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ABSTRACT

Evidence for this paper is based on the application of cross-section wage level and change regressions to national samples of teachers in determining the impact of unionization on teacher salaries. The findings contradict most of the previous empirical research (supporting a weak union paradigm and finding only marginal wage effects for unionization) and demonstrate that in 1977 unionized teachers and related teaching personnel had increased their wages by 12 to 21 percent more than nonunion workers. These findings are consistent with auxiliary evidence presented concerning the impact of unionization on the wages of nonteaching white-collar public employees. (Author/WD)

TEACHERS, UNIONS, AND WAGES IN THE 1970s:

UNIONISM NOW PAYS

by

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ABSTRACT

This paper provides evidence that unionization of teachers and related teaching personnel increases their wages relative to those for similar nonunion teachers substantially (by twelve to twenty-one percent) and that this union-nonunion wage differential has increased rapidly in the 1970s. The evidence for these conclusions is based upon the application of two different but complementary research designs, cross-section wage level regressions and cross-section wage change regressions, to national samples of teachers. These findings reverse the conclusions of most of the empirical research conducted during the 1970s, but are consistent with auxiliary evidence presented for the impact of unionization on the wages of nonteaching white-collar public employees.

I. Introduction

This paper provides evidence that unionization of teachers and related teaching personnel substantially increases their wages relative to those for similar nonunion teachers and that this union-nonunion wage differential has increased rapidly in the 1970s. Our evidence for these conclusions is based upon the application of two different but complementary research designs, cross-section wage level regressions and cross-section wage change regressions, to national samples of teachers. These findings reverse the conclusions of most of the empirical research conducted during the 1970s, but are consistent with auxiliary evidence we present for the impact of unionization on the wages of nonteaching white-collar public employees.

Several key theoretical and policy issues may be raised regarding the impact of unionizing public employees of any type.[1] Advocates of the "strong union" position hold that public employee unions have substantial market power. Such unions may play a key role in the political process, threatening to make an "end run" around normal collective bargaining procedures by making wages and other benefits issues in their support for both candidates and elected officials. Politicians are presumed to be more responsive to the highly focused benefit demands of an organized minority than to the broader concerns of the public at large. Moreover, the costs of acceding to public employee demands are diffused over time and over many taxpayers, while the costs to the public of enduring a strike are intensive and immediate. In addition, the demand for public services may be extremely price-inelastic, and the right to offer such services is often a legal monopoly or near-monopoly. Employment elasticities will then be quite low. In this situation Ehrenberg (1973, p. 378) suggests that

". . . market forces do not appear to be sufficiently strong to limit the size of real wage increases which state and local government employees may seek in the future." All of these strong union arguments appear to apply to educators. Teacher unions often play key roles in the political process, and governments require household purchases of the educational product through taxation regardless of whether the product is consumed.

Yet an alternative "weak union" set of arguments also appears to apply to public employee unions. Wage demands may be constrained by public opinion; in a time of tax revolts, it is difficult for public employees to demand sufficient real wage increases to match the inflation rate. More importantly, many white-collar public employees have a strong sense of professionalism, and it is often argued that professionalism is orthogonal to unionism. That is, many public employees have a nonunion "mindset" in which union membership and activity are seen as inappropriate and incompatible with professionalism. These arguments are particularly true for teachers, who are subject to the pressures of public opinion and have a very strong professional identity.

Virtually all the empirical research conducted in the 1970s lends at least implicit support to these weak union arguments, indicating that teacher unions increase relative wages only marginally.[2] Most recently, Perry (1979, p. 12) concluded in a study of nine diverse school districts that the impact of collective bargaining on ". . . average teacher salary, overall budget size, and percent of budget devoted to teacher salaries has not yet been substantial in aggregate terms."

We assess these two set of arguments as follows. In Section II, the methodological framework is set out, some of its limitations are reviewed, and the data used are described. In Section III the empirical results for teachers and other white-collar public employees are presented and evaluated. Section IV is a brief summary of the major conclusions.

II. Methodological Framework

We employ two different techniques for measuring the impact unionism has on relative wages. The first is a traditional method based upon cross-section wage regressions of the type:

$$LnWAGE_t = X_t\beta_t + U_t\gamma_t + \varepsilon_t \tag{1}$$

where LnWAGE is the natural logarithm of the wage rate, X is a vector of personal and job-related characteristics, β is a vector of corresponding coefficients, U is a binary variable indicating union membership for each individual, γ is the union status coefficient, and ε_t is the error term in time period t.[3] The equation is specified in the traditional semilog form so that estimates of γ approximate the proportionate impact of unionism on relative wages.

Unfortunately, not all relevant personal characteristics are observed for each teacher. In national samples, usually only race, sex, experience, education, and grade range taught (or specialty) are known. If a union wage premium does exist, then employers of union workers are likely to be more selective than they would otherwise be. That is, they may increase the quality of their workforce. Moreover, if employers have more information about teacher applicants than is contained in the data sets available to researchers, unmeasured personal characteristics embedded in the error term will be correlated with U, biasing the estimate of γ upward.

We can minimize this source of bias by employing a second method of measuring the impact of unionism suggested by Mellow (forthcoming) based upon changes in wages rather than wage levels. If one has information about the same teachers in a subsequent time period (t+1), a second wage equation can be identified:

$$\text{LnWAGE}_{t+1}=X_{t+1}\beta_{t+1}+U_{t+1}\gamma_{t+1}+\varepsilon_{t+1} \qquad (2)$$

Subtracting eq. (1) from eq. (2) yields the following difference equation:

$$\text{LnWAGE}_{t+1}-\text{LnWAGE}_{t}=(X_{t+1}-X_{t})\beta_{t+1} + X_{t}(\beta_{t+1}-\beta_{t}) + (U_{t+1}-U_{t})\gamma_{t+1} + U_{t}(\gamma_{t+1}-\gamma_{t}) + \phi \qquad (3)$$

where ϕ equals ε_{t+1} minus ε_t. Unmeasured personal characteristics common to both ε_{t+1} and ε_t and affecting wages similarly in both periods are netted out of ϕ, the wage change error term. Thus, the correlation between U and ε induced by these unmeasured personal characteristics (and the resulting upward bias in estimates of γ) is eliminated. Hence, in Section II we also present estimates of the union premium obtained from wage change regressions.

These two alternative approaches (wage level versus wage change regressions) differ in other respects. Wage level regressions provide estimates of the union wage premium for the average member. Wage change regressions, however, provide estimates more akin to the marginal premium, since a change in union status (i.e., becoming a union member) is used to estimate the premium.[4] The average premium is likely to exceed the marginal, since mobility and competition between union and nonunion sectors is usually strongest at entry points. Moreover, estimates of γ based upon changes in union status are more susceptible to any downward simultaneity bias (e.g., low wages induce growth in unionism).[5] On these grounds we would expect the wage change approach to provide a lower estimate of the impact of unionism on relative wages.

Two potential limitations of our methodology remain to be discussed. First, we have not considered "spillover" benefits, i.e., those benefits accruing to unorganized teachers because of the bargaining efforts of others. Previous research suggests that these benefits increase with union density, which is directly associated with large urban areas and particular geographic regions. Hence, we use a binary variable for the forty-four largest urban areas and twenty-six binary variables for geographic regions.[6] Since unionism grew during the period we examine, any failure in fully controlling for spillover benefits will exert a greater downward bias in the later years. A second potential limitation is that we only observe salaries and usual hours worked, not the full benefit package provided teachers. For union members in the economy at large, fringe benefits are larger than for nonunion members (Freeman [1978]), and there is some evidence that this is also true for teachers (e.g., Garms, Guthrie, and Pierce [1978]). Hence, omission of fringe benefits is also likely to exert a downward influence on our estimates. As we shall see in Section III, however, a full accounting of spillover benefits and fringe benefits would likely strengthen the results, leaving our basic conclusion intact.

The data used to estimate eqs. (1) and (3) are taken from the Current Population Survey (CPS). The CPS is a stratified random sample of about 56,000 United States households taken monthly by the United States Bureau of

Census. In the past, the May survey has contained information on the wages and union membership of household members. Because the CPS also has some limited longitudinal or panel properties, it is possible to match respondendents in May surveys a year apart. This property enables us to estimate eq. (3), the wage change equation, based upon those teachers (and other white-collar public employees) employed in both years. The years 1974 and 1977 are used for the wage level regressions, and the years 1974-75 and 1977-78 for the wage change regressions. We observe union membership status in all years,[7] but coverage by a collective bargaining contract only in 1977. Estimates based on coverage, however, are also discussed in Section III. Job-specific information is limited to occupation (grade range taught), geographic location (state/region, large metropolitan city), and for other public employees, governmental level (local, state, federal, postal).[8]

III. Empirical Results

Variables used in the wage level regressions for kindergarten through secondary teachers in 1974 and 1977 are listed along with their definitions and sample means in Table 1.[9] The same is true for the variables used in the wage level regressions for other white-collar public employees. Twenty-six state/region binary variables are not listed, but are included in the regressions. Table 2 presents the results of the wage level regessions for teachers and other white-collar public employees for the years 1974 and 1977. The signs, magnitudes, and significance levels of most of the coefficients for the traditional wage-determining variables for these groups are as expected and are

Table 1 Variables Used in Wage Level Regressions

Variable	Definition	Mean: Teachers 1974	Mean: Teachers 1977	Mean: W-C Public Employ. 1974	Mean: W-C Public Employ. 1977
Independent Variables:					
RACE	One if nonwhite, zero otherwise.	.07	.08	.10	.13
EXP	Years of experience (proxied by age - education - six).	16.14	13.75	23.91	19.96
EXPSQ	EXP squared.	407.15	313.20	743.28	572.53
SEX	One if female, zero otherwise.	.72	.70	.44	.47
FEXP	(EXP) x (SEX).	12.42	9.98	9.82	8.58
FEXPSQ	FEXP squared.	329.28	234.71	301.64	241.56
EDUC	Years of education completed.	16.39	16.27	13.15	13.58
LSMSA	One if in one of 44 largest SMSAs, zero otherwise.	.67	.35	.76	.42
UNION	One if member of union (or association similar to a union) at primary job, zero otherwise.	.30	.45	.24	.27
SEC	One if secondary teacher, zero otherwise.	.40	.41		
ELEM	One if elementary teacher, zero otherwise.	.45	.40		
KIND	One if kindergarten teacher, zero otherwise.	.05	.09		
FED	One if federal worker, zero otherwise.			.41	.40
POST	One if postal worker, zero otherwise.			.18	.16
STATE	One if state worker, zero otherwise.			.15	.19
PROF	One if professional worker, zero otherwise.			.27	.30
MANAG	One if manager or supervisor, zero otherwise.			.18	.20
Dependent Variable:					
LWAGE	Natural logarithm of the hourly wage.	1.57	1.69	1.65	1.80
No. Observations		617	1037	522	1027

Notes: Omitted dummy variables are OTHER (e.g., special education and adult education teachers) for the SEC, ELEM, and KIND variables; LOCAL for the FED, POST, and STATE variables; and CLERICAL (mostly clerical workers) for the PROF and MANAG variables. Binary control variables for twenty-six states/regions are not reported but are included in all the wage level regressions. Data are from the Current Population Survey, U.S. Bureau of the Census (see text for details).

Table 2 Wage Level Regressions for Teachers and White-Collar Public Employees

Independent Variables	Teachers 1974	Teachers 1977	W-C Public Employees 1974	W-C Public Employees 1977
Intercept	-.09	.51	.41	.26
	(-.47)	(3.98)	(2.87)	(2.66)
RACE	.02	.04	-.14	-.004
	(.40)	(.98)	(-2.45)	(-.12)
EXP	.03	.03	.03	.04
	(3.42)	(3.67)	(5.34)	(10.96)
EXPSQ	-.0004	-.0003	-.0005	-.0006
	(-1.95)	(-1.81)	(-4.57)	(-8.46)
SEX	.01	-.11	-.15	-.04
	(.13)	(-1.94)	(-1.56)	(-.64)
FEXP	-.01	-.002	-.01	-.02
	(-1.06)	(-.21)	(-1.35)	(-3.89)
FEXPSQ	.0002	-.0001	.0002	.0004
	(.63)	(-.53)	(1.13)	(3.52)
EDUC	.08	.06	.05	.06
	(7.20)	(8.75)	(6.19)	(10.44)
LSMSA	.11	.10	.08	.09
	(3.16)	(3.63)	(1.90)	(3.12)
UNION	.07	.21	-.01	.08
	(1.85)	(8.59)	(-.18)	(2.64)
SEC	-.10	-.15		
	(-2.04)	(-3.55)		
ELEM	-.14	-.14		
	(-2.74)	(-3.43)		
KIND	-.22	-.26		
	(-2.67)	(-4.75)		
FED			.28	.23
			(6.48)	(7.96)
POST			.28	.33
			(4.74)	(8.05)
STATE			.07	.11
			(1.36)	(3.35)
PROF			.26	.15
			(5.47)	(4.98)
MANAG			.16	.10
			(3.29)	(3.16)
R^2	.38	.36	.53	.51
No. Observations	617	1037	522	1027

Notes: The dependent variable in all regressions is the natural logarithm of the hourly wage. All coefficients are ordinary least squares estimates; t-statistics are in parenthesis below each coefficient. Omitted dummy variables are OTHER (e.g., special education and adult education teachers) for the SEC, ELEM, and KIND variables; LOCAL for the FED, POST, and STATE variables; and CLERICAL (mostly clerical workers) for the PROF and MANAG variables. Binary control variables for twenty-six states/regions are not reported here but are included in all the regressions. Data are from the Current Population Survey, U.S. Bureau of the Census (see text for details).

not dicussed here.[10] The estimates show that in 1974 the union wage premium for teachers was about seven percent (significant at the five percent level, one tail test), and that the union wage premium for other white-collar public employees was nonexistent. In 1977, however, the union wage premium estimated for teachers is twenty-one percent, and the estimated premium for other white-collar public employees is eight percent. Both estimates are significant at the one percent level. The estimated union premium for teachers in 1977 based upon coverage by a collective bargaining agreement rather than union membership is twenty-two percent, consistent with the result based on union membership. Based upon the union membership estimates, the teacher union premium rose by fourteen percentage points from 1974 to 1977 (a change significant at the one percent level, two tail test), and the union premium for other white-collar public employees rose from near zero to eight percent over the same period. Although slightly higher, the 1974 estimate for teachers is generally consistent with earlier studies, but the 1977 estimate indicates a dramatic increase in the union wage premium level regressions. Beginning with a sample of nonunion teachers, we estimate the wage premium associated with being a union member one year later. These estimates and comparable estimates for other white-collar public employees are presented in Table 3. As expected these estimated premiums are lower than those from the wage level regressions both for teachers and other white-collar public employees. For teachers the estimated premium for a union joiner in 1974-75 is four percent (but insignificant), while in 1977-78 the estimated premium is twelve percent (significant at the one percent level, one tail test). Hence, the estimated

Table 3 Wage Change Regressions for Teachers and White-Collar Public Employees

Independent Variables	Teachers 1974-75	Teachers 1977-78	W-C Public Employees 1974-75	W-C Public Employees 1977-78
Intercept	.00 (.00)	.12 (.44)	.62 (2.88)	.23 (1.51)
RACE	.05 (.64)	-.08 (-1.03)	-.02 (-.29)	-.09 (-1.89)
EXP	.00 (.03)	.01 (.83)	-.00 (-.09)	-.01 (-1.08)
EXPSQ	-.0001 (-.19)	-.0004 (-1.11)	-.0001 (-.63)	.0001 (.82)
FEXP	-.01 (-.62)	-.01 (-.76)	.01 (.48)	-.00 (-.24)
FEXPSQ	.0003 (.76)	.0005 (1.20)	-.0000 (-.02)	.0000 (.16)
EDUC	.00 (.06)	-.01 (-.76)	-.02 (-2.01)	-.01 (-.90)
ΔEDUC	.09 (.91)	.15 (1.99)	-.06 (-.42)	.00 (.02)
LSMSA	.03 (.70)	-.01 (-.12)	-.05 (-1.01)	.04 (1.14)
UNION JOINER	.04 (.69)	.12 (2.63)	-.05 (-.66)	.04 (.95)
SEC	.07 (.83)	-.01 (-.14)		
ELEM	.04 (.49)	.05 (.65)		
KIND	-.09 (-.71)	.01 (.07)		
FED			.05 (.82)	.01 (.15)
POST			.08 (.85)	.02 (.41)
STATE			-.02 (-.23)	-.06 (-1.42)
PROF			.00 (.07)	.07 (1.75)
MANAG			-.01 (-.23)	.05 (1.27)
R^2	.03	.08	.06	.06
No. Observations	318	233	280	343

Notes: The dependent variable in all regressions is the natural logarithm of the ratio of the 1974 (1977) wage to the 1975 (1978) wage, i.e., the approximate percentage change from one year to the next. All coefficients are ordinary least squares estimates; t-statistics are in parenthesis below each coefficient. Omitted dummy variables are OTHER (e.g., special education and adult education teachers) for the SEC, ELEM, and KIND variables; LOCAL for the FED, POST, and STATE variables; and CLERICAL (mostly clerical workers for the PROF and MANAG variables. Binary control variables for regional effects and for changes among the occupation and government level variables are not reported here but are included in all the regressions. The mean of UNION JOINER for teachers is 46 in 1974-75 and 67 in 1977-78; for the public employees it is 37 in 1974-75 and 42 in 1977-78. See text and notes to Table 1 for source of data and sample details.

change in the premium for teachers from 1974-75 to 1977-78 is eight percentage points (significant at the five percent level, two tail test). Although the estimated change in the union premium for other white-collar public employees is similar (nine percentage points), it is not statistically significant. Most of the other coefficients in both sets of wage change regressions are insignificant.

These results clearly suggest that the effective bargaining power of teacher unions (and public employee unions in general) has dramatically increased.[11] Moreover, the union premium for teachers appears at least as large as the union premium in the economy at large.[12] Hence, our findings tend to dispel the weak union agruments associated with public employee unions in general and with teacher unions in particular. It is not clear, however, whether the substantial gains made by teacher unions in recent years result from a self-motivated growth in their own bargaining strength, modifications in labor relations legislation for public employees, changes in public attitudes, a diminution over time of any downward simultaneity bias induced by the association between low wages and unionization efforts, or other factors--answers to these questions await further research.

IV. Concluding Remarks

Two opposing schools of thought regarding public employee unions, and especially teacher unions, suggest that such unions are likely to be, alternatively, powerful or weak. Heretofore, most empirical research has supported the weak union paradigm, finding only marginal wage effects for unionization. Using national data and two different research designs, we find that in 1977 unionization of teachers and related teaching personnel had increased their wages relative to those for similar nonunion workers by twelve to twenty-one percent, an increase in the union premium of eight to fourteen percentage points since 1974. This increase in the union wage premium exceeds but is consistent with the increase we find for nonteaching white-collar public employees.

Notes

[1]For an introduction to these issues, see Ehrenberg (1973), Freund (1974), Fogel and Lewin (1974), and Shapiro (1978). In 1977, about ninety percent of kindergarten through twelfth-grade school teachers were public employees.

[2]Zero to five percent was the range of increase in state average salaries found by Kasper (1970). Thornton (1971) found a one to four percent increase in the first three classification steps (but up to twenty-three percent in the top step) when he sampled cities with populations over 100,000 in 1969-70. Baird and Landon (1972) found a 4.9 percent increase in minimum salaries in their 1966-67 sample of 44 school districts having 24,000 to 50,000 students. Hall and Carroll (1973) found a 1.8 percent increase in mean salaries for unionized teachers in 118 districts in Cook County, Illinois. Lipsky and Drotning (1973) performed a detailed study of teacher salaries in all 696 districts in New York state for 1967-68. They found collective bargaining not significant in explaining 1968 salary variations across districts, but significant in explaining changes in district salaries from 1967 to 1968. Higher estimates (five to twelve percent) were obtained by Chambers (1977) in a study of eighty-nine districts in the six largest metropolitan areas in California

[3]Se shenfelter (1972), Oaxaca (1975), and Kalachek and Raines (1976), for applications of this methodological approach.

[4]Ideally, one could use observations on those who leave and those who join unions to measure the impact on relative wages. For teachers, however,

the measure for those who leave unions proved to be very cyclically sensitive (i.e., the measured premium was very large during downturns, but small and sometimes negative for upturns). Hence, we use estimates based upon union joiners in the wage change regressions.

[5]See Ashenfelter and Johnson (1972), Schmidt and Strauss (1976) and Lee (1978) for arguments that unionism and wages are simultaneously determined. In particular, Lee's finding that probability of union membership is positively related to the difference between the alternative union wage and the available nonunion wage suggests that within the teaching occupation any simultaneity bias in estimates of the effects of unionism on wages is likely to be negative.

[6]For evidence on density of unionism, see Chambers (1977). For an earlier example of this approach to controlling for spillover benefits, see Lipsky and Drotning (1973). The urban/region binary variables are also related to potential urban/regional cost of living differences.

[7]In 1974 the union status question was "Does . . . belong to a labor union?" In 1975 "on this (the primary) job" was added to the end. In 1977 and 1978 the question was "On this (the primary) job, is . . . a member of a union or an employee association similar to a union?"

[8]Duncan and Stafford (1979) argue that members of industrial labor unions have less desirable working conditions than nonunion members, hence that part of the union wage premium is a compensating wage differential.

[9]One set of variables warrants further explanation. Women teachers and public employees are permitted different coefficients for EXP and EXPSQ

(see FEXP and FEXPSQ). This is done for two reasons: (1) EXP is proxied by age minus education minus six, a more accurate proxy for men than women; and (2) the true coefficients may also differ (i.e., women may have different wage-experience profiles).

[10]Two exceptions are RACE and SEX (along with the FEXP variables). A significant sex differential is not present for teachers in 1974 or for other white-collar public employees in 1977, but is present for teachers in 1977 and for public employees in 1974. The FEXP and FEXPSQ variables, however, are strongly significant only for public employees in 1977. A significant RACE differential is present only for public employees in 1974.

[11]Mellow (forthcoming), for example, found union premiums in 1977-78 of twenty-one percent in wage level regressions for the economy at large and five to eight percent in wage change regressions.

[12]Note that under the assumptions made in Section II, the inclusion of fringe benefits in the analysis or a more precise accounting for spillover benefits is likely to make the increase in the total unionization effect on teacher wages even larger.

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