-.60 (0.07)	-22.5* (1.80)	616
1976	-1.5 (0.37)	-7.1 (1.44)	664	-6.9 (1.29)	-8.3 (1.33)	664	-9.3* (1.85)	-4.9 (0.86)	664	-21.3** (2.05)	2.1 (0.19)	664
1977	-2.5 (0.42)	-7.3 (1.11)	637	-1.9 (0.30)	-18.8** (2.53)	635	-4.2 (0.66)	-11.4 (1.61)	636	10.5 (0.94)	-28.5** (2.16)	637
1978	8.0 (1.22)	-15.8** (2.10)	655	-.10 (0.02)	-14.8** (2.03)	654	.20 (0.05)	-5.3 (1.37)	655	-4.7 (0.54)	-23.2** (2.20)	655
1979	4.0 (0.80)	-14.9** (2.53)	627	-.10 (0.02)	-9.5 (1.34)	628	-3.6 (0.69)	-8.2 (1.39)	627	-1.4 (0.15)	-29.1** (2.41)	627
1980	3.4*** (4.71)	-7.0 (0.90)	626	.20 (0.03)	-7.8 (1.00)	627	-5.1 (0.79)	0.3 (0.04)	627	-5.4 (0.51)	-6.8 (0.57)	627

<sup>a</sup>The percentage coefficients are calculated with the formula  $\% = e^B - 1$ , where B is the OLS log coefficient.

Absolute values of t-statistics in parentheses.

\*\*\*Significant at the .01 level,

\*\*Significant at the .05 level,

\*Significant at the .10 level (two-tailed tests).

costs, this pattern is most consistent beginning in 1973 or 1974. The strongest results appear for rape, robbery, assault, burglary, and auto theft. Some evidence for a similar pattern occurs for murder and larceny. Almost all results for manslaughter are insignificant.

The results for clearance rates (Table VI-6) were very disappointing. Many of the regressions were insignificant with  $R^2$ s ranging from .02 to .14 (see Appendix VI-4). Since all the explanatory variables taken together often could not explain a significant portion of the variation in clearance rates, the sizable number of insignificant results for arbitration and bargaining reported in Table VI-6 are not surprising. Moreover, many of the coefficients are highly unstable from year to year. For example, bargaining is associated with a 242% decrease in clearance rates for manslaughter in 1978 and a 55% increase in the rates the following year. Significant coefficients appear much more often for bargaining than arbitration. In most cases, bargaining is associated with decreased clearance rates. This suggests a reduction in productivity, which is opposite to the result for crime rates. However, for the reasons just mentioned we are unwilling to conclude that either bargaining or arbitration has had any consistent effect on clearance rates.

#### SUMMARY AND CONCLUSION

Given all the data problems associated with measuring police productivity and the (sometimes) conflicting results which we have obtained, we emphasize that our conclusions are limited and tentative. However, our Chapter VI analyses have produced some interesting results which in

turn suggest some tentative conclusions and some directions for future research.

We have found that both bargaining and arbitration typically are associated with increased costs in operating a police department. These results are quite consistent with our earlier findings of bargaining and/or arbitration's positive impacts on salaries and fringe benefits. In contrast, our measurements of productivity, whether through the use of input equations or output equations, indicate that bargaining and arbitration have opposite results. Bargaining is associated with more productive police officers, less productive civilians, and more productivity overall when productivity is measured by crime rates. Arbitration is associated with less productive police officers, more productive civilians, and less productivity overall when productivity is measured by crime rates. Keep in mind that because we have measured productivity strictly as a ratio of selected outputs to selected inputs, our results do not necessarily show that bargaining departments have more efficient work practices while arbitration departments have less efficient work practices.

Our input results (in Tables VI-1 through VI-4), however, are consistent with a "managerial response" hypothesis. More specifically, these results suggest that management responds to the increasing costs generated by bargaining by substituting less expensive civilians (and possibly capital goods such as cars and other equipment) for more expensive sworn officers. This substitution leads to increased productivity (as we have defined it) for sworn officers, either because there are fewer of them to perform the same amount of work or because

they are assigned in a more effective manner (i.e., to street jobs rather than to desk jobs). In contrast, for some reason(s) managements in bargaining cities in arbitration states apparently do not make these substitutions. We are unable to tell if arbitration somehow directly prevents these substitutions or if arbitration is an unwitting proxy for some unmeasured state or city characteristics. If arbitration in fact does have this direct impact, it may occur because arbitration enables police unions in arbitration states to resist trading off higher salaries and benefits for more efficient staffing practices as police unions in nonarbitration states might have done.

In our output analyses (Tables VI-5 and VI-6), we are unsure why bargaining and arbitration's associations with crime rates differ from each other, why these associations vary so much from year to year, and why these associations exist with some crimes but not others. Because the statistically significant CBA and ARB coefficients in Table VI-5 usually have opposing signs, these results indicate that bargaining is associated with substantially fewer reported rapes, robberies, assaults, burglaries, and auto thefts only in nonarbitration states (i.e., in arbitration states the CBA and ARB results tend to cancel each other). On the one hand, bargaining may contribute to a more effective managerial response against certain types of crimes, while arbitration may negate the bargaining influence. On the other hand, bargaining by itself somehow may be associated with lower crime rates for reasons beyond those examined in this research, while arbitration similarly may be associated unwittingly with higher crime rates. Because we have insufficient data to explain how or why police bargaining by itself should lead

to fewer crimes while arbitration should cancel this effect, we cannot demonstrate that the first explanation is the more likely of the two.

In contrast, neither bargaining nor arbitration appears to have any systematic association with clearance rates. Although our clearance rate measurements have little explanatory power, they suggest that bargaining and arbitration neither help nor hinder the efforts of police departments to clear crimes by arrests. In addition, the juxtaposition of the crime and clearance results presents a somewhat puzzling picture. The police presumably exercise little direct control over the initial reporting of crimes, yet bargaining and arbitration are systematically associated with several reported crime rates. In contrast, the police theoretically exercise considerable control over the clearance of crimes via arrests, yet bargaining and arbitration have no systematic association with clearance rates.

It is important to note that, because arbitration departments also bargain (we have treated the few nonunion departments in arbitration states as if they were in nonarbitration states), the net effects of bargaining and arbitration (compared to nonunion departments) should be determined by adding the coefficients for CBA and ARB in the various tables in this chapter. Because the CBA and ARB coefficients in both the input and output analyses usually have opposite signs their sum is often not significantly different than zero (i.e., the bargaining effect in one direction often is cancelled by an arbitration effect in the opposite direction). As an example, the Table VI-5 results indicate that crime rates in unionized departments in arbitration states are about the same as crime rates in nonunion departments in nonarbitration

states. As a result, both nonunion and arbitration departments are associated with higher crime rates than unionized departments in nonarbitration states. In turn, this pattern of results suggests that arbitration does not cause higher crime rates.

Finally, it is important to note -- once again -- that our research focus on productivity has been a limited one. We have not examined actual work practices (deployment, patrolling, response times, arrest and arrest processing methods, report writing, breaks, and so on), and we have not examined any data representing the non-law enforcement work (traffic control, order maintenance, social services, etc.) which constitutes 80-90 percent of the average police officer's work time. When this narrow focus is combined with the data interpretation warnings offered earlier, the conclusions we have reached in this chapter are tentative indeed. Accordingly, this chapter's analyses and results should be used as guides for future research rather than as definitive answers.

## CHAPTER VII

### DISCUSSION, CONCLUSIONS, AND RECOMMENDATIONS

Report authors often use their final chapter as a place to summarize the findings presented in earlier chapters. Because we have used the first chapter of this report as an executive summary, and because we have included topic-specific summaries at the end of most chapters, we see no need to summarize our findings a third time. Instead, we will use this final chapter to discuss and draw conclusions from our findings and offer suggestions for future research. The connecting theme among this chapter's topics is the need for a careful and wide-ranging assessment of bargaining and arbitration's numerous costs and benefits in order to render an informed verdict about bargaining, arbitration, and their impacts.

#### FORM vs. FUNCTION

##### Constitutionality and Acceptability

As we noted in our opening chapter, state appellate courts have ruled again and again that state interest arbitration laws are constitutionally permissible. Similarly, we are unaware of any state arbitration law which has been directly repealed or allowed to expire. Further, the willingness of states to adopt such statutes continues to the present, as Ohio implemented an arbitration law covering police and firefighters in 1983. In other words, arbitration is and continues to be a constitutional and acceptable mechanism for the resolution of public sector negotiating disputes. Consequently, we believe that normative assessments of

arbitration are more usefully made on the basis of how well or poorly arbitration works in practice rather than on how consistent or inconsistent arbitration is with some abstract form of representative government portrayed in a legal brief. As a result, in the following pages we assess some of the practical implications of our research results.

#### Functional Costs and Benefits

Following an earlier analysis (Feuille, 1979), we may identify three sets of benefits and two sets of costs which arbitration imposes. We can then use these benefits and costs as a framework with which to discuss and assess the findings in this report.

Protector of the public interest. As noted in Chapter I, other researchers (Ichniowski, 1982; Olson, et al., 1981; Wheeler 1975) have shown that the presence of an arbitration law substantially reduces strikes. If we assume that the public needs and wants to be protected from police strikes, we must conclude that police arbitration laws have done an excellent job of protecting the public interest.

Regulator of interest group conflict. A related benefit is arbitration's ability to absorb interest group conflict between public managers and public employees. Arbitration's finality means that it can be used to make binding and enforceable decisions which resolve the conflicting claims advanced by managers and employees. As a result, arbitration can regulate and absorb the competing interest group pressures which might otherwise lead to strikes or other disruptions. In so doing, arbitration contributes to political and social stability (although, other than counting strikes, it is not clear how to measure the stability that

arbitration helps create).

Guardian of employee interests. By definition, compulsory interest arbitration places unions and employers at the bargaining table in positions of approximately equal strength, for either side can say "no" to the other side's demands and put their case in front of an arbitrator — who is obligated to issue a "fair" or "balanced" award. As we saw in our Chapter III conceptual discussion, this feature of arbitration should make the procedure work to the advantage of employees who do not have the right to strike, for it eliminates the employer's ability to implement its own desired terms. If arbitration in fact works in this manner, we should see that arbitration enables police unions to obtain more favorable terms than they are able to obtain where arbitration is unavailable, ceteris paribus. Our findings indicate that arbitration indeed does protect employee interests, a point to which we shall return.

Inhibitor of representative government. Arbitration allows for public resources to be allocated in a relatively private manner by a nonelected third party who is only indirectly accountable for his or her decisions. Even though this delegation of authority is constitutionally permissible, it may be politically undesirable because it reduces public officials' accountability for the allocation of scarce public resources. However, this cost of arbitration is very difficult to measure empirically; instead, it is based upon a strongly normative view of how public allocation decisions should be made (Horton, 1975). In turn, this normative view invites contrary normative expression on behalf of arbitration (Krislov, 1977), and there is no known empirical formula for

deciding which of these views is more correct.

Inhibitor of genuine collective bargaining. Critics of arbitration assert that arbitration has a "chilling effect" upon the parties' efforts to negotiate an agreement, and over time has a "narcotic effect" as the parties habitually rely upon arbitrators to resolve their disagreements. The considerable debate over arbitration's actual impact on bargaining is influenced by personal preferences (Thompson and Cairnie, 1973; Feuille, 1975; Thompson and Cairnie, 1975) and by the research methods used to perform these assessments (Kochan and Baderschneider, 1978; Butler and Ehrenberg, 1981; Kochan and Baderschneider, 1981; J. Anderson, 1981a). The evidence seems to indicate that most of the time bargaining remains a viable process in the presence of arbitration, but there is no doubt that in some jurisdictions arbitration has weakened the parties' incentives to address and resolve their disagreements at the bargaining table.

#### FUNCTIONAL IMPACTS .

Our research results allow us to say little or nothing about arbitration's impact upon the incentives to negotiate, upon arbitration's functional compatibility with our form of representative democratic government, or upon the extent to which arbitration prevents strikes which otherwise might occur. However, our results do permit us to offer a variety of conclusions about how arbitration has affected the existence of police collective bargaining, guarded the employment interests of police officers, and affected the costs of delivering police services.

#### Existence of Police Collective Bargaining

As we saw in Table I-3 in Chapter I, we were able to identify the 1981 police bargaining status of 1,015 cities over 25,000 population. Of these cities, 388 were in arbitration states, and 382 (or 98.5 percent) bargained with the police. The other 627 cities were in nonarbitration states, and 315 (or 50.2 percent) bargained with the police. We do not need to perform a statistical test to see that the incidence of police collective bargaining is significantly higher where arbitration exists than where it does not.

Although there are reasons to believe that police collective bargaining would be more widespread in the arbitration states even if they had no arbitration laws (because of the lengthy history of police bargaining in those states, and because most of them statutorily protected police bargaining rights long before arbitration came along), arbitration nevertheless is associated with the almost universal existence of police bargaining. This association is easy to understand, for police officers need to bargain in order to participate in the arbitration process. More specifically, police officers need to bargain to convince management that inadequate (to the union) offers will result in the use of the arbitration procedure. Given this incentive to unionize and bargain, the surprising thing is not that police bargaining exists in 98.5 percent of the identified cities in arbitration states; rather, it is that six cities in those states still (in 1981) do not bargain!

#### Guarding Employee Interests

The data examined in Chapters III, IV, and V indicate that arbitra-

tion is associated with higher police salaries and more favorable (to the union) contracts. While the magnitude of these associations varies according to the research methods used, these associations are almost uniformly positive. In addition, our data show that police collective bargaining by itself also is associated with higher salaries and fringes. Consequently, our analyses indicate that police officers enjoy advantageous employment terms due to collective bargaining and that they receive even more advantageous terms when they bargain under an arbitration statute. In short, our results indicate that both collective bargaining and interest arbitration do an effective job of guarding the employment interests of police officers.

However, there is an important qualification to this conclusion: our disaggregated, state-specific findings indicate that arbitration's impact may vary from state to state. For example, if arbitration always and everywhere pushed up salaries and fringes, we would expect to find that police salaries and fringes in arbitration states have become ever more similar over time within each state and across all the arbitration states. However, we found that very little of these intrastate or interstate levelling effects had occurred. Instead, by 1981 the levels of salaries, fringes, and contract scores varied substantially both within and across the arbitration states. Further, many of the comparatively high salary and fringe benefits levels associated with arbitration also existed before arbitration was implemented (i.e., arbitration did not cause the high salaries and benefits in these states). Accordingly, it appears that arbitration's impacts may be much less monolithic than are commonly supposed, and hence arbitration may do a better job of

guarding employee interests in some states (and even in some cities) than in others.

These state-specific results, when combined with our arbitration use results, suggest perhaps the most important conclusion of this study; namely, that arbitration's greatest value to police officers and their unions may be its ability to prevent employees from being disadvantaged by management during the bargaining process. One example of this kind of "insurance" against loss is the fact that most of the ARBUSE coefficients in Chapters III, IV, and V were negative, which indicates that the actual users of the arbitral process tend to be those unions whose salaries, fringes, or contracts are somewhat less favorable than in other cities. This finding implies that these unions are using arbitration to overcome management's unwillingness to offer terms the unions can accept.

A second example of this insurance is the fact that through 1981 the relative levels of salaries and fringes in almost all the arbitration states showed no sign of decline. When we consider that most of our arbitration cities are in the Frost Belt states which have been experiencing losses of people and jobs to other parts of the country, we might expect that this relative erosion of the tax base would exert downward pressure on the rate of increase in salaries and fringes that these cities could pay or would pay. However, our state-specific results indicate that no such dampening of salaries or fringes has occurred in these arbitration states, and our time series results indicate that salaries have increased faster in arbitration states than elsewhere.

Further, our aggregate cross section results show that arbitration's influence on salaries has gotten larger, not smaller, during the 1978-81 years. In short, arbitration may have come into existence in several states which already enjoyed relatively high salaries and fringes, but it appears to have done an excellent job of maintaining (and perhaps even widening) these compensation advantages.

Our analyses indicate that arbitration's largest effect is on the union's ability to obtain favorable contract provisions, for the magnitude of the ARB coefficients in Chapter V (contracts) generally were larger than in Chapters III (salaries) and IV (fringes). In addition to arbitration's very strong association with total contract scores, arbitration had a very strong association with the items which constitute our intrusion into managerial prerogatives (MPI) subindex and also a very strong association with grievance procedure provisions (especially grievance arbitration). These results indicate that police officers in arbitration states have much stronger contractual protections than their colleagues in other states. In turn, these contractual protections enable police officers and their unions in arbitration states to challenge and appeal managerial decisions from a much stronger position than can their peers elsewhere. In other words, our results show that police officers and their unions in arbitration states have obtained a larger "voice" in police department operations than they would have otherwise.

When all these results are examined, we must conclude that arbitration has done a good job of guarding employee interests. Those police officers covered by arbitration laws appear to be better paid and have a stronger voice in departmental affairs than those who are not covered.

In addition, arbitration's effects exist independently of whatever effects bargaining by itself has had. In other words, police officers who bargain collectively are better off than those who do not, and those who bargain under arbitration laws are even better off.

These results should be pleasing to police unionists and to anyone else who believes that police should bargain from positions of equal strength with management. In contrast, these results should be disheartening to municipal managers and to anyone else who believes that collective bargaining or interest arbitration (or both) are inappropriate in the police service. We have no illusions that any of our results will cause people to change their normative views about police bargaining and arbitration, for previous experience (Kochan, 1978) suggests that opponents and proponents of bargaining and especially arbitration will selectively use those findings in this report which will help them advocate their causes and will ignore the rest. However, a complete evaluation of bargaining and arbitration requires that their effects on the delivery of police services also be considered.

#### The Delivery of Police Services

Level and mix of police employment. Our Chapter VI analyses showed that bargaining tilts police employment away from sworn officers and toward civilian employees, and that arbitration has the opposite effect. Because there are so many more sworn officers than civilian employees in each department, and because the ARB coefficients generally are larger than the CBA coefficients, the net effects on unionized police departments

in arbitration states are a higher level of total police employment and a shift in the mix of police employment toward sworn officers and away from civilians (compared to nonunion departments in nonarbitration states). In other words, management appears to respond to bargaining's higher salaries and benefits by substituting lower priced civilians for higher priced officers, but arbitration somehow negates this response.

Monetary costs. Both bargaining and arbitration are associated with increases in the cost of delivering police services. Chapters III and IV showed that police pay and fringes are higher where bargaining and arbitration exist than where they do not. Chapter VI showed that total police expenses are higher in bargaining cities and in arbitration cities even after controlling for the influences of higher salaries and numbers of employees. Taken together, our findings suggest that bargaining and especially arbitration increase the monetary cost of delivering a given bundle of police services to the community.

Ability to manage. As noted above, police unions in arbitration states have negotiated contract language which is more favorable (to the unions) than the language negotiated by their peers in other states. We collected no police operations data, though, so we cannot translate these higher contract scores directly into a precise impact upon police management's ability to manage the police department on a day-to-day basis. However, we did examine the possibility that crime rates and clearance rates have been affected by bargaining and arbitration, and our results are intriguing.

Our Chapter VI analyses showed that reported rates of rape, robbery, assault, and burglary are consistently and significantly negatively

associated with the presence of police bargaining during the 1971-80 period, and murder and auto theft rates are negatively associated with bargaining in about half of those years. This means that when we control for the influence of numerous city characteristics and regional patterns, there are fewer of these crimes reported in bargaining cities than in nonbargaining cities. In fact, whenever there is a significant association between a reported crime rate and bargaining (see Table VI-5), this association is almost always negative. However, just the opposite is true for arbitration: there are significantly higher rates of rape, robbery, assault, burglary, and auto theft reported in unionized cities located in arbitration states than in unionized cities elsewhere (again, after controlling for the influence of city characteristics and regional location). Taken together, these results indicate that unionized cities in arbitration states experience about the same amounts of reported crimes as do nonunion cities in nonarbitration states (i.e., arbitration apparently negates bargaining's association with lower crime rates).

Do these results indicate that bargaining somehow causes police managers to organize and deliver police services in a more effective manner but that arbitration somehow cancels this bargaining effect? Alternatively, do the Chapter VI regression coefficients simply report coincidental associations between bargaining and arbitration and reported crime rates? As another alternative, do bargaining and arbitration somehow serve as unintended proxies for unmeasured city or state characteristics which influence police employment, costs, and reported crime rates? In particular, are there systematic differences between arbitration

and nonarbitration states (or regions) which may be the actual causes of the different bargaining and arbitration results?

We may label these three potential explanations the "managerial response" theory, the "coincidence" theory, and the "location" theory of police bargaining and arbitration's impacts. We will not consider the coincidence theory any further, for there is nothing to consider. However, the other two potential explanations deserve scrutiny.

The centerpiece of the managerial response explanation lies with bargaining and arbitration's opposite impacts on the number of police officers and the number of police civilian employees. Our data show that management reacts to bargaining by hiring fewer sworn officers and more civilians, but that arbitration reverses these civilianization efforts and results in more officers and fewer civilians. We know that most police civilian employees work in clerical and communication technician jobs (i.e., they handle paperwork, answer the phone, dispatch calls, and so on). If management uses civilians to replace sworn officers in these kinds of jobs and then assigns the officers to street duty (i.e., to the patrol or detective divisions), it is possible that bargaining induces managers to deliver law enforcement services in a more effective manner than they would otherwise. In turn, this increased effectiveness (i.e., more sworn officers on the street) may result in fewer crimes being reported. However, we emphasize that this line of reasoning is highly speculative, for we have collected no data about police deployment and assignment practices nor have we examined the actual implementation of any civilianization programs. Further, this line of reasoning is constructed upon the assumption that the reorganiza-

tion of police services can have a substantial effect on the commission and reporting of crimes in a city.

In contrast to our bargaining results, our arbitration results indicate that arbitration is associated with more sworn officers, fewer civilians, and a negation of bargaining's association with lower crime rates. In turn, these results suggest that the presence of arbitration somehow restricts the effective delivery of police services. One possible explanation is the higher level of restrictive contract language in arbitration states than elsewhere; another is that arbitrators have issued awards which restrict management's ability to organize police services in a more effective manner (our arbitration award analyses are not yet sufficiently refined to empirically test this second possibility). However, this line of reasoning also is highly speculative, for we have collected no data about police deployment and assignment practices in arbitration cities. Further, this line of reasoning is constructed upon the assumption that arbitration by itself can be a dominant influence on how police departments organize themselves, and this is a tenuous assumption.

Before anyone concludes that arbitration is contributing to a crime wave in those states where arbitration exists, it is important to note that -- because arbitration and bargaining almost always exist together -- the bargaining and arbitration results tend to cancel each other. In other words, our data indicate that the reported crime rates in bargaining plus arbitration cities are similar to the levels and rates in nonunion plus nonarbitration cities.

Again, we emphasize that these interpretations of bargaining and

arbitration's impacts are speculative, for we have been unable to empirically examine the actual mechanisms or processes through which the statistically significant relationships in Chapter VI might have been implemented. In addition, our clearance rate analyses (see Table VI-6) produced primarily non-results: the bargaining and arbitration coefficients were rarely significant, they were often implausibly large and sometimes changed direction from one year to the next, and the overall equations explained only a tiny amount of the variation in clearance rates across cities. The police presumably exercise much more direct control over the clearance of crimes by arrests than they do over the reporting of crimes, yet clearance rates seem impervious to bargaining and arbitration while several crime rates appear to be highly influenced by them. Accordingly, this "managerial response" explanation is appropriately considered as a subject for future research rather than as a conclusive explanation.

Alternatively, it is possible that there are systematic differences in police department staffing practices and in reported crime patterns across arbitration and nonarbitration states. In Chapter I we saw that the vast majority of the arbitration cities are located in ten states in the Northeast and Upper Midwest (Massachusetts, Rhode Island, Connecticut, New York, New Jersey, Pennsylvania, Michigan, Wisconsin, Iowa, and Minnesota). We also saw that the vast majority of the bargaining cities outside of arbitration states are located in Illinois, Ohio, Florida, and especially California. If there are any systematic differences in police staffing practices and in reported crime patterns across these two groups of states, then the Chapter VI analyses may be reporting

bargaining and arbitration results which are spurious because these differences exist for other reasons. In particular, California has such a large number of cities in the bargaining without arbitration sample that systematically different staffing and crime patterns in that state alone could have produced the CBA coefficients in Chapter VI.

However, we emphasize that this locational interpretation is just as speculative as the effective management explanation just discussed. We have no data to indicate that the police practices and crime patterns are significantly different in the 14 states just mentioned. Further, even if some of these state practices or patterns are significantly different we have no ready explanation for why they exist.

In short, we have done a much better job of indentifying statistically significant relationships between bargaining or arbitration and police employment, costs, and crime levels than we have of explaining what these relationships mean or why they exist. We hope that future research will be more illuminating.

#### Costs and Benefits

Earlier we identified two tangible benefits which arbitration provides: it prevents strikes, and it enables police officers to bargain from a position of equal strength with management. We also identified one tangible cost associated with arbitration: it sometimes weakens the incentive to bargain. (Our emphasis here is on tangible costs and benefits, which means we are ignoring the intangible (i.e., unmeasurable) costs and benefits associated with greater or lesser amounts of political stability and political pluralism.)

In this report we have documented what some people would identify as another tangible benefit of arbitration: it induces more collective bargaining than would occur otherwise. However, the vast majority of our research results have documented what some people would identify as a very tangible cost of arbitration: it increases the cost of delivering police services. Our results show that arbitration is associated with higher salaries, higher police department costs, and more restrictive contract language (though our results are less clear about whether or not arbitration actually caused all of these things to occur). In other words, the consistent theme running through Chapters III, IV, V, and VI is that the price arbitration imposes for preventing strikes, fostering collective bargaining, and equalizing union and management bargaining strength may be higher than previously believed. This conclusion is strengthened by the fact that arbitration's influence on salaries, costs, and contract provisions has occurred in addition to whatever impact bargaining by itself has had on these outcomes.

There is nothing in this conclusion which is meant to suggest that arbitration is a bad thing. As with mediation, factfinding, and the right to strike, arbitration represents a value judgment about how negotiating impasses ought to be resolved. Because there is no formula which can accurately measure whether these labor relations value judgments are good, bad, or indifferent, we have not attempted to determine whether arbitration per se is wise or unwise public policy. Instead, we have analyzed some of the consequences of arbitration, and our analyses show that arbitration's benefits may come at a higher cost than previously identified. In turn, policy makers should decide whether these higher

costs represent an acceptable or unacceptable price to pay for fewer strikes, more collective bargaining, and equalization of bargaining power.

Finally, the analyses in this report have demonstrated that it is inappropriate to test arbitration's impacts by simply comparing arbitrated and negotiated outcomes in the same state. Consequently, we hope that this report will eliminate statements of the following type: "During the past year in this arbitration state, arbitrated wages increased an average of 6.7 percent while negotiated wages increased 6.8 percent. Therefore, arbitration had no effect on wages during this past year."

#### RESEARCH RECOMMENDATIONS

It is customary to close most research reports with the conclusion that "more research is needed," and this report is no exception. We believe that there are five methodological and substantive topic areas which would benefit greatly from additional research.

Longitudinal analyses. Most union impact studies are done at a single point in time. In contrast, our examination of the 1971-81 years (for salaries and fringes) and the 1975-81 years (for contract provisions) is highly unusual. We believe that our time series results offer an interesting comparison with our cross section results, and we believe our before and after state-specific analyses are particularly informative. In short, we believe our analyses have demonstrated the value of longitudinal examinations of union impacts, and we hope that other researchers

will use this approach. In particular, the history of public sector interest arbitration in this country indicates that a definitive before and after analysis of arbitration would require salary, fringe, and contract data back to 1965. Obviously, analyses of employment conditions going back that far can only be done on a longitudinal basis.

State-specific analyses. Our aggregated analyses suggest very substantial arbitration effects, but our disaggregated state-specific analyses suggest that arbitration's influence may be rather modest. In addition, there is information in each of Chapters III, IV, V, and VI which indicates that arbitration's impacts may be less uniform across states than previously believed. As a result, there is a clear need for very intensive examinations of how arbitration has worked in individual states. These intensive studies would profit especially from (a) more complete state-by-state data than we have been able to obtain and (b) an examination of how procedural differences in arbitration arrangements might have affected outcomes. As can be seen in Table I-4, we were unable to obtain complete 1980 information from any state, and the same can be said for the other years in our time period. Consequently, the possibility remains that more complete data would have led to different results. Similarly, J. Anderson (1981b) has shown how the actual operation of the Canadian federal government's arbitration procedure can affect how the parties use it and possibly the outcomes associated with it. As a result, such an analysis could be usefully performed in the states included in this study.

Bargaining, arbitration, and police employment. Our results show that management has responded to police bargaining in a time-honored

manner: bargaining is associated with higher salaries and fringes, so management has substituted lower priced civilians for higher priced officers. As a result, in bargaining cities there are fewer officers and more civilians, ceteris paribus, than in nonunion cities. Arbitration also is associated with higher salaries, so we expect that arbitration similarly would lead to substitutions of lower priced civilians in place of higher priced officers. Instead, arbitration seems to have precisely the opposite effect: more officers and fewer civilians.

As with most of our results, we cannot be certain if bargaining and arbitration are actually causing these changes in the mix of police department employees or if bargaining and arbitration are simply reporting employment changes which occurred for other reasons. Accordingly, an intensive examination of the employment mix in different police departments should isolate the direct and indirect effects that bargaining and arbitration have had on the kinds of personnel that police departments have used to perform particular jobs and functions.

Contract provisions and police practices. We have shown that police contracts contain more favorable (to unions) language in arbitration states than elsewhere, but we have been unable to determine what impacts the presence of this language has had on day-to-day police operations and practices. A very informative research effort would be the analysis of police department operations in nonunion, bargaining without arbitration, and bargaining with arbitration cities, paying particular attention to how police contract provisions have been used to affect management decisions. This kind of intensive and city-specific data would be quite expensive to obtain, but such a study would be one of the very few (and

perhaps the only one) to carefully measure how collective bargaining and interest arbitration have directly affected management's day-to-day ability to manage the delivery of police services. In addition, this type of research is necessary to fully understand the relationships discussed in the next paragraph.

Labor relations, crimes, and clearances. Similarly, research should focus on the associations we identified in Chapter VI between crime rates, bargaining, and arbitration. These findings are potentially the most controversial in the entire report and are certainly the most difficult to explain. After identifying the negative association between bargaining and selected crime rates and the positive association between arbitration and some crime rates, we admit that we are not sure what these statistical relationships mean. If bargaining and arbitration are simply inadvertent proxies for unmeasured city characteristics which are the real factors exerting the influences on crime rates, we are unable to explain what these city characteristics are and why they are associated in one direction with bargaining and in the opposite direction with arbitration. However, if bargaining and arbitration are actually causing these crime rates to be lower and higher, respectively, we are unable to explain why or how these labor relations arrangements are having such impacts on crimes (and on some crimes and not others). As a result, an intensive examination of the reasons for these reported relationships among bargaining, arbitration, and crimes should yield some interesting and useful findings. Such a study should also attempt to explain why crimes are associated with bargaining and arbitration but clearances are not.

A final word. Most good research efforts should generate more questions than they answer, and we hope that this report has done the same. Arbitration's impacts on bargaining outcomes have been researched very sparingly compared to its impacts on the bargaining process, and this report is a useful step toward a fuller assessment of what difference arbitration makes. We realize that arbitration evokes strongly positive and strongly negative opinions from most members of the labor relations community, and all these people ought to be able to find something in this report with which they can disagree. What is more important than any disagreements, though, is the impetus that this report provides toward more complete examinations of collective bargaining and interest arbitration in the public sector. If this report helps to spur such scrutiny, it will have made a contribution that goes beyond the merits of any of the results presented on the previous pages.

## APPENDIX I-1

## POLICE COLLECTIVE BARGAINING AND INTEREST ARBITRATION

Please answer the following questions, even if you do not have a collective bargaining relationship with a police employee organization. Please note that no individual municipality or respondent will be identified; the anonymity of your answers is guaranteed. Also, all questions apply to police organizations representing patrol officers and not to any separate superior officer organizations.

1. Name of municipality: \_\_\_\_\_
2. Does your municipality have a bargaining, negotiating, or meet-and-confer arrangement with a police employee organization representing patrol officers which results in a written contract, agreement, or memorandum of understanding?  
 Yes  
 No
3. If yes, what was the first year your municipality signed such written contract, agreement, or memorandum with the police?  
 Year \_\_\_\_\_
4. If yes, how many written contracts, agreements, or memoranda has your municipality signed with the police?  
 \_\_\_\_\_ contracts, agreements, or memoranda
5. Is your municipality covered by a compulsory interest arbitration procedure which mandates that arbitration be used to resolve negotiating impasses? (check one)  
 No  
 Yes, as a result of state law  
 Yes, as a result of city charter provision or local ordinance  
 Yes, as a result of an agreement with the police employee organization
6. If yes, in what year was this arbitration procedure first made available?  
 Year \_\_\_\_\_
7. If yes, how many times has your municipality and police employee organization received a written interest arbitration award to resolve negotiating impasses during the period since this arbitration procedure was made available?  
 \_\_\_\_\_ times If so, in what year(s): \_\_\_\_\_
8. Have the police in your municipality gone on strike?  
 No  
 Yes If so, in what year(s): \_\_\_\_\_

Please send this completed form plus your 1975-1981 police collective bargaining agreements (if any) and interest arbitration awards (if any) to:

Professor Peter Feuille  
 Institute of Labor and Industrial Relations  
 University of Illinois  
 504 East Armory Avenue  
 Champaign, Illinois 61820

THANK YOU very much for your cooperation, and please be assured of the anonymity of your responses.

APPENDIX I-2  
DATA SOURCES

The main data sources used in this project are cited below. Following each reference is a list of the specific data item(s) it contains.

I. City Characteristics Data

- A. Interuniversity Consortium for Political and Social Research (ICPSR) made available a magnetic tape containing all the information printed in U.S. Bureau of the Census, County and City Data Book, 1977 (Washington, D.C.: GPO, 1980). The variables taken from this tape include: FIPS SMSA number, FIPS State number, city land area, percent of city population over 65, percent of city population that is black, percent of city population over 18, percent of the city population above age 25 that have less than 5 years of education, percent of city population above age 25 that have a high school education or above, city median value of housing, and city average manufacturing wage (note that these data are available only at one point in time between 1970 and 1976).
- B. Certain city data are collected on a yearly or bi-yearly basis for the determination of revenue sharing expenditures. These data were taken from U.S. Department of the Treasury, Office of Revenue Sharing, Final State and Local Data Elements, General Revenue Sharing Entitlement Periods 3-13, 1971-81 (Washington, D.C.: GPO). The variables obtained here include city population, city per capita income, city intergovernmental transfers, and city adjusted local tax revenue.

II. Crime Data

Unpublished data for 1971-1980 were obtained from the Federal Bureau of Investigation (FBI). The FBI data include the number of crimes and clearances in each city for murder, manslaughter, rape, robbery, assault, burglary, larceny, and auto theft. Information on the number of police officers assaulted and killed was also gathered for these years.

III. Police Data

Two main sources of police employment and expenditure data were used.

- A. Under the Omnibus Crime Control and Safe Streets Act of 1968, as amended, the Law Enforcement Assistance Administration and the National Bureau of Justice Statistics collected selected data for virtually all U.S. cities over 25,000 population. Currently, data for 1971-1979 are available on tape from the ICPSR. These tapes include city data on full time and part time police employment, police payroll, police fringe benefits expenditures, police operations, and capital outlay expenditures. Data on selected intergovernmental transfer expenditures for police services are also available on the tapes.
- B. For many years the International City Management Association (ICMA) has conducted annual surveys of police employment and expenditure levels in U.S. cities. We obtained data from these surveys covering the 1970-81 period. The information includes the number of uniformed and civilian police employees, the length of the police work week, the starting and maximum salary

for a patrol officer, the number of years to the maximum step, the amount of longevity pay that accrues to patrol officers, the total salary expenditure for police employees, capital outlay and other police expenditures, social security expenditures, retirement expenditures, health and life insurance expenditures, the metropolitan status of each city, the form of government in each city, and regional location codes for each city.

#### IV. Legal Data

Data on the collective bargaining and arbitration law status of the states were obtained from Bureau of National Affairs, Government Employee Relations Report (GERR), Reference File (Washington, D.C.: BNA).

#### V. Collective Bargaining Data

We undertook a special survey of cities and supplemented our responses with data provided by Dr. Casey Ichniowski of the National Bureau of Economic Research. In a small number of cases information was obtained from individual state public employment relations board and state leagues of cities. These sources yielded information on whether individual cities bargained with their patrol officers, the length of time that the bargaining relationship has existed, whether and when a city experienced a strike by police officers, whether and when the police bargaining unit was covered by a compulsory arbitration statute, and whether and when compulsory arbitration was used to resolve an impasse in police bargaining. These data were used to generate information on the number of cities in each state that bargain with their police officers.

As a part of our survey we obtained collective bargaining agreements and arbitration awards covering patrol officers during the 1975-81 period. These agreements and awards were scored according to the instruments shown in other appendices of this report. We supplemented the information returned to us with agreements and awards gathered at public employment relations boards in several states.

#### VI. Private Sector Unionism Data

Data on the percent of private sector employees that belong to unions by state and SMSA location were obtained from Richard B. Freeman and James L. Medoff, "New Estimates of Private Sector Unionism in the United States," Industrial and Labor Relations Review, 32, 2 (January 1979), 143-174.

#### VII. Construction of Regional Variables

We followed the Census Bureau Classification Codes for Regions:

NORTHEAST	= Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, and Vermont
SOUTH	= Alabama, Arkansas, Delaware, District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia
NORTH CENTRAL	= Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, and Wisconsin
WEST	= Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, and Wyoming

### VIII. Other Factors

States that require or specifically list a grievance procedure in their bargaining laws include Alaska, Delaware, Florida, Minnesota, New Jersey, Oklahoma, Oregon, and Pennsylvania.

Alaska, Florida, Minnesota, and Pennsylvania specifically require grievance arbitration.

The following states require police negotiating impasses to be resolved by binding arbitration (effective date of statute is in parentheses): Alaska (1972), Connecticut (1975), District of Columbia (1980), Iowa (effective in 1975 for 1976 contracts), Maine--for non-cost items only (1974), Massachusetts (effective in 1974, repealed effective 1981), Michigan (1979), Minnesota (1973), Nebraska (1969), New Jersey (1977), New York (1974), Oregon (1973), Pennsylvania (1968), Rhode Island (1968), Washington (1973), and Wisconsin (1972).

### IX. Treatment of Missing Data

The 1981 crime rate, population, and per capita income data were not available. As a result, we used the 1980 values of these variables in the 1981 equations.

Some city characteristics data were not available. In these cases we used data for the county or SMSA that a city was located in as a proxy for the missing information.

### APPENDIX I-3

#### POLICE CONTRACT SCORING INSTRUMENT (130 items--1300 possible points)

#### WORKING CONDITIONS SUBINDEX (18 items--180 possible points)

##### Length of Work Week

- 0 = no provision
- 3.33 = more than 40 hours
- 6.67 = 40 hours
- 10 = less than 40 hours

##### Holiday Work

- 0 = no provision
- 10 = some provision

##### Staffing (MPI)

- 0 = no provision
- 5 = some provision without minimum levels specified
- 10 = minimum levels specified

##### Length of Work Day

- 0 = no provision
- 3.33 = more than 8 hours
- 6.67 = 8 hours
- 10 = less than 8 hours

##### Roll Call

- 0 = roll call attendance required before start of shift
- 5 = no provision
- 10 = roll call attendance occurs during shift

##### Type of Sidearm Carried (MPI)

- 0 = no provision
- 10 = some provision

##### Type of Ammunition Used (MPI)

- 0 = no provision
- 10 = some provision

(MPI) indicates an item that also is included in the intrusion into management prerogatives subindex.

## Armament in Squad Cars (MPI)

- 0 = no provision
- 10 = some provision

## Armament Off Duty

- 0 = no provision
- 5 = some provision
- 10 = do not have to carry weapon while off duty, or paid to carry

## Some Other Special Equipment (MPI)

- 0 = no provision
- 10 = some provision which requires that management provide special equipment

## Use of Arms/Deadly Force (MPI)

- 0 = no provision
- 10 = some provision

## Use of Less-Than-Deadly Force (MPI)

- 0 = no provision
- 10 = some provision

## Firearm Review Board (MPI)

- 0 = no provision
- 10 = some provision

## Firearm Qualification

- 0 = no provision
- 10 = some provision

## Pay Periods

- 0 = no provision
- 10 = some provision

## Facilities at Station (lockers, etc.)

- 0 = no provision
- 10 = some provision

## Shift Exchange (MPI)

- 0 = no provision
- 3.33 = permission of management required
- 6.67 = notification to management required
- 10 = provision allows shift exchange without notification to or approval from management

## Residency Requirement

- 0 = some provision requiring residency in the employing jurisdiction
- 3.33 = no reference
- 6.67 = provision allowing residency outside of the employing jurisdiction but within a stated radius
- 10 = provision allowing residency wherever the employee chooses

PAY SUPPLEMENTS SUBINDEX

(27 items--270 possible points)

## Overtime Pay (MPI)

- 0 = no provision
- 3.33 = straight pay or straight compensatory time off
- 6.67 = 1 1/2 pay (cash or compensatory time off)
- 10 = double time pay (cash or compensatory time off)

## Choice of Overtime Compensation (\$ or time off) (MPI)

- 0 = management decides or mutual decision
- 10 = employee decides

## Court Appearance Pay (MPI)

- 0 = no provision
- 2.5 = 2 hours or less minimum
- 5 = 2.1 to 3 hours minimum
- 7.5 = 3.1 to 4 hours minimum
- 10 = more than 4 hours minimum

## Court Appearance Pay Rate

- 0 = no provision
- 3.33 = straight pay or compensatory time off
- 6.67 = 1 1/2 pay or compensatory time off
- 10 = more than 1 1/2 pay or compensatory time off

## Standby Pay (MPI)

- 0 = no provision
- 5 = flat amount
- 10 = amount increases with the length of the standby period

## Call-In Pay (MPI)

- 0 = no provision
- 2 = flat pay; less than hourly rate
- 4 = 2 hours or less minimum
- 6 = 2.1 to 3 hours minimum
- 8 = 3.1 to 4 hours minimum
- 10 = more than 4 hours minimum

## Vacation Pay at Separation

- 0 = no provision
- 5 = some provision, limiting the amount of pay
- 10 = pay for all unused days

## Sick Leave Pay at Non-retirement Separation

- 0 = no provision
- 5 = some provision with limit on amount
- 10 = payment for all unused sick days

## Pay for Sick Leave upon Retirement

- 0 = no provision
- 5 = some provision with limit on amount
- 10 = payment for all unused sick days

## Education Incentive Pay

- 0 = no provision
- 5 = one time bonus for completion of educational program
- 10 = increase in salary for completion of educational program

## Pay for Out of Title Work

- 0 = no provision
- 5 = payment after some minimum number of days on the job
- 10 = pay with no minimum number of days required

## Severance Pay or Benefit

- 0 = no provision
- 10 = some provision

## Death Benefit

- 0 = no provision
- 10 = some provision

## Tuition Assistance

- 0 = no provision
- 5 = assistance provided for police-related courses or with management approval
- 10 = assistance for any courses

## Private Jobs

- 0 = no provision
- 3.33 = provision allows employees to take private jobs
- 6.67 = provision establishes minimum pay rates for private jobs
- 10 = provision establishes minimum pay rates and an allocation system

## Meal Allowance

- 0 = no provision
- 10 = some provision

## Initial Clothing Allowance

- 0 = no provision
- 3.33 = \$250 or less
- 6.67 = \$251 to \$400
- 10 = more than \$400 or furnished

## Clothing Allowance (Annual)

- 0 = no provision
- 3.33 = \$250 or less
- 6.67 = \$251 to \$400
- 10 = more than \$400 or furnished

## Replacement of Equipment

- 0 = no provision
- 5 = provision which provides for replacement of equipment damaged while on duty, with a limit on the amount
- 10 = provision which provides for full replacement of equipment damaged while on duty

## Longevity Pay Eligibility (first year longevity pay begins)

- 0 = no provision
- 2.5 = 15 years or more
- 5 = 10 to 14 years
- 7.5 = 5 to 9 years
- 10 = less than 5 years

## Longevity Pay Maximum Amount (Annual)

- 0 = no provision
- 2 = some provision
- 4 = \$499 or less
- 6 = \$500 to \$999
- 8 = \$1,000 to \$1,999
- 10 = \$2,000 or more

## Roll Call Pay

- 0 = no reference
- 10 = some provision

## Cost of Living Allowance

- 0 = no provision
- 5 = some provision with a limit
- 10 = unlimited provision

## Travel Allowance for Required Duties (e.g., court appearances)

- 0 = no provision
- 10 = some provision

## Shift Differential

- 0 = no provision
- 10 = some provision

## Special Assignment Pay

- 0 = no provision
- 10 = some provision

## Other Extra Pay

- 0 = no provision(s)
- 10 = some provision(s)

INDIVIDUAL SECURITY SUBINDEX  
(11 items--110 possible points)

## Vacancies - Promotions (MPI)

- 0 = no provision
- 3.33 = some provision that does not mention seniority
- 6.67 = some provision with seniority as a factor
- 10 = seniority governs

## Shift Assignment (MPI)

- 0 = no provision
- 3.33 = some provision that does not mention seniority
- 6.67 = provision including seniority as one factor
- 10 = seniority governs

## Posting and Bidding Provision for Vacancies and Assignments

- 0 = no provision
- 10 = some provision

## Involuntary Transfers (MPI)

- 0 = no provision or a provision allowing management to make changes at its discretion
- 3.33 = some provision that does not mention seniority
- 6.67 = seniority is a factor in assignment changes
- 10 = seniority governs

## Transfer Procedure (Voluntary Transfers) (MPI)

- 0 = no provision
- 3.33 = some provision that does not mention seniority
- 6.67 = provision including seniority as one factor
- 10 = seniority governs

## Layoff Procedure (MPI)

- 0 = no provision
- 3.33 = some provision that does not mention seniority
- 6.67 = provision including seniority as one factor
- 10 = seniority governs

## Justification for Layoff (MPI)

- 0 = no reference or layoffs are part of management's rights
- 5 = specific circumstances are specified that allow layoffs
- 10 = the standard of "necessity" is used to justify layoffs

## Recall from Layoff Procedure

- 0 = no provision
- 5 = some provision
- 10 = provision stating that last laid off is first recalled

## Safety Provision

- 0 = no provision
- 10 = some provision

## Maintenance of Standards (MPI)

- 0 = no provision
- 1.25 = some provision exists covering limited subjects
- 2.5 = some provision exists covering all subjects
- 3.75 = union must be notified before changes may be made (for limited subjects)
- 5 = union must be notified before any changes may be made
- 6.25 = a meeting or consultation must occur before changes may be made (limited subjects)
- 7.5 = a meeting or consultation must occur before any changes may be made
- 8.75 = changes must be negotiated (limited subjects)
- 10 = any changes must be negotiated

## Vacation Selection

- 0 = no provision
- 3.33 = some provision that does not mention seniority
- 6.67 = provision including seniority as one factor
- 10 = seniority governs

UNION SECURITY SUBINDEX  
(19 items--190 possible points)

Bargaining Agent Exclusive Recognition

- 0 = no provision
- 10 = some provision

Contract Duration

- 0 = continuous
- 5 = 4 years or more
- 10 = 1-3 years

Checkoff

- 0 = no reference
- 3.33 = voluntary
- 6.67 = automatic, unless employee objects
- 10 = mandatory

Union Security

- 0 = no reference or "open shop"
- 3.33 = maintenance of membership
- 6.67 = agency shop or fair share agreement
- 10 = union shop

Pay Parity

- 0 = no reference
- 10 = parity with firefighters

Financial Information Provided to Union

- 0 = no reference
- 10 = some provision

Bulletin Board

- 0 = no reference
- 5 = employer may regulate
- 10 = materials allowed without editing

Savings Clause

- 0 = no reference
- 10 = some provision

Released Time for Union Business

- 0 = no reference
- 3.33 = time off for grievances
- 6.67 = time off for grievances and negotiations
- 10 = time off for grievances, negotiations and other business

Office Space for Union Officer(s) or Steward(s)

- 0 = no reference
- 10 = some provision

Union Leave for Meetings and Conventions

- 0 = no reference
- 2 = some provision, deducting time from sick or personal leave
- 4 = 1-3 days
- 6 = 4-9 days
- 8 = 10-15 days
- 10 = more than 15 days

Bargaining Unit

- 0 = no reference
- 1.25 = patrol officers; an additional 1.25 points is added for each separate group included in addition to patrol officers (e.g., corporals, sergeants, lieutenants, captains, dispatchers/communications technicians, parking control agents, clerical; does not include detectives/plain clothes) [maximum of 10 points]

Union Use of Facilities During Nonwork Time

- 0 = no reference
- 10 = some provision

Management Rights (MPI)

- 0 = exhaustive, comprehensive (Type I) clause that is not subject to grievance procedure
- 2.5 = comprehensive clause (Type II) that is implicitly subject to the grievance procedure
- 5 = less comprehensive clause (Type III) that is implicitly subject to the grievance procedure
- 7.5 = clause uses "residual rights" language (Type IV), does not fall into other categories, or no provision
- 10 = clause specifically states that management rights may be subject to grievance or negotiation

No Strike: General Clause

- 0 = no strikes; union will act to prevent or eliminate stoppages
- 5 = no strikes without a mention of union responsibility
- 10 = no reference

No Strike: Definition of Strike [coded only if contract has a no strike clause, otherwise = 10]

- 0 = a "strike" is specifically defined in the clause
- 10 = no definition or no reference

No Strike: Discipline of Strikers [coded only if contract has a no strike clause, otherwise = 10]

- 0 = clause explicitly states that "striking" employees may be disciplined or discharged
- 10 = no reference

Zipper Clause

- 0 = provision states that both parties have had unlimited rights to bargain and waive all rights to further bargaining for the duration of the contract
- 5 = some other, less restrictive provision
- 10 = no provision

Subcontracting

- 0 = contract explicitly recognizes management's right to subcontract
- 3.33 = contract implicitly allows management to subcontract
- 6.67 = "management retains all customary rights" clause (i.e., only general language in management rights clause could address the issue)
- 10 = no reference

EQUITY SUBINDEX

(19 items--190 possible points)

Evaluation of Employees (Performance Appraisal) (MPI)

- 0 = no reference
- 3.33 = some provision
- 6.67 = provision providing employee the opportunity to see evaluation
- 10 = provision allows employee to respond/object to evaluation

Employee Access to Personnel File

- 0 = no reference
- 5 = some provision allowing employee access
- 10 = provision allows employee to respond

Investigative Bill of Rights (internal affairs) (MPI)

- 0 = no provision
- 5 = partial miranda rights
- 10 = full miranda rights (remain silent, representation)

Nondiscrimination

- 0 = no reference
- 10 = some provision

Grievance Definition (MPI)

- 0 = no provision
- 2.5 = grievances are narrowly defined; specific items are explicitly excluded from the grievance procedure
- 5 = grievances are disputes arising from the contract's terms and conditions
- 7.5 = grievances are disputes arising from the contract, personnel policies, or other aspects of work
- 10 = grievances are not specifically defined in the grievance procedure

Grievance Procedure: Representation by Union  
[equals zero if there is no grievance procedure]

- 0 = no reference
- 5 = no union representation prior to arbitration
- 10 = union representation occurs prior to arbitration

Grievance Time Limits at Initial Step

- 0 = no reference
- 3.33 = 5 days or less
- 6.67 = 6 to 10 days
- 10 = more than 10 days

Employer Response to Grievances

[equals zero if there is no grievance procedure]

- 0 = procedure does not require an employer to respond within a particular number of days
- 5 = grievance is denied if employer does not respond within a stated time period
- 10 = grievance is sustained if employer does not respond within a stated time period

Grievance Procedure (final step) (MPI)

- 0 = no reference
- 2 = police chief
- 4 = local official or council
- 6 = civil service officer or police commission
- 8 = advisory arbitration or factfinding
- 10 = binding arbitration

Limits on Arbitration

- 0 = certain types of grievances are not subject to arbitration
- 10 = all grievances may go to arbitration

## Limits on Arbitrator

- 0 = arbitrator is specifically limited beyond normal (cannot amend contract, etc.) constraints--e.g., limits on amount of back pay that may be awarded, etc.
- 10 = arbitrator faces only normal limitations (cannot amend contract)

## Cost of Arbitration

- 0 = loser pays or no reference
- 5 = cost is divided equally
- 10 = management pays

## Civil Service and Third-Party Entities

- 0 = no reference or the power of the third-party is not diminished by the contract
- 10 = some provision

## Disciplinary Procedures (MPI)

- 0 = no reference
- 5 = some provision without "just cause" or "cause" terminology
- 10 = some provision with "just cause" or "cause" terminology

## Rules Changes (MPI)

- 0 = management solely promulgates rules, or no reference
- 5 = changes in rules must be posted in advance
- 10 = changes in rules may be challenged by union in meeting or through grievance procedure

## Review of Discipline (MPI)

- 0 = no reference
- 5 = appeal to local officials or civil service or police commission
- 10 = appeal to grievance procedure

## Specification of Discipline (MPI)

- 0 = no provision
- 10 = some provision which restricts management's range of disciplinary options

## Officer Rights in Citizen Complaint Procedure

- 0 = no provision
- 10 = some provision

## Union-Management Committee

- 0 = no provision
- 5 = some provision to recommend changes
- 10 = some provision to mandate changes

FRINGE BENEFITS SUBINDEX  
(36 items--360 possible points)

## Dental Insurance (employee)

- 0 = no reference
- 3.33 = employee pays total amount
- 6.67 = employee pays part
- 10 = employer pays total amount

## Dental Insurance (family plan)

- 0 = no reference
- 3.33 = employee pays total amount
- 6.67 = employee pays part
- 10 = employer pays total amount

## Disability Insurance ("income protection")

- 0 = no reference
- 3.33 = employee pays total amount
- 6.67 = employee pays part
- 10 = employer pays total amount

## Health Insurance (employee)

- 0 = no reference
- 3.33 = employee pays total amount
- 6.67 = employee pays part
- 10 = employer pays total amount

## Health Insurance (family plan)

- 0 = no reference
- 3.33 = employee pays total amount
- 6.67 = employee pays part
- 10 = employer pays total amount

## Life Insurance

- 0 = no reference
- 3.33 = employee pays total amount
- 6.67 = employee pays part
- 10 = employer pays total amount

## Visual Insurance (employee)

- 0 = no reference
- 3.33 = employee pays total amount
- 6.67 = employee pays part
- 10 = employer pays total amount

## Visual Insurance (family plan)

- 0 = no reference
- 3.33 = employee pays total amount
- 6.67 = employee pays part
- 10 = employer pays total amount

## Worker's Compensation

- 0 = no reference
- 10 = some provision

## False Arrest Insurance

- 0 = no reference
- 10 = some provision

## Additional Insurance Protection (e.g., prepaid legal, etc.)

- 0 = no additional provision(s)
- 10 = some additional provision

## Vacation Accumulation

- 0 = no reference
- 10 = some provision

## Number of Holidays

- 0 = no provision
- 2.5 = 9 days or less
- 5 = 9.1 - 10.9 days
- 7.5 = 11 - 12.9 days
- 10 = 13 days or more

## Vacation Policy

- 0 = no provision
- 2.5 = 1-2 weeks after 1 year
- 5 = 2.1 to 3 weeks after 1 year
- 7.5 = 3.1 to 4 weeks after 1 year
- 10 = more than 4 weeks after 1 year

## Maximum Vacation

- 0 = no provision or no increase for length of service
- 2 = 3 weeks or less
- 4 = 3.1 to 4 weeks
- 6 = 4.1 to 5 weeks
- 8 = 5.1 to 6 weeks
- 10 = more than 6 weeks

## Sick Leave

- 0 = no reference
- 2.5 = 12 days or less per year
- 5 = 13 to 15 days per year
- 7.5 = 16 - 17 days per year
- 10 = more than 17 days per year (including unlimited)

## Sick Leave: Proof of Illness

- 0 = contract requires some evidence of illness to use sick leave
- 5 = no reference
- 10 = no proof is required

## Sick Leave: Advance Notice

- 0 = contract requires employee to give notice if he/she will be absent on a scheduled work day
- 5 = no reference
- 10 = no notice is required

## Maximum Accumulation of Sick Leave

- 0 = no provision for accumulation
- 2.5 = 100 days or less
- 5 = 101-149 days
- 7.5 = 150-199 days
- 10 = 200 days or more (including unlimited)

## Sick Leave Bank

- 0 = no reference
- 10 = some provision

## Extended Leave

- 0 = no reference
- 10 = some provision

## Personal Leave -- Amount

- 0 = no provision or tied to sick leave
- 2.5 = 1-2 days per year
- 5 = 3 days per year
- 7.5 = 4 days per year
- 10 = 5 or more days per year

## Personal Leave -- Notification

- 0 = prior approval of management is required
- 5 = no reference or more than 1 day notice is required
- 10 = less than 1 day notice is required

## Bereavement Leave

- 0 = no provision of deduction from sick leave
- 3.33 = 3 days or less
- 6.67 = 4 days
- 10 = 5 days or more

## Bereavement Leave Coverage

- 0 = immediate family (father, mother, spouse, children, in-law, brother, sister, grandparents)
- 10 = immediate family plus others

## Civil Leave (jury duty, subpoena, etc.)

- 0 = no reference
- 10 = some provision

## Parental Leave

- 0 = no reference
- 10 = some provision

## Educational Leave

- 0 = no reference
- 10 = some provision

## Military/VISTA/Public Service Leave

- 0 = no reference
- 10 = some provision

## Religious Leave

- 0 = no reference
- 10 = some provision

## Parking Facilities

- 0 = no reference
- 10 = employer will attempt to provide adequate parking facilities

## Laundry and Dry Cleaning

- 0 = no reference
- 10 = some provision

## Family Illness Leave

- 0 = no reference
- 10 = some provision

## Disability Leave (line of duty—if different from sick leave)

- 0 = no provision
- 5 = leave with a limit
- 10 = unlimited

## Pensions

- 0 = no reference (or if provision shows contributions are established by law or administratively)
- 5 = contributory plan
- 10 = noncontributory plan

## Selection of Training Course by Employee (MPI)

- 0 = no provision or employee had no prerogative to select course(s)
- 5 = opportunities posted, but management makes final decision
- 10 = employee had discretion in selection

---

(MPI) indicates an item that also is included in the intrusion into management prerogatives subindex

Intrusion Into Management Prerogatives Subindex  
(31 items--310 possible points)

Staffing  
Other Special Equipment  
Shift Exchange  
Overtime Pay  
Choice of Overtime Pay or Time Off  
Court Appearance Pay  
Call-In Pay  
Standby Pay  
Vacancies - Promotion  
Shift Assignment  
Transfer  
Layoff Procedure  
Justification for Layoff  
Maintenance of Standards  
Management Rights  
Evaluation of Employees  
Grievance Procedure (final step)  
Disciplinary Procedures  
Review of Discipline  
Bill of Rights  
Grievance Definition  
Selection of Training Course  
Specification of Discipline  
Involuntary Transfers  
Type of Sidearm Carried  
Type of Ammunition Used  
Armament in Squad Cars  
Use of Arms/Deadly Force  
Use of Less-Than-Deadly Force  
Firearm Review Board  
Rules Changes

Grievance Procedure Subindex  
(8 items--80 possible points)

Grievance Definition  
Representation by Union  
Time Limits  
Employer Response  
Final Step  
Limits on Arbitration  
Limits on Arbitrator  
Cost of Arbitration

Personnel Allocation Subindex (measures the extent the union has  
negotiated restrictions on management's ability to allocate or  
assign personnel to jobs)  
(13 items--130 possible points)

Staffing  
Shift Exchange  
Overtime Pay  
Choice of Overtime Pay or Time Off  
Court Appearance Pay  
Call-In Pay  
Standby Pay  
Vacancies - Promotions Procedure  
Shift Assignment  
Transfer Provision  
Involuntary Transfers  
Layoff Procedure  
Subcontracting

Disciplinary Protection Subindex  
(4 items--40 possible points)

Bill of Rights (internal investigations)  
Review of Discipline  
Disciplinary Procedures  
Specification of Discipline

Law Enforcement Policy Subindex (measures the extent the union has  
negotiated language which affects the performance of police-specific  
duties)  
(9 items--90 possible points)

Type of Sidearm Carried  
Type of Ammunition Used  
Armament in Squad Cars  
Firearm Review Board  
Use of Arms/Deadly Force  
Use of Less-Than-Deadly Force  
Officer Rights in Citizen Complaint Procedure  
Union-Management Committee  
Residency Requirements

## APPENDIX I-4

## ARBITRATION AWARD SCORING FORM

Page 1 of \_\_\_\_\_  
 Date Read \_\_\_\_\_

City Name \_\_\_\_\_  
 FIPS State Code \_\_\_\_\_  
 ICMA City Number \_\_\_\_\_  
 Years that arbitration award is effective 19\_\_ Scorer \_\_\_\_\_

## TYPE OF ARBITRATION USED

- \_\_\_\_\_ Conventional; i.e., the neutral hears the parties' arguments and formulates a binding outcome on the issues in question (codes: 1 = on all issues; 2 = on some issues).
- \_\_\_\_\_ Final Offer Arbitration with package selection; i.e., the neutral hears the parties arguments and must choose the final offer (covering all disputed issues) of the party that is most reasonable (codes: 1 = on all issues; 2 = on some issues).
- \_\_\_\_\_ Final Offer Arbitration with issue selection; i.e., the arbitrator resolves the dispute by choosing the final offer of one of the parties for each individual issue in dispute (codes: 1 = on all issues; 2 = on some issues).

## ARBITRATION PANEL AND AWARD

- \_\_\_\_\_ Was the dispute decided by a single arbitrator or a panel of arbitrators? (1 = single arbitrator; 2 = three member panel).
- \_\_\_\_\_ Is the employer's ability to pay in question in this dispute? (1 = yes, 0 = no).
- \_\_\_\_\_ Number of issues in dispute
- \_\_\_\_\_ Number of pages in award
- \_\_\_\_\_ Bargaining agent code (from contract scoring form)

\_\_\_\_\_  
 Name of neutral arbitrator (first, middle initial, last)

## DIRECTIONS

1. The purpose of this arbitration award scoring procedure is to determine the extent changes in police contracts occur via arbitration awards. As a result, your scoring of these awards must be consistent with your scoring of the police contracts.
  - a. Each scorer should score the arbitration awards from the same cities and states whose police contracts he scored.
  - b. When in doubt about how to score an arbitration issue, please check back and see how you scored that issue in the contract.
2. The issues contained in the arbitration awards will be scored in exactly the same manner as in the police contracts. Therefore, in the forms on the following pages you must list the "issue title" exactly as it appears in our contract scoring form. Keep in mind that arbitrators may label these issues differently than we have, so please be prepared to use your best judgment when translating issues into our language.
3. Each arbitration issue will be quantified on a favorableness to the union basis using the contract scoring format.
  - a. Issues which contain only a no provision/some provision dichotomy will be scored "0" or "1," as appropriate. For example, if such an issue is already in the contract, each side proposes new wording, and the arbitrator awards new wording, the status quo, employer's position, union's position, and arbitrator's award each will be scored "1." Obviously, this measuring method will not pick up all the changes in these issues, but it will pick up the award of new issues and the deletion of existing issues.
  - b. Issues which contain multiple scoring options must be very carefully assessed. For example, a change in union security from open shop (status quo = 1) to agency shop (arbitrator's award = 3) is easy to score. However, some change proposals or awards may not be substantial enough to warrant a change in numbers. For example, even though the union and the employer may propose some new wording in an already existing agency shop provision, and even though the arbitrator may award some new agency shop wording, the status quo, employer's position, union's position, and arbitrator's award each would be scored "3."
4. As a complete example, if an issue involved the ammunition used by police, its title would be "ammunition, type of". If there was no prior provision the "status quo" would be scored as zero. If the



APPENDIX III-1

BARGAINING AND MINIMUM SALARIES  
(Dependent Variable = lnMINSAL)

	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981
Intercept	6.208*** (.202)	6.262*** (.221)	6.391*** (.206)	6.387*** (.215)	6.482*** (.216)	6.229*** (.224)	6.602*** (.209)	6.853*** (.218)	7.190*** (.215)	7.386*** (.242)	7.020*** (.258)
LPOP	.0198*** (.0058)	.0207*** (.0063)	.0220*** (.0061)	.0189*** (.0060)	.0135** (.0062)	.0161** (.0067)	.0205*** (.0066)	.0170** (.069)	.0226*** (.0064)	.0309*** (.0069)	.0273*** (.0073)
DENSITY	.000009*** (.000001)	.00001*** (.000002)	.000007*** (.000001)	.000007*** (.000001)	.000007*** (.000001)	.000006*** (.000001)	.000009*** (.000002)	.000009*** (.000002)	.000007*** (.000002)	.000005*** (.000002)	.000006*** (.000002)
LPCI	.1594*** (.0330)	.1310*** (.0364)	.1019*** (.0346)	.0832** (.0358)	.1201*** (.0358)	.1607*** (.0353)	.1559*** (.0315)	.1606*** (.0319)	.1466*** (.0307)	.1641*** (.0350)	.2010*** (.0369)
LCRATE	.0501*** (.0115)	.0541*** (.0131)	.0433*** (.0130)	.0564*** (.0122)	.0549*** (.0128)	.0277** (.0134)	-.0038 (.0032)	-.0035 (.0033)	-.0060* (.0033)	.0111 (.0147)	-.0046 (.0147)
OPFWAGE	.1788*** (.0265)	.1772*** (.0289)	.1937*** (.0287)	.1965*** (.0278)	.1916*** (.0284)	.2050*** (.0281)	.0251** (.0099)	.0242** (.0103)	.0288*** (.0098)	.0204** (.0103)	.0156 (.0103)
ICTIV	-.0687*** (.0144)	-.0671*** (.0159)	-.0710*** (.0151)	-.0723*** (.0154)	-.0593*** (.0156)	-.0670*** (.0155)	-.0582*** (.0142)	-.0708*** (.0146)	-.0773*** (.0147)	-.0639*** (.0172)	-.0794*** (.0275)
CMGR	.0332*** (.0100)	.0330*** (.0107)	.0230*** (.0105)	.0162 (.0105)	.0230** (.0104)	.0198* (.0107)	.0150 (.0104)	.0133 (.0109)	.0194* (.0106)	.0101 (.0118)	.0340*** (.121)
LHOUSE	.1195*** (.0225)	.1337*** (.0246)	.1444*** (.0236)	.1746*** (.0243)	.1427*** (.0239)	.1243*** (.0239)	.1064*** (.0216)	.0870*** (.0219)	.0649*** (.0210)	.0346 (.0238)	.0459* (.0244)
BLACK	.0004 (.0005)	-.0003 (.0005)	.0006 (.0005)	.0003 (.0005)	-.0002 (.0005)	.0004 (.0005)	.0006 (.0005)	.0005 (.0005)	-.0005 (.0005)	-.0004 (.0005)	-.0006 (.0006)

APPENDIX III-1 (cont.)

BARGAINING AND MINIMUM SALARIES  
(Dependent Variable = lnMINSAL)

	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981
NEAST	-.0581*** (.0142)	-.0527*** (.0160)	-.0525*** (.0152)	-.0559*** (.0156)	-.0619*** (.0151)	-.0671*** (.0152)	-.0972*** (.0139)	-.1012*** (.0148)	-.0789*** (.0143)	-.0911*** (.0159)	-.0851*** (.0169)
SOUTH	-.1214*** (.0153)	-.1048*** (.0161)	-.1156*** (.0155)	-.0922*** (.0156)	-.0722*** (.0155)	-.0767*** (.0162)	-.1254*** (.0147)	-.1270*** (.0151)	-.1340*** (.0147)	-.1462*** (.0162)	-.1301*** (.0168)
WEST	.0357*** (.0134)	.0368*** (.0142)	.0407*** (.0136)	.0349*** (.0132)	.0424*** (.0132)	.0661*** (.0137)	.0667*** (.0126)	.0625*** (.0130)	.0378*** (.0130)	.0636*** (.0143)	.0886*** (.0146)
CBA	.0524*** (.0103)	.0667*** (.0111)	.0660*** (.0106)	.0538*** (.0106)	.0697*** (.0107)	.0828*** (.0112)	.0983*** (.0111)	.1002*** (.0115)	.0641*** (.0114)	.0678*** (.0125)	.0575*** (.0131)
F	117.0***	105.9***	104.4***	89.3***	82.7***	90.9***	94.3***	83.6***	71.8***	59.7***	66.6***
R <sup>2</sup>	.721	.719	.716	.683	.649	.652	.636	.605	.581	.563	.608
$\bar{R}^2$	.715	.713	.710	.676	.641	.644	.629	.597	.573	.554	.599
N	603	551	551	552	595	646	716	725	688	616	573

Standard errors in parentheses. \*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests). Unlike the figures in Table III-3, the figures here are log coefficients.

APPENDIX III-2

BARGAINING AND MAXIMUM SALARIES  
(Dependent Variable = lnMAXSAL)

	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981
Intercept	5.600*** (.201)	5.886*** (.221)	5.968*** (.206)	5.840*** (.223)	5.849*** (.215)	5.815*** (.220)	5.987*** (.208)	6.074*** (.204)	6.385*** (.211)	6.651*** (.242)	6.622*** (.252)
LROP	.0304*** (.0057)	.0267*** (.0063)	.0273*** (.0061)	.0244*** (.0063)	.0231*** (.0062)	.0232*** (.0066)	.0255*** (.0066)	.0244*** (.0064)	.327*** (.0062)	.0343*** (.0068)	.0336*** (.0071)
DENSITY	.000007*** (.000001)	.000008*** (.000002)	.000005*** (.000001)	.000007*** (.000002)	.000006*** (.000001)	.000006*** (.000001)	.000007*** (.000002)	.000008*** (.000002)	.000007*** (.000002)	.000006*** (.000002)	.000008*** (.000002)
LFCI	.1562*** (.0328)	.1523*** (.0365)	.1158*** (.0346)	.1067*** (.0370)	.1640*** (.0356)	.1602*** (.0347)	.1873*** (.0313)	.1991*** (.0299)	.1864*** (.0301)	.2256*** (.0350)	.226*** (.0359)
LCRATE	.0417*** (.0114)	.0532*** (.0131)	.0416*** (.0130)	.0610*** (.0126)	.0530*** (.0128)	.0404*** (.0132)	-.0034 (.0031)	-.0020 (.0031)	-.0072** (.0032)	.0196 (.0147)	.0056 (.0143)
OPWAGE	.1126*** (.0263)	.1156*** (.0290)	.1307*** (.0286)	.1263*** (.0288)	.1248*** (.0283)	.1375*** (.0277)	.0162* (.0099)	.0167* (.0097)	.0225** (.0096)	.0126 (.0103)	.0150 (.0100)
ICTTY	-.0699*** (.0143)	-.0703*** (.0159)	-.0784*** (.015)	-.0805*** (.0159)	-.0717*** (.0155)	-.0789*** (.0152)	-.0646*** (.0141)	-.0737*** (.0137)	-.0757*** (.0144)	-.0732*** (.0172)	-.0695*** (.0170)
CMGR	.0631*** (.0099)	.0596*** (.0107)	.0512*** (.0105)	.0391*** (.0108)	.056*** (.0104)	.0473*** (.0106)	.0492*** (.0103)	.0426*** (.0102)	.0549*** (.0104)	.0510*** (.0118)	.0715*** (.0118)
LHOUSE	.1858*** (.0224)	.1726*** (.0247)	.1947*** (.236)	.2290*** (.0251)	.1858*** (.0238)	.1909*** (.0235)	.1559*** (.0215)	.1455*** (.0205)	.1198*** (.0206)	.0743*** (.0238)	.0837*** (.0238)
BLACK	.0018*** (.0005)	.0012** (.0005)	.0015*** (.0005)	.0013** (.0006)	.0014*** (.0005)	.0015*** (.0005)	.0015*** (.0005)	.0016*** (.0005)	.0009* (.0005)	.0009 (.0006)	-.0002 (.0006)

APPENDIX III-2 (cont.)

BARGAINING AND MAXIMUM SALARIES  
(Dependent Variable =  $\ln \text{MAXSAL}$ )

	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981
NEAST	-.438*** (.0141)	-.0422*** (.0160)	-.0411*** (.0152)	-.0590*** (.0161)	-.0749*** (.0151)	-.0661*** (.0150)	-.0594*** (.0138)	-.0997*** (.0139)	-.0805*** (.0140)	-.0911*** (.0159)	-.0793*** (.0165)
SOUTH	-.1545*** (.0152)	-.1375*** (.0161)	-.1347*** (.0155)	-.1109*** (.0162)	-.0961*** (.0155)	-.0959*** (.0159)	-.1307*** (.0146)	-.2364*** (.0142)	-.2444*** (.0144)	-.2567*** (.062)	.2398*** (.0164)
WEST	.0479*** (.0133)	.0511*** (.0143)	.0528*** (.036)	.0422*** (.0137)	.0477*** (.0131)	.0650*** (.0135)	.0664*** (.0125)	.0564*** (.0122)	.02934** (.0127)	.0519*** (.0142)	.0718*** (.0142)
CBA	.0504*** (.0103)	.0576*** (.0111)	.0561*** (.0106)	.0528*** (.0110)	.0618*** (.0107)	.0656*** (.0110)	.0922*** (.0110)	.0858*** (.0108)	.0542*** (.0111)	.0455*** (.0125)	.0344*** (.0127)
F	143.6***	122.7***	122.4***	103.3***	100.7***	106.8***	113.0***	108.9***	89.3***	68.6***	75.6***
R <sup>2</sup>	.760	.748	.748	.714	.693	.687	.677	.666	.633	.597	.637
$\bar{R}^2$	.755	.742	.741	.707	.686	.681	.671	.660	.626	.588	.629
N	603	551	551	552	595	646	716	725	688	616	573

Standard errors in parentheses. \*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests). Unlike the figures in Table III-3, the figures here are log coefficients.

APPENDIX III-3

BARGAINING AND ARBITRATION AVAILABILITY IN 1976

	<u>lnMINSAL</u>	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>ln MAXSAL</u>	<u>\$MINSAL</u>	<u>\$MINSAL</u>	<u>\$MAXSAL</u>	<u>\$MAXSAL</u>
Intercept	6.1933*** (.2237)	6.2109*** (.2235)	5.7697*** (.2195)	5.7929*** (.2195)	-22918.4*** (2456.07)	-22702.12*** (2354.22)	-35367.17*** (2841.52)	-35040.44*** (284.56)
LPOP	.0158** (.0067)	.0156** (.0067)	.0227*** (.0066)	.2252*** (.0066)	162.479** (73.686)	159.525** (73.812)	282.902*** (85.2495)	279.15*** (85.492)
DENSITY	.000006*** (.000001)	.000006*** (.000001)	.000006*** (.000001)	.000006*** (.000001)	.065*** (.0163)	.065*** (.0163)	.0818*** (.0188)	.0806*** (.0189)
LPCI	.1601*** (.0352)	.159*** (.0352)	.1595*** (.0345)	.1582*** (.0346)	1665.03*** (386.32)	1649.97*** (386.84)	1973.21*** (446.95)	1953.14*** (448.056)
LCRATE	.027** (.0134)	.0279** (.0134)	.0395*** (.0131)	.0406*** (.0131)	252.783* (146.86)	264.06* (146.98)	466.681*** (169.91)	482.696*** (170.23)
OPWAGE	.1983*** (.0282)	.2005*** (.0282)	.1291*** (.0277)	.132*** (.0277)	2144.14*** (309.98)	2168.96*** (309.62)	1801.33*** (358.624)	1841.05*** (358.61)
ICITY	-.0694*** (.0155)	-.0692*** (.0155)	-.082*** (.0152)	-.0816*** (.0152)	-580.89*** (170.05)	-579.78*** (170.306)	-839.746*** (196.74)	-836.11*** (197.25)
OMER	.0203* (.0107)	.0198* (.0107)	.048*** (.0105)	.047*** (.0105)	209.51* (117.66)	203.19* (117.75)	584.65*** (136.12)	575.54*** (136.381)
LHOUSE	.1286*** (.024)	.1283*** (.024)	.1963*** (.2351)	.1957*** (.0236)	1480.26*** (263.09)	1478.77*** (263.54)	2661.44*** (304.383)	2655.66*** (305.244)
BLACK	.0006 (.0005)	.0005 (.0005)	.0017*** (.0005)	.0016*** (.0005)	8.5949 (5.8066)	8.0211 (5.7937)	21.902*** (6.7178)	20.9901*** (6.7104)

APPENDIX III-3 (cont.)

BARGAINING AND ARBITRATION AVAILABILITY IN 1976

	<u>lnMINSAL</u>	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>lnMAXSAL</u>	<u>\$MINSAL</u>	<u>\$MINSAL</u>	<u>\$MAXSAL</u>	<u>\$MAXSAL</u>
NEAST	-.0752*** (.0156)	-.074*** (.0157)	-.0762*** (.0153)	-.0743*** (.0154)	-869.65*** (171.74)	-858.2*** (171.9)	-1104.54*** (198.69)	-1081.85*** (199.1)
SOUTH	-.0695*** (.0165)	-.0714*** (.0164)	-.0868*** (.0162)	-.0895*** (.0161)	-559.707*** (180.8)	-580.62*** (180.069)	-886.157*** (209.17)	-92.034*** (208.562)
WEST	.0756*** (.0143)	.0774*** (.0143)	.0769*** (.0141)	.0745*** (.0141)	985.7*** (157.55)	970.217*** (157.42)	1143.55*** (183.277)	1114.1*** (182.33)
CBA	.7627*** (.0115)	.0751*** (.0119)	.0573*** (.0113)	.0563*** (.0117)	800.85*** (126.77)	781.96*** (130.77)	710.29*** (146.66)	690.21*** (151.46)
ARB	.0285** (.0132)	—	.0358*** (.013)	—	366.14** (145.15)	—	525.053*** (167.93)	—
CBA-ARB	—	.0252* (.01375)	—	.0302** (.0135)	—	338.55** (150.74)	—	462.56** (174.596)
F	85.2***	84.9***	100.7***	100.1***	79.1***	78.8***	97.8***	97.2***
R <sup>2</sup>	.6541	.6534	.6909	.6897	.6371	.6363	.6847	.6834
$\bar{R}^2$	.6454	.6457	.6841	.6828	.629	.6283	.6778	.6764
N	646	646	646	646	646	646	646	646

Standard errors in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at .10 level (two-tailed tests).

APPENDIX III-4

BARGAINING AND ARBITRATION AVAILABILITY IN 1981

	<u>lnMINSAL</u>	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>lnMAXSAL</u>	<u>\$MINSAL</u>	<u>\$MINSAL</u>	<u>\$MAXSAL</u>	<u>\$MAXSAL</u>
Intercept	7.0104*** (.2557)	7.0175*** (.2562)	6.6074*** (.245)	6.6182*** (.2457)	-24254.5*** 3745.9)	-24145.*** (3754.7)	-41012.02*** (4920.69)	-40809.97*** (4306.06)
LPOP	.0275*** (.0072)	.0271*** (.0073)	.0339*** (.0069)	.0333*** (.007)	385.5729*** (106.1902)	380.03*** (106.44)	586.74*** (121.633)	576.07*** (122.07)
DENSITY	.000007*** (.000002)	.000006*** (.000002)	.000009*** (.000002)	.000009*** (.000002)	.0981*** (.0286)	.0976*** (.0286)	.1605*** (.0327)	.1601*** (.0328)
LPCI	.1921*** (.0366)	.193*** (.0367)	.2087*** (.0351)	.2097*** (.0352)	2780.3491*** (536.6886)	2796.41*** (537.94)	3777.86*** (614.74)	3795.76*** (616.94)
LCRATE	-.0099 (.0146)	-.0081 (.0146)	-.0027 (.014)	-.0002 (.014)	-160.7581 (214.555)	-132.83 (214.5224)	-74.064 (245.756)	-27.088 (246.024)
OPWAGE	.0123 (.0102)	.013 (.0102)	.0099 (.0098)	.0108 (.0098)	161.9067 (149.9403)	173.3185 (150.1377)	172.895 (171.745)	190.374 (172.185)
ICITY	-.0814*** (.0173)	-.0807*** (.0173)	-.0727*** (.0166)	-.0716*** (.0166)	-1117.4583*** (253.4453)	-1105.75*** (253.98)	-1241.8*** (290.3)	-1221.82*** (291.27)
CMGR	.0356*** (.012)	.0353*** (.012)	.074*** (.0115)	.0736*** (.0115)	496.9813*** (175.5266)	491.745*** (175.92)	1232.18*** (201.05)	1224.32*** (201.74)
LHOUSE	.0518** (.0242)	.0513** (.0243)	.0928*** (.0232)	.0924*** (.0233)	923.6485*** (354.8148)	914.93*** (355.69)	1862.68*** (406.41)	1854.63*** (407.92)
BEBLACK	.0004 (.0006)	-.0005 (.0006)	.00006 (.0006)	.000006 (.0006)	2.8181 (8.7565)	2.2168 (8.77)	7.215 (10.0299)	6.293 (10.058)

APPENDIX III-4 (cont.)

BARGAINING AND ARBITRATION AVAILABILITY IN 1981

	<u>lnMINSAL</u>	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>lnMAXSAL</u>	<u>\$MINSAL</u>	<u>\$MINSAL</u>	<u>\$MAXSAL</u>	<u>\$MAXSAL</u>
NEAST	-.1189*** (.0194)	-.1143*** (.0193)	-.1319*** (.0186)	-.1273*** (.0185)	-1860.52*** (284.2217)	-1789.8*** (282.62)	-2616.65*** (325.554)	-5259.54*** (324.12)
SOUTH	-.1143*** (.0173)	-.1162*** (.0173)	-.1051*** (.0166)	-.1069*** (.0166)	-1579.65*** (253.584)	-1609.73*** (253.88)	-1813.66*** (290.46)	-1848.4*** (291.158)
WEST	.1061*** (.0153)	.1045*** (.0153)	.099*** (.0146)	.0979*** (.0147)	1759.1543*** (223.9196)	1733.96*** (224.84)	1976.39*** (256.48)	1953.89*** (257.86)
CBA	.0467*** (.0133)	.0466*** (.0135)	.0175 (.0128)	.0165 (.0129)	711.1173*** (195.3747)	710.42*** (197.29)	316.45 (223.79)	297.987 (226.26)
ARB	.0576*** (.0167)	—	.0897*** (.016)	—	889.91*** (254.34)	—	1674.42*** (281.018)	—
CBA*ARB	—	.0515*** (.0168)	—	.0847*** (.0161)	—	796.12*** (246.44)	—	1578.75*** (282.62)
F	63.8***	63.4***	76.2***	75.5***	64.7***	64.2***	86.7***	81.9***
R <sup>2</sup>	.6061	.6045	.6481	.6459	.6093	.6075	.6672	.6645
N	573	573	573	573	573	573	573	573

Standard errors in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at .10 level (two-tailed tests).

## APPENDIX III-5

## STATE-SPECIFIC SALARY EFFECTS IN 1981

	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>\$MINSAL</u>	<u>\$MAXSAL</u>
Intercept	6.9939*** (.2576)	6.5747*** (.2414)	-25,037.72*** (3,763.36)	-41,825.26*** (4,189.34)
LPOP	.0299*** (.0072)	.0413*** (.0068)	426.30*** (105.75)	728.08*** (117.72)
MI	.0726** (.0291)	.112*** (.0273)	1,212.39*** (425.63)	2,181.78*** (473.81)
RI	.1409 (.1011)	.0194 (.0948)	1,595.02 (1,477.58)	141.28 (1,644.83)
IA	-.0612 (.0373)	-.0507 (.035)	-942.98* (545.15)	-1,073.01* (606.85)
PA	.2926*** (.0897)	.2106** (.0841)	3,777.08*** (1,310.59)	3,393.30** (1,458.93)
CT	.218** (.0881)	.0518 (.0826)	2,435.11* (1,287.09)	447.10 (1,432.77)
WI	.0754** (.0335)	.0493 (.0314)	1,206.81** (488.98)	738.74 (544.33)
NY	.2639*** (.0892)	.1427* (.0836)	3,181.51** (1,303.18)	2,320.32 (1,450.69)
WA	.1269*** (.0362)	.1671*** (.0339)	2,077.08*** (528.48)	3,275.75*** (588.29)
MN	.0269 (.0311)	.1053*** (.0292)	393.05 (454.63)	2,058.65*** (506.09)
MA	.2268*** (.0872)	.0596 (.0817)	2,659.88** (1,274.15)	692.51 (1,418.38)
NJ	.2134** (.0865)	.1899** (.811)	2,412.70* (1,263.96)	2,945.41** (1,407.03)
OPPWAGE	.012 (.0101)	.0094 (.0094)	156.73 (146.92)	164.08 (163.55)
LPCI	.1674*** (.0371)	.1668*** (.0348)	2,386.08*** (541.99)	3,009.37*** (603.34)

## APPENDIX III-5 (cont.)

## STATE-SPECIFIC SALARY EFFECTS IN 1981

	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>\$MINSAL</u>	<u>\$MAXSAL</u>
ICITY	-.0788*** (.0171)	-.0677*** (.016)	-1,061.19*** (249.86)	-1,145.81*** (278.15)
DENSITY	.000005*** (.000002)	.000006*** (.000002)	.0758** (.0298)	.1016*** (.0332)
CMGR	.0395*** (.0122)	.0717*** (.0115)	555.03*** (178.54)	1,176.151*** (198.75)
CBA	.0495*** (.0131)	.0219* (.0123)	748.97*** (191.23)	399.91* (212.93)
LCRATE	.0036 (.0149)	.0048 (.0139)	-78.86 (217.43)	-122.84 (242.04)
PBLACK	.0006 (.0006)	.0004 (.0006)	1.48 (8.76)	-.1534 (9.75)
WEST	.0879*** (.0169)	.82*** (.0158)	1,479.07*** (246.68)	1,648.25*** (274.60)
SOUTH	-.121*** (.0174)	-.1085*** (.0163)	-1,675.02*** (254.08)	-1,880.12*** (282.84)
LHOUSE	.0756*** (.0262)	.127*** (.0245)	1,346.97*** (382.47)	2,500.94*** (425.76)
NEAST	-.2978*** (.0834)	-.1777** (.0782)	-3,788.78*** (1,218.42)	-3,009.57** (1,356.33)
F	39.9***	50.5***	40.7***	56.1***
R <sup>2</sup>	.6202	.6753	.6253	.6982
N	573	573	573	573

Standard errors in parentheses.

\*\*\*Significant at the .01 level;

\*\*Significant at the .05 level;

\*Significant at .10 level (two-tailed tests).

**CONTINUED**

**4 OF 5**

APPENDIX III-6  
TIME SERIES ANALYSIS OF BARGAINING AND ARBITRATION

	OLS		GLS	
	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>lnMINSAL</u>	<u>lnMAXSAL</u>
Intercept	6.3371*** (.0656)	5.7219*** (.0651)	5.8116*** (.085)	3.7403*** (.1424)
OPPWAGE	.1624*** (.0082)	.1014*** (.0081)	.1446*** (.0103)	.0508*** (.0192)
LPOP	.0209*** (.0019)	.0276*** (.0019)	.0116*** (.0025)	.0102** (.0046)
LPCI	.1435*** (.0105)	.1758*** (.0105)	.3199*** (.0013)	.7407*** (.0097)
LHOUSE	.1063*** (.0071)	.1598*** (.0070)	.0200*** (.0084)	-.0915*** (.0133)
LCRATE	.0312*** (.0039)	.0352*** (.0039)	.0385*** (.0042)	.0419*** (.0044)
CMGR	.0228*** (.0033)	.0534*** (.0032)	.0034 (.0043)	.0227*** (.0082)
DENSITY (000)	.0078*** (.0005)	.0068*** (.0005)	.0086*** (.0006)	.0066*** (.0009)
PBLACK	-.00002 (.0002)	.0012*** (.0002)	.0005** (.0002)	.0025*** (.0004)
ICITY	-.0699*** (.0047)	-.0742*** (.0047)	-.0600*** (.0061)	-.0470*** (.0115)
NEAST	-.0692*** (.0048)	-.0736*** (.0048)	-.0363*** (.0060)	-.0022 (.0108)
SOUTH	-.0921*** (.0050)	-.1113*** (.0049)	-.1056*** (.0065)	-.1465*** (.0122)
WEST	.0639*** (.0043)	.0659*** (.0043)	.0642*** (.0056)	.0779*** (.1049)
1972	.0499*** (.0065)	.0559** (.0064)	.0222*** (.0047)	.0317*** (.0036)
1973	.0998*** (.0065)	.1107*** (.0065)	.0702*** (.0047)	.0831*** (.0036)
1974	.1254*** (.0069)	.1318*** (.0068)	.0597*** (.0050)	-.0024 (.0036)
1975	.1984*** (.0068)	.2149*** (.0068)	.1349*** (.0050)	.0812*** (.0037)
1976	.2359*** (.0075)	.2512*** (.0075)	.1389*** (.0059)	.0153*** (.0042)

APPENDIX III-6  
TIME SERIES ANALYSIS OF BARGAINING AND ARBITRATION

	OLS		GLS	
	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>lnMINSAL</u>	<u>lnMAXSAL</u>
1977	.2864*** (.0079)	.3008*** (.0079)	.1774*** (.0064)	.0303*** (.0045)
1978	.3389*** (.0079)	.3641*** (.0079)	.2287*** (.0064)	.0896*** (.0045)
1979	.8738*** (.0092)	.3926*** (.0091)	.2309*** (.0079)	.0217*** (.0056)
1980	.4498*** (.0093)	.4745*** (.0092)	.3042*** (.0079)	.0961*** (.0057)
1981	.5334*** (.0093)	.5669*** (.0092)	.3881*** (.0080)	.1895*** (.0056)
CBA	.0622*** (.0035)	.0491*** (.0035)	.0496*** (.0037)	.0188*** (.0037)
ARB	.0250*** (.0042)	.0393*** (.0042)	.0172*** (.0043)	.0140*** (.0042)
F	1462.4***	1738.0***	---	---
R <sup>2</sup>	.8418	.8636	---	---
N	6588	6588	6588	6588

Standard errors in parentheses.

\*\*\*Significant at the .01 level;

\*\*Significant at the .05 level;

\*Significant at .10 level (two-tailed tests).

## APPENDIX III-7

## BARGAINING, ARBITRATION AVAILABILITY, AND ARBITRATION USE IN 1981

	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>\$MINSAL</u>	<u>\$MAXSAL</u>
Intercept	7.0335*** (.2594)	6.6197*** (.2516)	-23,855.18*** (3,793.85)	-40,834.85*** (4,414.51)
LPOP	.0249*** (.0074)	.0326*** (.0071)	350.41*** (107.79)	562.71*** (125.42)
WEST	.1096*** (.0154)	.098*** (.0149)	1,811.72*** (225.41)	1,950.01*** (262.58)
OPPWAGE	.0136 (.0105)	.011 (.0102)	188.57 (153.38)	201.37 (178.47)
ARBUSE	-.0273 (.0224)	-.0098 (.0217)	-438.53 (327.28)	-201.49 (380.83)
LPCI	.2046*** (.0377)	.2073*** (.0365)	2,939.72*** (551.09)	3,775.99*** (641.25)
DENSITY	.000008*** (.000002)	.000009*** (.000002)	.1164*** (.0301)	.1543*** (.0351)
LCRATE	-.0097 (.0151)	.0027 (.0147)	-146.99 (221.27)	-71.31 (257.47)
CBA	.0448*** (.0134)	.0158 (.013)	686.61*** (196.68)	292.04 (228.86)
ICITY	-.0807*** (.0178)	-.0734*** (.0173)	-1,095.59*** (260.71)	-1,246.21*** (303.36)
CMGR	.0309*** (.0123)	.0741*** (.0119)	421.66** (179.28)	1,230.21*** (208.60)
NEAST	-.1064*** (.0199)	-.1182*** (.0193)	-1,649.59*** (290.96)	-2,351.21*** (338.56)
PBLACK	-.0006 (.0006)	.00007 (.0006)	.2322 (8.82)	4.95 (10.26)
SOUTH	-.1095*** (.0174)	-.104*** (.0168)	-1,511.39*** (253.81)	-1801.98*** (295.34)
ARB	.0582*** (.0173)	.0867*** (.0167)	869.71*** (252.45)	1,578.86*** (293.75)
LHOUSE	.0408 (.0248)	.0944*** (.0241)	778.33** (363.32)	1,875.71*** (422.76)
F	58.3***	67.2***	59.1***	72.6***
R <sup>2</sup>	.6092	.6429	.6124	.6606
N	553	553	553	553

Standard errors in parentheses.

\*\*\*Significant at the .01 level;

\*\*Significant at the .05 level;

\*Significant at .10 level (two-tailed tests).

APPENDIX III-8

BARGAINING, ARBITRATION AVAILABILITY, CURRENT USE, AND PRIOR USE IN 1981  
(all cities)

	<u>lnMINSAL</u>	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>lnMAXSAL</u>	<u>\$MINSAL</u>	<u>\$MINSAL</u>	<u>\$MAXSAL</u>	<u>\$MAXSAL</u>
Intercept	6.9339*** (.2585)	6.9805*** (.2583)	6.4995*** (.2522)	6.5121*** (.2527)	-25,376.30*** (3,773.29)	-24,719.28*** (3,769.29)	-43,060.88*** (4,413.99)	-42,883.50*** (4,421.66)
LPOP	.0253*** (.0073)	.0227*** (.0074)	.0033*** (.0071)	.0326*** (.0072)	355.17*** (106.98)	318.62*** (108.06)	569.01*** (125.13)	563.74*** (126.76)
WEST	.113*** (.0153)	.1106*** (.0151)	.0968*** (.0149)	.0954*** (.0148)	1,873.48*** (222.67)	1,841.85*** (221.04)	1,943.00*** (260.46)	1,923.21*** (259.29)
OFFWAGE	.0136 (.0104)	.0139 (.0104)	.0117 (.0106)	.0118 (.0102)	187.50 (152.08)	191.51 (151.73)	211.70 (177.89)	212.36 (177.99)
ARBUSE	.0282 (.0221)	.0329 (.00222)	.0059 (.0215)	-.006 (.0217)	-469.22 (322.06)	-538.00* (323.49)	-150.88 (376.72)	-153.44 (379.47)
LECI	.2062*** (.0375)	.1988*** (.0374)	.2101*** (.3661)	.2075*** (.0366)	2,945.85*** (547.83)	2,842.53*** (546.30)	3,802.66*** (640.82)	3,766.26*** (640.85)
DENSITY	.000008*** (.000002)	.000007*** (.000002)	.000008*** (.000002)	.000008*** (.000002)	.1123*** (.2984)	.1072*** (.0299)	.1465*** (.0349)	.1466*** (.0351)
LCRATE	-.0069 (.015)	-.0029 (.0151)	.0016 (.0146)	.0017 (.0148)	-107.16 (219.06)	-48.27 (220.92)	2.16 (256.24)	4.00 (259.16)
PARB	.026 (.0191)	—	.0177 (.0186)	—	336.12 (278.95)	—	241.60 (326.30)	—
ICITY	-.0758*** (.0177)	-.0775*** (.0177)	.066*** (.0173)	-.0669*** (.0173)	-1,021.15*** (258.32)	-1,045.59*** (257.88)	-1,121.63*** (302.16)	-1,126.77*** (302.15)
OMGR	.0297** (.0122)	.0289** (.0122)	.0722*** (.0119)	.7232*** (.0119)	406.18** (177.69)	393.69** (177.44)	1,197.74*** (207.85)	1,199.61*** (208.15)

APPENDIX III-8 (cont.)

BARGAINING, ARBITRATION AVAILABILITY, CURRENT USE, AND PRIOR USE IN 1981  
(all cities)

	<u>lnMINSAL</u>	<u>lnMINSAL</u>	<u>lnMAXSAL</u>	<u>lnMAXSAL</u>	<u>\$MINSAL</u>	<u>\$MINSAL</u>	<u>\$MAXSAL</u>	<u>\$MAXSAL</u>
CBA	.0446*** (.0132)	.045*** (.0132)	.0191 (.0129)	.0197 (.0129)	674.80*** (192.89)	679.26*** (192.18)	341.79 (225.63)	350.32 (225.44)
NEAST	-.101*** (.0187)	-.1009*** (.0186)	-.1036*** (.0182)	-.1034*** (.0182)	-1,589.67*** (272.59)	-1,583.81*** (271.93)	-2,112.59*** (318.86)	-2,109.97*** (318.99)
BLACK	.0005 (.0006)	.0006 (.0006)	.0001 (.0006)	.00009 (.0006)	2.57 (8.78)	.6225 (8.82)	8.24 (10.27)	8.15 (10.35)
SOUTH	-.1073*** (.1715)	-.1069*** (.0171)	-.1058*** (.1673)	-.1063*** (.0167)	-1,456.23*** (250.36)	-1,457.98*** (249.76)	-1,817.69*** (292.86)	-1,824.30*** (292.98)
ARBAGE	.0058*** (.0021)	.0055*** (.002)	.008*** (.0021)	.0088*** (.002)	96.75*** (30.95)	89.18*** (29.48)	158.90*** (36.20)	170.42*** (34.57)
HOUSE	.0497** (.025)	.0559** (.025)	.105*** (.0243)	.1065*** (.0244)	928.80** (364.32)	1,016.29** (364.64)	2,091.69*** (426.16)	2,113.06*** (427.76)
PASTUSE	—	.0114** (.0057)	—	.0011 (.0056)	—	166.52** (83.22)	—	17.19 (97.63)
F	55.7***	56.1***	63.0***	62.9*	56.8***	57.3***	68.6***	68.5***
R <sup>2</sup>	.6137	.6152	.6428	.6422	.6183	.6201	.6622	.6619
N	553	553	553	553	553	553	553	553

Standard errors in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at .10 level (two-tailed tests).

APPENDIX IV-1  
 FRINGE BENEFITS IN 1980

	<u>lnFRNGCOP</u>	<u>\$FRNGCOP</u>
Intercept	-3.4694*** (1.3422)	-17,179.7*** (4,573.2)
LPOP	.1195*** (.0387)	548.4*** (131.8)
CMGR	.1948*** (.0655)	376.1* (223.1)
LPCI	-.0121 (.2009)	-94.0 (684.4)
OPPWAGE	.0204 (.5623)	125.8 (191.6)
DENSITY	.00002* (.00001)	0.1*** (0.03)
ICITY	.0377 (.0972)	184.8 (331.2)
LCRATE	.1594* (.0824)	442.0 (280.8)
WEST	0.0708 (.0839)	-157.8 (286.8)
NEAST	0.3215*** (.1039)	-1,343.6*** (354.0)
SOUTH	-.4794*** (.0927)	-1,753.7*** (315.9)
PBLACK	.0036 (.0032)	20.1* (10.9)
lnHOUSE	.3753*** (.1398)	1,675.3*** (476.3)
CBA	.2725*** (.0711)	791.3*** (241.9)
ARB	.1344 (.0970)	564.4* (330.4)
ARBUSE	.0455 (.1333)	51.3 (454.0)
PASTUSE	.0183 (.0325)	159.3 (110.9)
F	12.8***	17.0***
R <sup>2</sup>	.2524	.3127
N	565	565

The first column contains the OLS log coefficients, and the second column contains the dollar coefficients. Standard errors are in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

APPENDIX IV-2  
TOTAL COMPENSATION IN 1980

	<u>lnTOTCOMP</u>	<u>\$TOTCOMP</u>
Intercept	0.6618* (.3976)	-62,620.9*** (17,337.0)
LPOP	.5939*** (.0115)	1,745.6*** (499.6)
CBA	.0856*** (.021)	1,479.9 (917.1)
CMGR	.0265 (.0194)	-112.9 (345.7)
LPCI	.123** (.0595)	1,549.6 (2,594.6)
ARBUSE	-.0377 (.0395)	-2,463.1 (1,721.3)
OPPWAGE	.0226 (.0167)	454.1 (726.3)
DENSITY	.00002*** (.000003)	0.4*** (0.1)
ICITY	-.0381 (.0288)	-648.1 (1,255.7)
LCRATE	.0282 (.0244)	814.9 (1,064.4)
WEST	.0029 (.0249)	23.9 (1,084.3)
PASTUSE	.0241** (.0096)	1,230.0*** (420.4)
NEAST	-.1081*** (.0308)	-4,062.4*** (1,342.1)
PBLACK	.0005 (.001)	-7.7 (41.4)
SOUTH	-.1933*** (.0275)	-3,321.6*** (1,197.7)
ARB	.0879*** (.0287)	2,889.7** (1,252.5)
LHOUSE	.194*** (.0414)	5,196.5*** (1,805.6)
F	40.4***	11.1***
R <sup>2</sup>	.5283	.2228
N	565	565

Standard errors in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at .10 level (two-tailed tests).

APPENDIX IV-3  
COMBINED PAY SUPPLEMENTS AND  
FRINGE BENEFIT CONTRACT SCORES IN 1981

	<u>lnMONI</u>	<u>MONI (points)</u>
Intercept	6.4285*** (.9534)	283.7162* (151.7526)
LPOP	.0242 (.0253)	4.3484 (4.0284)
CMGR	.0372 (.0451)	8.0172 (7.182)
LPCI	-.3055** (.1499)	-37.5913 (23.8637)
ARBUSE	.0633 (.0604)	5.8827 (9.61)
OPPWAGE	.4985*** (.1404)	97.7169*** (22.3407)
DENSITY	-.0000004 (.000005)	.0002 (.0008)
ICITY	-.0619 (.0687)	-14.8786 (10.9296)
LCRATE	.0838 (.062)	9.6951 (9.8638)
WEST	-.2041*** (.0585)	-33.7726*** (9.3163)
PASTUSE	-.0322*** (.015)	-4.5697* (2.393)
NEAST	-.0284 (.0631)	-6.8107 (10.0487)
PBLACK	.0051** (.0022)	-.4393 (.3561)
SOUTH	.0309 (.0919)	3.0305 (14.6316)
ARB	.135* (.0771)	23.7701* (12.2731)
LHOUSE	.062 (.0953)	5.9412 (15.164)
CBAGE	.0026 (.0046)	.4126 (.7249)
BARGLIND	.0209** (.0087)	4.2196*** (1.3826)
F	6.2***	7.9***
R <sup>2</sup>	.1747	.2195
N	422	422

Standard errors in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at .10 level (two-tailed tests).

APPENDIX V-1  
TOTAL CONTRACT AND SUBINDEX SCORES IN 1981

	lnICI	ICI (pts.)	lnWCI	lnISI	lnUSI	lnEQI	lnMPI
Intercept	6.2892*** (.7125)	502.6* (268.7)	4.7361*** (1.1738)	1.8108 (.6.9413)	4.8483*** (.6089)	2.4788 (6.5788)	3.7916*** (1.3118)
LPOP	.0552*** (.0190)	20.62*** (7.16)	.0518* (.0313)	.1828 (.1850)	.0190 (.0162)	.4644*** (.1754)	.0970*** (.0350)
DENSITY	-.000001 (.000004)	-.00003 (.0014)	.00001 (.00001)	-.00002 (.00004)	.000001 (.000003)	-.00002 (.00003)	-.000003 (.000007)
LPCI	-.2466** (.1123)	-95.12** (42.33)	-.4623** (.1849)	-.2383 (1.0935)	-.1643* (.0959)	-.6634 (1.0364)	-.3844* (.2067)
LCRATE	.0682 (.0464)	24.08 (17.48)	.0899 (.0764)	.3504 (.4516)	.0168 (.0396)	.2798 (.4280)	.1055 (.0853)
OPWAGE	.3823*** (.1053)	157.37*** (39.69)	.2542 (.1734)	1.1806 (1.0253)	.2938*** (.0899)	2.0190** (.9718)	.3758* (.1938)
ICITY	.0135 (.0513)	-4.67 (19.34)	.0648 (.0845)	.3404 (.4997)	.0609 (.0438)	.3528 (.4736)	.0559 (.0944)
CMR	-.0074 (.0337)	0.62 (12.70)	-.1143** (.0555)	-.3008 (.3281)	-.0538* (.0288)	-.1895 (.3109)	-.0168 (.0620)
LHOUSE	.0859 (.0714)	34.48 (26.94)	.2099* (.117)	-.0224 (.6959)	.0253 (.0610)	-.0005 (.6596)	.2425* (.1315)
BLACK	-.0019 (.0017)	-0.46 (0.63)	-.0032 (.0028)	-.0027 (.0163)	-.0007 (.0014)	-.0102 (.0155)	-.0012 (.0031)
NEAST	-.0525 (.0476)	-28.67 (17.95)	-.2198*** (.0784)	-.0220 (.4638)	.1844*** (.0407)	.0645 (.4395)	-.1508* (.0876)
SOUTH	.0979 (.0743)	24.63 (28.01)	-.0251 (.1223)	.9338 (.7237)	.1172* (.0635)	.4301 (.6859)	.2326* (.1368)
WEST	-.2098*** (.0468)	-82.09*** (17.64)	-.3320*** (.0771)	-.6392 (.4557)	.0783* (.0400)	-1.3704*** (.4319)	-.3793*** (.0861)
EARLIND	.0167** (.0077)	7.42** (2.91)	0.287** (.0127)	.0454 (.0751)	-.0016 (.0066)	.0963 (.0712)	.0281** (.0142)
CBAGE	.0002 (.0034)	0.59 (1.29)	.0091 (.0056)	-.0119 (.0333)	.0051* (.0029)	-.0267 (.0315)	-.0015 (.0063)
ARB	.1736** (.0727)	64.47** (27.39)	.0444 (.1197)	.9482 (.7078)	.0349 (.0621)	.3676 (.6708)	.2854** (.1338)
ARBUSE	.0505 (.0467)	18.11 (17.60)	.0777 (.769)	-.3278 (.4546)	.0331 (.0399)	.2088 (.4309)	.0859 (.0859)
PASTUSE	-.0253** (.0114)	-9.21** (4.30)	-.0359* (.0188)	.0122 (.1110)	.000003 (.0097)	-.1095 (.1052)	-.0437** (.0210)
F	11.4***	12.2***	5.5***	2.5***	4.9***	4.2***	8.8***
R <sup>2</sup>	.296	.311	.153	.057	.136	.115	.239
N	422	422	422	422	422	422	422

Standard errors in parentheses.

\*\*\*Significant at the .01 level, \*\*Significant at the .05 level, \*Significant at the .10 level (two-tailed tests).

APPENDIX VI-1  
ICMA DATA FOR 1980

Independent Variable	Number of Police	Number of Police (excludes wage)	Number of Civilians	Total Expense
1) AFB	.188*** (.029)	.191*** (.030)	-.295*** (.060)	.080** (.034)
2) CFA	-.088*** (.027)	-.109*** (.027)	.095* (.055)	.049 (.030)
3) POP	.743*** (.041)	.758*** (.042)	.634*** (.101)	.170*** (.058)
4) MAXSAL	-.422*** (.074)	—	.341** (.155)	.768*** (.087)
5) Density	.033* (.018)	.007 (.018)	-.057 (.037)	.031 (.021)
6) Income	.340*** (.064)	.191*** (.059)	.253* (.133)	.016 (.074)
7) Percent Minority	.008*** (.001)	.010*** (.001)	.004 (.003)	.000 (.001)
<u>Clearances</u>				
8) Murder	-.006 (.005)	-.004 (.006)	.014 (.011)	.001 (.006)
9) Manslaughter	.009 (.006)	.009 (.006)	-.007 (.012)	.010 (.007)
10) Rape	-.010 (.007)	-.011 (.007)	-.006 (.014)	.001 (.008)
11) Robbery	.074*** (.025)	.058** (.025)	.057 (.051)	.027 (.028)
12) Assault	.023 (.025)	.017 (.025)	.089* (.050)	.011 (.028)
13) Burglary	-.101** (.045)	-.091** (.046)	.086 (.092)	.025 (.051)
14) Larceny	.159*** (.041)	.152*** (.043)	.175** (.086)	-.020 (.043)
15) Auto Theft	.143*** (.025)	.141*** (.026)	-.031 (.053)	.082** (.029)

## APPENDIX VI-1 (cont.)

ICMA DATA FOR 1980

<u>Independent Variable</u>	<u>Number of Police</u>	<u>Number of Police (excludes wage)</u>	<u>Number of Civilians</u>	<u>Total Expense</u>
<u>Crimes</u>				
16) Murder	.004 (.005)	.003 (.005)	-.007 (.010)	-.007 (.006)
17) Manslaughter	-.006 (.006)	-.006 (.006)	.008 (.012)	-.010 (.007)
18) Rape	.000 (.005)	-.000 (.005)	-.010 (.010)	-.003 (.005)
19) Robbery	-.010 (.010)	-.007 (.010)	-.018 (.020)	-.001 (.011)
20) Assault	.004 (.021)	.003 (.022)	-.047 (.044)	-.038 (.025)
21) Burglary	.046* (.025)	.045* (.026)	.049 (.051)	.055* (.028)
22) Larceny	-.046 (.021)	-.047 (.022)	.043 (.043)	.003* (.024)
23) Auto Theft	-.026** (.011)	-.025** (.011)	.022 (.023)	-.005 (.013)
24) Number of Civilians	.010 (.020)	.000 (.020)	—	.131*** (.023)
25) Manufacturing Wage (1972)	-.004 (.002)	-.020 (.022)	-.091** (.045)	.028 (.025)
26) Number of Police	—	—	.043 (.083)	.605*** (.046)
Constant	-4.411*** (.754)	-7.024*** (.615)	-11.895*** (1.510)	-5.738*** (.883)
R <sup>2</sup>	.9259	.9220	.7886	.9204
N	627	627	627	619

Standard errors in parentheses.

\*\*\*Significant at the .01 level;

\*\*Significant at the .05 level;

\*Significant at the .10 level (two-tailed tests).

## APPENDIX VI-2

EE DATA FOR 1977

<u>Independent Variable</u>	<u>Number of Full-time Employees</u>		<u>Total Expense</u>
	<u>With Wage</u>	<u>Without Wage</u>	
1) ARB	.139*** (.035)	.137*** (.036)	.115*** (.031)
2) CBA	-.023 (.033)	-.054* (.033)	.002 (.030)
3) POP/100,000	.048*** (.005)	.048*** (.005)	.028*** (.004)
4) Avg. Salary	-.234*** (.043)	—	.241*** (.041)
5) Density	-.026 (.020)	-.042** (.020)	.029 (.018)
6) Income	.371*** (.071)	.257*** (.069)	.444*** (.066)
7) Percent Minority	.004*** (.001)	.005*** (.001)	.002** (.001)
<u>Clearances</u>			
8) Murder	-.003 (.005)	-.003 (.006)	.002 (.005)
9) Manslaughter	.005 (.006)	.001 (.006)	-.002 (.005)
10) Rape	-.008 (.007)	-.009 (.008)	-.004 (.007)
11) Robbery	.067*** (.020)	.064*** (.020)	.038** (.018)
12) Assault	.144*** (.033)	.142*** (.033)	.108*** (.030)
13) Burglary	-.044* (.024)	-.045* (.025)	-.040* (.022)
14) Larceny	.117*** (.033)	.118*** (.033)	.105*** (.029)
15) Auto Theft	.344*** (.023)	.340*** (.023)	.293*** (.021)

## APPENDIX VI-2 (cont.)

EE DATA FOR 1977

Independent Variable	Number of Full-time Employees		Total Expense
	With Wage	Without Wage	
<u>Crimes</u>			
16) Murder	.010* (.005)	.010* (.005)	.001 (.005)
17) Manslaughter	.001 (.006)	.004 (.006)	.006 (.005)
18) Rape	.006 (.006)	.006 (.006)	.004 (.005)
19) Robbery	-.008 (.009)	-.007 (.009)	-.007 (.008)
20) Assault	.015 (.029)	.015 (.029)	.031 (.026)
21) Burglary	.081*** (.026)	.077*** (.025)	.069*** (.022)
22) Larceny	-.064*** (.025)	-.059** (.025)	-.075*** (.022)
23) Auto Theft	.023** (.011)	.023** (.011)	.003 (.010)
25) FT Employees	—	—	.286*** (.018)
Constant	-.296 (.591)	-.812 (.593)	4.088*** (.529)
R <sup>2</sup>	.8637	.8590	.9012
N	871	871	871

Standard errors in parentheses.

\*\*\*Significant at the .01 level;

\*\*Significant at the .05 level;

\*Significant at the .10 level (two-tailed tests).

## APPENDIX VI-3

EE DATA FOR 1979

Independent Variables	Number FT Sworn Police		Number FT Non-Sworn Employees	Total Expense
	Including Wage	Excluding Wage		
1) ARB	.259*** (.022)	.259*** (.023)	-.393*** (.052)	-.010 (.019)
2) CBA	-.057** (.022)	-.117*** (.022)	.076 (.051)	.065*** (.018)
3) Population	.731*** (.031)	.748*** (.032)	.479*** (.093)	.018 (.033)
4) Number Sworn	—	—	.442*** (.082)	.807*** (.029)
5) Number Non-Sworn	.084*** (.016)	.068*** (.016)	—	.127*** (.013)
6) Avg. Sworn Salary	-.399*** (.056)	—	.740*** (.130)	.641*** (.047)
7) Avg. Non-Sworn Salary	-.064* (.037)	—	-.195** (.085)	.109*** (.030)
8) Density	.055*** (.014)	.036*** (.014)	-.027 (.031)	.016 (.011)
9) Income	.306*** (.048)	.111** (.044)	.061 (.113)	.071* (.040)
10) Percent Minority	.007*** (.001)	.009*** (.001)	.002 (.002)	.001 (.001)
<u>Clearances</u>				
11) Murder	-.001 (.004)	-.001 (.004)	.001 (.010)	-.006* (.003)
12) Manslaughter	.006 (.004)	.006 (.004)	-.006 (.009)	-.001 (.003)
13) Rape	-.001 (.007)	-.008 (.007)	-.005 (.015)	-.011** (.005)
14) Robbery	.040*** (.015)	.035** (.016)	.026 (.035)	-.001 (.012)
15) Assault	-.010 (.024)	-.020 (.025)	.088 (.055)	.027 (.019)

APPENDIX VI-3 (cont.)

EE DATA FOR 1979

Independent Variables	Number FT Sworn Police		Number FT Non-Sworn Employees	Total Expense
	Including Wage	Excluding Wage		
16) Burglary	-.050 (.035)	-.068* (.037)	.114 (.081)	0.058** (.028)
17) Larceny	.042 (.036)	.061 (.037)	.028 (.082)	.001 (.029)
18) Auto Theft	.131*** (.020)	.123*** (.021)	-.139*** (.048)	.034** (.017)
<u>Crimes</u>				
19) Murder	.006 (.004)	.007* (.004)	-.000 (.001)	.002 (.003)
20) Manslaughter	-.005 (.004)	-.005 (.004)	.000 (.001)	-.000 (.003)
21) Rape	-.004 (.004)	-.003 (.004)	-.001 (.001)	-.006* (.003)
22) Robbery	-.001 (.006)	-.003 (.007)	.003 (.014)	.000 (.005)
23) Assault	.043** (.020)	.045** (.021)	-.037 (.046)	-.027 (.016)
24) Burglary	.061*** (.016)	.069*** (.017)	-.053 (.038)	.013 (.013)
25) Larceny	-.053*** (.019)	-.049** (.020)	.152*** (.044)	-.001 (.016)
26) Auto Theft	-.004 (.008)	-.007 (.008)	.006 (.018)	-.002 (.006)
Constant	-4.551*** (.480)	-6.113 (.464)	-9.918*** (.011)	4.339*** (.410)
R <sup>2</sup>	.9490	.9438	.8108	.9670
N	776	776	776	776

Standard errors in parentheses.

\*\*\*Significant at the .01 level;

\*\*Significant at the .05 level;

\*Significant at the .10 level (two-tailed tests).

APPENDIX VI-4

OUTPUT EQUATIONS FOR 1980

Independent Variables	Murder		Manslaughter		Rape		Robbery	
	Crime Rate	Clearance Rate	Crime Rate	Clearance Rate	Crime Rate	Clearance Rate	Crime Rate	Clearance Rate
CEA	.158** (.064)	.188 (.249)	-.067 (.072)	-.010 (.874)	.282*** (.070)	.137 (.205)	.502*** (.069)	-.098 (.115)
ARB	-.217*** (.070)	-.170 (.292)	.098 (.080)	1.57 (.983)	-.485*** (.077)	-.233 (.226)	-.595*** (.076)	-.017 (.126)
Number of Police	.106 (.100)	-.248 (.419)	-.176 (.114)	.569 (1.45)	.116 (.110)	.159 (.324)	.469*** (.109)	-.324* (.180)
Population	.175*** (.040)	.260* (.145)	-.632*** (.045)	.137 (.428)	.333*** (.044)	.266** (.126)	.376*** (.043)	.044 (.072)
Income	-.463*** (.164)	-3.81*** (.711)	-.070 (.186)	-.421 (.310)	-.853*** (.180)	-1.01* (.529)	.263 (.178)	-.247 (.293)
% Pop. > 65	.072*** (.00)	.072** (.030)	-.004 (.009)	-.288 (.195)	-.004 (.008)	.016 (.024)	-.011 (.008)	.012 (.014)
% Minority	.038 (.003)	.009 (.011)	-.002 (.033)	-.029 (.040)	-.018*** (.003)	.007 (.009)	.023*** (.003)	-.003 (.005)
% Pop. > 18	-.012** (.006)	-.002 (.028)	.008 (.007)	.095 (.175)	.015** (.007)	-.021 (.020)	.014** (.007)	.003 (.011)
% Pop. > 25 w/H.S. Educ.	-.006 (.033)	.021 (.014)	.004 (.004)	-.028 (.061)	.003 (.004)	.009 (.011)	-.015*** (.004)	-.007 (.006)
Density	.007 (.009)	-.222 (.17)	.009 (.009)	-.266 (.581)	-.020** (.009)	-.210 (.132)	.038*** (.009)	-.059 (.073)
Region 1	.341*** (.094)	-.701* (.369)	-.187* (.107)	.604 (1.24)	.184* (.103)	-.320 (.301)	.087 (.102)	-.105 (.168)
Region 2	.321*** (.021)	-.615* (.363)	-.006 (.104)	.495 (1.63)	.092 (.100)	-.397 (.290)	.055 (.099)	.015 (.164)
Region 3	.163* (.096)	-.247 (.367)	-.011 (.109)	.991 (1.17)	.081 (.106)	-.223 (.305)	.096 (.105)	.078 (.173)
Region 4	.064 (.109)	.255 (.409)	.135 (.123)	.388 (1.43)	.111 (.119)	-.007 (.342)	.048 (.118)	.191 (.194)
Region 5	.097 (.109)	.089 (.410)	-.021 (.124)	.575 (1.24)	.054 (.120)	.087 (.344)	.198* (.119)	.186 (.197)
Region 6	-.224* (.115)	.018 (.414)	.422*** (.131)	1.58 (1.75)	.087 (.126)	-.313 (.361)	.108 (.125)	.111 (.206)
Region 7	-.223 (.140)	-.715 (.492)	.383** (.159)	1.39 (1.75)	.200 (.154)	-.261 (.439)	.099 (.152)	-.431* (.251)
Number Police Civilians	-.010 (.047)	-.134 (.193)	.024 (.053)	-.388 (.787)	.252*** (.052)	-.041 (.150)	.283*** (.051)	.035 (.084)
Constant	-6.54*** (1.83)	26.9*** (7.54)	-3.66* (2.08)	1.93 (27.5)	-2.95 (2.00)	7.88 (5.80)	-8.30*** (1.99)	-.874 (3.25)
R <sup>2</sup>	.3459	.1375	.3268	.1496	.4122	.0434	.5629	.0320
N	627	492	627	492	627	604	627	627

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APPENDIX VI-4 (cont.)  
OUTPUT EQUATIONS FOR 1980

Independent Variables	Assault		Burglary		Larceny		Auto Theft	
	Crime Rate	Clearance Rate	Crime Rate	Clearance Rate	Crime Rate	Clearance Rate	Crime Rate	Clearance Rate
	Rate	Rate	Rate	Rate	Rate	Rate	Rate	Rate
CBA	.107 (.069)	.033 (.070)	.165*** (.039)	.002 (.069)	.052 (.038)	-.050 (.063)	.244*** (.048)	-.053 (.105)
ARB	-.193** (.077)	-.068 (.76)	-.174*** (.043)	-.074 (.075)	-.032 (.042)	.004 (.069)	-.173*** (.055)	-.066 (.115)
Number of Police	.427*** (.109)	-.253 (.109)	.209*** (.062)	-.253 (.107)	.281*** (.060)	-.277** (.098)	.486*** (.075)	-.432*** (.163)
Population	.080* (.043)	-.021 (.043)	.122*** (.025)	-.032 (.043)	.062*** (.024)	-.003 (.039)	.172*** (.030)	-.066 (.065)
Income	-.712*** (.179)	.120 (.177)	-.227** (.101)	-.204 (.174)	-.447*** (.098)	-.175 (.160)	.245** (.123)	-.903*** (.266)
% Pop. > 65	.004 (.008)	.010 (.008)	.008 (.005)	.017** (.008)	.003 (.005)	.012 (.008)	-.020*** (.006)	.035*** (.013)
% Minority	.011*** (.003)	.003 (.003)	.101*** (.002)	.003 (.003)	.003* (.002)	.003 (.003)	-.003 (.002)	.011** (.005)
% Pop. > 18	-.009 (.007)	-.007 (.007)	-.007 (.039)	-.006 (.007)	.002 (.004)	-.015** (.006)	.006 (.005)	-.013 (.010)
% Pop. > 25 w/H.S. Educ.	-.002 (.004)	.002 (.036)	-.002 (.002)	.002 (.004)	.006*** (.002)	.006** (.003)	-.013*** (.003)	.022*** (.005)
Density	.002 (.009)	.025 (.044)	-.001 (.005)	-.043 (.044)	-.024*** (.005)	.056 (.040)	.028*** (.007)	-.199*** (.067)
Region 1	-.006 (.103)	-.144 (.132)	.077 (.058)	-.059 (.100)	.082 (.056)	-.139 (.092)	.047 (.071)	-.161 (.153)
Region 2	.011 (.099)	-.170* (.099)	.025 (.057)	-.085 (.098)	.010 (.055)	-.039 (.090)	.019 (.069)	-.175 (.149)
Region 3	.017 (.105)	-.062 (.105)	.029 (.060)	.012 (.103)	.081 (.058)	-.032 (.095)	-.016 (.072)	-.110 (.157)
Region 4	.140 (.118)	-.104 (.118)	.073 (.067)	.041 (.116)	.120* (.065)	.019 (.106)	.079 (.082)	-.042 (.177)
Region 5	.044 (.119)	-.028 (.119)	.059 (.067)	.053 (.117)	.091 (.065)	.076 (.108)	.201** (.082)	-.057 (.179)
Region 6	.118 (.125)	-.034 (.125)	.077 (.071)	-.002 (.123)	.051 (.069)	.106 (.113)	-.009 (.086)	.026 (.188)
Region 7	.109 (.153)	-.468*** (.152)	.014 (.087)	-.327** (.149)	.078 (.084)	-.482*** (.137)	.060 (.105)	-.649*** (.228)
Number Police Civilians	.304 (.051)	-.021 (.051)	.183*** (.029)	.018 (.050)	.200*** (.028)	.101** (.046)	.106*** (.035)	.202*** (.077)
Constant	6.05*** (1.99)	-2.37 (1.97)	-.738 (1.13)	-.276 (1.94)	2.78** (1.09)	-.475 (1.78)	-5.16*** (1.38)	6.32** (2.96)
R <sup>2</sup>	.2859	.0296	.3913	.0350	.2450	.0873	.3500	.1162
N	627	626	627	627	627	627	627	627

Standard errors in parentheses.

\*\*\*Significant at the .01 level; \*\*Significant at the .05 level; \*Significant at the .10 level (two-tailed tests).

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