# IS THERE REALLY A FACULTY UNION SALARY PREMIUM?

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Studies of the effects of unions on collegiate faculty salaries are inconclusive. Some estimate a significant union premium, but such estimates suffer from endogeneity between unions and wages, non-random measurement error, and failure to adjust for local cost-of-living differences. By using data from the National Study of Postsecondary Faculty (NSOPF, 1988–2004) as well as other sources to identify institution-specific factors omitted from previous studies, the authors estimate significantly smaller union premia than those found by other researchers.

Conventional wisdom holds that collective bargaining increases the wages of unionized workers relative to their nonunion counterparts. Such wisdom has substantial theoretical and empirical support and appears to apply to both the private and public sectors.[1] In the higher-education labor market, where most unionized institutions are state-operated, the theoretical impact of faculty unionization on wages is less clear. Tuition, a common source of funds for faculty raises, is often capped by external agencies and may be unresponsive to unionization. The bargaining position of university faculty might not be as strong as that of other public employees whose services may be viewed as being more "essential." Further, at some higher education institutions, the stated motivation for collective bargaining has not been to increase faculty wages but rather to better address bureaucratic institutions, create formal grievance processes, and articulate clear criteria for promotion and other personnel actions. At the same time, faculty unions may provide increased opportunities to lobby state legislatures and policy makers for increased funding. Indeed, some faculty unions have joined the large K-12 teachers' unions in hopes of increasing their leverage while promoting increased wages for educators. The absence of a profit motive, moreover, may make university administrations less resistant to union demands.[2] Of course, in some locations the bargaining power of faculty unions is enhanced by the right to strike whereas others lack this right.

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[1] See, for example, Bahrami et al. (2009); Freeman (1986); and Lewis (1990).

[2] See Kaufman and Hotchkiss (2006).

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In the end, the impact of unions on faculty salaries in higher education is an empirical question that has been inadequately addressed in the past.

Since the 1970s, a number of studies with widely varying methodological approaches and results have examined the effects of faculty unions on wages. The earliest studies compared average salaries (or their growth rates) across institutions using data from the American Association of University Professors (AAUP). Of these, Birnbaum (1974, 1976), Morgan and Kearney (1977), and Brown and Stone (1977) found positive union wage premia, whereas Marshall (1979) found none. Later studies applied regression techniques to the AAUP data 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 whereas Kesselring (1991) and Rees (1993) estimated impacts that were zero or negative. In two notable studies, Guthrie-Morse et al. (1981) and Hu and Leslie (1982) found that although unionization is 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*. This distinction is important, given the geographical distribution of faculty unionization. According to data compiled by Moriarty and Savarese (2006: 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. The geographical dispersion of faculty raises the question of whether unions cause higher nominal salaries or whether they are simply more likely to organize in higher-cost (and higher-salary) regions.

A faculty member's salary depends in complex ways on both institution-specific variables and his or her own characteristics. Hence reliance on institution-level data obscures important variation across individuals that could be exploited to estimate more precisely the effects of union representation. Recent studies have benefited from the availability of micro-level survey data on individual faculty members. For example, Barbezat (1989) and Ashraf (1992) used data from the 1977 *Survey of the American Professoriate*; 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 were highly divergent. Barbezat estimated a statistically insignificant union advantage of about 1.3–1.6%. Ashraf provided a range of point 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 a different approach, but these results are based on a sample that includes two-year colleges. This is problematic given Ashraf's (1998) estimate of a statistically significant 8% wage premium for two-year colleges versus zero for all other institutions.

To date, no study has combined repeated observations of institutions with micro-level observations of faculty. Unionization may be correlated with unobservable institutional variables that impact faculty salaries. If this is the case, then previous estimates of the union wage premium will be biased. Moreover, no studies using faculty-level data have controlled for differences in cost of living. Theoretically, such costs are important: we expect wage differentials in equilibrium to reflect the values of individuals' human capital and the costs of obtaining consumption goods and amenities.[3] Empirically speaking, omitting living costs can have important consequences, as is evident from the findings of Guthrie-Morse et al. (1981) and Hu and Leslie (1982). If unionization is positively correlated with living costs, then estimates of union impacts that ignore this relationship will overstate the union wage premium.

In this paper, we address the limitations of past research in several ways. First, we incorporate data from the two most recent rounds of the NSOPF, which have not been exploited by previous researchers. Second, by pooling the four existing NSOPF surveys,

[3] See Dumond et al. (1999).

we identify institution-specific factors omitted from previous studies, the exclusion of which can bias estimates of the union wage premium. Third, we explicitly account for cost-of-living differences to estimate the real salary difference attributable to faculty unions. Fourth, we address measurement error in the NSOPF measure of unionization that likely caused previous researchers to overestimate the wage premium. Fifth, we control for potential endogeneity between wages and unionization. Finally, to acknowledge the large variation across states in the legal environment and attitudes toward unionization, we estimate models with state fixed effects.

### Model Specification

The approach we use in this paper is to estimate a log-wage equation of the form

$$(1) \qquad \ln(Y_{ijts}) = \beta Union_{jts} + \delta X_{ijts} + \gamma Z_{jts} + \alpha S_s + \eta T_t + v_j + \varepsilon_{ijts}$$

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 an indicator variable for 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 represents binary variables representing the individual survey years. The error term in this equation contains two components: $v_j$ represents an institution-specific error and $\varepsilon_{ijts}$ 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 when faculty collectively bargain.

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

$$(2) \qquad \ln(Y_{ij}) = \beta Union_j + \delta X_{ij} + \gamma Z_j + u_{ij}.$$

Equation (2) is a special case of equation (1) that ignores the state-level and time-level heterogeneity and forces the error term $u_{ij}$ to equal $v_j + \varepsilon_{ijts}$. Estimating equation (2) by ordinary least squares (OLS) has three potential shortcomings. First, because the institution-specific error term $v_j$ is common for all faculty members at the same institution, the composite error term $u_{ij}$ is correlated across observations within institutions. OLS ignores this clustering, so estimates of β will be inefficient and standard errors will be incorrect. Second, equation (2) ignores the unobserved institutional-level heterogeneity that can be accounted for by using panel methods. Finally, equation (2) suppresses potentially important unobserved state-level information (e.g., collective bargaining legislation, support of education establishments) that influences 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, 1998, and 2004. Each cycle uses a similar two-stage sampling process. In the first stage institutions are sampled and in the second faculty members are sampled from within the selected institutions.[4] 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 individual surveys, faculty at each institution are asked about their professional experience and background, responsibilities and workload, compensation, demographic characteristics, and opinions.

[4] 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. As we point out in note 16, correcting for this oversampling using NSOPF weights does not significantly alter regression coefficients.

Sample sizes for the original NSOPF and for the sample used in our analysis are given in Table 1. Over all four cycles, a total of 78,310 faculty and 1,900 institutional questionnaires have been returned.[5] The NSOPF queries instructors, lecturers, tenure-track, and tenured faculty members at two-year and four-year institutions of higher education. Previous studies (e.g., Ashraf (1998, 1999)) have suggested that differential impacts of unionization can be expected across institution type and faculty status. In order to estimate these effects as precisely as possible, we focus our analysis on full-time faculty members at four-year colleges and universities, which reduces the sample to 44,330 faculty observations at 1,160 institutions. Of these, we exclude 9,220 faculty whose principal activity is neither teaching nor research, 3,630 who had missing explanatory variables, and 200 whose institution failed to indicate whether faculty on their campus collectively bargain. As part of the survey process, the NSOPF replaces missing observations with imputed data. An additional 6,680 observations generated in this manner were omitted. Finally, we excluded 530 faculty whose basic salary received from their institution was less than $20,000 or greater than $350,000 in 2004 dollars.[6] After deleting these observations, a final sample of 24,070 faculty at 1,060 different institutions remains. Table 2 shows the panel structure of the institutional observations over time. About half of all institutions are observed once, a quarter are observed twice, about a sixth are observed three times, and eight percent appear in all four surveys.

### Measures of Unionization

The institutional questionnaire asks the following question:

*Q1*: Are any full-time faculty and instructional staff legally represented by a union (or other association) for purposes of collective bargaining?

In all previous union research using the NSOPF, *Q1* is used to construct a binary variable (called *Union* in this paper) that is equal to one if *Q1* is answered positively. Utilizing this institutional-level variable, however, introduces a severe measurement problem. Consider, for example, the University of California system, in which adjuncts engage in collective bargaining but tenure-track faculty do not.[7] Administrators at these institutions answered *Q1* in the affirmative. Under the NSOPF methodology, this affirmative answer was assigned to all faculty at those institutions, resulting in a systematic measurement error for tenure-track faculty. A similar problem arises in the state of Florida, where faculty unions represent permanent faculty and librarians but not adjuncts. Administrators in Florida's public higher education institutions responded to *Q1* positively, so that *Union* was incorrectly assigned a value of one for adjuncts at these institutions. If the measurement error were random and independent of the true value of *Union*, then estimates of $\beta$ would be attenuated toward zero.[8] However, the measurement error implied by *Q1* is not random. Under this survey scheme, an

[5] As we note elsewhere, many institutions were sampled multiple times. It is possible that individual faculty members may have been sampled more than once, but this would be purely coincidental, and the NSOPF does not identify which, if any, faculty were repeatedly sampled. Faculty and institution counts are rounded to the nearest 10 to comply with NCES confidentiality requirements.

[6] The rationale for excluding very high and very low incomes is threefold. First, these exclusions are similar to those used by Monks (2000), and using similar exclusions allows easier comparisons with prior literature. Second, faculty at the extreme ends of the salary distribution are more likely to represent coding errors or "non-regular" faculty. For example, in the 1988 and 1993 surveys (the only years for which data exist on the duration of faculty contracts), more than 60% of faculty with salaries below $20,000 held single-term appointments. Similarly, faculty whose earnings are at the highest salary levels are more likely to hold administrative or non-teaching positions. Third, when the sample is expanded to include incomes below $20,000 or above $350,000, the qualitative conclusions of the paper do not change.

[7] The one exception in the UC system is at Santa Cruz, where tenure-track faculty are represented by a union as well.

[8] Moreover, the coefficients on the other explanatory variables would be biased in unpredictable ways. For more on these points, see Greene (2008: 325–27).

*Table 1.* Sample Size

| | Number of Institutions | | Number of Faculty | |
|---|---|---|---|---|
| *Year* | *NSOPF* | *Sample* | *NSOPF* | *Sample* |
| 1988 | 450 | 310 | 8,380 | 3,840 |
| 1993 | 970 | 480 | 25,780 | 6,970 |
| 1999 | 890 | 490 | 18,040 | 4,580 |
| 2004 | 1,020 | 590 | 26,110 | 8,670 |
| **Sum** | **1,900** | **1,060** | **78,310** | **24,070** |

*Note:* Sample sizes are rounded to the nearest 10 to comply with NCES disclosure requirements. Columns may not sum to totals due to rounding. Because many institutions are repeatedly sampled over different years, the total number of institutions does not equal the sum of institutions over the four years.

*Table 2.* Replication Pattern for Institutions

| *Times Observed* | *Survey Year* | *Number of Institutions* | | *Percent of Institutions* | |
|---|---|---|---|---|---|
| 4 | 1988, 1993, 1999, 2004 | 80 | 80 | 8 | 8 |
| 3 | 1988, 1993, 1999 | 20 | 160 | 2 | 15 |
| | 1988, 1993, 2004 | 20 | | 2 | |
| | 1988, 1999, 2004 | 30 | | 3 | |
| | 1993, 1999, 2004 | 90 | | 8 | |
| 2 | 1988, 1993 | 20 | 240 | 2 | 23 |
| | 1988, 1999 | 30 | | 3 | |
| | 1988, 2004 | 20 | | 2 | |
| | 1993, 1999 | 50 | | 5 | |
| | 1993, 2004 | 50 | | 5 | |
| | 1999, 2004 | 70 | | 7 | |
| 1 | 1988 | 90 | 570 | 8 | 54 |
| | 1993 | 140 | | 13 | |
| | 1999 | 120 | | 11 | |
| | 2004 | 220 | | 21 | |
| **Totals** | | **1,060** | **1,060** | **100** | **100** |

*Note:* Sample sizes are rounded to the nearest 10 to comply with NCES disclosure requirements. Columns may not sum to totals due to rounding.

institution without any union representation will be correctly assigned a value of zero for *Union*. Institutions with some portion of their faculty represented by a union will assign a value of one to *Union* for all faculty members, regardless of each subgroup's true collective bargaining status. Thus, individual faculty observations of *Union* equal to zero are likely to be correct whereas observations of *Union* equal to one are potentially incorrect.

The impact of this systematic measurement error on the *Union* coefficient is ambiguous. In the UC system (and institutions with a similar unionization structure), using *Union* causes a false positive to be disproportionately assigned to higher-paid tenure-track faculty. In the Florida system (and institutions with a similar structure), however, *Union* produces a false positive on lower-paid adjunct professors. Thus, if more observations fall into the "California" than into the "Florida" situation, the *Union* coefficient is likely to be overestimated. If there are more Florida-type observations than "California" ones, $\beta$ will be underestimated.

To avoid this measurement problem, we make use of 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 Savarese (2006). The NCSCBHEP data identify the date of initial collective bargaining for four faculty subgroups within all U.S. institutions: full-time permanent faculty, part-time permanent faculty, adjuncts, and librarians. Using these data, we construct a variable (*Unionsubgroup*) that corrects the misclassification of unionization at institutions that write collective bargaining agreements with portions of their instructional staff. *Unionsubgroup* equals one if the individual faculty member's subgroup at an institution is part of a collective bargaining agreement. To be clear, *Unionsubgroup* correctly assigns values of one to adjuncts and zero to tenured and tenure-track individuals in the UC system, and zero to adjuncts and one to permanent faculty at the Florida institutions. Since the NCSCBHEP data are sub-institutional data, *Unionsubgroup* does not suffer from systematic measurement error that arises from an overly broad survey question.[9]

### Measures of Salary

The NSOPF faculty survey asks numerous questions regarding the financial compensation of individuals, including the value of the basic contract and opportunities for faculty to earn additional income from their institution. 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 was 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, overload courses, and internal research monies. 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. In this case, unionization would increase *Total Salary* relative to *Basic Salary*. Alternatively, a union may frown on such payments and bargain to curtail them, or they may bargain into the basic contract what were previously considered extra duties in exchange for increased *Basic Salary*, resulting in a smaller difference between *Basic Salary* and *Total Salary*. For the entire sample, the correlation between *Basic Salary* and *Total Salary* is 0.92, 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 strong geographical pattern of unionization. In the NSOPF data, the mid-Atlantic Census region and California contain 46% of all unionized faculty observations but only 23% of total faculty observations, suggesting that on average, faculty are much more likely to collectively bargain if they live in these areas. Since the cost of living in these regions is relatively high, failure to account for those differences can cause the union wage premium to be overestimated.[10]

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, which provides a comprehensive measure of

[9] To illustrate the magnitude of the problem, of the 24,070 observations in our sample, *Union* incorrectly assigns a value of one to 1,030 faculty members. Of these, 270 reside in California and 90 in Florida. In inflation-adjusted 2004 dollars, the mean of basic salary for the 1,030 false positives is $66,387, in comparison with a mean of $63,720 for the 4,490 faculty for whom both *Union* and *Unionsubgroup* assign values of one. Hence it appears that use of the *Union* variable exaggerates the salary difference attributable to union status relative to the more precisely measured *Unionsubgroup*.

[10] 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.

living cost. The ACCRA index, however, is compiled only for metropolitan areas. Thus, use of the ACCRA data would eliminate from our analysis 7,380 individuals at 290 non-metropolitan institutions. These observations comprise 31% of our combined sample of 24,070.

In order to retain the rural data in our sample and to improve the precision of our regression estimates, we 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 $t$.[11] Our cost-of-living index is defined as

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

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 (www.bls.gov/cex/home.htm). The coefficient of 0.7 ensures that the *Rent Index* equals one when a county's quality-adjusted apartment rents equal the national median. Our use of the *Rent Index* is justified in two ways. First, most of the geographical variation in the ACCRA index is due to differences in housing costs. As evidence of this relationship, a regression of the ACCRA index on all of its own components (housing, groceries, utilities, transportation, healthcare, and miscellaneous) yields a standardized coefficient of 0.70 on housing; a regression of the ACCRA index on its housing component alone has a slope coefficient of 0.40 with a standardized value of 0.98 and an $R^2$ of 0.95. Clearly, geographical variation in housing costs explains more of the variation in total cost of living than what is suggested by its 30% weight in the index. Second, the *Rent Index* approximates the ACCRA index fairly well. For observations with valid ACCRA and rent data, the correlation between these variables is 0.62.

Because the ACCRA index is not measured in rural areas, we have no way to check whether the *Rent Index* is a reasonable measure of cost-of-living differences in these locations. There are, however, reasons to believe that the *Rent Index* under-corrects for living costs relative to ACCRA. First, to the extent that other local costs of living are positively correlated with rents, variation in the *Rent Index* will understate differences in these costs. Second, the distribution of the ACCRA index is highly skewed and is substantially underestimated by the *Rent Index* in a few dozen of the highest-cost counties in the country. These counties are located in the states of New York, New Jersey, and California, which happen to be three of the states with the highest rates of faculty unionization. Hence, the results presented in the next section using rent-adjusted salaries will tend to overestimate the union salary premium relative to an adjustment procedure that more accurately measures true cost-of-living differences. Though we rely most heavily on the *Rent Index* because of its inclusion of both rural and urban areas, we also report some results using the ACCRA data for comparison.

The *Rent Index* is used to construct two rent-adjusted salary measures: *RA Basic Salary* = *Basic Salary* / *Rent Index* and *RA Total Salary* = *Total Salary* / *Rent Index*. In this paper, we refer to the technique of dividing salaries by a cost-of-living index as the "complete" adjustment process.

Dumond et al. (1999) 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, or in workers being willing to accept lower wage offers, or both. Thus, higher prices in more-desirable locations may overstate the cost of achieving a given utility level. In addition, higher prices induce consumers to change their utility-maximizing consumption bundle so that 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*

[11] We use rent data from the 1990 Census for the 1988 and 1993 NSOPF surveys, and from the 2000 Census for the 1999 and 2004 surveys.

(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. recommended a regression-based partial-adjustment procedure that uses $\ln Y$ as the dependent variable and $\ln P$ and its square as explanatory variables.[12] Because it does not restrict the regression coefficients on prices, this partial-adjustment method is preferable over either the complete-adjustment or no-adjustment approaches. To be thorough, we report the results of all three methods below.

**Descriptive statistics**

As a first look at the union wage premium, consider the descriptive statistics presented in Table 3. The first two rows of Table 3 show means and standard deviations of *Basic Salary* and *Total Salary* paid to faculty by collective bargaining status as defined by *Union-subgroup*. Unionized faculty average $3,680 (6.1%) more *Basic Salary* and $3,851 (6.0%) more *Total Salary* than non-unionized faculty.[13] After dividing these salary measures by the *Rent Index*, these differences fall by more than 40%, to $2,074 (3.4%) and $2,152 (3.3%), respectively. This dramatic drop suggests the importance of accounting for cost-of-living differences even when using an index that likely understates the true cost of living. 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 more likely to hold the rank of full professor, and are more likely to be at public institutions[14] and at schools with higher student enrollments.

An important drawback of the data is the small amount of time variation in unionization. Although 21.9% of observed institutions and 18.9% of observed faculty engage in collective bargaining, fewer than ten of 1,060 observed institutions changed collective bargaining status within the four periods in our sample. This implies that identification of the union wage premium relies upon variation in union status between institutions rather than variation within institutions.

## Econometric Evidence

To facilitate comparison with previous studies, we first estimate equation (2) using ordinary least squares (OLS) to regress *Basic Salary* and *Total Salary* on *Union*, X, and Z. The explanatory variables in X contain all faculty variables listed in Table 3, 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 represent total student full-time equivalent (FTE) enrollment and its square, six binary variables indicating the Carnegie Classification of the institution (doctoral granting, comprehensive, liberal arts) interacted with public or private control, and time binaries. In order to make direct comparison with past studies, we first use *Union* as the measure of collective bargaining.

Panel A of Table 4 presents results using *Basic Salary* as the dependent variable. The union premium declines steadily over time, from 8.5% in 1988 to 6.3% in 2004. The 7.8% union premium estimated using the 1993 survey is close to Monks's (2000) estimate of

[12] 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 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).

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

[14] The higher incidence of unionization at public institutions is a result of the U.S. Supreme Court decision in NLRB v. Yeshiva University, 444 U.S. 672 (1980). In this case, the Court ruled that faculty at Yeshiva University were essentially managerial employees and therefore lacked collective bargaining rights given to private-sector employees under the National Labor Relations Act. Collective bargaining by faculty at public institutions is governed by state law and is not subject to the Yeshiva decision.

*Table 3*. Descriptive Statistics: Sample Means
(Standard Deviations in Parentheses)

| | | *Unionized* | | *Non-Unionized* |
|---|---|---|---|---|
| Basic Salary | Real salary, 2004 base year | 63,622 | > | 59,942 |
| | | (21,403) | | (27,161) |
| Total Salary | Real payments, 2004 base year | 68,342 | > | 64,491 |
| | | (24,222) | | (31,520) |
| RA Basic Salary | Basic Salary divided by *Rent Index* | 62,706 | > | 60,632 |
| | | (21,065) | | (26,337) |
| RA Total Salary | Total Salary divided by *Rent Index* | 67,381 | > | 65,229 |
| | | (23,895) | | (30,741) |
| Exp | Years of experience at current institution | 12.61 | > | 10.61 |
| | | (9.90) | | (9.57) |
| Degexp | Years of experience since earning highest degree | 16.40 | > | 15.13 |
| | | (10.14) | | (10.18) |
| Female | Binary = 1 if female | .348 | = | .360 |
| | | (.476) | | (.480) |
| Married | Binary = 1 if currently married | .727 | < | .745 |
| | | (.446) | | .435 |
| Wasmarried | Binary = 1 if previously married | .111 | = | .104 |
| | | (.315) | | (.305) |
| Hispanic | | .052 | > | .040 |
| | | (.221) | | (.197) |
| Indian | | .011 | = | .009 |
| | | (.107) | | (.098) |
| Asian | | .077 | > | .060 |
| | | (.267) | | (.237) |
| Black | | .046 | < | .054 |
| | | (.210) | | (.225) |
| Pacific | | .002 | > | .001 |
| | | (.044) | | (.031) |
| Lecturer | Binary = 1 if academic rank is lecturer | .027 | = | .030 |
| | | (.161) | | (.171) |
| Instructor | Binary = 1 if academic rank is instructor | .033 | < | .069 |
| | | (.179) | | (.254) |
| Assistant | Binary = 1 if academic rank is assistant professor | .252 | < | .306 |
| | | (.434) | | (.461) |
| Associate | Binary = 1 if academic rank is associate professor | .293 | > | .271 |
| | | (.455) | | (.444) |
| Full | Binary = 1 if academic rank is professor | .384 | > | .302 |
| | | (.486) | | (.459) |
| Tenured | Binary = 1 if tenured | .674 | > | .517 |
| | | (.468) | | (.499) |
| Tentrack | Binary = 1 if on tenure track | .251 | = | .262 |
| | | (.433) | | (.439) |
| Bachelors | Binary = 1 if highest degree earned is a bachelors | .009 | < | .013 |
| | | (.095) | | (.113) |
| Masters | Binary = 1 if highest degree earned is masters | .163 | < | .200 |
| | | (.369) | | (.400) |
| Profession | Binary = 1 if highest degree is professional | .041 | < | .059 |
| | | (.197) | | (.235) |

*continued*

*Table 3.* Descriptive Statistics: Sample Means (Standard Deviations in Parentheses) Continued

| | | *Unionized* | | *Non-Unionized* |
|---|---|---|---|---|
| Doctorate | Binary = 1 if highest degree earned is Ph.D. or equivalent | .786 | > | .727 |
| | | (.409) | | (.445) |
| Citizen | Binary = 1 if U.S. citizen | .911 | = | .916 |
| | | (.284) | | (.277) |
| Funded | Binary = 1 if scholarly activity is funded by external agency | .314 | < | .345 |
| | | (.464) | | (.475) |
| Firstjob | Binary = 1 if current job is first since graduating | .388 | = | .402 |
| | | (.487) | | (.490) |
| Articles | Total refereed articles published | 14.11 | < | 15.08 |
| | | (26.35) | | (29.32) |
| Nonref | Total non-refereed articles published | 8.06 | > | 7.20 |
| | | (37.86) | | (22.35) |
| Books | Total books published | 4.12 | = | 3.91 |
| | | (13.26) | | (15.99) |
| Presentation | Total research presentations made | 35.98 | = | 37.97 |
| | | (86.96) | | (96.95) |
| Pubdoc | Binary = 1 if institution is public, doctoral granting | .360 | > | .338 |
| | | (.480) | | (.473) |
| Privdoc | Binary = 1 if institution is private, doctoral granting | .018 | < | .144 |
| | | (.133) | | (.351) |
| Pubcomp | Binary = 1 if institution is public, comprehensive | .502 | > | .175 |
| | | (.500) | | (.379) |
| Privcomp | Binary = 1 if institution is private, comprehensive | .035 | < | .116 |
| | | (.185) | | (.320) |
| Publa | Binary = 1 if institution is public, liberal arts | .025 | > | .012 |
| | | (.157) | | (.111) |
| Privla | Binary = 1 if institution is private, liberal arts | .025 | < | .143 |
| | | (.158) | | (.350) |
| Enrollment | Total Student FTE (thousands) | 12.75 | > | 11.08 |
| | | (9.25) | | (10.63) |
| N | Number of Faculty Observations | 4,560 | | 19,510 |
| $N_j$ | Number of Institutions | 210 | | 960 |

*Note:* >,< represent statistical differences using a paired t-test at the .05 level.

7.3% using the same survey.[15] When all four surveys are pooled, the estimated union premium is 7.4%. All *Basic Salary* estimates are, like those of Ashraf (1998, 1999) and Monks, statistically significant and economically important.[16]

[15] Our sample differs from that of Monks in that he included two-year schools and excluded faculty who earned less than $1,000 or more than $300,000 in 1999 dollars (as opposed to our sample, which excludes observations less than $20,000 or greater than $350,000 in 2004 dollars). We also augment Monks's independent variables by adding institutional enrollment and its square, and we use faculty field codes for 32 disciplines in place of Monks's ten broader field variables.

[16] The NSOPF provides faculty and institutional weights that, if used, can simulate a representative sample of the nation's professoriate. Using these weights, however, precludes correcting the standard errors of regression results for clustering at the institutional level. We estimated all econometric models reported in this paper using weighted regressions but not correcting for clustering. In the pooled OLS results of Table 4, estimates of the union coefficient differed only at the third decimal place. Standard errors using the weighted data not corrected for clustering were typically 30% smaller than

*Table 4.* Estimates of Wage Premium Using Union

| *Sample* | *1988* | *1993* | *1999* | *2004* | *Pooled* | *RE* |
|---|---|---|---|---|---|---|
| | | | **Panel A: Basic Salary** | | | |
| β | .085*** | .078*** | .066*** | .063*** | .074*** | .056*** |
| | (.018) | (.014) | (.014) | (.013) | (.010) | (.008) |
| $R^2$ | .662 | .601 | .644 | .653 | .636 | .629 |
| | | | **Panel B: Total Salary** | | | |
| β | .081*** | .071*** | .065*** | .062*** | .070*** | .053*** |
| | (.017) | (.014) | (.013) | (.013) | (.009) | (.008) |
| $R^2$ | .645 | .569 | .632 | .648 | .621 | .616 |
| N | 3,840 | 6,970 | 4,580 | 8,670 | 24,070 | 24,070 |
| $N_j$ | 310 | 480 | 490 | 590 | 1,060 | 1,060 |

*Notes:* All regressions contain the independent variables listed in Table 3, squares of institutional and degree experience, the square of enrollment, time binaries, and binaries for 32 academic fields. Robust standard errors corrected for clustering within institution are presented in parenthesis.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

The final column of Table 4 presents random-effects estimates of the pooled *Union* wage premium. The estimate of 5.6% is considerably smaller than the pooled OLS estimate. The random effects estimator uses the inherent panel nature of the data by explicitly accounting for the institutional-specific error term $v_j$. A drawback of random effects is the assumption that the $v_j$ are uncorrelated with the independent variables—an assumption that is problematic if unobserved institutional characteristics influence faculty unionization. It would be preferable to use the fixed-effects estimator; however, the small amount of within-institution variation in *Union* renders this estimator impracticable.

Panel B of Table 4 presents the *Total Salary* union premium. Again, the highest estimate of the union premium occurs in 1988 and then declines steadily over subsequent surveys. This lends some credence to the hypothesis that the impact of faculty unions may have fallen over time and is consistent with Barbezat (1989), who found that the wage impact of unionization peaks about four years after organization and dies out after an additional four years. Relative to the estimates using *Basic Salary*, the estimated *Total Salary* premium is smaller in individual years and for the pooled estimates, a pattern that repeats itself in subsequent estimations. 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.

The estimates in Table 4 have at least three shortcomings. As previously explained, they do not account for measurement error in *Union*, for local cost-of-living differences, or for unobserved state-level heterogeneity. The importance of these concerns is demonstrated in Table 5. Here, we examine the *Basic Salary* premium by substituting *Unionsubgroup* for *Union*, incrementally adding cost-of-living adjustments and state-level fixed effects. Comparing the annual estimates in Panel A of Table 5 with those in Panel A of Table 4, the wage premium based upon *Unionsubgroup* is between 0.4 and 1.5 percentage points smaller than that found using *Union*. The difference is even larger in the pooled random-effects estimate, which drops from 5.6% to 3.8% when *Unionsubgroup* is employed. Since *Union* and *Unionsubgroup* differ only in that *Unionsubgroup* correctly measures the bargaining status of

those using the correction. Similar differences occurred in the estimates of β presented in subsequent tables.

*Table 5.* Estimates of Wage Premium Using *Unionsubgroup* and *Basic Salary*

| *Sample* | *1988* | *1993* | *1999* | *2004* | *Pooled* | *RE* |
|---|---|---|---|---|---|---|
| **Panel A: No Cost-of-Living Adjustment** | | | | | | |
| β | .071*** | .074*** | .061*** | .053*** | .066*** | .038*** |
| | (.016) | (.014) | (.015) | (.014) | (.010) | (.008) |
| $R^2$ | .658 | .600 | .643 | .652 | .634 | .627 |
| **Panel B: Complete Rent-Adjusted Cost of Living** | | | | | | |
| β | .022 | .022* | .030** | .011 | .022** | .011 |
| | (.016) | (.013) | (.014) | (.011) | (.008) | (.007) |
| $R^2$ | .652 | .585 | .627 | .643 | .623 | .617 |
| **Panel C: Partial Rent-Adjusted Cost of Living** | | | | | | |
| β | .033** | .035** | .043*** | .018 | .033*** | .019** |
| | (.016) | (.014) | (.015) | (.012) | (.009) | (.008) |
| $R^2$ | .676 | .618 | .650 | .666 | .648 | .643 |
| **Panel D: Partial Rent-Adjusted Cost of Living and State Fixed Effects** | | | | | | |
| β | –.014 | .007 | –.004 | –.032** | –.008 | –.007 |
| | (.021) | (.016) | (.016) | (.016) | (.010) | (.008) |
| $R^2$ | .700 | .641 | .666 | .684 | .663 | .657 |
| F-test of State FE | 24.63*** | 5.15*** | 8.45*** | 6.44*** | 7.64*** | |
| N | 3,840 | 6,970 | 4,580 | 8,670 | 24,070 | 24,070 |
| $N_j$ | 310 | 480 | 490 | 590 | 1,060 | 1,060 |

*Notes:* See notes to Table 4. F-test of State fixed effects reports an F-test on the null hypothesis that the included state fixed effects have no impact on faculty *Basic Salary*.

each job classification on each campus, these results suggest an upward bias in the use of *Union* from misclassifying higher-paid faculty as a result of the problematic institutional-level question *Q1*.

Panels B and C of Table 5 indicate the importance of accounting for local cost-of-living differences. The 6.6% pooled OLS estimate of Panel A falls to 2.2% when using the complete adjustment process and to 3.3% when using the partial rent adjustment process. Similar large declines occur in the individual years and in the random-effects estimator. Not surprisingly, the partial-adjustment approach produces estimates that are somewhere between those from the no-adjustment and complete-adjustment approaches. Note also that after adjusting for cost of living, the apparent decline in the union premium over time is less evident, although in 2004 it is smaller than in previous years and statistically insignificant.

As Freeman and Valletta (1988) suggested for public employees in general and Hosios and Siow (2004) suggested for higher education faculty specifically, the legislative environment in which the faculty union operates can have significant effects on the union wage premium. There are differences across states in the legality and the scope of bargaining, the duty of universities to bargain, and the right to strike. They are large and difficult to measure. In order to account for their impacts, we introduce state fixed effects. Of course, these fixed effects also capture impacts on faculty wages from differences in state support for higher education, public sentiment toward collective bargaining, and other state-level, unobserved, time-invariant factors. In the presence of state fixed effects, β is best thought of as a within-state estimator that reveals the union wage premium relative to faculty without collective bargaining agreements within the same state.

*Table 6.* Estimates of Wage Premium Using *Unionsubgroup* and *Total Salary*

| *Sample* | *1988* | *1993* | *1999* | *2004* | *Pooled* | *RE* |
|---|---|---|---|---|---|---|
| **Panel A: No Cost-of-Living Adjustment** | | | | | | |
| β | .065*** | .066*** | .058*** | .050*** | .061*** | .036*** |
| | (.016) | (.014) | (.015) | (.013) | (.009) | (.008) |
| $R^2$ | .641 | .568 | .630 | .646 | .619 | .614 |
| **Panel B: Complete Rent-Adjusted Cost of Living** | | | | | | |
| β | .017 | .015 | .027* | .008 | .017** | .008 |
| | (.015) | (.013) | (.014) | (.011) | (.008) | (.008) |
| $R^2$ | .636 | .553 | .615 | .638 | .608 | .604 |
| **Panel C: Partial Rent-Adjusted Cost of Living** | | | | | | |
| β | .027** | .029** | .040*** | .014 | .027*** | .016** |
| | (.015) | (.014) | (.015) | (.012) | (.009) | (.008) |
| $R^2$ | .658 | .584 | .637 | .660 | .632 | .628 |
| **Panel D: Partial Rent-Adjusted Cost of Living and State Fixed Effects** | | | | | | |
| β | –.023 | –.002 | –.006 | –.034** | –.014 | –.011 |
| | (.020) | (.017) | (.017) | (.016) | (.010) | (.009) |
| $R^2$ | .680 | .605 | .651 | .675 | .645 | .640 |
| F-test of State FE | 50.97*** | 6.33*** | 5.67*** | 7.62*** | 7.64*** | |
| N | 3,840 | 6,970 | 4,580 | 8,670 | 24,070 | 24,070 |
| $N_j$ | 310 | 480 | 490 | 590 | 1,060 | 1,060 |

*Notes:* See notes to Table 4. F-test of State fixed effects reports an F-test on the null hypothesis that the included state fixed effects have no impact on faculty *Total Salary*.

The partial rent-adjustment estimates augmented by state-level fixed effects are presented in Panel D of Table 5. The importance of state fixed effects is plain. The largest union coefficient is 0.7% (1993 *Basic Salary*); and all are statistically insignificant except the 2004 estimate, which is both negative and statistically significant. Further, for each regression, F-tests reject the null hypothesis that state fixed effects do not explain variation in faculty wages. Though we cannot be certain that state fixed effects are good proxies for a state's legal environment, the insignificant βs in the presence of these suggests that there are no within-state wage premia earned by faculty under collective bargaining agreements.

Table 6 applies the estimation routines of Table 5 to *Total Salary*. As in the case of Table 5, the estimated wage premium is significantly reduced by using *Unionsubgroup* rather than *Union* and is reduced further by accounting for local cost-of-living differences. For the preferred partial adjustment process, almost all estimates fall by more than half relative to the uncorrected measures presented in Table 4. The OLS pooled estimate of 2.7% and the random-effects estimate of 1.6% are close to the average annual dues charged by faculty unions of around 1%.[17] Like those of Table 5, estimates of the union premium are negative and

[17] There are apparently no comprehensive surveys of faculty union dues. However, in the two most heavily unionized states, the California Faculty Association (which represents faculty in the California State University system) and the New York Professional Staff Congress (representing faculty in the City University of New York system), dues are charged in an amount equaling 1.05% of gross salary (see www.calstatela.edu/univ/cfa/join_cfa.htm and www.psc-cuny.org/membership.htm). The United University Professions (representing faculty in the State University of New York system) charges 1% (see www.uupinfo.org/membership/memberapp.pdf).

statistically no different than zero (except for the 2004 sample) in the presence of state fixed effects. Table 6 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 5, every estimate of the *Total Salary* wage premium is smaller.

A natural question to ask is whether these results might be sensitive to the use of *Rent Index* rather than to the more comprehensive ACCRA index as a measure living costs. To check this, we ran the *Unionsubgroup* and *Basic Salary* regressions as reported in Table 5, but we employed the ACCRA cost-of-living adjustment on the subsample for which it is available.[18] The results are presented in Table 7. Notably, the values are almost all lower than those in Table 5, which suggests that the *Rent Index* employed in Table 5 is a conservative measure of cost-of-living differences. Comparing the no-cost-of-living-adjusted estimates in Panel A of the two tables, those in Table 7 average about 1.1 percentage points smaller than those from the full sample in Table 5, but they remain positive and statistically significant. The complete-adjustment approach in Panel B, however, results in statistically significant *negative* union premia in all years and in the pooled sample—on average about 11.4 percentage points lower than the rent-adjusted estimates. This is no doubt a consequence of the skewness of the ACCRA index previously noted. 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. With the preferred partial-adjustment procedure in Panel C, the random-effects estimates and those from 2004 are negative, and none are statistically different from zero. When state fixed effects are added in Panel D, all point estimates other than those for 1993 are negative, and all but the random-effects estimate are statistically no different from zero. These results are statistically similar both to those of Panel C and to those of Panel D of Table 5, indicating that when a comprehensive cost-of-living measure such as the ACCRA index is used to adjust nominal wages, the absence of a significant union premium does not depend critically on the inclusion of state fixed effects.[19]

### Endogeneity

The discussion of state fixed effects above suggests the possibility that inter- and intrastate differences in attitudes and legislation may contribute to a nonrandom pattern of unionization across institutions. As a matter of fact, Freeman (1978) found that the estimated union wage premium overstates the union impact because unionized schools tend to have been higher-paying prior to organization. However, one can imagine faculty demand for unionization to be negatively correlated with their compensation. If low wages induce faculty to unionize, then estimates of the union premium ignoring this will be biased downward. Conversely, if there are unobserved variables that result in both higher salaries and higher probabilities of unionization, then the effects of those variables will be incorrectly attributed to the measure of union, and estimates will be biased upward. To correct for the possibility of such biases, we need an instrument that is correlated with unionization and uncorrelated with the error term in equation (1).

A good candidate for such an instrument is the prior level of unionization in the state in which a faculty member lives. States with histories of high levels of unionization out-

[18] Results using *Total Salary*, analogous to Table 6, are very similar and are available on request by emailing john.krieg@wwu.edu.

[19] In addition to measuring the effects of state-level unobservables such as the legislative environment and support for higher education, the state dummies may also partially reflect interstate differences in living costs that are not measured by our *Rent Index* relative to the more broadly defined but geographically limited ACCRA index.

*Table 7.* Estimates of Wage Premium Using *Unionsubgroup*, *Basic Salary*, and ACCRA

| *Sample* | *1988* | *1993* | *1999* | *2004* | *Pooled* | *RE* |
|---|---|---|---|---|---|---|
| **Panel A: No Cost-of-Living Adjustment** | | | | | | |
| β | .065*** | .059*** | .037** | .050*** | .057*** | .024** |
| | (.023) | (.021) | (.019) | (.017) | (.013) | (.010) |
| $R^2$ | .666 | .594 | .630 | .654 | .630 | .623 |
| **Panel B: Complete ACCRA-Adjusted Cost of Living** | | | | | | |
| β | –.056* | –.105*** | –.092*** | –.129*** | –.100*** | –.084*** |
| | (.030) | (.028) | (.027) | (.022) | (.017) | (.012) |
| $R^2$ | .630 | .490 | .554 | .621 | .569 | .560 |
| **Panel C: Partial ACCRA-Adjusted Cost of Living** | | | | | | |
| β | .028 | .011 | .006 | –.016 | .006 | –.009 |
| | (.023) | (.019) | (.017) | (.015) | (.012) | (.010) |
| $R^2$ | .683 | .613 | .638 | .671 | .645 | .640 |
| **Panel D: Partial ACCRA-Adjusted Cost of Living and State Fixed Effects** | | | | | | |
| β | –.038 | .005 | –.008 | –.026 | –.007 | –.026** |
| | (.029) | (.021) | (.020) | (.019) | (.013) | (.010) |
| $R^2$ | .708 | .632 | .651 | .685 | .656 | .651 |
| F-test of State FE | 20.59*** | 11.14*** | 9.74*** | 8.28*** | 10.64*** | |
| N | 2,360 | 4,810 | 3,360 | 6,170 | 16,690 | 16,690 |
| $N_j$ | 200 | 340 | 360 | 430 | 770 | 770 |

*Note:* See notes to Table 4.

side higher education are more likely to have sentiment, legislation, and policies making unionization of faculty more likely. To capture this effect, we use as an instrument for *Unionsubgroup* the 1964 percentage of nonagricultural workers in an institution's state that were union members (*Mem64*).[20] We choose 1964 because among all four-year institutions listed by the NCSCBHEP data, unionization began in the late 1960s, peaked in the early 1970s, and was virtually complete by the mid-1980s. By choosing our instrument to predate faculty unionization, we eliminate the possibility that faculty unions caused unionization in other areas of the state. Thus, 1964 state union membership should be highly correlated with faculty unionization but sufficiently distant in the past to be uncorrelated with the error term in the faculty salary equation during the sample period. We use Wooldridge's (2002: 623) recommended two-stage instrumental-variables technique. At the first stage, we estimate a logit equation in which *Unionsubgroup* is regressed on *Mem64* and the other explanatory variables in the wage equation; we then employ the predicted probability from this logit as an instrument for *Unionsubgroup* in a second-stage IV regression. To avoid bias and loss of precision, and to facilitate comparison with Tables 5 and 6, we estimate these regressions using the full sample.

Results of this procedure are reported in Table 8. First consider Panel C at the bottom of the table, which reports the logit coefficients on *Mem64* from the first stage regression. These coefficients are all positive and statistically significant at the .01 level, indicating that *Mem64* is a strong predictor of

[20] This is taken from Hirsch et al. (2001) and available under the title "State Union Membership Density in the U.S., 1964–2008" at unionstats.gsu.edu.

*Table 8.* Instrumental-Variables Estimates of the Union Wage Premium

| *Sample* | *1988* | *1993* | *1999* | *2004* | *Pooled* | *RE* |
|---|---|---|---|---|---|---|
| **Panel A: Basic Salary** | | | | | | |
| No Cost of Living Adjustment | .021 | .038 | –.019 | –.023 | .022 | .042 |
| | (.048) | (.036) | (.036) | (.034) | (.026) | (.028) |
| $R^2$ | .699 | .638 | .664 | .681 | .661 | .654 |
| Complete Cost of Living Adjustment | –.087 | .020 | –.017 | –.010 | .031 | .049* |
| | (.053) | (.037) | (.037) | (.034) | (.027) | (.029) |
| $R^2$ | .667 | .604 | .642 | .659 | .633 | .626 |
| Partial Cost of Living Adjustment | –.027 | .020 | .0005 | –.028 | –.0007 | .018 |
| | (.046) | (.033) | (.033) | (.032) | (.025) | (.024) |
| $R^2$ | .700 | .641 | .666 | .684 | .663 | .658 |
| **Panel B: Total Salary** | | | | | | |
| No Cost of Living Adjustment | .010 | .017 | –.037 | –.029 | .003 | .027 |
| | (.048) | (036) | (.037) | (.035) | (.027) | (.029) |
| $R^2$ | .679 | .602 | .649 | .672 | .643 | .638 |
| Complete Cost of Living Adjustment | –.098* | –.0006 | –.035 | –.016 | .012 | .034 |
| | (.054) | (.037) | (.037) | (.033) | (.026) | (.030) |
| $R^2$ | .652 | .571 | .628 | .652 | .618 | .613 |
| Partial Cost of Living Adjustment | –.046 | .00006 | –.019 | –.038 | –.021 | –.001 |
| | (.047) | (.033) | (.034) | (.032) | (.025) | (.025) |
| $R^2$ | .680 | .605 | .651 | .675 | .645 | .641 |
| **Panel C: First-Stage Logit** | | | | | | |
| Coefficient of *Mem64* in First-Stage Logit | .077*** | .090*** | .107*** | .123*** | .092*** | .092*** |
| | (.021) | (.013) | (.014) | (.013) | (.006) | (.006) |
| $\chi^2$ test of 1st Stage Regression (p-values) | 230.11 | 354.40 | 331.16 | 439.05 | 1025.3 | 1025.3 |
| | (.0000) | (.0000) | (.0000) | (.0000) | (.0000) | (.0000) |
| N | 3,840 | 6,970 | 4,580 | 8,670 | 24,070 | 24,070 |
| $N_j$ | 310 | 480 | 490 | 590 | 1,060 | 1,060 |

*Notes:* See notes to Table 4. State binaries are also included. The coefficient of *Mem64* in the first-stage logit and the $\chi^2$ test of first-stage regression all correspond to the no cost-of-living adjustment regressions. We found qualitatively similar results for the first stage of the complete and partial cost-of-living adjustment regressions.

future faculty unionization. States with high union membership in 1964 are more likely to have subsequent higher-education unionization.[21] A chi-square test of each first stage regression suggests that these logits explain a statistically significant proportion of the variation in institutional union membership.

Like the OLS results using state-level fixed effects of Tables 5 and 6, the two-stage union wage premia estimated in Panels A and B of Table 8 are generally statistically insignificant. Of course, this may be due to larger standard errors generated by two-stage least squares. Concentrating on the pooled random-effects results, the largest estimates in each panel are those associated with the complete cost-of-living adjustment. This method yields *Basic Salary* and *Total Salary* premia of 4.9% and 3.4%, respectively, although the latter is not statistically significant. With the preferred partial cost-of-living adjustment, these decline to 1.8% and –0.1%, respectively. Taken as a whole, the results of Table 8 agree substantially with those of Tables 5 and 6 that incorporate state fixed

[21] The magnitude of the *Mem64* coefficient is also large—its coefficient of the pooled first stage estimate of 0.092 corresponds to an elasticity of 2.39.

effects. This may be because the first-stage estimators used in Table 8 rely on state-level variation in *Mem64*. Whatever the case, the preferred partial cost-of-living estimates found in Table 8 suggest little substantial impact of faculty unions on wages.

## Conclusion

In this paper we have examined the impacts of faculty unions on faculty salaries in four-year colleges and universities, employing the most comprehensive dataset available. Using panel-data methods to exploit the availability of repeated institutional observations, we estimated considerably smaller union salary effects than those obtained using OLS applied to individual cross-sections, as has been common in previous studies. We corrected for measurement error in the union variable, for differences in local costs of living, for unobserved state-level fixed effects, and for endogeneity between unionization and wages. After making each of these adjustments, we found that the estimated union wage premium remains positive but is of a much smaller magnitude than both OLS estimates and those found by prior researchers. Making all of these adjustments simultaneously reduces the union wage premium to statistical insignificance.

It is important to note that the weak effect that unions have on salaries does not necessarily indicate that they are ineffective advocates for their members. The presence of faculty unions may result in improvements in amenities, benefits, and working conditions. Further, unions may develop in order to help enforce contract rights, delineate promotion processes, create a grievance structure, or simply help faculty deal with the bureaucratic nature of many institutions. Indeed, these benefits alone may be sufficient to offset the cost of union dues even in the absence of salary effects. Further, unionization of one subgroup of faculty may have spillover effects for other subgroups. Evaluating the importance of the presence of spillovers or non-pecuniary benefits of collective bargaining is beyond the scope of this paper but certainly merits further investigation.

The absence of a large union salary premium is consistent with the theoretical arguments of Tullock (1994) and Hosios and Siow (2004), who pointed out that faculty unions are in the unenviable position that, unlike unions in the for-profit private sector, operate in an environment in which there are few rents to be bargained over. Further, it is not clear whether unions are able to enhance university revenue through increases in either tuition or, for public institutions, state support. As Tullock (1994: 200) concluded, “Unions may be able to shift funds around among the faculty but . . . there is little chance of increasing total salaries.”

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