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Collective Bargaining and Community College Faculty: What Is the Wage Impact?

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Abstract Studies that measure the impacts of collective bargaining on the salary of faculty in two-year colleges are limited. Most studies of faculty unions have used data that combine faculty in both two-year and four-year institutions. Recent work has demonstrated that past estimates of the impacts of unions on full-time faculty salaries in higher education suffer from multiple data, methodological, and statistical problems. This paper addresses these deficiencies, and the results support the claim that collective bargaining increases faculty salaries in two-year institutions, though by less than previously documented.

Keywords Union wage premium · Higher education

The last four decades have produced a number of studies examining the impacts of unions on faculty wages in higher education. However, most of these studies have concentrated on faculty at four-year schools. Considerably less attention has been given to faculty at two-year institutions—a group that is expanding quickly and whose members are more likely to be unionized than their four-year counterparts. Given increasingly tight constraints on state budgets, combined with few options for legislators to balance those budgets without cutting higher education, understanding the impacts of collective bargaining on faculty wages is important and topical.

To the extent that faculty at two-year colleges have been included in studies of union effects, they have typically been pooled together with faculty at four-year institutions. Only rarely have investigators attempted to measure effects separately by institution type. In an early set of studies, Birnbaum (1974, 1976) applied matched-pairs comparisons to institution-level data from the American Association

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of University Professors (AAUP). He found that among four-year colleges and universities the rate of salary growth was significantly higher for unionized than for nonunionized institutions, but that among two-year colleges the union differential was not statistically significant. Later studies applied regression techniques to control for variation in institutional characteristics such as type (AAUP or Carnegie classification), public versus private control, and faculty composition by rank. Using this approach Leslie and Hu (1977) and Freeman (1978) found positive union effects, but they did not report separate union differentials by two- or four-year type. In one of the few such studies to do so, Rees (1993) estimated union premia of about 3.8% for four-year schools and 7.4% for two-year schools using cross-section data. Using a fixed-effects model applied to panel data, however, Rees estimated statistically insignificant or negative union impacts, depending on the sample period. Because a faculty member's salary depends in complex ways both on institution-specific variables and on his or her own characteristics, the use of institution-level data in these early studies obscures important variation across individuals that can be exploited to more precisely estimate the effects of union representation.

A second wave of studies beginning in the late 1980s benefited from the availability of micro-level survey data on individual faculty members. Barbezat (1989) and Ashraf (1992) used data from the 1977 *Survey of the American Professoriate*, while Ashraf (1997) used the 1988 *National Study of Postsecondary Faculty* (NSOPF), and Ashraf (1998, 1999) and Monks (2000) used the 1993 round of the NSOPF. Here again, the results are highly divergent. Barbezat estimated a statistically insignificant union advantage of about 1.3–1.6%. Ashraf (1997, 1998) provided a range of estimates, both positive and negative, that differ widely across institutional and faculty characteristics. Monks estimated a union premium of 7.3% using one model specification and 14% using another. However, all of these results are based on samples that mix two- and four-year institutions. Given that the compensation structure is very different between two- and four-year schools, it is unclear what these prior studies indicate about unionization at either type of institution.

To date, the only study of which we are aware that has used micro-level data to understand the wage impacts of unionization on faculty members at two-year schools has been Ashraf (1998). He estimated an 8.4% wage benefit from unionization for two-year colleges, versus zero for four-year schools. However, there are reasons to be skeptical about this result. As noted by Hedrick et al. (2011), all studies described above suffer from a number of limitations. First, none consider the impact of local differences in living costs. According to data compiled by Moriarty and Savarese (2006, p. 88), the vast majority of unionized faculty are located in the West, Mid-Atlantic, and Midwest, with about half located in the relatively high-cost states of California and New York. This is important, given the findings of Guthrie-Morse et al. (1981) and Hu and Leslie (1982) from institution-level data that although unionization may be associated with higher nominal salaries, if differences in cost of living are taken into account then real salaries at unionized institutions are on average lower. Second, none of these studies have combined repeated observations of institutions with micro-level observations of faculty. This presents a problem: it is possible that the higher pay of unionized schools is not a function of unionization but instead of unobserved institution-specific factors. Under these circumstances, the estimates of the union wage premium based on cross-section data will not reflect the impact on salaries of unionization.

In this paper we re-examine the impact of unions on faculty at two-year colleges, utilizing the approach of Hedrick et al. to avoid several limitations of past research. First, by focusing solely on two-year institutions, we eliminate the potential bias that arises when these institutions are pooled with their four-year counterparts. Second, we incorporate data from the two most recent rounds of the NSOPF, which have not been utilized by previous researchers. By pooling these data and applying panel data techniques, we control for institutional-specific factors correlated with unionization. Third, we explicitly account for cost-of-living differences to estimate the real salary difference attributable to faculty unions. Fourth, we address measurement error implicit in the NSOPF data, which appears to have biased upwards prior estimates of the union wage premium. Finally, we acknowledge the large variation across states' legal systems and attitudes toward unionization by estimating models with state fixed effects.

Model Specification

As is common among studies of wage determination, we estimate a model in which the dependent variable is the natural logarithm of salary. Specifically:

$$\ln\left(Y_{ijts}\right) = \beta Union_{jts} + \delta \mathbf{X}_{ijts} + \gamma \mathbf{Z}_{jts} + \alpha \mathbf{S}_s + \eta \mathbf{T}_t + v_j + \varepsilon_{ijts} \tag{1}$$

where Y_{ijts} is a measure of salary for the i^{th} faculty member at institution j during time t in state s. *Union* is a binary variable equaling one in the presence of collective bargaining, and **X** and **Z** are matrices of individual and institutional variables, respectively. State-level binary variables, **S**, control for unobserved state-level heterogeneity in faculty salaries, such as might arise from differences in legislative support or unobserved amenities. **T** is a matrix of binary variables representing the individual survey years. The error term in this equation contains two components: v_j, which represents an institution-specific error, and ε_{ijts}, which is associated with a particular faculty member at that institution. The coefficient of interest in (1) is β, which approximates the percentage change in wages associated with collectively bargaining.

Previous faculty-level studies of the wage premium used cross-sectional data for a single time period to estimate a model of the form:

$$\ln\left(Y_{ij}\right) = \beta Union_j + \delta \mathbf{X}_{ij} + \gamma \mathbf{Z}_j + u_{ij}. \tag{2}$$

Equation 2 is a special case of Eq. 1 that ignores the state-level and time-level heterogeneity and has an error term u_{ij} equal to $v_j + \varepsilon_{ijs}$. Estimating Eq. 2 by ordinary least squares is problematic for at least three reasons. First, because all faculty at an institution share the common institution-specific error term v_j, the composite error term u_{ij} is correlated across observations within institutions. Because it ignores this clustering, the OLS estimator of β will be inefficient and standard errors will be incorrect. Secondly, Eq. 2 ignores the unobserved institutional-level heterogeneity that can be identified using panel methods. Finally, Eq. 2 omits potentially important unobserved state-level variables (e.g. collective bargaining legislation, support of education establishments) that influence faculty salaries.

Data

Description

The NSOPF is conducted about every five years by the National Center for Education Statistics (NCES) of the U.S. Department of Education. To date it has been administered four times: in 1988, 1993, 1999, and 2004. Each cycle uses a similar two-stage sampling process in which institutions are sampled first and then faculty members are sampled from within the selected institutions.[1] In the institutional questionnaire, a representative of the school's administration is asked about institutional characteristics, policies, faculty benefits, and whether any faculty at the institution are represented by a union for the purposes of collective bargaining. In separate surveys, faculty at each institution are asked about their professional experience and background, responsibilities and workload, compensation, demographic characteristics, and opinions.

The four cycles of the NSOPF generated 78,310 faculty observations at 1,900 two- and four-year institutions. Because compensation systems are likely different between these types of institutions, observations from four-year institutions are deleted from this analysis. This reduces the sample to 20,900 faculty observations at 640 two-year colleges.[2] Of these, we exclude 2,380 faculty whose principal activity is not teaching, 3,390 who had missing explanatory variables, and 40 whose institution failed to indicate whether faculty on their campus collectively bargain. We omit an additional 4,330 observations for which some missing responses were replaced by imputed values. Because of the inherent salary differences expected between full-time and part-time workers, we also exclude 4,270 faculty who work part-time. Finally, we exclude 20 faculty whose basic salary received from their institution was less than $2,500 in 2004 dollars. After deleting these observations, a final sample of 6,480 faculty at 610 different institutions remains. Table 1 shows the panel structure of the institutional and individual observations over time. About two-thirds of institutions and 45% of faculty observations are observed once, almost a quarter of institutions and one-third of faculty are observed twice, 7.8% of institutions and 10.2% of faculty are observed three times, and 1.5% of institutions and 4% of faculty are observed four times.

Measures of Unionization

In all previous union research using the NSOPF, unionization was indicated by a binary variable (called *Union* in this paper) based on an institution-level question that asked whether any full-time faculty and instructional staff were legally represented by a union for the purpose of collective bargaining. As pointed out by Hedrick et al., this can result in serious measurement-error bias if only some faculty at an institution are represented. For example, in the University of California system,

[1] The NSOPF treats each campus in a multi-campus system as a separate institution for sampling purposes. It oversamples doctoral granting institutions and faculty members who are either women or minorities, or who teach in the humanities.

[2] Faculty and institution counts are rounded to the nearest 10 to comply with NCES confidentiality requirements.

Table 1 Panel structure of the data

Times observed	Survey year	Number of institutions		Percent of institutions		Number of faculty observations		Percent of faculty observations	
4	1988, 1993, 1999, 2004	10	10	2	2	260	260	4	4
3	1988, 1993, 1999	0	50	0	8	80	1,100	1	17
	1988, 1993, 2004	10		2		190		3	
	1988, 1999, 2004	10		2		140		2	
	1993, 1999, 2004	30		5		700		11	
2	1988, 1993	10	140	2	23	190	2,160	3	33
	1988, 1999	10		2		140		2	
	1988, 2004	10		2		120		2	
	1993, 1999	50		8		750		12	
	1993, 2004	20		3		460		7	
	1999, 2004	40		7		490		8	
1	1988	40	420	7	68	220	2,950	3	46
	1993	130		21		1,450		22	
	1999	100		16		470		7	
	2004	150		25		810		13	
Totals		610	610	100	100	6,480	6,480	100	100

Sample sizes are rounded to the nearest ten to comply with NCES disclosure requirements. Columns may not sum to totals due to rounding.

adjuncts engage in collective bargaining but tenured and tenure-track faculty do not. The *Union* variable incorrectly assigns union status to the higher-paid regular faculty, biasing the estimate of β upward. In Florida, however, permanent faculty collectively bargain but adjuncts do not, resulting in a bias in the opposite direction. The net effect of this measurement error is ambiguous and dependent upon the frequencies of union representation of faculty subgroups on a campus.

We avoid this problem by using a periodic comprehensive survey of unionized institutions from the National Center for the Study of Collective Bargaining in Higher Education and the Professions (NCSCBHEP) compiled by Moriarty and Savarse (2006). The NCSCBHEP data identify the year of initial collective bargaining for four faculty subgroups within all U.S. institutions: full-time permanent faculty, part-time permanent faculty, adjuncts, and librarians. We use these data to construct a variable (*Unionsubgroup*) that corrects the misclassification of unionization at institutions having collective bargaining agreements with portions of their instructional staff. Because *Unionsubgroup* correctly identifies the individual faculty member's collective bargaining status, it does not suffer from the systematic measurement error in the *Union* variable that arises from the NSOPF's overly broad survey question.

Measures of Salary

The NSOPF faculty survey asks numerous questions regarding individuals' financial compensation. From these we construct two measures: *Basic Salary* and *Total*

Salary. *Basic Salary* represents payments made to faculty in exchange for fulfilling their basic annual contract and is the measure used by Monks (2000) and Ashraf (1998, 1999). *Total Salary* is equal to *Basic Salary* plus other supplementary payments from the faculty's institution such as summer teaching and overload courses. Unions may impact *Basic Salary* and *Total Salary* differently. For instance, institutions could respond to unionization by creating optional faculty duties external to the basic contract, thereby increasing *Total Salary* relative to *Basic Salary*. Alternatively, a union may frown on such payments and bargain to curtail them, or may bargain into the basic contract what were previously considered extra duties in exchange for increased *Basic Salary*, reducing the difference between *Basic Salary* and *Total Salary*. For the entire sample, the correlation between *Basic Salary* and *Total Salary* is 0.93, suggesting that any systematic differences that occur are relatively small. To be thorough, we report results using both measures of salary.

Cost of Living

Both the NSOPF and the NCSCBHEP data identify a very strong geographical pattern of unionization among two-year colleges. In the NSOPF data, the mid-Atlantic Census region and California contain 40.5% of all unionized faculty observations but only 27% of total faculty observations, suggesting that on average, faculty are more likely to collectively bargain if they live in these areas. Since these regions are relatively expensive, failure to account for cost of living differences can cause the union wage premium to be overestimated.[3]

Arguably the most reliable and widely used measure of local geographical differences in living costs currently available is the ACCRA cost of living index published quarterly by the Council for Community and Economic Research. The ACCRA index is based on the prices of 57 commodities and services, providing a comprehensive measure of living cost. However, the ACCRA index is compiled only for metropolitan areas. Use of the ACCRA data would thus eliminate from our analysis 2,520 faculty observations at 230 rural institutions—almost 39% of our faculty observations.

To retain the rural data in our sample, we follow Hedrick et al. and use data from the decennial U.S. Census to construct an alternative cost-of-living measure that approximates the ACCRA index. Define $Rent\ Ratio_{kt}$ to be the ratio of median gross quality-adjusted apartment rents in county k at time t to median gross rents for the U. S. at time t. This cost-of-living index is defined as:

$$Rent\ Index_{kt} = 0.7 + 0.3 \times (Rent\ Ratio_{kt}). \tag{3}$$

The weight of 0.3 in our *Rent Index* is the weight on housing costs in the ACCRA index and is based on the Consumer Expenditure Survey conducted by the U.S. Bureau of Labor Statistics.[4] The coefficient of 0.7 ensures that the *Rent Index* equals one when a county's quality-adjusted apartment rents equal the national median. For a discussion of the accuracy of the *Rent Index* relative to the more comprehensive

[3] For example, the ACCRA cost of living index (discussed below) during 2004 in California and the mid-Atlantic states averaged 28.1% higher than the rest of observations in the data.

[4] www.bls.gov/cex/home.htm

ACCRA index, see Hedrick et al. In brief, there are reasons to believe that use of the *Rent Index* rather than the ACCRA measure will, if anything, tend to overestimate the union salary premium. While we rely most heavily on the *Rent Index*, we also report some results using the ACCRA data for comparison.

We use the *Rent Index* to construct two rent-adjusted salary measures: *RA Basic Salary* = *Basic Salary/Rent Index* and *RA Total Salary* = *Total Salary/Rent Index*. We refer to this approach as the "complete" adjustment process.

Dumond et al. (1999) have pointed out that equilibrium wages vary across locations less than proportionately with living costs. Attractive local amenities may result in land prices being bid up and/or in workers being willing to accept lower wage offers. Thus higher prices in more-desirable locations may overstate the cost of achieving a given utility level. In addition, consumers alter their utility-maximizing consumption bundle in response to price differences, so a true constant-utility cost index would rise less rapidly than a fixed-weight price index. For these reasons, if Y is nominal salary and P is a price index such as the *Rent Index* (centered at 1.0 rather than at 100), then the complete-adjustment approach that uses $\ln(Y/P)$ as the dependent variable in the log wage equation potentially over-corrects for true cost-of-living differences. As an alternative, Dumond et al. (1999) recommend a regression-based partial-adjustment procedure that uses $\ln Y$ as the dependent variable and $\ln P$ and its square as explanatory variables.[5] Because it does not restrict the regression coefficients on prices, we prefer this partial-adjustment method over either the complete-adjustment or no-adjustment approaches, but we report the results of all three methods below.

Descriptive Statistics

Table 2 presents sample means and standard deviations for both unionized and nonunionized faculty as defined by the *Union* variable. Unionized faculty average \$9,689 (21.6%) more *Basic Salary* and \$10,478 (21.4%) more *Total Salary* than non-unionized faculty.[6] After dividing these salary measures by the *Rent Index*, these differences fall by about one-third, to \$6,566 (14.0%) and \$7,113 (13.9%), respectively, highlighting the importance of accounting for cost of living differences. The remaining salary differences may be further explained by the fact that faculty at unionized institutions average more experience (both in their current position and since earning their highest degree), are less likely to hold the rank of instructor, are more likely to hold masters degrees and to teach at larger institutions (as measured by enrollment).

One drawback of the data is that there is relatively little time variation in unionization within institutions. Although 58.7% of observed institutions and 55.6% of observed faculty engage in collective bargaining, only about ten out of 610 institutions changed collective bargaining status over the four periods in our sample.

[5] The two methods are equivalent if the coefficients on $\ln P$ and $(\ln P)^2$ are equal to 1 and zero respectively. The inclusion of the squared term allows for $\ln Y$ to increase at a decreasing rate with $\ln P$. In all of the regressions reported in Tables 4, 5 and 6 below that use the partial-adjustment approach, the coefficient on $\ln P$ is between zero and one and the coefficient on $(\ln P)^2$ is negative or statistically no different from zero, consistent with the theoretical prediction of Dumond et al. (1999).

[6] All dollar figures are in base year 2004.

Table 2 Descriptive statistics: sample means (standard deviations in parentheses)

		Union=1		*Union*=0
Basic Salary	Real salary, 2004 base year	54,336 (22,696)	>	44,737 (27,111)
Total Salary	Real payments, 2004 base year	59,229 (26,91)	>	48,850 (28,109)
RA Basic Salary	Basic Salary divided by *Rent Index*	53,455 (22,134)	>	47,002 (26,510)
RA Total Salary	Total Salary divided by *Rent Index*	58,289 (26,904)	>	51,300 (27,558)
Exp	Years of experience at current institution	12.10 (9.13)	>	9.71 (8.45)
Degexp	Years of experience since earning highest degree	16.26 (9.63)	>	14.38 (9.54)
Female	Binary=1 if female	.467 (.499)	=	.484 (.499)
Married	Binary=1 if currently married	.718 (.450)	=	.732 (.442)
Wasmarried	Binary=1 if previously married	.143 (.350)	=	.152 (.359)
Hispanic		.055 (.229)	<	.069 (.253)
Indian		.013 (.114)	=	.014 (.117)
Asian		.039 (.194)	>	.029 (.169)
Black		.075 (.263)	<	.097 (.296)
Pacific		.002 (.044)	=	.001 (.032)
Lecturer	Binary=1 if academic rank is lecturer	.004 (.065)	=	.003 (.059)
Instructor	Binary=1 if academic rank is instructor	.380 (.486)	<	.450 (.497)
Assistant	Binary=1 if academic rank is assistant professor	.113 (.316)	=	.124 (.329)
Associate	Binary=1 if academic rank is associate professor	.136 (.343)	=	.123 (.329)
Full	Binary=1 if academic rank is professor	.232 (.423)	>	.145 (.352)
Tenured	Binary=1 if tenured	.673 (.469)	>	.359 (.479)
Tentrack	Binary=1 if on tenure track	.153 (.360)	=	.168 (.373)
Bachelors	Binary=1 if highest degree earned is a bachelors	.103 (.304)	=	.116 (.321)
Masters	Binary=1 if highest degree earned is masters	.670 (.470)	>	.620 (.485)
Profession	Binary=1 if highest degree is professional	.018 (.135)	=	.020 (.139)
Doctorate	Binary=1 if highest degree earned is Ph.D. or equivalent	.176 (.381)	=	.184 (.387)
Citizen	Binary=1 if U.S. citizen	.982 (.133)	=	.980 (.140)
Funded	Binary=1 if scholarly activity is funded by external agency	.085 (.280)	=	.083 (.276)
Firstjob	Binary=1 if current job is first since graduating	.434 (.496)	=	.416 (.493)
Enrollment	Total Student FTE (thousands)	5.39 (3.84)	>	4.65 (4.52)
N	Number of Faculty Observations	3,580		2,900
N_j	Number of Institutions	360		260

>,< represent statistical differences using a paired *t*-test at the 5% level. Sample sizes are rounded to the nearest 10 to comply with NCES disclosure requirements.

Thus identification of the union wage premium relies mostly upon cross-sectional variation in union status between institutions rather than variation within institutions over time.

Econometric Evidence

To facilitate comparison with other studies, we first estimate Eq. 2 by ordinary least squares, treating *Basic Salary* and *Total Salary* as functions of *Union*, **X**, **T**, and **Z**. The explanatory variables in **X** contain all faculty variables listed in Table 2, the squares of institutional and degree experience, and 32 binary variables indicating the faculty member's general field of study. The institutional variables in **Z** are total student full-time equivalent enrollment and its square.

Panel A of Table 3 presents results using *Basic Salary* as the dependent variable. The estimated union premium ranges from 9.4% in the 1999 survey to 13.1%, in 2004. The 10.9% union premium for 1993 is close to Ashraf's (1998) estimate of 8.4% using the same survey. When all four surveys are pooled, the estimated union premium is 11.5%.

The final column of Table 3 presents random-effects estimates of the pooled *Union* wage premium. The estimate of 9.8% is about 15% smaller than the pooled OLS estimate. The random-effects estimator makes use of the inherent panel nature of the data by explicitly accounting for the institutional-specific error term v_j. However, the random-effects estimator assumes that the v_j are uncorrelated with the independent variables—which may be questionable if unobserved institutional characteristics influence faculty unionization. Though the fixed-effects estimator would be preferable in this case, it is impracticable due to the limited within-institution variation in *Union*.

Panel B of Table 3 presents the *Total Salary* union premium. Again, the highest estimate of the union premium occurs in the latest survey. Indeed, the slight upward trend in the estimates suggests that the impact of faculty unions at two-year schools may have increased over time. Relative to the estimates using *Basic Salary*, the estimated *Total Salary* premium is smaller in three of the four individual years, for the pooled estimates, and for the random-effects estimates. Since *Total Salary* includes payments for extra faculty duties, the lower wage premium using *Total Salary* suggests either that unions may incorporate these into the basic annual contract or that unionization limits opportunities for faculty to earn extra pay.

As noted above, there are reasons to be suspicious of these results: they do not account for measurement error in *Union*, for local cost of living differences, or for unobserved state-level heterogeneity. To address these concerns we re-estimate the *Basic Salary* premium, substituting *Unionsubgroup* for *Union* and incrementally

Table 3 Estimates of wage premium using *Union*

	1988	1993	1999	2004	Pooled	RE
			Panel A: Basic Salary			
β	0.103***	0.109***	0.094***	0.131***	0.115***	0.098***
	(0.031)	(0.019)	(0.024)	(0.028)	(0.014)	(0.013)
R^2	0.503	0.294	0.412	0.446	0.351	
			Panel B: Total Salary			
β	0.064**	0.103***	0.098***	0.110***	0.104***	0.088***
	(0.026)	(0.019)	(0.026)	(0.029)	(0.014)	(0.013)
R^2	0.536	0.318	0.410	0.433	0.364	
N	650	2,860	1,110	1,860	6,480	6,480

Standard errors in parenthesis. *** {**} [*] represent statistical significance at the 1% {5%} [10%] level. Sample sizes are rounded to the nearest 10 to comply with NCES disclosure requirements.

adding cost of living adjustments and state-level fixed effects. Results are given in Table 4. Comparing the annual estimates in Panel A between Tables 3 and 4, the wage premium based upon *Unionsubgroup* is smaller in three of the four years than that found using *Union*. The difference is largest in the preferred random-effects estimate, which drops from 9.8% to 8.6%. Since *Union* and *Unionsubgroup* differ only in that *Unionsubgroup* correctly measures the bargaining status of each job classification within a campus, these results suggest an upward bias in the use of *Union* from misclassifying faculty as a result of the problematic institution-level collective-bargaining question asked on the NSOPF.

In Panels B and C of Table 4 we correct for local cost of living differences. The 8.6% random-effects estimate of Panel A falls to 4.4% when using the complete adjustment process and to 5.7% when using the preferred partial rent adjustment process. Similar large declines occur in the individual years and in the pooled estimates, again highlighting the importance of accounting for living costs. Note also that after adjusting for cost of living, the apparent increase in the union premium over time is less evident.

Freeman and Valletta (1988) and Hosios and Siow (2004) point out that the legislative environment in which a faculty union operates can be an important factor in its success in securing higher compensation for its members. There are large differences across states in the legality and scope of bargaining, the duty of institutions to bargain, and the right to strike. We account for these differences by introducing state fixed effects. Of course, these fixed effects also capture impacts on

Table 4 Estimates of wage premium using *Unionsubgroup* and *Basic Salary*

	1988	1993	1999	2004	Pooled	RE
			Panel A: No Cost of Living Adjustments			
β	0.092***	0.105***	0.121***	0.110***	0.109***	0.086***
	(0.032)	(0.022)	(0.021)	(0.027)	(0.014)	(0.013)
R^2	0.498	0.291	0.418	0.435	0.347	
			Panel B: Complete Rent-Adjusted Cost of Living			
β	0.038	0.041**	0.077***	0.061***	0.053***	0.044***
	(0.028)	(0.020)	(0.020)	(0.023)	(0.013)	(0.012)
R^2	0.441	0.242	0.364	0.407	0.297	
			Panel C: Partial Rent-Adjusted Cost of Living			
β	0.061*	0.068***	0.088***	0.058**	0.068***	0.057***
	(0.031)	(0.023)	(0.022)	(0.025)	(0.014)	(0.013)
R^2	0.512	0.300	0.430	0.481	0.363	
		Panel D: Partial Rent-Adjusted Cost of Living with State Fixed Effects				
β	0.031	0.023	0.078	−0.009	0.031*	0.028*
	(0.051)	(0.024)	(0.053)	(0.033)	(0.017)	(0.017)
R^2	0.565	0.327	0.502	0.597	0.406	
N	650	2,860	1,110	1,860	6,480	6,480

Standard errors in parenthesis. *** {**} [*] represent statistical significance at the 1% {5%} [10%] level. Sample sizes are rounded to the nearest 10 to comply with NCES disclosure requirements.

faculty wages from differences in state support for higher education, public sentiment towards collective bargaining, and other state-level, unobserved, time-invariant factors.

The partial rent adjustment estimates augmented by state-level fixed effects are presented in Panel D of Table 4. In the presence of state fixed effects, the preferred random-effects estimate falls from 5.7% to 2.8%. For each regression, F-tests reject the null hypothesis that state fixed effects have no explanatory power. The considerable reductions in β suggest that prior estimates of the union wage premium confounded unobservable state characteristics with the impacts of unions.

Table 5 applies the estimation routines of Table 4 to *Total Salary*. As in the case of Table 4, the estimated wage premium is generally reduced by using *Unionsubgroup* rather than *Union* and further reduced after accounting for local cost of living differences. For the preferred partial adjustment process, the estimates are one-half to two-thirds the size of estimates that do not account for living costs. The preferred combination of institution-level random effects with partial cost of living adjustment and state fixed effects generates an estimated premium of 3.0%. Table 5 also corroborates the earlier finding that unions have a smaller impact on *Total Salary* than on *Basic Salary*. Relative to the *Basic Salary* estimates of Table 4, the estimates of the *Total Salary* wage premium are either smaller or of similar magnitude.

A natural question is whether these results might be sensitive to the use of *Rent Index* as a measure living costs rather than the more comprehensive ACCRA index.

Table 5 Estimates of wage premium using *Unionsubgroup* and *Total Salary*

	1988	1993	1999	2004	Pooled	RE
			Panel A: No Cost of Living Adjustments			
β	0.056**	0.104***	0.122***	0.090***	0.100***	0.083***
	(0.028)	(0.021)	(0.023)	(0.027)	(0.014)	(0.013)
R^2	0.533	0.316	0.414	0.425	0.361	
			Panel B: Complete Rent-Adjusted Cost of Living			
β	0.002	0.041**	0.077***	0.041*	0.044***	0.040***
	(0.026)	(0.020)	(0.022)	(0.023)	(0.013)	(0.013)
R^2	0.472	0.263	0.356	0.394	0.308	
			Panel C: Partial Rent-Adjusted Cost of Living			
β	0.029	0.074***	0.089***	0.042*	0.062***	0.056***
	(0.028)	(0.022)	(0.024)	(0.025)	(0.014)	(0.013)
R^2	0.545	0.323	0.425	0.463	0.375	
		Panel D: Partial Rent-Adjusted Cost of Living with State Fixed Effects				
β	0.002	0.041*	0.073	−0.007	0.031*	0.030*
	(0.038)	(0.024)	(0.056)	(0.034)	(0.018)	(0.017)
R^2	0.598	0.353	0.495	0.564	0.417	
N	650	2,860	1,110	1,860	6,480	6,480

Standard errors in parenthesis. *** {**} [*] represent statistical significance at the 1% {5%} [10%] level. Sample sizes are rounded to the nearest 10 to comply with NCES disclosure requirements.

As a check, we estimated regressions using *Unionsubgroup* and *Basic Salary* as in Table 4, but using the ACCRA cost of living adjustment on the subsample for which it is available. Results are presented in Table 6. Comparing the no-cost-of-living-adjusted estimates in Panel A of the two tables, those in Table 6 are generally similar to those from the full sample in Table 4. The complete-adjustment approach in Panel B, however, results in *negative* union premia in all years and in the pooled samples—on average about eight to ten percentage points lower than the rent-adjusted estimates. This is no doubt a consequence of two things. First, the ACCRA index is positively skewed. Following the argument of Dumond et al. (1999), individuals in the highest-cost areas (which happen to be the most highly unionized) may be more likely to substitute other goods (such as commuting time) for housing costs, so that the ACCRA index may overestimate actual living costs in these locations (and underestimate "real" income), thus causing the "true" union premium to be underestimated. Secondly, the rent index likely understates true cost of living differences between locations, causing some of this variance to be attributed to unions. With the preferred partial-adjustment procedure in Panel C, the wage premium is positive for all individual years save 2004, and the preferred random-effects estimate is 3.9%. Adding state fixed effects to this model reduces the random-effects estimate to 2.3%, compared with the 2.8% estimate found under the rent adjustment process. Regardless of the cost-of-living adjustment used, these estimates are less than half of the 7.4% and 8.4% estimates of Rees (1993) and Ashraf (1998), respectively.

Table 6 Estimates of wage premium using *Unionsubgroup* and *Basic Salary*

	1988	1993	1999	2004	Pooled	RE
			Panel A: No Cost of Living Adjustments			
β	0.139***	0.103***	0.104***	0.122***	0.114***	0.098***
	(0.030)	(0.027)	(0.026)	(0.031)	(0.017)	(0.016)
R^2	0.641	0.264	0.452	0.446	0.349	
			Panel B: Complete ACCRA-Adjusted Cost of Living			
β	−0.026	−0.047	−0.033	−0.084***	−0.057***	−0.052***
	(0.036)	(0.031)	(0.035)	(0.028)	(0.019)	(0.018)
R^2	0.526	0.196	0.342	0.376	0.267	
			Panel C: Partial ACCRA -Adjusted Cost of Living			
β	0.097***	0.066**	0.071**	−0.023	0.045**	0.037*
	(0.033)	(0.032)	(0.029)	(0.039)	(0.020)	(0.019)
R^2	0.653	0.271	0.459	0.495	0.365	
		Panel D: Partial ACCRA -Adjusted Cost of Living with State Fixed Effects				
β	−0.005	0.052	0.114*	−0.024	0.022	0.023
	(0.053)	(0.039)	(0.068)	(0.035)	(0.026)	(0.025)
R^2	0.730	0.294	0.520	0.592	0.402	
N	400	1,580	670	1,330	3,970	3,970

Standard errors in parenthesis. *** {**} [*] represent statistical significance at the 1% {5%} [10%] level. Sample sizes are rounded to the nearest 10 to comply with NCES disclosure requirements.

Conclusion

Employing the most comprehensive dataset available, this paper examines the impacts of unions on two-year faculty salaries. Using random-effects estimators to exploit the availability of repeated observations of institutions spread out over 16 years, we estimate considerably smaller union salary effects than those found using OLS applied to individual cross-sections as has been done in prior studies. We correct for measurement error in the union variable, for differences in local costs of living, and for unobserved state-level fixed effects. After making each of these adjustments, the estimated union wage premium declines; after making all of these adjustments simultaneously, our preferred random-effects estimates of the union wage premium are 2.8% (*Basic Salary*) and 3.0% (*Total Salary*).

It is important to note that in some ways these results may understate the impact of unions on wages. In the presence of state fixed effects, these estimates are best thought of as comparisons of union wages against non-union wages within a given state. It is possible that unionization of one institution has spillover effects on the wages of non-unionized institutions within that state. For instance, in order to attract job applicants, a non-unionized institution may have to match salary offers from unionized colleges. Thus, unionization may raise salaries of all instructors (whether they are at unionized or non-unionized schools), leading to a smaller measured union wage premium. This is consistent with the reduction in the estimated union impact when state fixed effects are added to the model. Whether or not fixed effects are included, each of these estimates is substantially less than those found by the only other micro-level survey focusing on two-year schools.

Another caveat is that the relatively small impact unions have on salaries does not indicate that they are ineffective advocates for their members. Faculty unions may bring about better working conditions, amenities, and benefits. They may help enforce contracts, delineate promotion processes, create a grievance structure, or simply help faculty deal with the bureaucratic nature of many institutions. Indeed, absent a wage premium, these potential benefits may be sufficient to offset the cost of union dues. Further, unionization of one subgroup of faculty may have spillover effects for other subgroups. Evaluating the importance of the presence of spillovers of non-pecuniary benefits of collective bargaining is beyond the scope of this paper, but certainly warrants additional investigation.

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