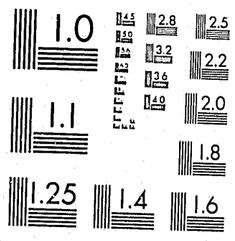

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EXECUTIVE SUMMARY

THE IMPACT OF COLLECTIVE BARGAINING AND INTEREST ARBITRATION ON POLICING

by

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Executive Summary of the
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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

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.

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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 asistance 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.

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

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

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

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.

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

[a] A lower court decision overturning Connecticut's law was dismissed by the state supreme court on non-constitutional grounds.

[b] E.D. means equally divided, which has the effect of upholding the lower court decision.

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

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 signficant 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.

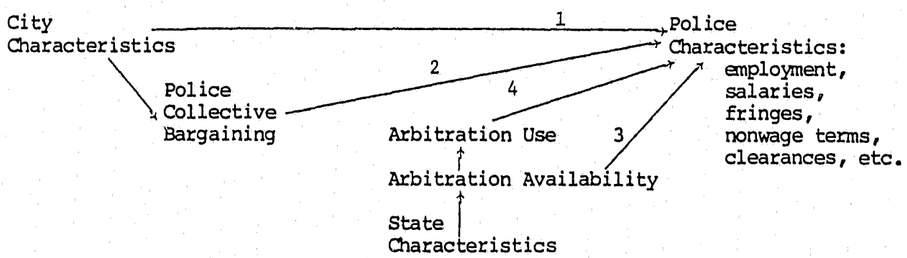
Bargaining vs. arbitration. In our analyses, one key objective has been to differentiate between any impacts that police unionism or collective bargaining has had versus any impacts that arbitration has had. Some previous research on police unionism (usually dealing with the unions' impacts on wages; 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 explictly 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[a]	1,077
Number of cities in our sample in 1981[b]	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[c]	1,963
Number of contract-years included[c]	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.

[a]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).

[b]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.

[c]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[a]	Number (Percent) of Cities Which Bargain With Police	Number of Cities in Arbitration States[b]	Bargaining Cities Covered By Arbitration Law		Number (Percent) of Covered Cities That Used Arbitration[d]
				State	Local[c]	
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)

Source: Mail survey and field visits by the researchers.

[a]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.

[b]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.

[c]The cities in this column are located in nonarbitration states.

[d]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[a]	Number of Bargaining Cities[b]	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[a]	Number of Bargaining Cities[b]	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.

[a] 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.

[b] 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 <u>all</u> 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.

<u>Levelling effects</u>. 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.

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