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Introduction

A large body of literature suggests that unions affect the distribution of wages across workers. These impacts manifest themselves in several ways, and at several levels of aggregation. There is strong empirical evidence that unionization reduces wage dispersion or the variance of wages across all employees, job classifications, and skill levels at the macroeconomic and inter-industry levels.[5] It is less clear how unions affect the distribution of wages across employees who differ by skill levels, job classification, seniority, or pay. At the inter-firm level, some unions, such as the United Auto Workers, attempt to standardize wages across competitors and thereby reduce inter-firm wage dispersion. At the intra-firm level, both the goal and the effects of collective bargaining are less clear. Past literature suggests the primary objective of unions is to raise overall wages, but unions may simultaneously decide to pursue egalitarian strategies that flatten the wage distribution across skill levels and/or job classifications. On the other hand, unions may bargain to increase remuneration to workers with greater seniority and tilt the wage distribution across seniority upwards. Unions may reduce wage dispersion both within and across establishments as well, via a flattening of the wage distribution across skill groups.[6] There is supporting empirical evidence that unions flatten the distribution of wages across skill groups, but results have been limited due to a lack of observations of individual outcomes at both unionized and nonunionized firms.

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[5] See Freeman (1994), Card (1992, 1996, 2001), Gosling and Machin (1994), Gosling and Lemieux (2001).

[6] Freeman (1980) shows this happens between white- and blue-collar positions within firms.

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One industry in which high-quality worker-level data are observed at numerous firms, both with and without unionization, is higher education. Universities are comprised of faculty members in diverse fields of specialization with widely differing salaries. In addition, there is substantial variation in collective-bargaining status across institutions, particularly among public institutions, and in many cases across different faculty groups within the same institution. This combination of influences provides an excellent opportunity to investigate the impacts of collective bargaining on the wage distribution within and across establishments in the same industry.

In a recent paper Hedrick et al. (2011) find that faculty unions have little impact on the average real salaries of full-time faculty at four-year institutions.[7] This naturally brings up the question: If unionization has little impact on average full-time faculty real salaries, then what, if any, are the pecuniary impacts of faculty unions? To the extent that income is presumably the most important benefit of employment, a corollary question is why faculty unionize in the first place. One might expect a waning of interest in faculty unionization in higher education, corresponding to the recent slowing trend for unionization throughout the economy. However, Berry, Savarese, and Boris (2012) document overall participation in collective bargaining in higher education grew 8% over a five year period and argue recent NLRB rulings will increase this growth among private institutions. Although the explanations of why unions form are diverse and include non-economic determinants[8], one possible explanation is that faculty unions lead to a more egalitarian wage distribution, to the benefit of a majority of faculty at a particular institution.

In this paper we use data from the National Study of Postsecondary Faculty (NSOPF) to examine the impacts of collective bargaining on the distribution of wages across full-time faculty in different disciplines. Our results should be of interest both to researchers and policy makers concerned with faculty labor markets in higher education, and to labor economists more generally. Some additional motivation for our paper comes from the fact that most faculty unions are at public institutions. Previous studies[9] have assessed the broad impacts of public sector unionization on wage dispersion, but not at the intra-firm level. Given the ascendancy of public sector unions, it is important to gain a deeper understanding of their effects. Using an

[7] Notably, the paper does not assert that faculty unions have no impacts on full time faculty; rather, using the most comprehensive data and empirical methodologies available, there is no discernible, statistically significant impact on average real salary.

[8] See Hammer and Berman (1981) and Krieg et al. (2013).

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

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instrumental variables approach, we find that unions cause the variance of wages across disciplines to shrink. However, the tightening of the wage distribution does not change average wages; collective bargaining thus benefits those at the lower end of the wage distribution and hurts those at the top end.

Unions and the Distribution of Wages

Union Effects on the Wage Distribution

Studies of union impacts on the distribution of wages date back at least to Friedman (1956), who hypothesized that unions would increase wage dispersion. Card et al. (2004) provide a summary of the major literature in the intervening period. A significant proportion of this literature deals with the macroeconomic and inter-industry impacts of unions on wage dispersion (Lucifora, 1999). At these scales, a preponderance of the evidence indicates that unions raise overall wages and flatten the wage distribution, particularly when the bargaining units are highly centralized.[10]

Similarly, there is considerable evidence that wage dispersion is decreased at the inter- and intra-establishment levels. These results were most strongly presented in a landmark paper and follow-up article by Freeman (1980, 1982). More recently, Frandsen (2012) uses a regression discontinuity approach based upon union certification elections to track individual earnings. Using data for U.S. workers from a variety of industries, he shows that unionization flattens the wage distribution: lower-paid workers gain, while higher-paid workers gain little or nothing.[11] By contrast, we find intra-establishment causality through the use of an instrumental variable, and specific to the higher education industry.

A union-caused flattening of the wage distribution, on an intra-firm basis, may be either the direct result of the bargaining process, or an indirect result of workers with different productivities selecting to work at unionized versus non-unionized institutions. With regard to the first possibility, bargaining may result in higher-wage workers receiving a smaller union premium than lower-wage workers, or higher-wage workers suffering a larger loss under

[10] Recent analyses using older data support these results. Firpo et al. (2009) utilize a quantile regression technique and data on over 250 thousand U.S. males from the 1983-85 Outgoing Rotation Group (ORG) supplement of the Current Population Survey to show that unionization reduces overall wage dispersion by raising low-wage groups and lowering high-wage groups. Chernozhukov et al. (2013) utilize a more general distribution regression technique, along with ORG data from 1979 and 2008, and find that de-unionization increases between-group inequality.

[11] In an exception to the majority of the literature, however, Spetz et al. (2011) found little evidence that unionization explains any of the variance in nurse's wages.

unionization than lower-wage workers, or a redistribution income from higher-wage to lower-wage workers. If unions bargain for the median faculty member, then the wage distribution curve flattens out (i.e., there is wage compression). On the other hand, unions may also alter the wage distribution indirectly by attracting high productivity workers from low-paid disciplines and repelling higher productivity workers from high-paid disciplines. While it is difficult to disentangle these direct and indirect effects, both could be the result of unionization.

In the case of higher education, a faculty member in a non-unionized setting can be considered to engage in Nash bargaining with a price-discriminating university administration. Wage negotiations occur on an individual basis and wage outcomes primarily reflect differing outside options of faculty members. In general, individuals with higher opportunity costs receive higher wages. Under unionization, the situation becomes a bilateral monopoly between the union and the university. Unions collectively bargain, and there is little opportunity for individual faculty members to take advantage of opportunity cost differences (except at time of initial hire or in the case of a competing job offer).[12] Subsequent to hiring, increases in salary are typically based on measurable criteria such as rank and years of experience, rather than increased opportunity cost. Individuals with high opportunity costs but limited mobility lose economic rents (or quasi-rents), which are transferred to individuals with lower opportunity costs. This is the theory often discussed at the macroeconomic or industry level (Dell'Aringa and Pagani, 2007; Barth and Zweimüller, 1993) and it is consistent with the theoretical predictions of Hosios and Siow (2004) regarding faculty unions.

Despite the recent downturn in state support for public higher education, public universities continue to receive a majority of their funding from the state or via means influenced by the state (e.g. state tuition controls or advisories).[13] There is little reason to believe that unionized institutions are able to elicit greater state appropriations, at least on a systematic basis. It follows that unionization would have a minimal impact on average faculty salary. However, while the total salary pool at a particular institution may be unaffected by unionization, the allocation of the available funds may change.

To the extent that earlier studies of faculty unions have considered distributional impacts, they have tended to focus more on how unions affect the distribution of salaries across rank rather than across disciplines (Guthrie-Morse et al., 1981; Hu and Leslie, 1982; Barbezat, 1989;

[12] To a lesser degree, additional individual bargaining may occur at times of promotion, typically through a proxy (e.g., a college dean bargaining on behalf of a faculty member with a provost).

[13] See Mitchell, Palacios, and Leachman (2014).

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Hosios and Siow, 2004; Martinello, 2009). Only two studies of which we are aware have addressed unionization's impact on the distribution of salaries across disciplines, and the results have largely been suggestive rather than conclusive. Barbezat (1989) tested the equality of mean salaries across selected liberal arts fields and found statistically significant differences at nonunionized institutions, but not at unionized schools. Hosios and Siow (2004) found that the difference between the salaries of the highest-paid and lowest-paid disciplines fell under unionization at doctoral institutions but either increased or was unchanged at other types of institutions.

Below, we examine the distributional impacts of faculty unions at a much more disaggregated level by academic discipline. We estimate the separate effects of unions on the overall level of salaries and on the spread of the distribution, allowing us to identify which faculty groups (if any) gain, and which lose under collective bargaining.

Wage Distribution and Union Formation

Uniformity in wages is a well-documented result of industrial unions, at least at the macroeconomic level (Bennett and Kaufman, 2007). Thus, perceived wage inequality is likely to contribute to union formation. In higher education, if unions are unable to affect the average level of faculty wages as Hedrick et al. (2011) suggest, then they may refocus their emphasis on altering the distribution of wages. In particular, faculty with below-average wages might be motivated to form unions in hopes of obtaining wage gains at the expense of their higher-paid colleagues. If a majority of faculty in relatively low-salary disciplines benefit at the expense of a minority of higher-paid faculty, then median voter arguments suggest that unionization votes are likely to succeed. There are some unique aspects of higher education that make it well suited for our analysis. First, this labor market provides a case in which the basic tasks performed by faculty (teaching, research and service) are similar, but there are large and observable differences in salaries across disciplines among non-unionized faculty. These differences make it easier to identify wage effects among unionized faculty. Second, the higher education data provide an opportunity to develop a repeated cross-section of micro-level observations on faculty salaries with an extensive set of faculty- and institutional-level of controls to help reduce omitted variable bias that may confound the observed effects of unions on the wage distribution.

Model Specification and Data

Model Specification

We estimate a log-wage equation of the form:

(1) $\ln(Y_{ijkts}) = \alpha Union_{ijts} + \mathbf{\theta D}_{ijkts} + \mathbf{\beta D}_{ijkts} \cdot Union_{ijts} + \mathbf{\delta X}_{ijts} + \mathbf{\gamma Z}_{jts} + \mathbf{\lambda S}_{s} + \mathbf{\eta T}_{t} + \nu_{j} + \varepsilon_{ijkts}$

where Y_{ijkts} is a measure of salary for the i^{th} faculty member at institution j in discipline k at time t in state s. *Union* is a binary variable that indicates whether the faculty member's subgroup (defined as tenured or tenure-track versus non-tenure-track) is covered by a collective-bargaining agreement. **D** is a vector of dummy variables each representing one of 25 academic disciplines, **X** represents a vector of individual observables, and **Z** represents institutional variables including measures of local cost of living described below. State-level binary variables, **S**, control for unobserved state-level heterogeneity in faculty salaries, and **T** is a set of binary variables representing the individual survey years. The error term in this equation contains two components: ν_j, which represents an institution-specific error, and ε_{ijkts}, which is associated with a particular faculty member at that institution. The coefficients of interest in (1) are α, which measures the overall percentage change in wages attributable to unions at an institution, and **β**, which measures the discipline-specific impacts of collective bargaining on wages. The sum of α and β_k is the overall impact of unionization on academic discipline k.

Data Description

The NSOPF is conducted roughly every five years by the National Center for Education Statistics (NCES) of the U.S. Department of Education. To date it has been administered in 1988, 1993, 1999, and 2004. Each cycle has used a similar two-stage sampling process. At the first stage institutions are sampled, and in the second stage faculty members are sampled from within the selected institutions. Faculty members are asked about their professional experience and background, responsibilities and workload, compensation, demographic characteristics, and opinions. In a separate institutional questionnaire, a representative of the school's administration is asked about institutional characteristics, policies, faculty benefits, the total number of full-time and part-time faculty, student enrollment, and whether any faculty at the institution are represented by a union for the purposes of collective bargaining. Over all four cycles, a total of 78,310 faculty and 1,900 institutional questionnaires have been returned.

The NSOPF queries instructors, lecturers, tenure-track, and tenured faculty members at two-year and four-year higher education institutions. Previous studies (e.g., Ashraf (1998, 1999)) suggest that differential impacts of unionization can be expected across institution type and faculty status. In order to estimate these impacts as precisely as possible, we focus our analysis on full-time faculty members at four-year colleges and universities. This reduces the sample to

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44,330 faculty observations at 1,060 institutions. After eliminating faculty whose principal activity is neither teaching nor research, those who had missing or imputed values of any explanatory variables, and those whose basic salary received from their institution was less than $20,000 or greater than $350,000 in 2004 dollars,[14] we have a final sample of 24,180 faculty at 1,060 different institutions. Table 1 presents the number of institution and faculty observations by survey year and Table 2 presents the replication pattern of institutions. While 8% of institutions are observed in all four surveys, nearly half of institutions appear in the NSOPF in at least two of the survey years. On average, we observed 22.8 individuals per institution per year.[15]

Table 1

Sample Size

	Number of Institutions		Number of Faculty	
Year	NSOPF	Sample	NSOPF	Sample
1988	450	310	8,380	3,890
1993	970	480	25,780	7,010
1999	890	490	18,040	4,610
2004	1,020	590	26,110	8,670
Sum	1,900	1,060	78,310	24,180

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.

[14] 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), over 60% of faculty with salaries below $20,000 held single-term appointments. Similarly, faculty 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.

[15] 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.

Table 2

Replication Pattern for Institutions

Times Observed	Survey Year	Number of Institutions		% 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.

Hedrick et al. (2011) demonstrate that the survey question the NSOPF uses to measure the presence of faculty unionization incorrectly identifies the presence of unionization for some full-time faculty. This error is based upon the fact that the NSOPF assigns a single measure of unionization to an institution despite the possibility that an institution may have collective bargaining agreements with one faculty type (for instance, non-tenure track) and not another (tenured/tenure-track). We correct these measurement errors using data from the National Center for the Study of Collective Bargaining in Higher Education and the Professions (NCSCBHEP) compiled by Boris (2006). The NCSCBHEP data identify the presence of collective bargaining at an institution separately for full-time permanent faculty, full-time adjunct faculty, and librarians.

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Unfortunately, during the sample period tenured and tenure-track union status does not vary much within institution over time. Although 17.8% of observed institutions and 19.1% of observed faculty engage in collective bargaining, fewer than 10 of the 1,060 institutions switched collective b8argaining status for their tenure/tenure-track faculty within the four surveys in our sample and fewer than 30 institutions switched bargaining status among their non-tenure track faculty. This limited number of switchers implies that identification of the union impacts on both the overall level of wages and on their distribution relies upon variation in union status between institutions rather than within-institution variation over time. Because of the limited variation in union status within institutions, we estimate equation (1) using linear random effects models. It is also important to note that because the *Union* variable measures the presence of collective bargaining for groups of faculty within an institution and not an individual's choice to be a union member, $\alpha + \beta_k$ measures the impact of collective bargaining on the wages of covered faculty and not the impact on wages of being a member of a union.

Measure of Salary

The NSOPF faculty survey asks numerous questions regarding 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 utilize *Basic Salary* which represents payments made to faculty in exchange for fulfilling their basic annual contract and is the measure used by Monks (2000) and Hedrick et al. (2011).[16] *Basic Salary* accounts for general inflation by choosing 2004 as the base year and adjusting all dollar figures in prior surveys upwards using the CPI.

Both the NSOPF and the NCSCBHEP data identify a very 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 significantly 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

[16] An alternative NSOPF measure is *Total Salary* which is equal to *Basic Salary* plus other supplementary payments from the faculty's institution such as elective 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 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 estimate all equations using *Total Salary* rather than *Basic Salary* and find no substantive differences between the results so, for brevity's sake, we report only *Basic Salary* herein.

cause the union wage premium to be overestimated. We therefore adjust salaries for cost of living using an index constructed from apartment rent data taken from the decennial U.S. Census. This rent index closely approximates the well-known ACCRA index published by the Council for Community and Economic Research, but has much wider geographic coverage. A detailed discussion of this procedure is given by Hedrick et al. (2011).

Distribution of Salaries

Faculty observations in the NSOPF are categorized into 25 different academic fields. Table 3 orders fields by average *Basic Salary* adjusted for the cost of living using the rent index. The overall rent-adjusted average *Basic Salary* is $60,996; unionized faculty average $62,706, about 3.5% more than the non-union average of $60,596. The data demonstrate significant variation between faculty in different fields. The lowest average salary is $50,601 in the visual and performing arts and the highest is $91,919 for faculty in legal professions and studies. A simple t-test of mean comparison demonstrates significant differences in remuneration between the union and non-union sectors for a majority of fields.

Table 3 previews our econometric results. The median rent-adjusted cost of living salary is $55,569. Among the nine academic fields that average a salary below the median, the average benefit of unionization (the "Difference" column in Table 3) is $5,130, compared with $281 for the sixteen academic fields above the median suggesting that unionization benefits lower paid disciplines more than higher paid ones.

The apparent union wage premium in Table 3 may be partly explained by differences in factors other than unionization. Table 4 demonstrates that faculty at unionized institutions average greater experience, both after earning their highest degree and at their current institution, and they are more likely to hold the advanced ranks of associate and full professor, to hold doctorates, and to be at larger schools. All variables in Table 4, as well as the cost of living measures[17] and squares of experience and institutional enrollment, are components of **X** and **Z** in equation (1).

[17] Because equilibrium wages are likely to vary less across locations than living costs, Dumond et al. (1999) suggest including the log and the log squared of the cost of living measure on the right hand side of a regression, a process followed here.

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Table 3

Basic Salary, Adjusted with Rent Index, by Discipline

	Basic Salary		Union		Non-Union		
	Obs.	Mean	Obs.	Mean	Obs.	Mean	Diff.
Arts-visual and performing	1,590	50,601	290	54,284	1,290	49,766	4,518***
English language and literature/letters	1,750	51,126	360	55,752	1,390	49,923	5,828***
Foreign languages/ literature/linguistics	1,210	51,470	210	57,001	1,000	50,280	6,720
Library science	60	51,500	10	56,633	40	49,828	6,804
Parks/recreation/leisure/ fitness studies	250	51,618	60	56,228	200	50,278	5,950***
Communication/journ./ comm. Tech	510	52,354	100	53,869	420	52,002	1,867
Education	1,900	53,313	440	59,329	1,460	51,507	7,821***
Area/ethnic/cultural/ gender studies	70	53,873	20	62,106	50	50,322	11,783***
Philosophy, religion & theology	1,090	54,596	150	61,238	940	53,534	7,704***
Family/consumer sciences, human sciences	180	56,131	30	57,333	140	55,862	1,470***
Other	400	54,760	90	57,121	310	54,049	3,072
Multi/interdisciplinary studies	60	57,070	10	54,117	50	57,765	-3,649
Architecture and related services	180	57,462	50	62,271	130	55,696	6,575**
Mathematics and statistics	1,060	58,113	200	61,797	860	57,242	4,554***
Public admin./social services	220	58,793	50	62,502	180	57,850	4,651*
Psychology	930	59,490	190	62,929	740	58,596	4,332***
Social sciences (except psych) & history	3,000	60,193	650	61,843	2,350	59,736	2,107**

	Basic Salary		Union		Non-Union		
	Obs.	Mean	Obs.	Mean	Obs.	Mean	Diff.
Computer/info. Sciences/support tech	610	64,232	140	67,115	470	63,364	3,751
Physical sciences	1,520	64,697	280	65,902	1,250	64,430	1,472
Agriculture/natural resources/related	410	67,368	90	66,287	320	67,658	-1,372
Biological and biomedical sciences	1,880	69,224	270	67,777	1,610	69,466	-1,690
Business/management/ marketing/related	1,610	69,361	320	71,177	1,290	68,916	2,260
Health professions/clinical sciences	2,190	71,470	320	70,107	1,870	71,704	-1,598
Engineering technologies/technicians	1,170	73,065	210	70,468	960	73,632	-3,164*
Legal professions/studies	330	91,919	40	83,571	290	93,179	-9,608*
Overall	24,180	60,996	4,590	62,706	19,590	60,596	2,110***

Note: *** {**} [*] represent statistical significance at the 99% {95%} [90%] level.

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Table 4

Descriptive Statistics: Sample Means & Standard Deviations

		Unionized		Non-Unionized
Basic Salary	Real salary, 2004 base year	63,626 (21,365)	>	59,906 (27,131)
Exp	Years at current institution	12.62 (9.89)	>	10.59 (9.56)
Degexp	Years of experience since earning highest degree	16.40 (10.15)	>	15.12 (10.17)
Female	Binary = 1 if female	.347 (.476)	=	.361 (.480)
Married	Binary = 1 if currently married	.727 (.446)	<	.745 .436
Wasmarried	Binary = 1 if previously married	.111 (.314)	=	.104 (.306)
Hispanic		.051 (.221)	>	.041 (.197)
Indian		.012 (.107)	=	.010 (.098)
Asian		.077 (.267)	>	.060 (.237)
Black		.047 (.212)	<	.054 (.226)
Pacific		.002 (.044)	>	.001 (.031)
Lecturer	Binary = 1 if academic rank is lecturer	.027 (.161)	=	.030 (.171)
Instructor	Binary = 1 if academic rank is instructor	.033 (.179)	<	.069 (.254)
Assistant	Binary = 1 if academic rank is assistant professor	.251 (.434)	<	.307 (.461)

		Unionized		Non-Unionized
Associate	Binary = 1 if academic rank is associate professor	.294 (.456)	>	.271 (.445)
Full	Binary = 1 if academic rank is professor	.385 (.487)	>	.302 (.459)
Tenured	Binary = 1 if tenured	.675 (.468)	>	.518 (.500)
Tentrack	Binary = 1 if on tenure track	.250 (.433)	=	.262 (.440)
Bachelors	Binary = 1 if highest degree earned is a bachelors	.009 (.095)	<	.013 (.113)
Masters	Binary = 1 if highest degree earned is masters	.163 (.369)	<	.201 (.400)
Profession	Binary = 1 if highest degree is professional	.041 (.197)	<	.059 (.235)
Doctorate	Binary = 1 if highest degree earned is Ph.D. or equivalent	.787 (.410)	>	.726 (.446)
Citizen	Binary = 1 if U.S. citizen	.912 (.284)	=	.916 (.278)
Funded	Binary = 1 if scholarly activity funded by external agency	.314 (.464)	<	.345 (.475)
Firstjob	Binary = 1 if current job is first since graduating	.388 (.487)	=	.401 (.490)
Articles	Total refereed articles published	14.11 (26.34)	<	15.06 (29.27)
Nonref	Total non-refereed articles published	8.06 (37.77)	>	7.20 (22.32)
Books	Total books published	4.13 (12.25)	=	3.94 (16.03)
Presentation	Total research presentations made	35.97 (86.78)	=	37.91 (96.77)

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		Unionized		Non-Unionized
Pubdoc	Binary = 1 if institution is public, doctoral granting	.360 (.480)	>	.339 (.473)
Privdoc	Binary = 1 if institution is private, doctoral granting	.018 (.133)	<	.144 (.351)
Pubcomp	Binary = 1 if institution is public, comprehensive	.503 (.500)	>	.175 (.380)
Privcomp	Binary = 1 if institution is private, comprehensive	.036 (.185)	<	.116 (.320)
Publa	Binary = 1 if institution is public, liberal arts	.025 (.157)	>	.012 (.111)
Privla	Binary = 1 if institution is private, liberal arts	.026 (.158)	<	.143 (.350)
Enrollment	Total Student FTE (thousands)	12.76 (9.25)	>	11.09 (10.64)
N	Number of Faculty Observations	4,590		19,590
N_j	Number of Institutions	210		850

Note: >,< represent statistical differences using a paired t-test at the 5% level.

Empirical Results

Base Results

Using institutional-level random effects, the estimated coefficients on the union variable (α) and the interaction terms for each field (β_k) from equation (1) are summed to calculate the percentage change in *Basic Salary* for faculty in each of the 25 academic disciplines.[18] These results are given in the first column of Table 5, which lists disciplines ranked from lowest to highest average *Basic Salary*. Consistent with Hedrick et al. (2011), the estimated impact of *Union* on overall wages, α = -0.045, is near zero and statistically insignificant (t =-1.70).[19] Despite the absence of an overall union impact, Table 5 reports that faculty in a number of

[18] The results using the ACRAA cost of living adjustment follow a similar pattern and are available from the authors.

[19] All standard errors have been corrected for the clustering of faculty within institution.

disciplines gain large and statistically significant wage benefits. For instance, unionized faculty in area and ethnic studies fields are estimated to earn 11.1% higher salaries than non-unionized faculty in these fields. Other faculty with statistically positive gains from unionization are those in foreign languages, English, and philosophy. On the other hand, faculty in fields such as business, agriculture, engineering, computer science, and the health and legal professions earn less under unionization than their non-unionized counterparts. In general, those fields that suffer the greatest declines are precisely those with the highest opportunity cost faculty, and vice versa.

An alternate view of the impact of unions on the distribution of salaries amongst full-time faculty is provided in Figure 1, where the estimated percentage change in *Basic Salary* ($\alpha + \beta_k$) reported in Table 5 is plotted against average *Basic Salary* for each discipline. The graph underscores the flattening of the wage distribution curve, whereby unionized faculty in high-income fields experience relative reductions in salary while other faculty in low-income fields see their salaries increase. As is demonstrated by the regression line in Figure 1, for each $1,000 increase in a discipline's average *Basic Salary*, the discipline can expect to lose four-tenths of one percentage point of the estimated wage premium associated with unionization.

Table 5

Union Impact on Real Basic Salary, All Observations

			(1)	(2)	(3)
		Specification	RE	2SLS, RE	2SLS, FE
	Discipline	N	$\alpha + \beta_k$	$\alpha + \beta_k$	$\alpha + \beta_k$
Arts	Visual and performing arts	1,590	0.022 (.022)	.108** (.046)	0.108* (.056)
Eng	English language and literature	1,750	0.043*** (.016)	.142*** (.045)	0.144*** (.055)
Foreign	Foreign languages/literature	1,210	0.057*** (.017)	.124*** (.044)	0.151*** (.055)
Libr	Library sciences	60	-0.036 (.040)	-.041 (.091)	-0.033 (.101)
Rec	Parks/recreation/leisure	250	0.025 (.029)	.059 (.059)	0.062 (.067)

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			(1)	(2)	(3)
		Specification	RE	2SLS, RE	2SLS, FE
	Discipline	N	$\alpha + \beta_k$	$\alpha + \beta_k$	$\alpha + \beta_k$
Comm	Communication/ journalism	510	-0.045* (.025)	.012 (.057)	0.015 (.066)
Edu	Education	1,900	0.014 (.013)	.092** (.040)	0.089* (.052)
Area	Area/ethnic/cultural/ gender studies	70	0.111*** (.041)	.164 (.106)	0.142 (.106)
Phil	Philosophy, religion & theology	1,090	0.038* (.020)	.172*** (.048)	0.158*** (.059)
Other	Other	220	-0.045* (.031)	.082 (.057)	0.079 (.078)
Family	Family/consumer sciences	180	0.013 (.029)	.167** (.071)	.182** (.078)
Multi	Multi/interdisciplinary studies	60	-0.022 (.052)	.159 (.111)	0.175 (.116)
Arch	Architecture and related services	180	0.047 (.034)	.224*** (.086)	0.237** (.095)
Math	Mathematics and statistics	1,060	0.016 (.018)	.072 (.048)	0.082 (.06)
PubAdmin	Public admin./social services	220	0.0001 (.029)	.031 (.057)	0.03 (.065)
Psych	Psychology	930	-0.011 (.019)	.017 (.052)	0.024 (.062)
SocSci	Social sciences and history	3,000	0.01 (.013)	.094** (.043)	0.107** (.055)
Comp	Computer sciences	610	-0.041* (.023)	-.010 (.054)	-0.009 (.064)
Psciences	Physical sciences	1,520	-0.004 (.015)	.055 (.046)	0.064 (.056)
Ag	Agriculture/natural resources	410	-0.06** (.025)	.056 (.094)	0.062 (.066)

			(1)	(2)	(3)
	Specification		RE	2SLS, RE	2SLS, FE
	Discipline	N	$\alpha + \beta_k$	$\alpha + \beta_k$	$\alpha + \beta_k$
Bio	Biological and biomedical sciences	1,880	-0.025 (.017)	-.003 (.050)	0.04 (.06)
Bus	Business/management/ marketing	1,610	-.068*** (.021)	-.027 (.055)	-0.038 (.066)
Health	Health professions/clinical sciences	2,190	-0.065*** (.021)	-.112** (.050)	-0.075 (.064)
Engin	Engineering and related	1,170	-0.038** (.019)	.040 (.052)	0.044 (.062)
Legal	Legal professions and studies	330	-0.153** (.0164)	-.272*** (.100)	-0.265** (.111)

Note: *** {**} [*] represent statistical significance at the 99% {95%} [90%] level. Standard errors of $\alpha+\beta_k$ corrected for clustering at the institutional level are in parenthesis. All models include all variables displayed in Table 4 and squares of enrollment, Exp, Degexp, and binary variables indicating survey period. The random effects (RE) models also include binaries for each state.

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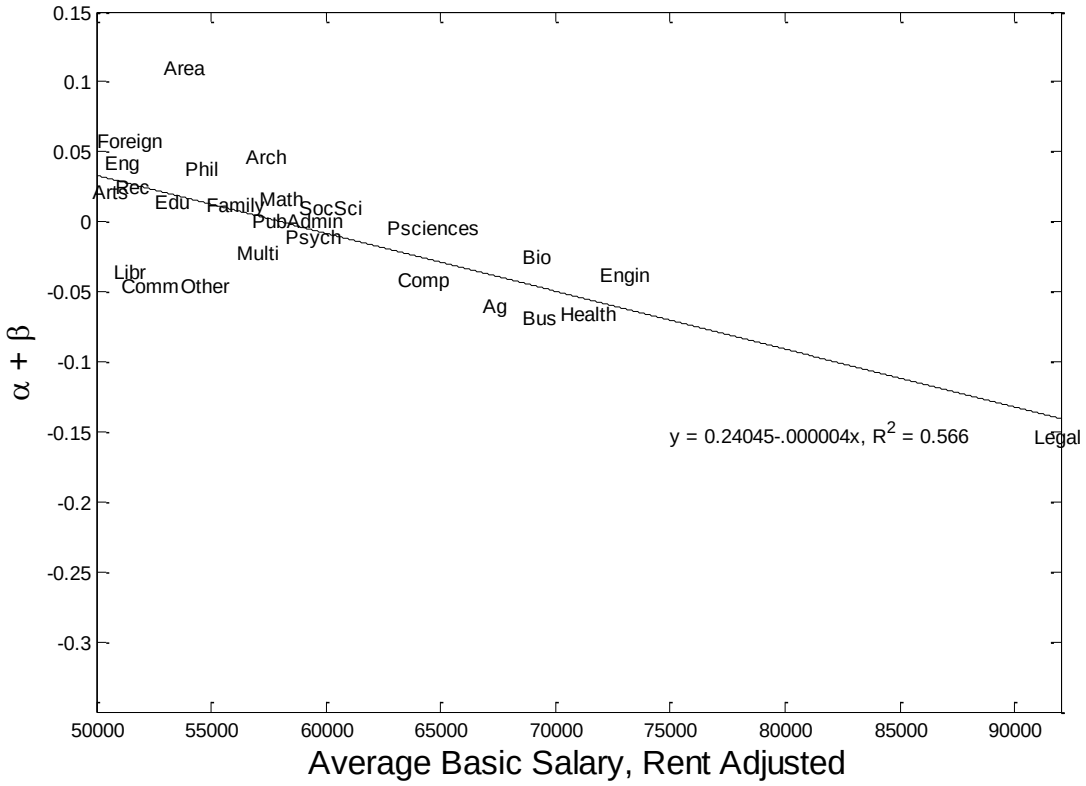
Figure 1

Discipline-Specific Impacts of Unionization



Causation and Sensitivity Tests

The results presented this far are suggestive of wage compression caused by unionization. However, alternative explanations are possible. For instance, if institutions with relatively homogeneous inter-disciplinary salaries are more likely to unionize, then one would expect the results in Figure 1 to occur not because unions caused them, but instead simply because unions were more likely to form under these conditions.[20] To determine the causal impact of unions on faculty wage distributions, we control for this type of selection bias using an instrumental variable. As suggested by Krieg et al. (2013), a candidate for such an instrument is the prior level

[20] As mentioned earlier, unions may also cause faculty to select towards or away from unionized institutions. The impacts of sorting on the wage distribution is another mechanism, along with bargaining, by which unions can affect the wage distribution.

of private sector unionization in the state in which the institution is located. States with histories of high levels of unionization are more likely to have sentiment, legislation, and policies that make the unionization of faculty more likely. To be specific, our instrument is the percentage of nonagricultural workers in an institution's state that were unionized during 1964 (*Mem64*).[21] As demonstrated in Figure 2, among all 4-year institutions listed by NCSCBHEP, unionization began in the late 1960s, peaked in the early 1970s, and was nearly complete by the mid-1980s. By choosing our instrument to predate faculty unionization, we eliminate any mechanical correlation between university unions and *Mem64*. We use Wooldridge's (2010) recommended two-stage instrumental variables technique. In the first stage, we estimate separate probits for tenured/tenure track and non-tenure track faculty.[22] Each probit regresses *Union* on *Mem64*, **Z** and **T** and from each, we save the predicted probabilities of unionization. In the second stage, we utilize these predicted probabilities as instruments for the variable *Union* in equation (1).

Identifying the impact of unions on the wage distribution in this manner relies on the dual assumptions that *Mem64* is uncorrelated with the error term in (1) and *Mem64* is correlated with *Union*. As to the first assumption, we believe this is justified for at least two reasons. First, it seems unlikely that the extent of unionization in a state forty years in the past is correlated with the unexplained portion of an individual's wages. However, even if states with high union activity in 1964 are likely to have higher university salaries later due to unobserved state-level effects, these will be purged from (1) by the inclusion in that equation of the state-level fixed effects, $\mathbf{S}_s$. With respect to the correlation between *Mem64* and *Union*, the first stage coefficients of 0.053 (t = 12.5) for tenured/tenure track and 0.037 (t = 5.82) for non-tenure track are statistically significant and large in magnitude. For instance, the tenure/tenure-track coefficient is equivalent to a 1.2% percentage-point increase in the likelihood that an institution unionizes for each percentage-point increase in its state's 1964 unionization rate.[23]

[21] *Mem64* varies considerably across states with the highest being Michigan (44.8%) to South Carolina (7%). The average institution experienced a *Mem64* of 28%. This data is available under the title "State Union Membership Density in the U.S., 1964-2008" at unionstats.gsu.edu.

[22] We treat tenured/tenure track and non-tenure track observations differently because unions can represent either, one, or both types of faculty on a campus depending upon state law. For instance, the University of California system has non-tenure track faculty represented but not tenured/tenure track.

[23] An additional regression using an alternative instrument – the percent of a state's workforce that was unionized 24 years prior to the NSOPF survey – yields results highly similar to those from the Mem64 regression. The benefit of the alternative instrument is that it exhibits variation within states; however, it suffers with respect to ease of exposition, as well as the fact that the 1980 observation occurs after most of the unionization in higher education. Results of the alternative regression are available upon request.

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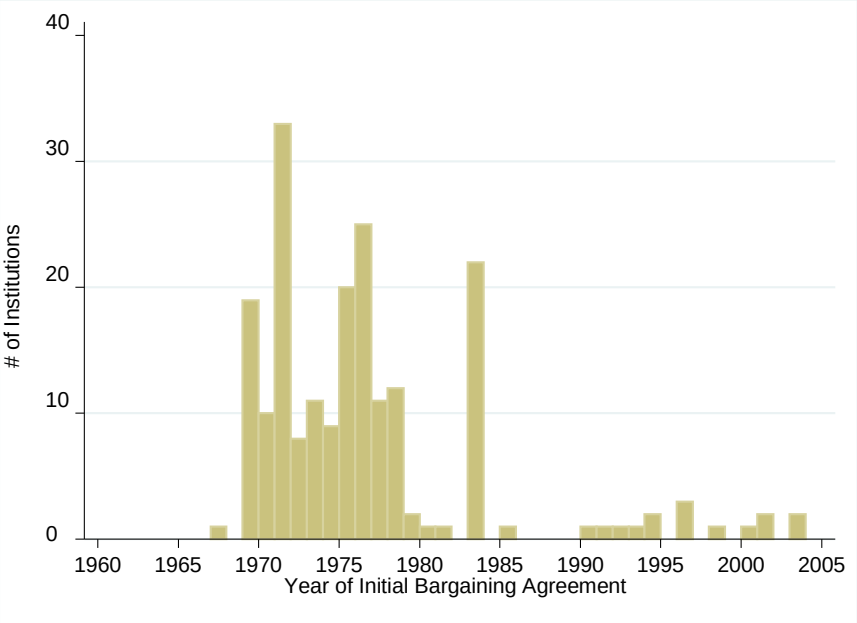
Figure 2

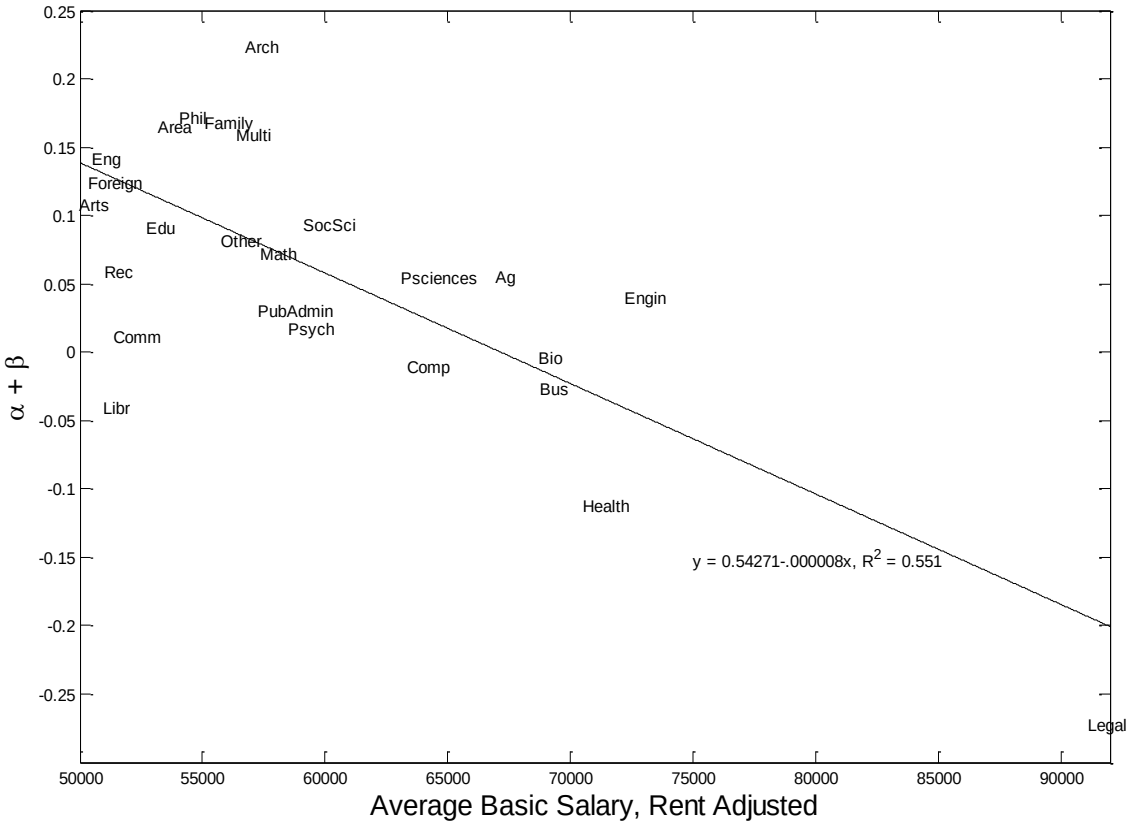
Dates of Initial Unionization for Tenured and Tenure Track Faculty



Source: Boris (2006).

The results of the instrumental variables approach are produced in the second column of Table 5 and graphically presented in Figure 3. A few differences relative to the base model are apparent. First, using the instrument, the estimated impact of *Union* on overall wages, $\alpha = 0.082$, is considerably larger than the α estimated with the base model though, like the base model, this estimate is statistically insignificant ($t = 1.43$). As is clear from the second column of Table 5, this higher value of α reduces the number of fields that experience a decrease in *Basic Salary* as a result of unionization. Second, as expected from any two-stage estimation process, the standard errors produced using the instrumental variable technique are considerably larger than those of the base model. Even in light of this, fields such as the arts, English, foreign languages, education, and philosophy experience positive, statistically significant union wage premia while high-paid fields such as the legal and health professions experience negative and significant estimates. Finally, as demonstrated in Figure 3, for each additional $1,000 averaged by a discipline, the estimated wage premium declines by eight-tenths of a percentage point, double that estimated in the base model.

Figure 3
Discipline-Specific Impacts of Unionization, 2SLS



Another concern involves the potentially strong assumption implicit in random effects that the institution-specific effect, v_j, is uncorrelated with any of the included independent variables. Relaxing this assumption by estimating (1) with fixed effects has the drawback of identifying the impact of unions based upon fewer than 40 institutions that changed union status over the observed period. Moreover, the majority that did switch status did so only for their non-tenure track faculty. Thus, a fixed-effects model has the potential to produce estimates based upon a non-representative group of faculty at a non-random group of institutions. In addition, because no observed institutions changed locations, a model that includes institutional level fixed effects precludes the use of state-level fixed effects. However, given these concerns, we report the results of equation (1) using *Mem64* as an instrument and institution-level fixed effects in the

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third column of Table 5. The similarities between the second and third columns are striking. Both the signs and magnitudes of the estimates are substantively the same for almost all fields.[24]

Extensions

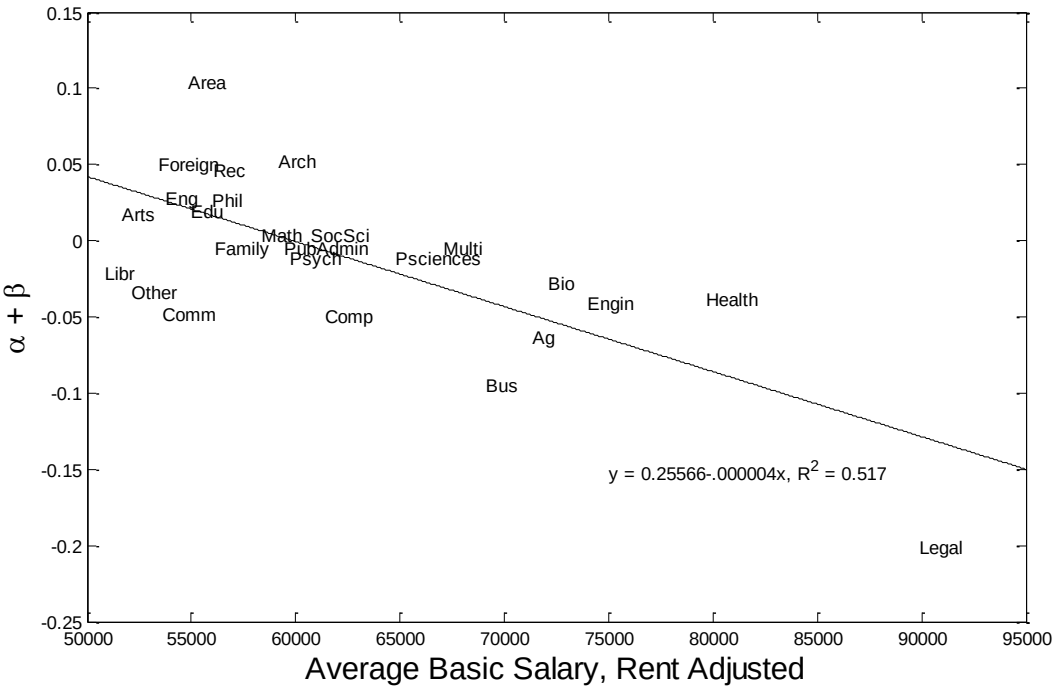
Faculty Type

One concern with the preceding analysis is that it mixes tenured and tenure track faculty with non-tenure track observations. While non-tenure track observations make up a minority of total observations (19.1%), it is conceivable that non-tenure track unions have different objective functions than unions representing tenure track faculty members. To explore the effect of combining these two groups, we drop the non-tenure track observations and re-estimate equation (1) for just tenured and tenure track faculty members. Results from this exercise are displayed in Figure 4. Like the earlier OLS results, the overall impact of unions on wages is small, negative, and insignificantly different than zero (α = -0.033, t = -1.45). There is also no substantive difference in differences between fields with the more highly paid fields receiving less at unionized institutions relative to non-unionized ones. Like the results in Figure 1, an increase in average salary of $1,000 reduces a discipline's union wage premium by four-tenths of one percentage point. Given this, combining non-tenure and tenure track faculty appears not to change the overall finding that unions tend to level wages across disciplines.

[24] The presence of state level dummy variables in the random effects model and their necessary exclusion from the fixed effects model precludes comparison of the two model using the standard Hausman test. When we drop the state level dummy variables from the random effects model and perform a chi-squared test of equality between the α and β_k's from the institutional-level fixed effect model, we fail to reject the null that the coefficients differ from each other (p = 0.158).

Figure 4

Discipline-Specific Impacts of Unionization, Tenure Track Faculty Only



Institution Type

Because different types of institutions have different missions and potentially different pools of labor from which to draw, it is possible that these differences might be related to the finding that unionization benefits lower-paid faculty at the expense of higher-paid faculty. For instance, if the faculty markets for doctoral institutions are more competitive than those for liberal arts schools, one might expect unionization to have impacts on the wage distribution that differ by institution type. We estimate equation (1) separately for three classes of institutions: comprehensive, doctoral, and liberal arts schools.[25] In all three cases, there is a negative relationship between the size of the union wage premium and a discipline's average *Basic Salary*, though this is statistically significant only at doctoral and comprehensive institutions. The largest impact appears in comprehensives, which experience a decline in the union wage premium of seven-tenths of one percentage point for each $1,000 increase in a discipline's average salary. Doctoral institutions exhibit a slightly smaller six-tenths of one percentage point reduction. Liberal arts schools demonstrate an even more negative relationship between the

[25]As defined by the Carnegie classification system of higher education institutions.

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union wage premium and average salary, though this slope is not statistically significant. For liberal arts schools, the estimated α is -0.135, though this is statistically insignificant (t = -0.97). In contrast, the estimated impact of unions on overall wages was much smaller at doctoral institutions (α = 0.044, t = 0.35) and comprehensive universities (α = 0.112, t = 0.113).

Union Persistence and Growth

The empirical results shed some light on why faculty unions may persist even in the absence of an overall union wage premium. Given that unions tilt the wage distribution in favor of lower-paid disciplines at the expense of higher-paid ones, it is possible that a majority of faculty benefit from unions and, through a median voter mechanism, push for their continuance or establishment on non-unionized campuses. Figure 5 depicts the cumulative distribution of union impacts on faculty salaries. When the entire sample using *Basic Salary* is analyzed, 51.7% of faculty are estimated to earn more under unionization. [26] However, this result becomes more striking when the sample is divided into institution type as is demonstrated in Figure 6. Within comprehensive universities, 78.5% of faculty receive a positive wage benefit from unionization while just over 17% of faculty at doctoral institutions gain. At liberal arts universities, only 8% of observations experience the positive wage benefits of collective bargaining.[27] This may contribute to the fact that among the 20 most recent switches from non-union to union status among higher education institutions, a majority were comprehensive universities while no liberal arts institutions formed new unions.[28]

[26] This percentage rises to 56.7% when *Total Salary* is used as the salary measure

[27] This may occur because the variance of wages across disciplines in liberal arts institutions is significantly lower than the variance at other types of schools leaving less room for unions to impact the distribution.

[28] Among the 20 most recent switches from non-union to union status included 10 comprehensive universities, 3 doctoral institutions, and 7 community/technical colleges. Source: Boris (2006).

Figure 5

Cumulative Distribution Function of Union Wage Effects on Basic Salary

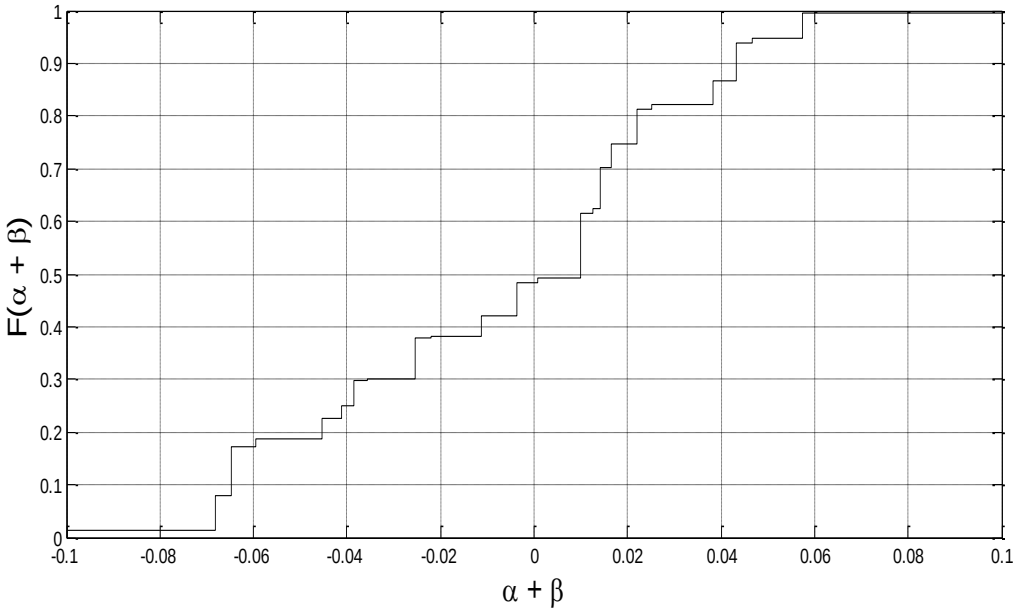
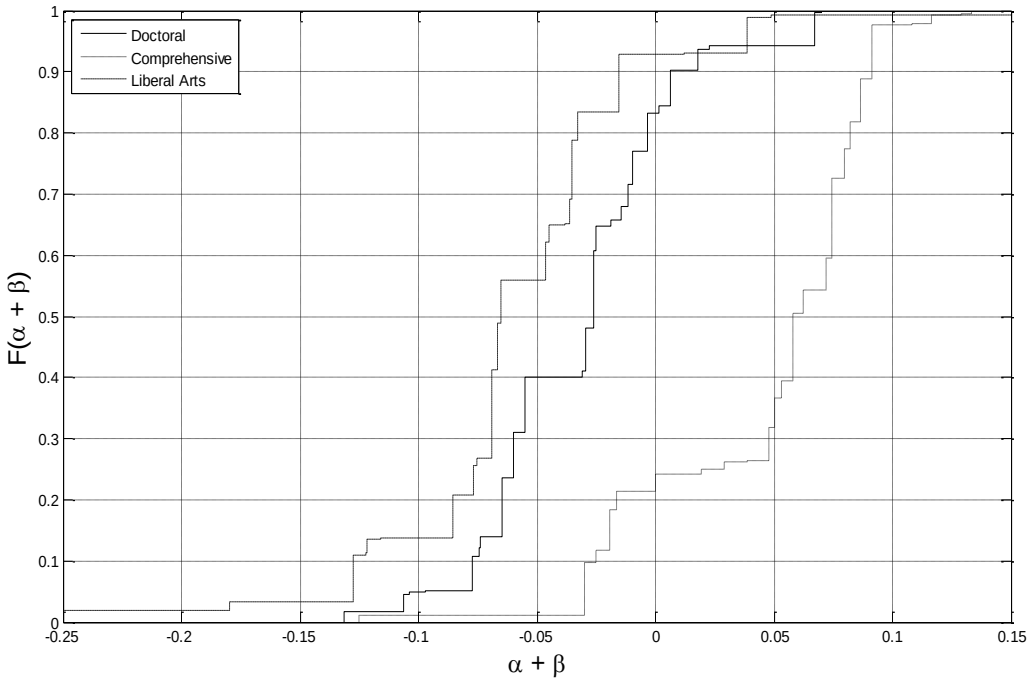


Figure 6

Cumulative Distribution Function of Union Wage Effects on Basic Salary, By Institution Type



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Clearly it would be naïve to argue that only faculty who stand to gain financially will vote for unionization and only those who lose will vote against it; there are obviously other important factors such as effects on fringe benefits, working conditions, and faculty governance, as well as differential interest in unionization between faculty in different disciplines. Nevertheless, these findings provide some support for the median voter theorem premise for unionization.

So far, we have focused on the wage premia earned by full-time faculty members. Depending upon state laws, however, part-time faculty may also participate in collective bargaining. If part-time faculty receive a wage premium and are part of the same bargaining unit as full-time faculty, they would be likely to join forces with full-time faculty who stand to gain and to vote for unionization. Of the 780 institutions that had collective bargaining agreements in 2004, 56% had agreements that covered part-time faculty. A majority of those institutions, 55%, had both full-time and part-time faculty who bargain collectively. While the data do not allow one to ascertain when full-time and part-time faculty are part of the same bargaining unit, it is likely that this is the case at many institutions. In the only study to date that has attempted to measure the union wage for part-time faculty, Monks (2007) finds a statistically significant 10% premium for those who participate in collective bargaining. His findings are suggestive that part-time faculty might vote along with full-time faculty who expect to gain from collective bargaining.

Discussion and Conclusions

Using a comprehensive dataset on full-time faculty members in U.S. colleges and universities, we find that unionization causes a significant flattening of the distribution of wages across academic disciplines. These effects control for individual-level measures that impact salary such as experience, tenure, rank, academic productivity, and demographics. The flattening of the distribution of wages is strongest at comprehensive institutions, also present in doctoral institutions, and less likely to occur at liberal arts schools. This flattening generates gains at the lower end of the wage distribution at the expense of higher-paid faculty, with no detectable change in overall average salaries. The distribution of expected gains from unionization is somewhat positively skewed; that is, the gains are positive at the median of the wage distribution. This positive skew is more prevalent at doctoral and comprehensive institutions and provides some explanation for the widespread success of unionization votes and the extreme rarity of decertification efforts.

There is reason to believe that these results may understate the actual shift in the wage distribution across disciplines. As Card et al. (2003) point out, the statistical challenge is to

develop a methodology that controls for the effects that the presence of unions may have on the level and distribution of wages in the non-union sector. Once unions are formed, the benefits they convey may draw workers away from the non-union sector. Equally plausible is that some workers may wish to avoid the more structured environment that unions provide. The resulting selection bias distorts estimates of union impacts using statistical methods such as those employed herein. If, for example, workers with high opportunity costs avoid union jobs, the resulting selection bias will lead to underestimation of the union/non-union wage differential. Self-selection will also increase the variation in wages between establishments while decreasing the variation within both union and non-union establishments.[29]

Of course, faculty choose to unionize for a wide variety of reasons in addition to salary gains. For example, some may be motivated by expectations or hopes of more attractive teaching loads, better fringe benefits, and/or stronger faculty governance. The question of what factors determine unionization is one that deserves further investigation. Another interesting question concerns the effects of this wage compression on faculty satisfaction. This has important implications for faculty governance and the ability of faculty in diverse disciplines to cooperate on matters of common interest. Finally, the flattening of the wage distribution across disciplines within an institution has implications for the quality of faculty hired at that university. If low-opportunity-cost disciplines are paid more at unionized than at non-unionized schools, one might expect unionized institutions to attract and keep higher-quality faculty in these disciplines. As a result, one might expect a shift of educational and research quality from non-unionized to unionized institutions in low-cost fields and from unionized to non-unionized institutions in high cost fields. We leave these issues for further exploration.

[29] For a further discussion of these issues, see Lemieux (1993, 1998), Card (1996), and Abowd and Farber (1982).

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