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MEASURING THE EFFECT OF ARBITRATION ON WAGE LEVELS: THE CASE OF POLICE OFFICERS

Orley Ashenfelter
Dean Hyslop

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ABSTRACT

In this paper we provide an empirical evaluation of the effect that the provision of an arbitration statute has on the wage levels of police officers. We analyze the effect of arbitration on wages by comparing wage levels across political jurisdictions and over time using a sample of states. Two complementary data sources are used: panel data on state level wages of police officers, and individual level data on police officers from Decennial Censuses. The empirical results from both data sets are remarkably consistent and provide no robust evidence that the presence of arbitration statues has a consistent effect on overall wage levels. On average, the effect of arbitration is approximately zero, although there is substantial heterogeneity in the estimated effects across states.

Orley Ashenfelter
Industrial Relations Section
Firestone Library
Princeton University
Princeton, NJ 08544
and NBER

Dean Hyslop
Department of Economics
Bunche Hall
UCLA
Los Angeles, CA 90095
dhyslop@paua.sscnet.ucla.edu

Introduction

In the public sector, where strikes are often legally prohibited, the advantages of binding arbitration for the day to day functioning of the industrial relations system are widely acknowledged. (See, especially, Lester (1984).) At the same time, however, there has been continuing concern over the possibility that the presence of an arbitration statute may distort and increase wage levels artificially above what would otherwise prevail. In this paper we provide an empirical evaluation of the effect that the provision of an arbitration statute has on the wage levels of police officers. Since police officers are a very homogeneous class of workers, and since arbitration statues are both present and absent in roughly the same proportion among the non-Southern states that we study, this group provides a fruitful evaluation design.

Although there is a considerable literature attempting to measure the effect of collective bargaining in the public sector on wages[1], attempts to measure the effect of the arbitration institution on wages face a major conceptual problem. In the standard model of the arbitration process, due to Stevens (1966) and Farber and Katz (1979), negotiated and arbitrated wages are not expected to differ because negotiations take place "in the shadow of arbitration." Thus, the comparison of arbitrated and negotiated wages provides no information on the overall impact on wage levels of the workers covered by the arbitration statute.

Our approach to solving this problem is to compare overall wage levels across

[1]A comprehensive, but dated survey is Freeman (1986).

political jurisdictions and over time in order to isolate the impact of the arbitration statute. Approximately one third of states introduced binding arbitration to resolve labor relations disputes involving police officers at various dates over the past 30 years. Thus, it is possible to provide both across-state and within-state estimates of the effect of arbitration on wage levels. Our analysis focuses on states in three regions: the Northeast, the Midwest and the West, and uses data from two complementary sources. First, we have constructed a panel data set on state level wages of police protection workers in these states. Approximately half of the states in these regions ultimately adopted arbitration statutes, while the remaining states were selected for comparison. Second, we have constructed an individual level data set of police officers from the 1970, 1980 and 1990 Decennial Censuses for these same states. This latter data set permits us to control for possible heterogeneous changes in police demographics across states and over time.

The empirical results provide no strong evidence that the presence of arbitration statutes has had any effect on overall wage levels. There is some weak evidence that wage levels may actually be lower as a result of arbitration statutes, although there is also some evidence of heterogeneity in the estimated effects. These results may come as a surprise to some, but they are by no means inconsistent with plausible theories of the role of arbitration in the wage determination process.

The first section of the paper outlines the motivation for the empirical tests and the design of the data collection. The data and results are described in the second and

third sections of the paper, and these are followed by a brief conclusion and discussion of the limitations of the results.

I. Motivating the Measurement Method

Arbitration systems for public sector workers, especially for police officers and firefighters, are typically implemented because these workers are forbidden the right to strike. Once collective bargaining has been permitted, arbitration systems are a natural procedure for resolving what might otherwise be very protracted negotiations.

The now standard model of arbitrator behavior predicts that to survive arbitrators must attempt to make decisions similar to those that would have been made by other arbitrators had they been in the same position.[2] This is the only strategy that will guarantee that the parties will continually select them. This implies that negotiating parties can predict the central tendency of any award that is likely to be made by an arbitrator. As a result, if the parties are risk neutral, they will rationally propose settlements that are distributed around this central tendency. The result is that negotiated settlements mimic the central tendency of arbitrator awards.

This argument is quite general and independent of the type of arbitration institution adopted. If the parties are not risk neutral, however, then the negotiated settlements need not equal the central tendency of arbitration awards. The difference

[2]That is, arbitrators are said to be statistically exchangeable (or random). This assumption forms the basis of the models used by Farber and Katz (1979) and Farber (1980) and others. The logical foundation for this assumption, and a summary of the evidence to support it, is contained in Ashenfelter (1987).

between arbitrated and negotiated settlements will reflect any differences in the risk preferences of the party. In general, the more risk averse party is disadvantaged and will accept a lower negotiated settlement in order to avoid the risk associated with the arbitration system.[3] According to this theory, the negotiated settlements do not provide a useful comparison group for determining the impact of arbitration on wages because of the spillover between arbitrated and negotiated settlements. The same argument is equally applicable to the use of non-union workers, who would otherwise be covered by the arbitration statute, as a basis for comparison. The mere presence of an arbitration statute creates incentives for the wages of all workers to be treated similarly.[4]

There is some good evidence that, as predicted, negotiated and arbitrated wages do not differ significantly. In careful studies both Bloom (1981) and Currie (1989) conclude that this is the case.

The interesting question that this analysis raises, but does not answer, is, what determines the central tendency of arbitrator awards? One possibility is that arbitrators look to previously negotiated settlements. If this is uniformly the case, then arbitrated and negotiated settlements are not only equal, they must remain constant at the same level! It is certainly possible that this is the case with some arbitration systems in some periods, but it seems unlikely that it could exist indefinitely. Since it is unclear what determines the central tendency of arbitrator awards, and since the presence of long-

[3]See especially Farber (1980).

[4]This is also the argument made by Freeman, Ichniowski, and Lauber (1985) with respect to collective bargaining coverage in the public sector.

term implicit contracts is likely to introduce considerable specificity in the employment relationship, it is essentially an empirical question whether the presence of an arbitration statute affects the overall wage level.

This discussion makes it clear that to evaluate the effect of an arbitration law on the wage level it is necessary to use a comparison group that falls outside the coverage of the arbitration statute. In this study we use two different comparison groups for this purpose. We use individual states as controls for themselves (by comparing pre and post-arbitration wages) and we use adjacent states as controls (comparing states with and without arbitration). In our sample we have selected a total of 33 states from the Northeast, the Midwest, and the West. In addition to the geographic variation across the sample, there is also variation in whether arbitration was adopted by the sample states and, for those states that did adopt arbitration, the date at which arbitration became effective. In particular, during the sample period, 18 of the selected states introduced binding arbitration for police officers at various dates between 1969 and 1988. In addition, there were a variety of different types of arbitration adopted across the sample. (Lester (1984) provides a careful study of nine of the jurisdictions in the present sample.)

Police officers in all these states form very homogeneous groups by comparison with virtually any other occupation. In addition, the police officers in all these states tend to belong to specialized unions of police officers or equivalent fraternal orders. Strikes by police officers are illegal in all states.

II. Data

There are a variety of sources for data on police officer compensation. We use data from two complementary sources. First, we use a long time series of the average payroll cost per full-time equivalent employee of local police protection workers, which comes from various issues of Public Employment, compiled by the U.S. Department of Commerce. These data cover police officers and other police protection employees. These are the workers who are generally directly covered by the arbitration statute and they comprise the vast majority of police officers and support personnel in the states covered.

Second, we have constructed a micro data sample of police officers' annual earnings and average hourly wages from the 1970, 1980 and 1990 Decennial Censuses for the same group of states. For the 1970 census, we use data from both the "15%, One-in-One-Hundred" and the "5%, One-in-One-Hundred" public use micro samples (PUMS), giving approximately a 2 percent random sample; for the 1980 and 1990 censuses, we use data from the 5-percent PUMS. The sample includes police and detectives, supervisors, and sheriffs and marshals who are employed by state or local administrations. The sample was also restricted to individuals aged 25-50, who worked at least 40 weeks the previous year, and whose annual earnings were at least $5000 (in 1990 dollars) and below the topcode limit in effect in each year. The wage and earnings information cover the previous calendar year (i.e. 1969, 1979 and 1989).

The average hourly wage measure we use was computed as annual earnings divided by the number of weeks worked multiplied by usual hours per week.[5]

We have taken the date at which coverage became effective for the arbitration statute from Valletta and Freeman (1988).[6] The list of states, together with the effective adoption periods, and the number of observations in each state-year cell from the three censuses are indicated in Table 1. There is an ambiguity in the categorization of New York because arbitration was enacted for the City of New York two years in advance of the legislation for the State of New York. We have adopted the later date, but using the earlier date in the empirical analysis has no material effect on the results.

The characteristics of the census samples by year and arbitration provisions are described in an appendix, in Table A1. The demographic characteristics of the samples appear reasonably stable over time, although there has been a significant increase in the education level of police officers and the fraction female. Wages and annual earnings have increased over time in states both with and without arbitration provisions. The increase appears to have been greater for states with arbitration, but this may be due to the changing composition of states with arbitration and heterogeneity across states.

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[5] As the 1970 census did not include the number of usual weekly hours last year, we computed the average hourly wage for 1969 using the number of hours worked last week.

[6] Valletta and Freeman categorize the availability of arbitration as follows: "no provision", "specifically prohibited", "voluntary" (both parties must consent), "discretionary" (administrative agency may initiate, either unilaterally or upon the request of either party", and "mandatory" (required by statute). We treat the latter two categories as representing binding arbitration.

III. Empirical Results

State Level Analysis

We begin our analysis by providing some basic facts about average monthly compensation of police officers in the states under consideration in table 1. Table 1 divides the states into three regions -- the Northeast, Midwest, and West -- and, for each region, orders the states by the date at which an Arbitration statute came into effect. It is apparent from the table that there is substantial variation across states in both the level of compensation in 1992 and the growth rates during the period 1961-92. For example, compensation levels varied from $1801 in West Virginia to $3556 in California, while the average growth in real compensation ranged from 0.66 percent in Indiana to 2.52 percent in New York. There is some systematic regional variation in compensation levels and growth rates, which are generally higher in the Northeast and West than the Midwest. However, there is also considerable variation within regions. Also, there is no apparent relationship between the growth rates and the date of adoption of arbitration.

A very straightforward initial analysis of the data is contained in Figure 1 and Table 2. For each region, we have constructed an unweighted index of the (logarithm of) wages for states that eventually adopted arbitration and subtracted from it a similar index of the wages for states that never adopted arbitration. The time series of these

proportionate wage differences are graphed in Figure 1, while Table 2 presents the means across alternative "pre-" and "post-" arbitration periods. The pre-arbitration period is the period before any state in the region adopted arbitration; similarly, the post-arbitration periods are defined as the period from when the last state in the region adopted an arbitration statute. For the Northeast the pre- and post- arbitration periods are 1961-68 and 1979-92 respectively,[7] and the corresponding periods for the Midwest and West are 1961-69 and 1985-92, and 1961-73 and 1988-92. The dates defining these periods are labeled on Figure 1.

The first column of Table 2 contains the mean of the wage difference index for each of the three regions for the pre-arbitration periods defined above. As is apparent from Figure 1 and Table 2, wages of Police Officers in the arbitration states in the Northeast and Midwest were on average 15 and 20 percent higher than in the non-arbitration states over this period. Wages in the Western states, on the other hand, were 5 percent lower in the states which later adopted arbitration. This implies that a simple cross-section comparison would provide a very misleading measure of the effect of arbitration on wage levels. The second column contains the corresponding average wage difference indexes during the post-arbitration periods. The pattern of wage differences is similar to that observed in the pre-arbitration periods: wages are comparatively higher in the Northeast and Midwest "Arbitration" states, and lower in the

[7] Massachusetts repealed its compulsory arbitration statute in 1981. The analysis presented here ignores this change, but the results are qualitatively unaffected by this factor -- for example, omitting Massachusetts from the analysis does not change the qualitative nature of the results.

West "Arbitration" states.

A very simple estimate of the effect of the arbitration statutes on wages is presented in the third column of Table 2. This estimate is computed by comparing the wage differences in the pre- and post-arbitration periods for each region. The "difference-in-differences" point estimates of the effect of arbitration vary across the three regions: -6 percent in the Northeast, 4.8 percent in the Midwest and 1.9 percent in the West. The estimates for the Northeast and Midwest states are each statistically different from zero, while the estimate for the Western states is not. Although informative and easy to present, the simple comparisons in Table 2 and Figure 1 do not take advantage of all the data available. For example, several other estimators of the effect of arbitration could be constructed using different groups of comparison states and/or comparison periods.

We now turn to a series of regressions that exploit the data more fully. The results of this analysis are presented in Table 3. We first present results using all the states in the sample in row (1) of the table. Column (1) contains the estimated arbitration effect on the logarithm of wages from a regression that controls for individual year effects and state effects, and that allows arbitration to have a constant proportionate effect on wage levels in each state. This specification finds that arbitration reduces wages by a statistically insignificant 0.5 percent. One possibility is that arbitration may directly affect wage growth. To examine this conjecture, the specification reported in column (2) also includes an arbitration trend term, which grows

linearly from the effective date of arbitration. The results for this specification are very close to those in column (1): the estimated effect of arbitration on the wage level is 0.5 percent (and statistically insignificant), and the effect on wage growth is almost zero.

Columns (3) and (4) repeat the analysis, but allow state-specific time trends. The results of these specifications again find no statistically significant constant effect of arbitration on the level of Police wages. However, the results in column (4) suggest that arbitration reduces wage growth by a statistically significant -0.35 percent per year.

In order to evaluate the robustness of these results, we repeat the analysis for each region separately in rows (3), (4), and (5). The results for states in the Northeast in row (2) imply two consistent findings: first, arbitration is estimated to have a statistically significant negative effect on wages of between 2.6 and 4.3 percent; and second, there is no evidence that arbitration affects the rate of wage growth in these states. In contrast, for the Midwestern states, the results in columns (1) -- (3) show a significant positive effect of arbitration on the level of wages of between 2 and 3.5 percent. However, when state-specific time trends in wages are allowed (column (4)), the estimates find arbitration has a -0.6 percent effect on wage growth. Also, the estimates for the Western states find no evidence of statistically significant arbitration effects on either the level or growth of Police wages.

Finally, we also allowed the effect of arbitration to vary across states in specifications analogous to those presented in Table 3, and tested the joint significance and equality of these effects. With the exception of the Western subsample of states,

the test statistics indicate that the arbitration effects are jointly significant and that they are not equal. This latter result serves to emphasize that, although the average effect of arbitration on wage levels is small and negative, this masks considerable heterogeneity in the estimated effects by state.

Census Analysis

We now turn to the analysis using micro data from the decennial censuses. Table 4 contains estimates of the effect of arbitration on the (logarithm of) annual earnings and the (logarithm of) average hourly wages of police officers using a variety of specifications and samples. The results in columns (1) -- (4) pertain to annual earnings, while those in columns (5) -- (8) pertain to hourly wages. In order to control for possible common unobservable factors across individuals which might bias the precision of the results, all of the specifications allows for a common random component of error across observations in each state-year cell.

The first row in Table 3 presents the results using the full sample of states from each of the three regions we consider. The model in column (1) includes just an arbitration statute indicator variable, which varies across states and over time. This specification suggests that the presence of an arbitration statute has a statistically significant positive effect on police salaries, and raises salaries by 8 percent. The model in column (2) includes observable individual specific demographic characteristics in the regression. The result is to reduce the effect of arbitration to 5 percent, which is

still statistically significant at conventional levels. In column (3), state dummy variables are included in the model to control for heterogeneity across states in salaries. In this specification, the estimated effect of arbitration is negative (-1.7 percent), but not statistically different from zero. The final specification, presented in column (4), includes year dummy variables to control for common economy-wide factors that may affect salaries. The estimated effect of arbitration is again negative (-3.8 percent), but not statistically different from zero. Columns (5) -- (8) present results for analogous specifications for the logarithm of average hourly wages. The results are qualitatively the same as for earnings: in the absence of state and year controls, the estimated effect of arbitration is positive and statistically significant; however, there is no statistical evidence of an arbitration effect once we control for state and year effects.

In order to evaluate whether there are any regional differences in the effect of arbitration on police wages and earnings, we repeat the above analysis separately for each of the three regions. The results are presented in rows (2), (3) and (4) of Table 3 for states in the Northeast, the Midwest and the West respectively. The results are broadly in line with those for the full sample in row (1). The single exception to the finding that arbitration has no effect on police earnings and wages once state and year effects are controlled for is in the Midwest, where the effect of arbitration on wages and earnings is estimated to be 3-4 percent.

Finally we consider the effect of arbitration statutes on the variability in wages and salaries. One plausible hypothesis, if arbitrators tend to propose settlements

around the central tendency of possible outcomes, is that arbitration reduces the dispersion in outcomes. To evaluate this hypothesis, we computed the state and year specific standard deviations of the logarithm of individual annual earnings and hourly wages, and compared the averages in samples with and without arbitration. We have computed both the state-year specific standard deviations using the raw data, and also the standard deviations for regression adjusted residuals from the regression specifications in Table 4, rows (4) and (8), for annual earnings and hourly wages respectively. The results are contained in Table 5. Row (1) presents the results of this exercise using the raw data. The estimated effect of arbitration of the dispersion of earnings is negative but not significantly different from zero. The effect on the dispersion in hourly wages is also negative (-0.012) and has a t-value of 1.5, implying some weak statistical evidence that arbitration does reduce the variability in wage outcomes. Row (2) presents the results for the standard deviations of the regression-adjusted residuals: the results are qualitatively the same as those in row (1). All of these results suggest that any effect of an arbitration statute on the dispersion in wage rates is very small.

IV. Conclusion

It is well known that the presence of an arbitration system influences negotiated settlements because they are negotiated in the shadow of the arbitrated decisions. This is an extreme example of a spillover effect and it makes the measurement of the

effect of the arbitration system on wage levels very difficult. Comparisons of arbitrated and negotiated outcomes provide no evidence on the key question, so other comparisons must be used.

In this paper we have used comparisons of political jurisdictions with each other and with themselves at different points in time to estimate the effect of arbitration statutes on wage levels. Comparisons using both state-level time-series data and micro-level data from the Census give remarkably similar results. The estimates tell a complicated story. On average, there is no strong evidence that arbitration tends to raise wage levels, but there is considerable evidence that this average effect masks considerable heterogeneity in effects across states. There is some weak evidence that arbitration may reduce the variability of wages. Although there is some evidence that arbitration lead to an increase in police wages in the Midwest, accounting for differential time trends in wage growth across states reverses this result. Since the estimated effects do appear to differ by state, it is possible that this reflects the way that the parties have interacted in the manipulation of the central tendency of arbitral awards. Lester (1984) provides many anecdotes indicating that the parties strive to manipulate these systems through the internal comparisons they engender. The evidence in this paper suggests that the average effect on wage levels of this behavior is small.

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Figure 1a: Northeastern States

STATA™

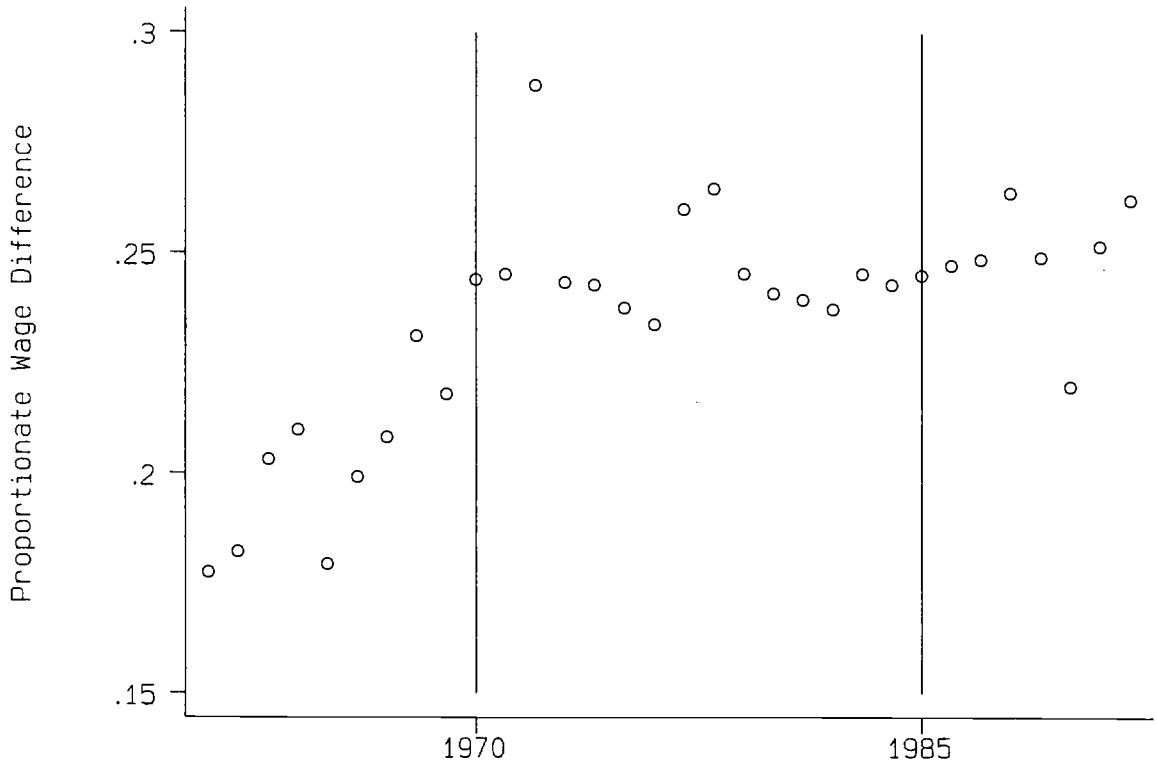

Figure 1b: Midwestern States

STATA™

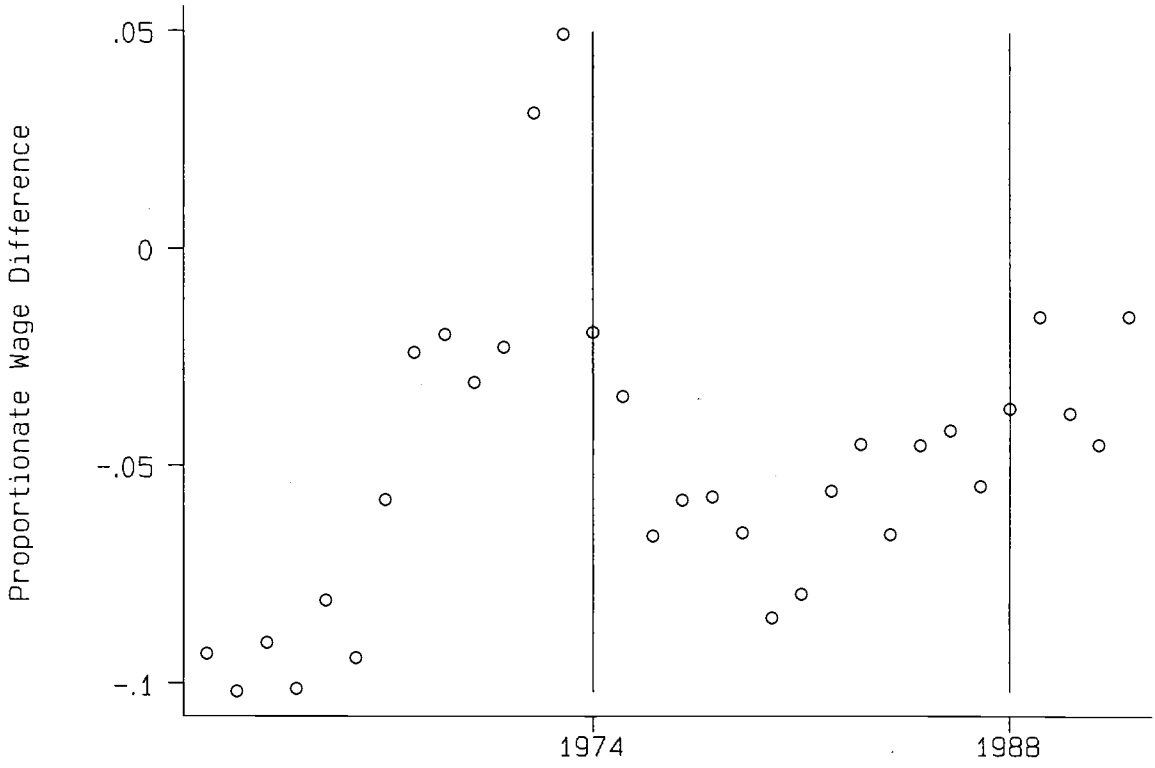

Figure 1c: Western States

Table 1: Police Protection Services Compensation, And Real Growth Rates

	Year Arbitration Adopted	Average Monthly Compensation, 1992 ($1990)	Annual Real Percentage Growth, 1961-92
		Northeastern States	
Pennsylvania	1969	2651	1.45
Rhode Island	1969	2509	1.48
Maine	1970	2129	1.33
Massachusetts	1975[(a)]	3007	1.64
Connecticut	1976	3122	1.78
New Jersey	1978	3048	1.40
New York	1978	3488	2.52
District of Columbia	1979	3044	1.04
Delaware	Never	2631	1.39
New Hampshire	Never	2457	1.54
Maryland	Never	2764	1.59
Vermont	Never	2446	1.72
		Midwestern States	
Michigan	1970	2772	1.05
Nebraska	1970	2257	1.44
Wisconsin	1973	2485	0.93
Minnesota	1974	2735	1.28
Iowa	1976	2323	1.37
Illinois	1985	2765	1.10
Ohio	1985	2432	0.92
Indiana	Never	1966	0.66
Kansas	Never	2119	1.24
Missouri	Never	2102	0.90
North Dakota	Never	1880	0.72
South Dakota	Never	1834	0.95
West Virginia	Never	1801	0.87
		Western States	
Oregon	1974	2760	1.31
Washington	1974	2965	1.50
Nevada	1988	3095	1.75
Alaska	Never	3626	0.95
Arizona	Never	2716	1.45
California	Never	3556	1.41
Hawaii	Never	3210	1.37
Idaho	Never	2098	1.22

Notes: Arbitration adoption dates obtained from NBER *Public Sector Bargaining Law* data set (see Valletta and Freeman, 1988), and updated from US Department of Labor, *Monthly Labor Review*

(various). Dates refer to statutes that require mandatory arbitration, or discretionary arbitration initiated by either party or the administrative agency.
[a] Compulsory Arbitration in Massachusetts was repealed in 1981.

Table 2: The Proportionate Wage Difference Between States That Did and Did Not Eventually Adopt Arbitration for Police Officers

	Average Difference Pre-Adoption	Average Difference Post-Adoption	Difference-in-Differences
(1) Northeastern States	0.145 (0.008)	0.085 (0.008)	-0.060 (0.012)
(2) Midwestern States	0.201 (0.006)	0.249 (0.005)	0.048 (0.008)
(3) Western States	-0.049 (0.014)	-0.030 (0.006)	0.019 (0.015)

Notes: Standard errors are in parentheses. The Pre- and Post-adoption periods for the Northeast are 1961-68 and 1979-92 respectively; for the Midwest, 1961-1969 and 1985-1992; and for the West, 1961-1973 and 1988-1992.

Table 3: The Effect of Arbitration on the Natural Logarithm of Average Monthly Compensation of Police Officers

	(1)	(2)	(3)	(4)
(1) Full Sample of States:				
Level effect	-0.005	-0.005	-0.013	-0.013
	(0.006)	(0.008)	(0.008)	(0.008)
Growth effect (/100)	-.---	-0.008	-.---	-0.345
		(0.06)		(0.15)
(2) Northeastern States:				
Level effect	-0.044	-0.036	-0.026	-0.026
	(0.010)	(0.012)	(0.013)	(0.013)
Growth effect (/100)	-.---	-0.110	-.---	0.004
		(0.09)		(0.24)
(3) Midwestern States:				
Level effect	0.035	0.032	0.020	0.015
	(0.008)	(0.010)	(0.010)	(0.010)
Growth effect (/100)	-.---	0.039	-.---	-0.582
		(0.07)		(0.21)
(4) Western States:				
Level effect	0.018	0.014	0.002	0.005
	(0.016)	(0.021)	(0.002)	(0.022)
Growth effect (/100)	-.---	0.056	-.---	-0.410
		(0.18)		(0.36)
State Controls	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes
State-specific trends	No	No	Yes	Yes

Notes: Standard errors are in parentheses.

Table 4: The Effect of Arbitration on Annual Earnings and Average Hourly Wages of Police Officers

	Dependent Variable							
	Natural Logarithm of Annual Earnings				Natural Logarithm of Hourly Wages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Proportionate Effect of Arbitration							
(1) Full Sample of States	0.079	0.051	-0.017	-0.038	0.105	0.078	0.021	-0.027
	(.038)	(.037)	(.024)	(.031)	(.042)	(.040)	(.025)	(.032)
(2) Northeastern States	0.066	0.025	0.004	-0.037	0.088	0.051	0.042	-0.035
	(.048)	(.040)	(.036)	(.045)	(.058)	(.049)	(.031)	(.041)
(3) Midwestern States	0.139	0.104	-0.057	0.036	0.180	0.147	-0.018	0.044
	(.052)	(.043)	(.013)	(.011)	(.057)	(.047)	(.018)	(.012)
(4) Western States	0.001	-0.017	0.019	-0.017	0.017	-0.001	0.038	-0.022
	(.098)	(.093)	(.066)	(.082)	(.107)	(.099)	(.070)	(.086)
Demographic Covariates	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year Controls	No	No	No	Yes	No	No	No	Yes

Notes: Standard errors are in parentheses. All specifications control for random cluster effects across State-Year cells. Demographic controls include age, age squared, years of schooling, and indicators for sex, race, and marital status.

Table 5: The Effect of Arbitration on the Variability of Annual Earnings and Hourly Wages

	Dependent Variable	
	Standard Deviation Log(Annual Earnings)	**Standard Deviation Log(Hourly Wages)**
(1) Raw Data	-0.008	-0.012
	(.009)	(.008)
Mean of Dependent Variable	0.307	0.330
(2) Regression-adjusted Residuals	-0.002	-0.009
	(.008)	(.007)
Mean of Dependent Variable	0.284	0.311

Notes: Standard errors are in parentheses. The data are the standard deviations of the 99 state-year cells. The differences are weighted by the state-year cell sizes. In row (2) the standard deviations pertain to regression-adjusted residuals from the specifications in Table 4, columns (4) and (8) for earnings and wages respectively.