

What Do Unions Do for Professionals? Evidence from Faculty Unionization in U.S. Higher Education*

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Abstract

About 45 percent of U.S. union members now work in management, professional, and related occupations, yet most evidence on what unions do comes from blue-collar jobs and from occupations paid on fixed salary schedules. Faculty unionization in U.S. higher education offers a setting to study what bargaining does where pay is set individually and tied to productivity: faculty have unionized at different institutions in different years, and academic careers follow an observable ladder from assistant to full professor. We combine the staggered timing of faculty unionization between 1993 and 2023 with institution-level salaries and headcounts from IPEDS and restricted-use microdata from the Survey of Doctorate Recipients (SDR), which allow us to follow doctorate-holding faculty over time. Unionization does not raise average salaries at any tenure-line rank: under parallel trends, we rule out gains of more than about \$900 a year for assistant professors and \$1,400 for full professors, roughly a tenth of the public-sector union wage gap. Unionization does change employment. Total faculty employment is unchanged, but the median unionizing institution employs four to eight fewer assistant professors than comparable institutions, a decline that follows an expansion in the years before unionization. Faculty who stay at unionizing institutions see faster salary growth than faculty elsewhere, largest for non-tenure-track faculty, and among tenure-line faculty concentrated below full professor, while exits and movement along the tenure track do not change. The findings broaden the margin of adjustment to collective bargaining from pay alone to the composition and careers of the academic workforce.

Keywords: faculty unionization, academic labor markets, higher education

JEL Codes: I23, J44, J51

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1 Introduction

Unions are no longer a blue-collar institution: about 45 percent of U.S. union members now work in management, professional, and related occupations ([U.S. Bureau of Labor Statistics, 2026](#)). Yet the evidence on what unions do comes overwhelmingly from the sectors in which they have traditionally organized, such as manufacturing, construction, and occupations paid on fixed salary schedules ([Blanchflower and Bryson, 2004](#); [Freeman, 1986](#)). Unions are increasingly appearing in less traditional sectors as well: among graduate students and faculty ([Mirabello et al., 2024](#)), medical residents ([Rooke-Ley et al., 2025](#)), and journalists ([Cohen and de Peuter, 2020](#)), whose work rests on specialized knowledge and whose pay is often tied to individual productivity.

Theory points in several directions, and there is little evidence on which forces dominate in these labor markets. A common pay scale may push the most productive workers to leave, even as a formal channel for complaints makes others more likely to stay ([Hirsch and Schumacher, 1998](#); [Freeman, 1980a](#)), a union facing an employer with little room to raise pay may win more in job security and workload than in wages, and the employer may respond through whom it hires rather than what it pays. It remains open whether bargaining among professionals raises pay once pre-existing differences between unionizing and other employers are accounted for, whether it changes whom employers hire and keep, and whether its effects on employers and on individual careers move together.

We study these questions for faculty in U.S. higher education, following both institutions and their faculty through the same unionization events. We combine the staggered timing of faculty unionization at U.S. colleges and universities between 1993 and 2023, dated by the recognition of each institution’s first faculty bargaining unit, with two panels: institution-level salaries and headcounts by rank from IPEDS, and restricted-use waves of the Survey of Doctorate Recipients (SDR), which follow individual doctorate holders and identify their employers. Earlier U.S. studies compare organized with unorganized institutions in the cross-section or in pooled panels with institution effects ([Freeman, 1978](#); [Barbezat, 1989](#); [Hedrick](#)

et al., 2011); we instead compare institutions and faculty before and after each unionization with those not yet unionized, as Baker et al. (2026) do for Canadian faculty. Observing each unionization in both employer averages and individual careers also separates what unionization does to an institution from what it does to the faculty who remain there, a distinction that matters whenever unionization changes who works for the employer (Frandsen, 2021). And because we measure the employment margin alongside pay, we can show that employment, not pay, is where unionization registers most clearly.

Higher education offers two advantages for this design. Faculty have unionized at different institutions in different years over several decades (Figure 1), and by 2024 bargaining units represented about 27 percent of U.S. faculty (Herbert et al., 2024). And academic careers follow an observable ladder, from assistant to full professor and onto or off the tenure track, so effects can be traced across career stages.

Unionization does not raise what institutions pay their tenure-line faculty. Average salaries within each rank are flat before unionization and do not rise after it; the only significant estimate is a small decline for associate professors, which is sensitive to the choice of comparison group. Under parallel trends, the estimates rule out salary gains of more than about \$900 a year for assistant professors and \$1,400 for full professors, roughly a tenth of the public-sector union wage gap over the same period (Kulkarni and Hirsch, 2021).

Unionization does change whom institutions employ. Total faculty employment is unchanged, but the median unionizing institution, with 66 assistant professors, employs roughly four to eight fewer of them than comparable institutions.

At the individual level, faculty who stay at unionizing institutions see salaries rise 3.3 log points faster than faculty elsewhere after unionization. Among tenure-line faculty, the gains are concentrated among those who were assistant or associate professors at unionization, at 10 to 11 log points, while full professors gain nothing; for associate professors, the gains arise among faculty who move beyond that rank. The largest gains are for faculty off the tenure track, whose salaries rise by 23 log points, although the estimate is imprecise, and

covers only non-tenure-track faculty with research doctorates. Unionization changes neither the rate at which faculty leave their institution nor their movement along the tenure track.

The decline in assistant professors does not come from higher pay, since average assistant-professor salaries do not rise. The likelier sources are the non-wage terms that faculty contracts set, such as job security, workload, and grievance procedures (Section 2.3). Where pay is set individually and anchored to outside offers, bargaining acts on the terms and number of positions more than on the pay attached to them.

The main threat to the design is that faculty organize in response to how their institutions are changing, so that unionizing institutions would have followed different paths even without a union. Evidence on contingent-faculty organizing suggests instead that elections track the legal and political climate for bargaining rather than local distress (Bennett, 2023), and we compare each unionizing institution with institutions not yet unionized, drawing never-unionized controls from a sample matched exactly on control, degree level, Carnegie class, and faculty size before unionization. In the SDR, we test directly whether unionization changes who leaves, using a sample that keeps movers and assigns each individual to the institution where they began. Section 5.4 discusses these threats and others, including anticipation during organizing campaigns.

Studies of unions in traditional settings find wage premiums and compressed pay structures (Freeman, 1980b, 1982; Card, 1996; Blanchflower and Bryson, 2004; Hirsch, 2004), but much smaller effects in designs that isolate newly organized units (DiNardo and Lee, 2004) or exploit the staggered adoption of state bargaining laws (Frandsen, 2016). Closest to our two-tier design, Frandsen (2021) finds large negative effects of unionization on establishments but small effects on their workers' earnings, a gap driven by changes in who works there. We find a related separation in a professional labor market: no premium at the employer, an employment response, and pay gains for individual faculty that do not appear in the employer average.

Studies of U.S. faculty unions find small salary premiums that shrink once the higher

pay of organizing institutions is accounted for (Freeman, 1978; Rees, 1993; Hedrick et al., 2011), with clearer effects on the structure of pay: contracts raise the return to seniority and lower the return to publications (Barbezat, 1989). Among Canadian faculty, unions had a negligible effect on average pay (Hosios and Siow, 2004), and Baker et al. (2026) find that salary floors raised pay at the bottom of the distribution with little effect on employment, and little average gain from certifications after 1996, which span our period. Our tenure-line results agree with theirs, our largest individual gain, for non-tenure-track faculty, the lowest-paid group in the SDR (Table 2), matches their bottom-of-distribution result, and the decline in assistant professors contrasts with their employment null.

The rest of the paper proceeds as follows. Section 2 describes faculty unions and the legal framework for bargaining, and Section 3 sets out the predictions of theory and prior evidence. Section 4 describes the data and Section 5 the empirical strategy. Section 6 presents institution-level estimates (Section 6.1), worker-level estimates (Section 6.2), and the comparison between them (Section 6.3). Section 7 interprets the results and concludes.

2 Institutional Background

2.1 Faculty unions in U.S. higher education

Faculty bargain collectively through a union that has been certified or recognized as the exclusive bargaining agent for a defined unit of faculty at an institution, so the terms it negotiates cover every faculty member in the unit, members and non-members alike. Since *Janus v. AFSCME*, public-sector unions may not charge non-members agency fees,¹ and right-to-work laws do the same for private employers in the states that have adopted them, so membership can fall well short of coverage. A unit may cover tenure-track faculty, non-tenure-track faculty, or both, and it may span a single campus or an entire state system. As of January 2024, 775 such units represented 402,217 faculty, about 27 percent of all U.S.

¹585 U.S. 878 (2018).

faculty, at more than 600 institutions in 30 states and the District of Columbia (Herbert et al., 2024, p. 17). Representation is concentrated in the public sector: it covers 52 percent of faculty at public two-year institutions and 33 percent at public four-year institutions, but only 8 percent at private non-profit four-year institutions (Herbert et al., 2024, Figure 4, p. 21). The gap is a product of law, which differs sharply between the two sectors.

2.2 Legal framework and the waves of unionization

No single law governs faculty bargaining in the United States. At public institutions, bargaining rights are set by state public-sector labor law, which varies across states and over time: some states impose a duty to bargain on public employers, others permit bargaining without requiring it or grant only a right to “meet and confer,” and a few prohibit public-sector bargaining outright (Freeman, 1986; Valletta and Freeman, 1988). States also differ in which subjects are bargainable and whether public employees may strike, and several cover public universities under a separate higher-education statute. Faculty organizing followed the spread of these laws. Michigan extended bargaining rights to public employees in 1965, New York in 1967, and Pennsylvania in 1970, and the faculty of Central Michigan University, recognized in 1969, were among the first at a four-year institution to bargain under such a law.

At private institutions, faculty fall under the National Labor Relations Act (NLRA). The National Labor Relations Board (NLRB) first asserted jurisdiction over private non-profit universities in 1970.² A decade later, in *NLRB v. Yeshiva University*, the Supreme Court held that faculty who exercise effective authority over academic policy are managerial employees, outside the Act’s protection.³ The decision stalled organizing among tenure-track faculty at private institutions, and the number of private-sector faculty units declined in the years that followed (Herbert et al., 2024, p. 28). Non-tenure-track faculty, who typically have

²*Cornell University*, 183 NLRB 329 (1970), reversing *Trustees of Columbia University* (1951).

³444 U.S. 672 (1980). Separately, *NLRB v. Catholic Bishop of Chicago*, 440 U.S. 490 (1979), and *Bethany College*, 369 NLRB No. 98 (2020), limit the Board’s jurisdiction over religiously affiliated institutions.

no role in shared governance, are generally not excluded under *Yeshiva*, and they account for nearly all private-sector organizing since: two-thirds of today’s faculty units at private four-year institutions represent non-tenure-track faculty alone (Herbert et al., 2024, Table 1, p. 22). In *Pacific Lutheran University* (2014), the Board narrowed the managerial exclusion, placing the burden of proof on the institution and finding that the full-time non-tenure-track faculty before it were not managerial.⁴

Faculty unionization came in two waves (Appendix Figure A1). The first, from the late 1960s through the 1970s, was overwhelmingly public and included a large share of two-year colleges (Table 1, last column). Early units mostly represented tenure-track faculty, often together with non-tenure-track colleagues in a single unit; separate non-tenure-track units became common only later (Herbert and van der Naald, 2024, pp. 182–183, as cited in Herbert et al., 2024, p. 28). Unionization then proceeded slowly for three decades before accelerating again after 2013 (Figure 1). This second wave is private and non-tenure-track (units recognized since 1993 by type and control, and institutions by their mix of units, are in Appendix Tables A1 and A2). Private-sector faculty units grew from 84 in 2012 to 150 in 2024 (Herbert et al., 2024, Table 6, p. 28), and of the faculty units formed between 2013 and 2023, 83 percent included non-tenure-track faculty and 72 percent represented them alone (Herbert et al., 2024, p. 30).

The legal regime also determines which institutions can be treated at all. Public institutions in states without enabling law, and tenure-track faculty at most private institutions after 1980, could not organize however their circumstances evolved, so some institutions are never treated for reasons of law rather than of choice. The enabling laws were in place by the 1980s (Freeman, 1986), before our estimation window opens in 1993. Later changes mostly restricted bargaining where it already existed, as in Wisconsin in 2011, which also repealed the bargaining rights granted to University of Wisconsin faculty in 2009, and Iowa in 2017, or extended it to public employees other than university faculty, as in Colorado in 2020 and

⁴361 NLRB 1404 (2014).

Virginia in 2021. Unionization in our window therefore reflects organizing under an existing legal regime more than changes in the law.

The institutions we study, those whose faculty first unionized in 1993 or later (Section 5.2), look little like the popular picture of faculty unions (Table 1). Fifty-seven percent are public, against 93 percent of institutions unionized before 1993. Twenty-one percent are R1 or R2 doctoral universities, against 8 percent of earlier-unionized institutions and 4 percent of never-treated institutions, and the share of associate’s colleges, 32 percent, is half that among earlier-unionized institutions (annual counts by Carnegie class are in Appendix Figure A2). Never-treated institutions are more often private and rarely research universities, which is why the institution-level design matches on institutional type (Section 5.2).

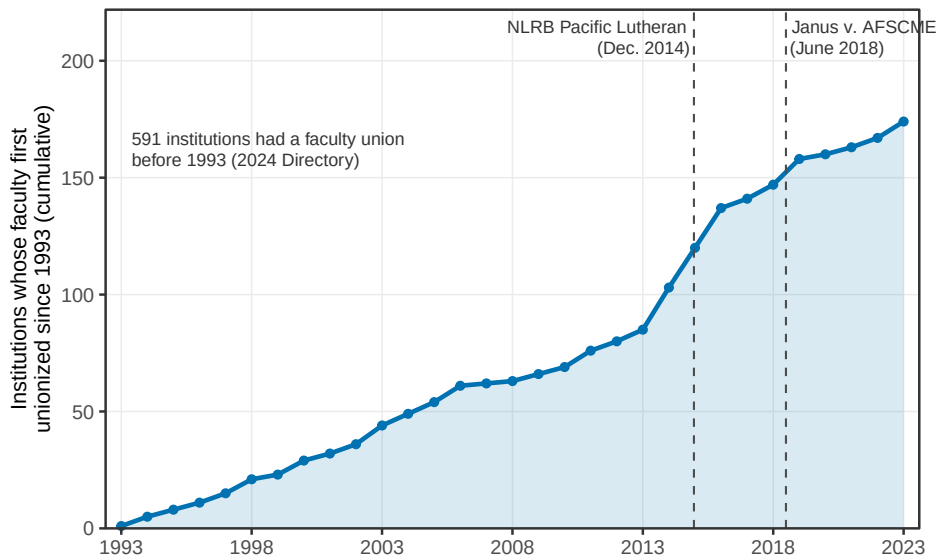


Figure 1: Cumulative number of institutions whose faculty first unionized, 1993–2023

Note: Running total of institutions in the institution-level analysis sample (Section 5.2) whose first faculty bargaining unit (tenure-track or non-tenure-track) was recognized between 1993 and 2023, by year of recognition; 174 institutions by 2023. The annotation gives the number of institutions whose first faculty unit was recognized before 1993 (591), counted from the 2024 Directory by institution; these institutions are not in the estimation sample and are not plotted. The Directory lists currently active units only, so recognitions later ended are not counted. Annual counts by Carnegie class are in Appendix Figure A2. Recognition dates from the 2024 Directory (Herbert et al., 2024). Dashed lines mark the NLRB decision in *Pacific Lutheran University* (December 2014; 361 NLRB 1404) and *Janus v. AFSCME* (June 2018; 585 U.S. 878).

Table 1: Institutional characteristics by treatment group, unionization in 1993 or later

Characteristic	Treated post-1993	Never-treated	Unionized pre-1993
Institutions	174	4,607	569
Public	0.57	0.37	0.93
Private NFP	0.43	0.63	0.07
4-year	0.78	0.64	0.47
2-year	0.22	0.29	0.53
Trade/vocational	0.00	0.06	0.00
HBCU	0.02	0.03	0.02
Land-grant	0.03	0.03	0.04
<i>Carnegie classification</i>			
R1/R2 Doctoral	0.21	0.04	0.08
Masters	0.21	0.10	0.18
Baccalaureate	0.10	0.12	0.04
Associates	0.32	0.17	0.64
Specialized/Other	0.11	0.14	0.03
Carnegie missing	0.05	0.43	0.03
Median enrollment size (INSTSIZE)	2	2	3

Note: Institutions in the institution-level analysis panel (IPEDS, 1984–2023, for-profit institutions excluded), before matching. “Treated post-1993”: first faculty bargaining unit recognized in 1993 or later. “Never-treated”: no faculty unit recognized. “Unionized pre-1993”: first faculty unit recognized before 1993; these institutions are excluded from estimation and shown for comparison. Shares are fractions of institutions in each group. Carnegie classification sourced from IPEDS HD files (available from 1994); shares computed over all institutions in the group (missing counted separately). *INSTSIZE*: 1 = <1,000 students; 2 = 1,000–4,999; 3 = 5,000–9,999; 4 = 10,000–19,999; 5 ≥ 20,000.

2.3 Recognition and bargaining

Recognition is the point at which the employment relationship changes in law. After an organizing campaign, a union becomes the bargaining agent by winning a representation election, conducted by the NLRB or a state labor board, or through voluntary recognition by the institution, usually after a card check (Herbert et al., 2024, p. 12). Where the governing law imposes a duty to bargain, recognition obliges the institution to negotiate over the terms of employment and bars it from changing those terms unilaterally while bargaining is under way.⁵ Negotiation then produces a first contract, whose provisions take effect on ratification or are phased in over its term. Of the 174 institutions whose first faculty unit was recognized between 1993 and 2023, 140 had ratified a first contract by 2024, and for these the median lag from recognition to first contract is two years, with an interquartile range of one to two years (Herbert et al., 2024).

What a union can bargain over depends on how pay is set without one. Tenure-line faculty are paid individually rather than by a schedule attached to the job: salaries are set at hire against a national market, adjusted by merit increases, renegotiated in response to outside offers, and raised with promotion along a ladder from assistant to full professor (Barbezat, 1989; Ransom, 1993). For tenured faculty, tenure already provides much of the job security that unions elsewhere negotiate for.

Our reading of a sample of faculty contracts shows that they cover four broad areas. Pay provisions set salary floors and scales and negotiate across-the-board and cost-of-living raises. Career provisions govern evaluation, promotion, and tenure, and establish grievance procedures. Employment provisions regulate appointment, renewal, seniority, layoffs, and job security. Finally, contracts set workload and benefits and formalize faculty participation in governance. The non-pay provisions can matter most to research faculty, who expect little from collective wage setting: lighter teaching loads, clearer standards for tenure and

⁵*NLRB v. Katz*, 369 U.S. 736 (1962). State public-sector boards, including those of Michigan and New York, apply the same rule.

promotion, a formal role in governance, and protection of academic freedom.

We date treatment at recognition, using the year in which the institution’s first faculty bargaining unit was recognized, as recorded in the 2024 Directory (Section 4.1), rather than at the first contract, and we refer to this date as the institution’s unionization. Recognition is the earliest date at which the institution’s legal obligations change, and an institution can respond to an organized faculty before any contract is signed. The Directory also records it more completely, for 98 percent of faculty units against 90 percent for first-contract ratification (Herbert et al., 2024, p. 14, nn. 13–14), and because ratification dates are missing mainly for units still without a contract, dating treatment at the contract would condition on reaching one. Section 5.4 discusses what this choice implies for the timing of effects.

3 Predictions from Theory and Prior Evidence

Most evidence on what unions do comes from settings in which pay is attached to the job: a contract sets a wage for each position, and a union raises pay by raising that schedule. Academic pay works differently: tenure-line faculty are paid individually, through starting salaries, merit increases, outside offers, and promotion (Section 2.3), and a faculty union bargains over the rules that govern these salaries, over the terms of the jobs on the ladder, and over the jobs off it. This section applies the standard account of unions (Freeman, 1982; Blanchflower and Bryson, 2004) to that setting and states what it predicts for each outcome the paper measures, in the order the results take them up. The institution panel tests the predictions for pay level and employment (Sections 3.1–3.2); the SDR tests the pay-level prediction again, along with those for career stages and for exits and promotion (Sections 3.3–3.4); and the comparison of the two tiers tests the last (Section 3.5).

3.1 Pay level

Unions raise pay where they can capture rents, and the classic estimates find a premium for covered workers ([Blanchflower and Bryson, 2004](#); [Hirsch, 2004](#)). For 1994–2018, longitudinal estimates from displaced workers put that premium near 15 percent, and cross-sectional estimates put the public-sector gap at about half the private-sector one ([Kulkarni and Hirsch, 2021](#)). Designs that isolate newly organized units find much less: comparing narrow union election wins with narrow losses, [DiNardo and Lee \(2004\)](#) find wage effects close to zero. The faculty literature reaches a similar place. Organized institutions already paid more before they organized, so cross-sectional comparisons overstate the premium ([Freeman, 1978](#)); individual-level data from the late 1970s put it typically below two percent ([Barbezat, 1989](#)); and institution fixed effects and local living costs shrink or reverse it ([Rees, 1993](#); [Hedrick et al., 2011](#)), although larger premiums appear in the 1990s cross-section ([Monks, 2000](#)). In the early studies the premium varies with how long an institution has been organized, and in [Freeman \(1978\)](#) it grows with years organized.

Two forces pull in opposite directions in academia. Pay for research faculty is anchored to outside offers in a national market, so an institution that already pays to retain its faculty has little room to pay more. Against this, universities hold substantial monopsony power over tenure-track faculty, though not over non-tenure-track faculty ([Goolsbee and Syverson, 2023](#)), and faculty who stay at one institution earn less than similar faculty who move ([Ransom, 1993](#)). Where employers hold such power, a union can raise pay without reducing employment ([Dodini et al., 2022](#)). The first force predicts a positive premium smaller than in the settings where unions are usually studied ([Hedrick et al., 2011](#); [Hosios and Siow, 2004](#)); the second leaves room for a larger one, so the size of the premium, and whether it is positive at all, is an empirical question. Because faculty premiums grow with years organized, any effect should build in the years after unionization rather than appear at it.

3.2 Employment and the composition of the faculty

Whether higher labor costs reduce covered employment depends on what the contract governs. If the employer keeps the right to manage employment, a higher negotiated wage moves employment down the labor demand curve (Nickell and Andrews, 1983); if wages and employment are bargained jointly, employment can stay at or above its non-union level (McDonald and Solow, 1981). The cost to the employer need not come through pay. Contracts also set job security, workload limits, and grievance procedures, and in matched employer–employee data unionization has large negative effects on establishments but small effects on the earnings of their workers (Frandsen, 2021). In academia the margin of adjustment is the composition of the faculty as much as its size: faculty employment has shifted over several decades toward non-tenure-track positions (Zhang et al., 2021), for whom universities face elastic supply (Goolsbee and Syverson, 2023).

The prediction is that an employer that retains control over staffing responds to unionization with fewer covered entry-level tenure-line positions, and that any substitution toward non-tenure-track faculty depends on whether they are in the bargaining unit. Because the cost need not come through pay, the prediction does not require a salary premium.

3.3 Pay across career stages

Unions change how pay is set as much as how much is paid. Collective agreements replace individually negotiated raises with salary floors, scales, and across-the-board increases, which compress pay within the employer (Freeman, 1980b, 1982) and raise the wages of less-skilled workers more than those of the more skilled (Card, 1996). Faculty evidence shows the same shift. Unionization raised the return to seniority and lowered the returns to publications and post-degree experience (Barbezat, 1989), and among Canadian faculty, contractual salary floors raised pay at the bottom of the distribution and left the top unaffected (Baker et al., 2026). A common schedule raises pay most where individually set pay sits furthest below it, and least where pay reflects accumulated merit and outside offers. The prediction is that

gains are largest for junior faculty and for faculty off the tenure track, and smallest for full professors.

The monopsony evidence implies a competing prediction. Faculty who stay at their institution fall behind faculty who move ([Ransom, 1993](#)), and monopsony power over tenure-track faculty increases with rank, being largest over full professors ([Goolsbee and Syverson, 2023](#)). If contracts raise the return to seniority, gains are largest for long-serving faculty, who are concentrated above the entry rank. Because the SDR follows individuals by rank, it can distinguish the two predictions.

3.4 Exits and promotion

Unions give workers voice as an alternative to exit ([Hirschman, 1970](#)), and union workers quit less ([Freeman, 1980a](#)). Compression works the other way: a common scale pays the most able workers less than they could earn elsewhere, so union jobs retain a less able mix of high-skill workers ([Hirsch and Schumacher, 1998](#)). The reverse change among public school teachers shows the same force: when Wisconsin ended bargaining over seniority-based salary schedules, districts that adopted flexible pay raised salaries for high-quality teachers and drew them from other districts ([Biasi, 2021](#)). Among faculty, holding salary and benefits constant, unionization raises the retention of associate and full professors, though not of assistant professors, and only several years after the representation election ([Rees, 1994](#)). The net effect of unionization on exits is therefore ambiguous, and the sorting channel predicts that any increase is concentrated among senior, highly paid faculty.

Contracts also write down the rules for advancement. Seniority carries more weight in promotion decisions in union work groups, though by less than commonly supposed ([Abraham and Medoff, 1985](#)), and faculty contracts set the procedures for evaluation, promotion, and tenure (Section [2.3](#)). These procedures can change how quickly faculty move up the ladder.

3.5 Employer averages and individual careers

The two data tiers measure different objects. The institution panel measures the average pay and headcount of each rank, over whoever holds the rank in each period; the SDR follows individual faculty through unionization. Effects on an establishment can differ from effects on the workers who remain there when unionization changes who works there ([Frandsen, 2021](#)). If unionization changes hiring into a rank, promotion out of it, or who leaves, the average pay of a rank and the pay of the faculty who stay can move differently, so the two tiers can disagree without either being wrong.

4 Data

4.1 Union recognition dates

Recognition dates come from the 2024 Directory of Bargaining Agents and Contracts in Institutions of Higher Education ([Herbert et al., 2024](#)). For each bargaining unit, the Directory records the bargaining agent, the employee groups the unit covers, the year the unit was recognized, and the institution’s IPEDS identifier, which links it to both panels. An institution is treated from the first year in which it recognizes a unit that includes tenure-track or non-tenure-track faculty, whether the unit covers one group or both, and it remains treated in every later year. Institutions with no such unit are never treated. The treatment date is thus the date faculty acquire a bargaining agent, not the date a first contract takes effect (Section 2.3). The same date defines treatment in both panels. A unit that spans the campuses of a multi-campus system carries no single IPEDS identifier in the Directory; we assign its recognition year to every campus that IPEDS lists as a member of that system, and drop the few units that cannot be linked to any campus. Because the Directory lists the units in place in 2024, an institution whose only faculty unit decertified before then is coded as never treated and enters the comparison group. An institution whose unit decertified

before the sample window, and whose union left no lasting effect, is a valid control. One whose unit was active during the window is not: if its union moved pay or employment in the same direction as at institutions that remain unionized, its presence among the controls biases the estimates toward zero.

4.2 Individual panel: Survey of Doctorate Recipients

The Survey of Doctorate Recipients (SDR), conducted by the National Center for Science and Engineering Statistics (NCSES), is a longitudinal survey of individuals who hold a research doctorate in science, engineering, or health from a U.S. institution ([National Center for Science and Engineering Statistics, 2025](#)). Its sample is drawn from the Survey of Earned Doctorates, the census of new U.S. research doctorates, and is refreshed with new graduates at each cycle, while sampled individuals are resurveyed in later waves. We use the fifteen waves fielded between 1993 and 2023. The public-use SDR does not identify a respondent’s employer. The restricted-use files report the employing institution, which is what allows individuals to be linked to their institution’s recognition date. Despite this coverage, the SDR remains surprisingly underused as a source on academic careers. [Stansbury and Rodriguez \(2026\)](#) show its value, using the 1993–2021 waves to trace the careers of U.S. doctorate recipients through tenure-track placement, tenure, and pay.

We restrict the panel to respondents employed at a postsecondary institution in the survey reference week. A robustness specification further restricts the wage sample to faculty employed at research universities. The outcomes are annual salary on the principal job in 2019 dollars, faculty rank, tenure status, and employment status at the next wave; age and seniority (years with the current employer) enter as covariates. [Appendix B](#) describes how these variables are constructed and the features of the survey design that bear on the panel.

The analysis uses two samples. The stayer sample, used for the wage and promotion analyses, keeps individuals whose employing institution is the same in every wave in which it is reported. The transition sample, used for the retention analysis, keeps movers as well

and takes each individual’s treatment timing from the first institution at which they are observed. Appendix Tables [A3–A4](#) summarize both samples by treatment group. Faculty at institutions unionized during the window are concentrated in private research universities, while those at institutions unionized before 1993 are mostly at public institutions. Appendix Figure [A3](#) plots mean salaries by treatment group and institutional control.

4.3 Institution panel: IPEDS

The institution panel comes from the IPEDS survey of instructional staff salaries, which reports total salary outlays and headcounts of full-time instructional faculty by academic rank ([National Center for Education Statistics, 1984–2023](#)). We divide outlays by headcount to obtain average salary in 2019 dollars and construct both measures overall and for five rank groups: professor, associate professor, assistant professor, non-tenure-track (instructors and lecturers), and faculty without academic rank. The first four mirror the SDR. We keep faculty without academic rank as a separate group because in 2001–02 many institutions moved their faculty out of that category, mostly into the instructor rank; totals that include them are close to continuous across the change, while totals over the ranked categories alone are not. The panel covers 1984–2023, excluding the uncollected years 1986, 1988, and 2000, and drops private for-profit institutions. Appendix Table [A5](#) reports the matched institution sample used in estimation (Section [5.2](#)).

The two panels differ in whom they cover and what they measure. IPEDS covers all full-time instructional faculty, whatever their degree or field, and its salary is an institution average that moves with the composition of the faculty as well as with pay. The SDR covers only doctorate holders in science, engineering, and health, including those in non-faculty positions, but it observes each person’s own salary, rank, and tenure status, so it can separate changes in pay from changes in who is employed.

Table [2](#) describes the two estimation samples. In the institution panel, unionizing institutions resemble their matched controls in the period before unionization. Their average

salaries are 1 to 4 percent higher at every rank, and the median unionizing institution employs as many assistant professors as the median control, 66. They are larger in the senior ranks, with medians of 71 professors and 78 associate professors against 56 and 60.

Table 2: The two estimation samples

<i>Panel A. Institution panel (IPEDS), last period before unionization</i>				
	Mean salary (2019 \$)		Median headcount	
	Treated	Controls	Treated	Controls
Overall	82,897	80,462	172	141
Professor	116,424	114,121	71	56
Assoc. Professor	87,547	85,433	78	60
Asst. Professor	74,314	73,446	66	66
Non-tenure-track	59,821	57,752	30	34
Institutions (overall salary sample)	149	1,468		

<i>Panel B. Individual panel (SDR), stayer sample</i>		
	Treated in window	Never treated
Persons	13,698	36,851
Person-waves	36,048	97,602
Age	46.9	48.5
Years with employer	10.6	11.4
Federally supported work, prior year	0.53	0.37
Public institution	0.40	0.68
Research university	0.87	0.55
<i>Mean salary (2019 \$)</i>		
Overall	91,265	82,111
Professor	136,828	108,963
Assoc. Professor	102,735	84,953
Asst. Professor	89,670	74,025
Non-tenure-track	58,817	44,727

Note: Panel A: main institution sample (balanced panel, exact matching on sector, degree level, Carnegie class, and faculty-size class before unionization; Section 5.2). Treated: institutions recognizing a first faculty unit in 1993–2023, measured in the last period before unionization. Controls: matched never-treated institutions, weighted by their matching weights and measured in the same periods as the treated institutions. Salary is the institution average, total outlays divided by headcount. Each rank group is a separate balanced sample; the numbers of treated institutions by rank group are in Tables 3 and 4. Panel B: SDR stayer sample (Section 4.2), all waves pooled; faculty at institutions unionized before the sample window are omitted (Appendix Table A3). Shares are over person-waves with the characteristic reported. In both panels, Overall includes faculty without academic rank.

5 Empirical Strategy

We estimate the effect of faculty unionization on the salary trajectories and career progression of university faculty by exploiting the staggered timing of faculty unionization. Institutions, or the individuals employed at them, are compared before and after their institution’s faculty first unionize, relative to institutions not yet unionized. The institution panel observes employers annually and allows us to build the comparison group by exact matching on institutional type; the SDR follows the same doctorate holders across biennial waves and so holds the worker fixed.

5.1 Staggered difference-in-differences

Treatment is the first recognition of a faculty bargaining unit at the unit’s institution (Section 4.1) and does not reverse. Let G_i denote the period in which it occurs, with $G_i = 0$ if the institution does not unionize within the sample window. Because treatment is defined at the institution, the estimates are intent-to-treat effects of unionization at the employing institution rather than effects of individual membership or coverage: membership is voluntary for most faculty (Section 2.1), and a faculty member at a unionizing institution need not belong to the recognized unit, as when only non-tenure-track faculty are represented.

We estimate the group-time average treatment effects $ATT(g, t)$ of Callaway and Sant’Anna (2021b) with the `did` package (Callaway and Sant’Anna, 2021a). Each compares the outcome change of cohort g between a base period $b(g)$, the last period observed before g , and period t with that of units not yet unionized by t , so already-treated units never serve as controls (Goodman-Bacon, 2021; Sun and Abraham, 2021). Identification rests on no anticipation and on parallel trends conditional on X_i : the matching cell in the institution panel, and age and seniority in the SDR.

We report the simple aggregate, a cohort-size-weighted average of all post-treatment $ATT(g, t)$, and event studies that average across cohorts at each event time $e = t - g$. Event

times $e \in [-5, 10]$ enter the aggregation and $e \in [-5, 5]$ survey periods are reported in both tiers. In the institution panel we use a universal base period, so every coefficient, before or after unionization, is measured relative to $b(g)$ and pre-period coefficients trace cumulative drift ahead of unionization. Standard errors come from a multiplier bootstrap clustered at the institution, and event-study bands are simultaneous across event times.

5.2 Institution-level design

The institution panel is estimated on the SDR’s time grid. Calendar years are grouped into periods ending at each SDR wave (two-year periods before 1993), outcomes are averaged within each period, and the year of unionization is assigned to the first period at or after it, as in the SDR. Event time then means the same thing in both tiers, so differences between their dynamics reflect population and outcome rather than how time is measured. The outcomes are log average real salary and log faculty headcount, overall and for professors, associate professors, assistant professors, and non-tenure-track faculty. Faculty without academic rank enter only the overall measures, because only a handful of treated institutions report them in every period (Section 4.3).

The treated cohorts are institutions that first unionized between 1993 and 2023, the SDR window, so that both tiers estimate effects of the same wave of organizing. Institutions unionized before 1993 are dropped: they are treated throughout and cannot serve as controls, and their own unionization falls outside the window. The panel still begins in 1984, so the earliest cohorts retain their pre-unionization periods. Each outcome is estimated on a balanced panel of institutions that report it in all 19 periods between 1984 and 2023. The salary survey’s coverage shifted over the window, most sharply in 2001–02, when many institutions moved faculty from the no-academic-rank category into ranked categories (Section 4.3); in an unbalanced panel such changes in who reports would register as changes in pay. Balancing retains 149 of 173 matched treated institutions for overall salary and about 100 for each tenure-line rank, mostly institutions that have reported since the 1980s.

Controls are institutions not yet unionized by period t , both never-unionized and later-unionizing, the same comparison as in the SDR. The never-treated institutions are drawn from a matched sample. We match exactly on the interaction of institutional control (public or private not-for-profit), degree level, Carnegie class, and one of six faculty-size classes, each measured before unionization: control and degree level from an institution’s first IPEDS directory record, Carnegie class from the earliest classification, and size from average full-time instructional headcount over its first three salary reports. For most institutions the earliest classification is the 1994 edition, which is built from degree and federal-support data for 1989–91 ([Carnegie Foundation for the Advancement of Teaching, 1994](#)) and so predates every post-1993 unionization. The match is estimated on the post-1993 cohorts and re-estimated within each outcome’s balanced sample. Never-treated institutions are reweighted to the treated distribution across cells, and treated institutions in cells without a never-treated counterpart are dropped, so the estimand is the effect among treated institutions with a comparable never-treated institution. No further covariates enter. Appendix Figure [A4](#) reports balance before and after weighting.

Four alternatives accompany the main specification: (i) an unbalanced panel of all institution-periods, which shows how much changes in reporting move the estimates; (ii) the balanced panel without matching weights; (iii) a Sun–Abraham event study with institution and period fixed effects on the unbalanced matched panel ([Sun and Abraham, 2021](#)); and (iv) later-unionizing institutions as the only controls, which asks less of comparability because every institution eventually unionizes, at the cost of the latest cohort serving only as a control. Splits by institutional control and unionization era use the main specification (Appendix Tables [A6](#) and [A7](#)).

5.3 Individual-level design

In the SDR, t indexes survey waves by sequence rather than calendar year, because the interval between waves is sometimes three years rather than two. G_i is the first wave at or

after the unionization of the individual’s institution, and event time is measured in survey periods. Individuals whose institution unionized at or before the first wave are always treated and contribute no identifying variation. Because the first wave is 1993, this includes 1993 unionizations, so the SDR’s treated cohorts begin with 1994 unionizations, whereas the institution panel, which starts in 1984, also identifies the 1993 cohort.

The wage and promotion analyses use the stayer sample and the retention analysis the transition sample (Section 4.2). In the transition sample treatment is anchored to the first institution at which an individual is observed, so the retention estimates are effects of unionization at the origin institution on the probability of leaving it. The retention outcome equals one if an individual observed at the next wave has moved to another institution, left academic employment, become unemployed, or left the labor force; individuals not observed at the next wave are excluded rather than counted as leavers. Because most respondents appear in only a few of the fifteen waves, the panel cannot be balanced, and each $ATT(g, t)$ is estimated in repeated-cross-section form: it compares the individuals observed in t with those observed in $b(g)$ rather than within-person changes, retaining the person identifier for inference.

Controls are individuals at institutions not yet unionized by t , including never-unionized institutions, the same definition as in the institution panel, so that the two tiers share a comparison group. Unlike the institution panel, the SDR does not match employers on institutional type. With individual data, we instead condition directly on the worker characteristics that most shape pay and exit over a career, age and seniority (years with the current employer), and restricting the sample to research universities, a more homogeneous set of employers, yields qualitatively similar wage estimates (Figure 4). Each $ATT(g, t)$ is estimated with the doubly robust estimator of Sant’Anna and Zhao (2020). We deliberately exclude faculty rank, tenure status, hours, and work activity: unionization could change each of them, which makes them outcomes rather than controls. Standard errors are clustered at the current institution in the stayer sample, which does not change by construction, and at

the first-observed institution in the transition sample. Estimates are not weighted by the SDR survey weights, which are designed to recover population totals and means. Unweighted and weighted estimators target the same treatment effect unless it varies across the survey’s sampling strata (Solon et al., 2015).

Splitting by current rank conditions on a variable unionization may change: if it alters promotion, the composition of each rank differs between treated and control institutions after unionization. We therefore also split by baseline rank, fixed before treatment: the rank at the last wave before unionization for individuals at eventually unionized institutions, and the rank at the first wave observed for those at never-unionized institutions. Because the two groups are classified at different career stages, the eve of unionization versus panel entry, differences between the current-rank and baseline-rank estimates should not be read as a decomposition into promotion and within-rank pay effects. The baseline-rank specifications are estimated with and without the age and seniority covariates.

5.4 Threats to identification

Five features of the setting bear on the identifying assumptions or on how the estimates should be read. Section 6 returns to each where it bears on the estimates.

Selection into unionization. Institutions unionized after 1993 differ from never-unionized institutions in levels (Table 1), but fixed differences are removed by differencing. Parallel trends fails only if faculty organize in response to how their outcomes are *changing*. The leading case is organizing in response to deteriorating pay or working conditions: treated institutions would then have been on a declining path even without unionization, and the estimates would understate any gain. Alternatively, unionization may track the legal and institutional climate for collective bargaining rather than local distress (Section 2.2), in which case its timing carries little information about pre-existing trajectories. Evidence on contingent-faculty organizing at private institutions fits this view: certification elections

are more likely in states with more liberal legislatures and more prior contingent-faculty elections, and are unrelated to an institution’s enrollment or number of part-time faculty (Bennett, 2023). The event-study pre-periods test this directly.

Anticipation during organizing campaigns. Recognition ends an organizing campaign. During it, administrations may raise pay or improve conditions to weaken support for the union (Freeman and Kleiner, 1990), and faculty who oppose the union may begin to leave. Any such response in $b(g)$ violates no anticipation; pre-emptive raises would bias the wage effect toward zero. The coefficients just before recognition show whether outcomes move ahead of it. One pre-recognition pattern fits an organizing campaign: assistant-professor employment rises in the periods before recognition (Section 6.1.2), although no pre-recognition estimate is individually significant.

SDR selection and composition. Membership in the stayer sample is defined over an individual’s entire observed panel, including waves after unionization. If unionization changes who stays, the stayer sample at treated institutions is selected on a post-treatment outcome, and the wage estimates describe faculty who remain rather than those employed at unionization. If faculty with the best outside options disproportionately leave, as models of union wage compression predict (Section 3), the stayer-sample wage effects are biased downward relative to the effect on the initial faculty. More generally, because each $ATT(g, t)$ compares whoever is observed in t and $b(g)$, a change in who responds or remains moves the estimates as a change in outcomes would. The 2015 sample expansion alters the sample without reflecting individual behavior and is not counted as exit. The stayer and transition samples bracket the problem: the first holds employers fixed at the cost of conditioning on staying, while the second does not condition on staying but anchors treatment to where an individual began.

Survey timing. Event time is measured in survey periods in both tiers (Section 5.2), so $e = 0$ spans zero to two years after unionization and a period does not correspond to a fixed number of years. In the institution panel, the first treated period pools up to three years, which can include years before unionization and bias estimates downward.

Recognition versus first contract. Provisions that could change pay, promotion, and job security take effect only once a first contract is ratified, typically two years after recognition (Section 2.3), or roughly one survey period. Effects that run through contract terms should therefore not appear at once: the period of recognition, $e = 0$, mostly precedes any contract and combines bargaining status with anticipation of its terms, so such effects should emerge from $e = 1$ and build over time. A small estimate at $e = 0$ is consistent with this timing rather than evidence against an effect. Dating treatment at recognition may understate the effect of the contract itself, but it avoids conditioning the treatment date on the outcome of bargaining.

6 Results

6.1 Employer Response

This subsection tests the two predictions of Section 3 that concern institutions: how unionization changes the average pay of the faculty an institution employs (Section 3.1), and how it changes their number and mix (Section 3.2). The institution panel covers the full faculty, including those who enter and leave each rank, at every institution in the matched sample. All estimates use the main specification (Section 5.2): a balanced panel of institutions that report the outcome in all 19 survey periods from 1984 to 2023, with not-yet-unionized controls and never-unionized controls exactly matched on pre-unionization characteristics. Because first contracts follow recognition by about one survey period (Section 5.4), effects that run through contract terms should appear from $e = 1$.

Average salaries within rank barely move after unionization; a small decline for associate professors is the only significant estimate. Total employment does not change, but the number of assistant professors decline post unionization.

6.1.1 Institution-average salaries

Unionization does not raise average salaries within any rank (Table 3). The overall average salary, which pools full-time instructional faculty of all ranks, including those without academic rank, falls by 1.5 log points (standard error 0.9), significant only at the 10 percent level. The estimates for professors (-0.7 log points), assistant professors (-0.5), and non-tenure-track faculty (-0.5) are small and insignificant. Associate professors are the exception, at -1.7 log points (standard error 0.8), significant at the 5 percent level. Because the overall estimate lies below every rank estimate but the associate one, part of it reflects shifts in the rank mix.

The event studies show no movement before unionization (Figure 2). For the three tenure-line ranks, every pre-unionization estimate lies within 0.9 log points of zero, and none is significant. The associate-professor series is the cleanest: its pre-unionization estimates lie within 0.3 log points of zero, its post-unionization estimates settle between -1.0 and -1.5 log points, and only the unionization-period estimate is individually significant. The non-tenure-track series is noisier, with a pre-unionization estimate of -2.2 log points two periods before unionization and post-unionization estimates that alternate in sign.

The associate-professor estimate holds in a within-institution Sun–Abraham event study (-1.9 log points) and when matching on latest rather than pre-unionization characteristics (-1.8). It shrinks to an insignificant, less precise -1.2 with only later-unionizing institutions as controls, and to -0.5 without matching weights, so it depends on comparing treated institutions with never-unionized institutions of the same type. On the unbalanced panel, estimates are larger at every rank (-4.0 for associate, -3.1 for professor, and -2.6 for assistant professors), reflecting changes in which institutions report each rank, which the

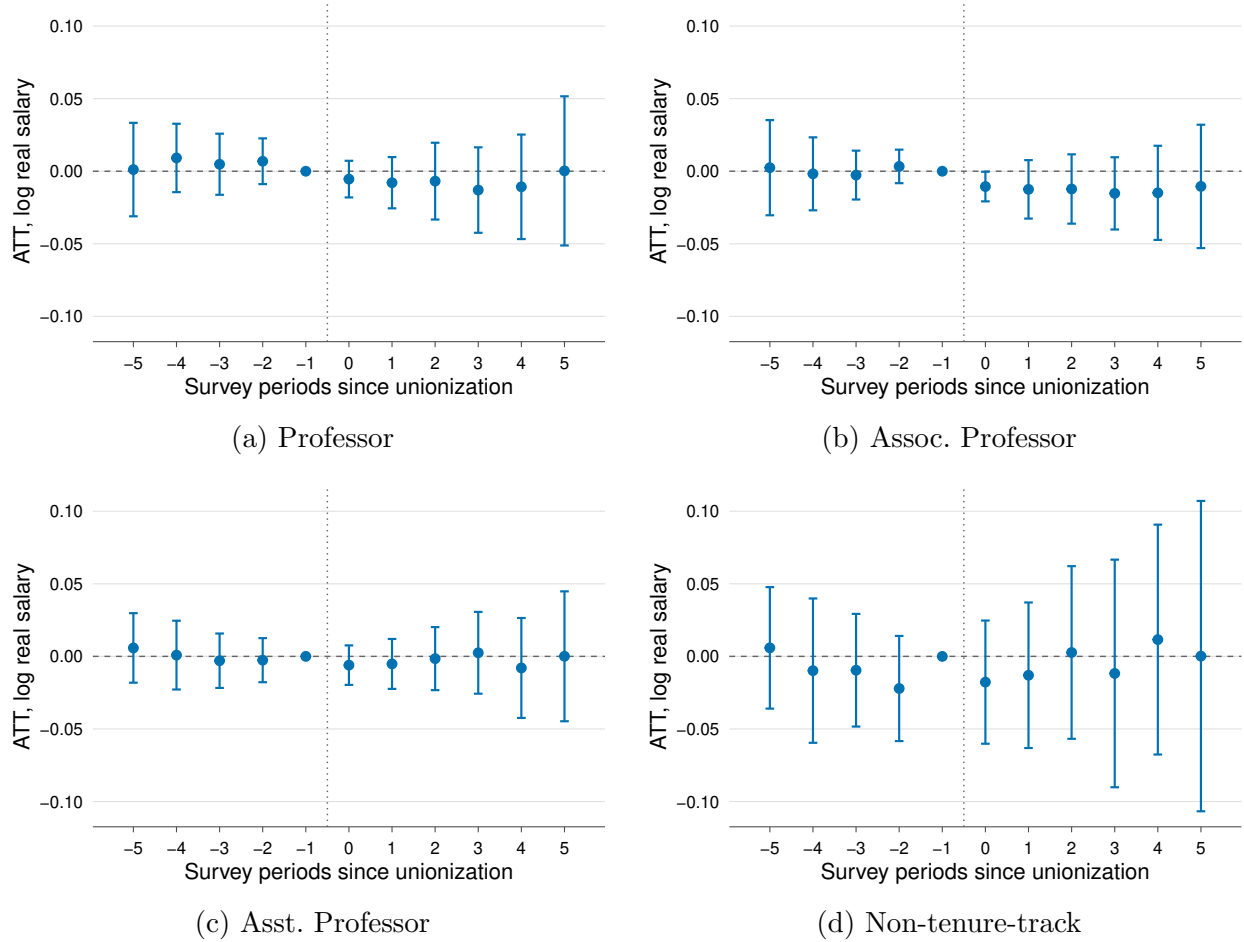


Figure 2: Institution-average log real salary around unionization, by rank group, IPEDS

Note: Callaway–Sant’Anna event-study estimates of the effect of unionization on log institution-average real salary (2019 dollars) within each rank group. Event time is in SDR survey periods; estimates are relative to the last period before unionization ($e = -1$). Balanced panel of institutions observed in all 19 periods, 1984–2023; controls are not-yet-unionized institutions, including never-unionized institutions exactly matched on pre-unionization characteristics. Bars show 95 percent simultaneous confidence bands from a multiplier bootstrap clustered by institution. Panels share a vertical axis. Faculty without academic rank are not shown (four to seven treated institutions).

Table 3: Institution-average salary effects by rank group and specification, IPEDS

	Main	Unbalanced	Unmatched	Within-inst.	Later-treated
Overall	−0.015*	−0.025***	−0.017**	−0.019**	−0.019
	(0.009)	(0.007)	(0.008)	(0.008)	(0.014)
<i>N</i> treated inst.	149	173	150	173	150
Pre-unionization mean	11.296				
Professor	−0.007	−0.031*	0.005	−0.024**	−0.002
	(0.010)	(0.016)	(0.009)	(0.011)	(0.016)
<i>N</i> treated inst.	101	155	102	155	102
Pre-unionization mean	11.630				
Assoc. Professor	−0.017**	−0.040***	−0.005	−0.019***	−0.012
	(0.008)	(0.013)	(0.007)	(0.007)	(0.012)
<i>N</i> treated inst.	102	146	103	146	103
Pre-unionization mean	11.361				
Asst. Professor	−0.005	−0.025**	0.008	−0.010	0.004
	(0.009)	(0.011)	(0.008)	(0.008)	(0.012)
<i>N</i> treated inst.	102	146	103	146	103
Pre-unionization mean	11.196				
Non-tenure-track	−0.005	0.044	−0.005	−0.008	−0.015
	(0.023)	(0.030)	(0.022)	(0.019)	(0.025)
<i>N</i> treated inst.	59	167	62	167	62
Pre-unionization mean	10.981				

Note: Callaway–Sant’Anna (2021) simple aggregate ATT. Treated group restricted to institutions unionizing (first faculty unit recognized) in 1993–2023; institutions unionized before 1993 are dropped entirely. Panel years 1984–2023. Rank structure mirrors the individual-level SDR analysis: Professor, Assoc. and Asst. Professor separate, Instructor and Lecturer pooled into non-tenure-track as combined salary outlay divided by combined headcount. Overall covers all full-time instructional faculty, including no academic rank. Years are binned onto the SDR survey waves (each year assigned to the first wave at or after it; 2-year bins before 1993), outcomes averaged within bin, and unionization assigned to the first wave at or after the unionization year. Unless noted: balanced panel (institutions observed in all 19 wave bins, separately for each outcome and rank group); controls are not-yet-treated institutions (never-treated plus those unionized after the period); controls exactly matched to the treated on sector $\times$ degree level $\times$ Carnegie class $\times$ faculty-size class, all measured before unionization, with the match re-run within each balanced sample. Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Unbalanced: all institution-periods, matched on the full sample. Unmatched: balanced, no matching weights. Within-inst.: Sun–Abraham event study with institution and period fixed effects on the unbalanced matched panel, aggregated over post-unionization cells. Later-treated: balanced, never-treated institutions dropped, so controls are institutions unionized after the period (unweighted). Pre-unionization mean: average among treated institutions in the main sample in the last wave bin before unionization, the event-study base period. Log real salary, 2019 dollars.

balanced panel holds fixed (Section 5.2).

The salary estimates differ little by sector or era. The associate-professor decline is concentrated at private not-for-profit institutions (-2.4 log points, standard error 1.1), with none at public institutions (-0.1), and the pooled estimate lies between the two (Appendix Table A6). Cohorts unionized in 1993–2003 and from 2004 on give insignificant estimates at every rank (Appendix Table A7). The era split is not a decomposition: each era is re-matched and loses the other era’s institutions as controls, which is why the pooled overall and associate-professor estimates lie outside the range of the two. Only 33 institutions unionized before 2004 survive balancing, about 16 per rank.

One explanation for the absence of a premium can be checked against these estimates: first contracts lag recognition, but the event studies show no gain building with time since unionization, which argues against the lag as the main explanation.

The institution panel therefore shows no pay premium at any tenure-line rank. Under parallel trends, the estimates rule out gains larger than about 1.2 log points for professors and assistant professors and any gain for associate professors; for non-tenure-track faculty they cannot bound gains below about four log points.

These bounds can be set against the closest estimates for faculty elsewhere. With the same estimator applied to Canadian universities, Baker et al. (2026) find that unionization raised institution-average faculty salaries by 2.1 percent in the first year after certification and by 4.7 percent by the sixth. Because event time here is measured in survey periods, the period after unionization ($e = 1$) spans one to four years after it and $e = 3$ spans five to nine, the closest counterparts to those horizons. The upper ends of the simultaneous 95 percent bands for overall salary are 0.45 log points at $e = 1$ and 1.9 at $e = 3$, and the bounds for each tenure-line rank also lie below the Canadian effects at both horizons. The comparison does not imply that U.S. faculty unions achieve less. The Canadian gains come almost entirely from certifications between 1970 and 1995; for those between 1996 and 2022, the period of the unionizations we study, Baker et al. (2026) find effects mostly near zero.

In both countries, recent faculty unionization has not raised average salaries. The headline Canadian estimates, 2.4 percent after one year and 6.1 percent after six, follow the same workers over time, an estimand closer to that of the SDR stayer analysis (Section 6.2.1) than to institution averages.

6.1.2 Employment

Unionization does not change the size of the faculty, but it reduces the number of assistant professors (Table 4). Total full-time instructional faculty falls by 3.2 log points (standard error 2.2), and the estimates for professors (-2.4), associate professors (-0.3), and non-tenure-track faculty ($+2.3$, standard error 7.8) are far from significant. The number of assistant professors falls by 13.0 log points (standard error 3.8), significant at the 1 percent level and the largest estimate in the institution panel.

The decline holds across specifications, sectors, and eras: -11.7 log points without matching weights, -17.6 with only later-unionizing controls, and -14.0 with matching on latest characteristics, all significant at the 5 percent level or better. It is similar at public (-12.6) and private not-for-profit (-13.6) institutions and negative in both eras (-21.1 for the 15 institutions unionized before 2004, -8.5 for the 88 unionized after). The within-institution Sun–Abraham estimate is about half as large, -6.8 log points with a standard error of 3.9.

The dynamics complicate the causal reading (Figure 3). After unionization, assistant-professor headcount falls steadily, from 3.8 log points below its pre-unionization level in the unionization period to 13.8 log points five periods later. Before unionization it rises: relative to the last pre-unionization period, it is 7.1 log points lower five periods before and 1.9 lower two periods before. No pre-unionization estimate is individually significant, but together they describe an expansion that then reverses. Total headcount follows the same, weaker shape, with pre-unionization estimates rising from -3.4 to zero and post-unionization estimates between -0.5 and -3.0 .

Whether the decline is an effect of unionization depends on what the expansion would

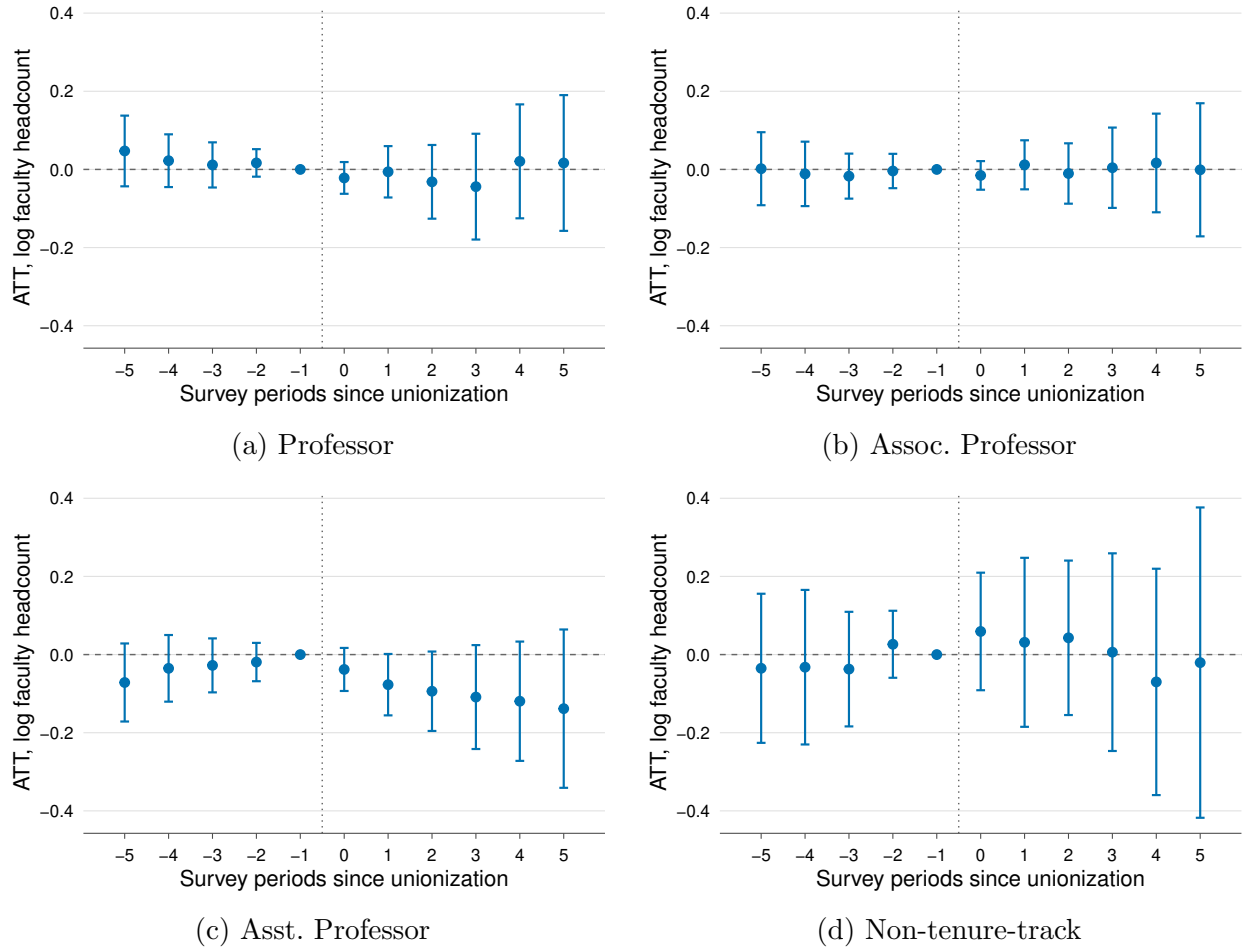


Figure 3: Log faculty headcount around unionization, by rank group, IPEDS

Note: Callaway–Sant’Anna event-study estimates of the effect of unionization on log full-time instructional faculty headcount within each rank group. Sample, controls, matching, timing, and confidence bands as in Figure 2. Panels share a vertical axis.

Table 4: Faculty headcount effects by rank group and specification, IPEDS

	Main	Unbalanced	Unmatched	Within-inst.	Later-treated
Overall	−0.032 (0.022)	−0.060** (0.026)	−0.027 (0.019)	−0.014 (0.020)	−0.119*** (0.036)
<i>N</i> treated inst.	151	173	152	173	152
Pre-unionization mean	5.264				
Professor	−0.024 (0.037)	−0.162*** (0.062)	−0.035 (0.033)	−0.030 (0.031)	−0.081 (0.051)
<i>N</i> treated inst.	106	155	107	155	107
Pre-unionization mean	4.306				
Assoc. Professor	−0.003 (0.040)	−0.066 (0.081)	0.001 (0.040)	0.035 (0.037)	−0.080 (0.052)
<i>N</i> treated inst.	105	150	106	150	106
Pre-unionization mean	4.309				
Asst. Professor	−0.130*** (0.038)	−0.155* (0.092)	−0.117*** (0.037)	−0.068* (0.039)	−0.176*** (0.040)
<i>N</i> treated inst.	103	151	104	151	104
Pre-unionization mean	4.312				
Non-tenure-track	0.023 (0.078)	0.019 (0.101)	0.122 (0.075)	0.066 (0.086)	0.051 (0.124)
<i>N</i> treated inst.	76	167	77	167	77
Pre-unionization mean	3.586				

Note: Callaway–Sant’Anna (2021) simple aggregate ATT. Treated group restricted to institutions unionizing (first faculty unit recognized) in 1993–2023; institutions unionized before 1993 are dropped entirely. Panel years 1984–2023. Rank structure mirrors the individual-level SDR analysis: Professor, Assoc. and Asst. Professor separate, Instructor and Lecturer pooled into non-tenure-track as combined salary outlay divided by combined headcount. Overall covers all full-time instructional faculty, including no academic rank. Years are binned onto the SDR survey waves (each year assigned to the first wave at or after it; 2-year bins before 1993), outcomes averaged within bin, and unionization assigned to the first wave at or after the unionization year. Unless noted: balanced panel (institutions observed in all 19 wave bins, separately for each outcome and rank group); controls are not-yet-treated institutions (never-treated plus those unionized after the period); controls exactly matched to the treated on sector $\times$ degree level $\times$ Carnegie class $\times$ faculty-size class, all measured before unionization, with the match re-run within each balanced sample. Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Outcome is log faculty headcount. Unbalanced: all institution-periods, matched on the full sample. Unmatched: balanced, no matching weights. Within-inst.: Sun–Abraham event study with institution and period fixed effects on the unbalanced matched panel, aggregated over post-unionization cells. Later-treated: balanced, never-treated institutions dropped, so controls are institutions unionized after the period (unweighted). Pre-unionization mean: average among treated institutions in the main sample in the last wave bin before unionization, the event-study base period.

otherwise have done. If it would have continued, the decline understates the effect. If it was transitory, part of the decline is its reversal. The data cannot distinguish the two. The later-treated column fits the same pattern: with only later-unionizing controls, headcount effects are more negative at every tenure-line rank and for total faculty (-11.9 log points, against -3.2), as expected if institutions expand their junior faculty in the periods before their own unionization, when they serve as controls for earlier cohorts.

The decline is what a model of employment adjustment predicts if unionization raises the cost of entry-level tenure-line positions (Section 3.2), but that cost is not a higher salary: average assistant-professor pay does not rise, and the gains of assistant professors who stay may reflect growth that predates unionization (Section 6.2.2). The candidates are the non-wage terms that collective agreements set, such as job-security provisions, workload limits, grievance procedures, and the expectation of terms not yet bargained (Section 2.3), none of which the data measure. Nor can the headcount data say whether the decline comes from fewer hires into the rank or faster promotion out of it: tenure attainment in the SDR does not change, but its standard error of 5.1 percentage points leaves room for either. There is no evidence of substitution toward non-tenure-track faculty, the second margin in Section 3.2, whose headcount estimate is positive but small relative to its standard error. With total employment flat and that an imprecise non-tenure track estimate, however, a modest shift away from the tenure track cannot be ruled out.

6.2 Effects on Workers

The institution panel shows what unionization does to average pay and employment; the SDR shows how the pay and careers of the same people evolve. It tests the predictions for which faculty gain (Section 3.3) and for exits and promotion (Section 3.4), and gives a second test of the pay-level prediction (Section 3.1). Wage and career-progression estimates use the stayer sample and retention estimates the transition sample (Section 4.2). All are Callaway–Sant’Anna estimates against not-yet-treated faculty, conditional on age and seniority, with

standard errors clustered by institution (Section 5.3).

Faculty who stay at unionizing institutions see faster salary growth after unionization, concentrated below the rank of full professor. A cumulative drift before unionization cannot be ruled out, however, so these gains cannot be attributed to unionization with confidence. The exception is non-tenure-track faculty, whose gain is larger and noisier but shows no such drift. Exits and movement along the tenure track do not change.

6.2.1 Salary trajectories of continuing faculty

Stayers' salaries rise 3.3 log points faster after unionization than those of not-yet-treated faculty (standard error 0.017), and 3.8 log points faster at research universities (standard error 0.015), both significant at the 5 percent level (Table 5). Each pre-unionization coefficient in Figure 4 is small and indistinguishable from zero, but together they do not rule out a gradual cumulative drift. The pooled estimate may therefore partly reflect faster salary growth already under way, and does not, on its own, identify an effect of unionization on pay.

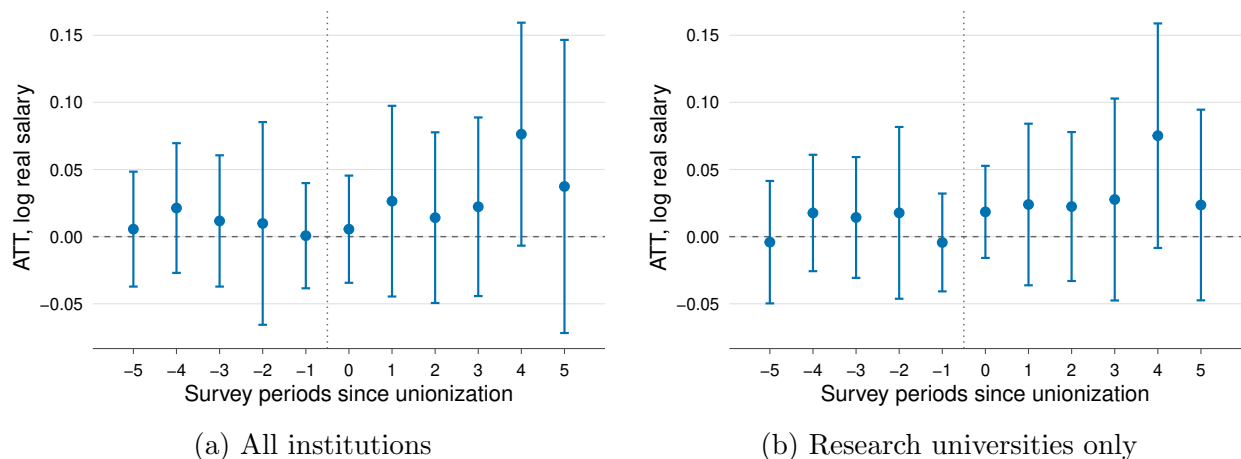


Figure 4: Log real salary around unionization, observed stayers: all institutions and research universities

Note: Callaway–Sant’Anna dynamic aggregation, stayer sample, not-yet-treated controls, conditional on age and seniority. Event time in SDR survey periods. Bars are 95 percent simultaneous confidence bands from a multiplier bootstrap clustered by institution.

Such a drift could come from changes in the sample’s composition: because the panel is

Table 5: Salary effects among observed stayers, SDR

	Log real salary
All observed stayers	0.033** (0.017)
Research universities only	0.038** (0.015)

Note: Callaway–Sant’Anna simple aggregate effect on log real salary. Stayer sample: individuals whose employing institution is the same in every wave in which it is reported. Not-yet-treated controls, conditional on age and seniority; standard errors clustered by institution.

unbalanced, each comparison contrasts the individuals observed in two periods rather than following the same people (Section 5.1), so a change in who is observed moves the estimates as a change in pay would. Split by institutional control, the pooled estimate loses precision: salaries of stayers rise 3.3 log points at public institutions and 1.9 at private ones, and neither estimate is significant on its own (Appendix Table A8 and Figure A5).

6.2.2 Salary growth across career stages

By baseline rank (Figure 5; middle column of Table 6), faculty who were assistant professors on the eve of unionization gain 11.0 log points (standard error 0.039), associate professors 9.9 (0.033), and full professors nothing (-0.4 , standard error 0.032); dropping the age and seniority covariates leaves the pattern intact. Full professors show no gain by current rank either (1.1 log points, standard error 0.027). For associate professors, the estimates two to four periods after unionization are individually significant, but as for the pooled sample, a cumulative drift cannot be ruled out for either gain.

The current-rank estimates place the associate-professor gain beyond that rank. Among faculty who are associate professors in a given period, the estimate is 3.4 log points (standard error 0.025) and not significant (first column of Table 6). Because the baseline-rank estimates follow individuals across promotions, the gain for faculty who were associate professors at unionization arises among those followed beyond the rank, where a promotion to full professor would appear. This is consistent with contracts that formalize promotion to full professor

(Section 2.3) or tie raises to rank (Wassell et al., 2015), and with faster promotion itself (Section 3.4). One qualification is that baseline rank is assigned at different career-stage dates: on the eve of unionization for the treated and at panel entry for controls (Section 5.3).

A gain concentrated below the top of the ladder is the pattern compression predicts (Section 3.3). Collective agreements typically replace individually negotiated raises with salary floors and across-the-board increases (Wassell et al., 2015), which matter least for senior faculty whose pay reflects accumulated merit increases and outside offers. The competing prediction, that a contract rewarding seniority raises pay most for long-serving faculty above the entry rank, finds no support at the top of the ladder, to the extent that rank tracks length of service: full professors gain nothing.

Table 6: Salary effects by rank definition, SDR

	Current rank	Baseline rank	
	+ controls	+ controls	No controls
Professor	0.011 (0.027)	−0.004 (0.032)	0.008 (0.024)
Assoc. Professor	0.034 (0.025)	0.099*** (0.033)	0.122*** (0.021)
Asst. Professor	0.073** (0.033)	0.110*** (0.039)	0.086** (0.043)
Non-tenure-track	0.194* (0.105)	0.234** (0.100)	0.271*** (0.104)
No rank system	−0.010 (0.037)	0.036 (0.040)	0.050 (0.036)

Note: Current rank is the rank in each period. Baseline rank is the rank at the last wave before unionization for faculty at unionized institutions and at the first wave observed for controls, so the two groups are classified at different career-stage dates. “+ controls” adds age and seniority. The difference between columns is not a decomposition into promotion and within-rank effects.

Non-tenure-track faculty show the largest gains in the SDR, and the only ones not preceded by a steady rise in pay. Faculty who were off the tenure track at unionization see their salaries rise by 23.4 log points (standard error 0.100), and faculty off the tenure track in a given period by 19.4 (0.105; Table 6). The estimate is imprecise: its 95 percent confidence interval runs from about 4 to 43 log points, and the pre-unionization estimates swing in both

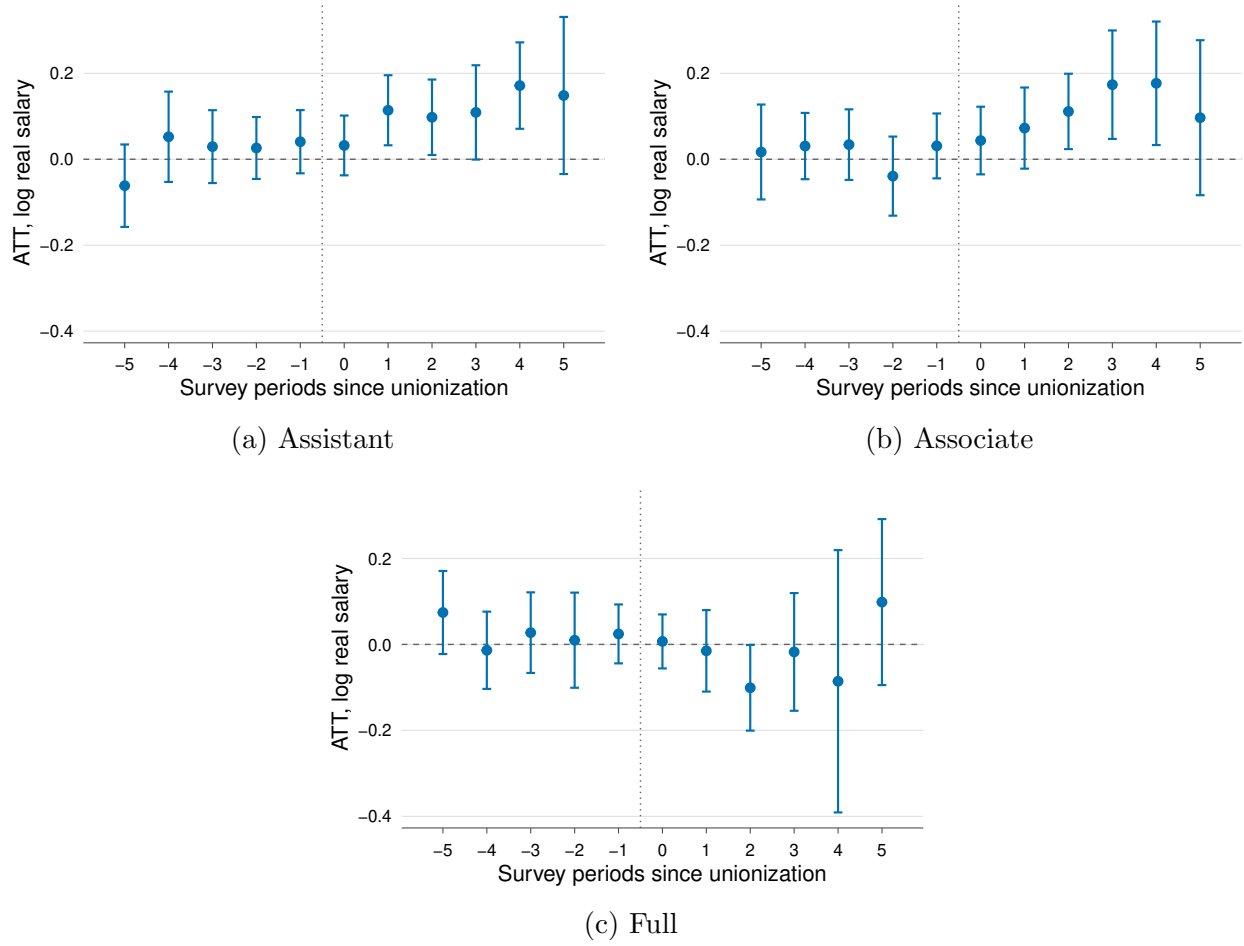


Figure 5: Log real salary around unionization, by baseline faculty rank

Note: Baseline-rank specification with age and seniority covariates (middle column of Table 6). Baseline rank is the rank at the last wave before unionization for faculty at unionized institutions and at the first wave observed for controls. Neither specification controls for current rank. Bands as in Figure 4. Panels share a vertical axis.

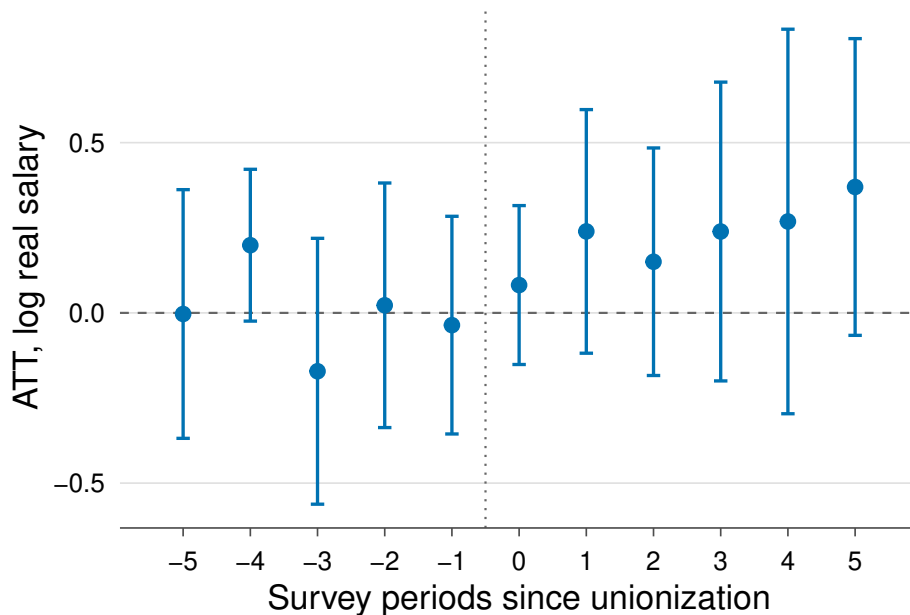


Figure 6: Log real salary around unionization, non-tenure-track faculty at baseline
Note: Same specification as Figure 5, drawn on its own vertical axis.

directions (Figure 6). The pattern is the one compression predicts, with the largest gains off the tenure track (Section 3.3).

There are two qualifications. The splits by institutional control do not sharpen the result: at public institutions the pre-unionization swings exceed the effect, and at private institutions a pre-unionization estimate is significant (Appendix Figure A6). And the SDR covers only non-tenure-track faculty who hold research doctorates, a narrow slice of the contingent workforce. We therefore read the estimate as the worker-level pay gain least exposed to pre-existing growth, not as a precise measure of its size.

6.2.3 Mobility and career progression

Unionization does not change the probability that faculty leave their institution. In the transition sample, which keeps movers and anchors treatment to the first institution at which each individual is observed, the probability of leaving within one wave rises by an insignificant 0.9 percentage points (standard error 1.5; Table 7). Leaving is defined broadly, as a move to another institution, an exit from academic employment, unemployment, or

leaving the labor force, so the null covers two opposing channels (Section 3.4): exits by faculty whose outside options improve relative to a compressed scale, and the reduction in quits that union voice can produce (Freeman, 1980a).

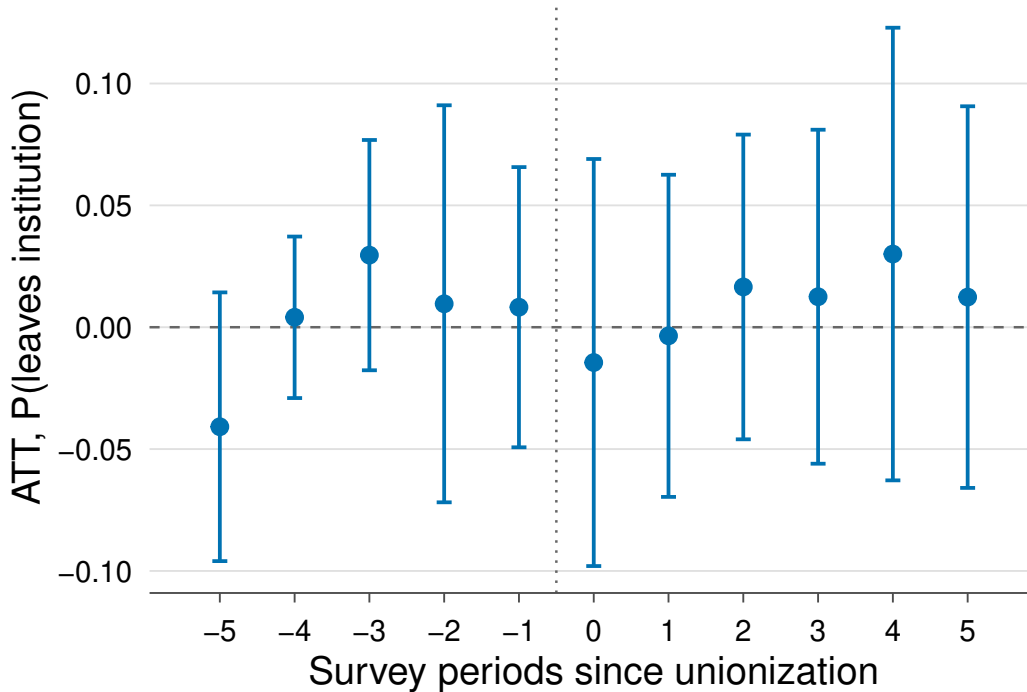


Figure 7: Probability of leaving within one wave around unionization, SDR

Note: Transition sample: movers kept, treatment anchored to the first institution at which each individual is observed. Leaving equals one for a move to another institution, an exit from academic employment, unemployment, or leaving the labor force by the next wave; individuals not observed at the next wave are excluded. Bands as in Figure 4.

The splits by current rank do not overturn this. Full professors, among whom the sorting channel predicts any increase in exits to concentrate (Section 3.4), are 3.1 percentage points more likely to leave, but the estimate is significant only at the 10 percent level and is one of several splits, so we do not read it as evidence that unionization drives out senior faculty. The estimates for assistant professors and faculty without a rank system are small and insignificant. The associate-professor split has a significant pre-unionization coefficient, and the non-tenure-track estimate of 7.8 percentage points is smaller than the largest pre-unionization swing in its event study, so neither can be interpreted. Neither the public nor the private estimate is significant (Appendix Table A8 and Figure A7).

Table 7: Leaving and career-progression effects, SDR

	ATT
<i>Panel A. Leaving within one wave</i>	
Leaves, all	0.009 (0.015)
Professor	0.031* (0.018)
Assoc. Professor	0.007 (0.036)
Asst. Professor	−0.014 (0.031)
Non-tenure-track	0.078** (0.039)
No rank system	0.018 (0.038)
<i>Panel B. Career progression</i>	
Attains tenure (TT at origin)	−0.006 (0.051)
Converts to tenure track (NTT at origin)	0.031 (0.023)

Note: Panel A: probability of leaving the first-observed institution by the next wave, transition sample; rank splits are by current rank. The associate-professor split has a statistically significant pre-unionization coefficient, and the non-tenure-track split's largest pre-unionization swing exceeds its estimate. Panel B: tenure attainment by the next wave among tenure-track faculty, and movement onto the tenure track among faculty off it, stayer sample. Public and private estimates are in Appendix Table A8.

The stable exit rate limits selection in the stayer salary estimates, whose sample would be selected on a post-treatment outcome if unionization changed who stays (Section 5.4). A wholesale change in who remains is unlikely, but a change in *which* faculty exit is not ruled out; the direct test is the leaving effect estimated separately by pre-unionization salary.

Contracts codify promotion and tenure procedures (Section 3.4), but unionization leaves movement along the tenure track unchanged. The probability that a tenure-track faculty member attains tenure by the next wave has a point estimate of -0.6 percentage points, with a standard error of 5.1 that rules out neither large positive nor large negative effects; among faculty who begin off the tenure track, the probability of moving onto it rises by an insignificant 3.1 percentage points (standard error 2.3; Panel B of Table 7).

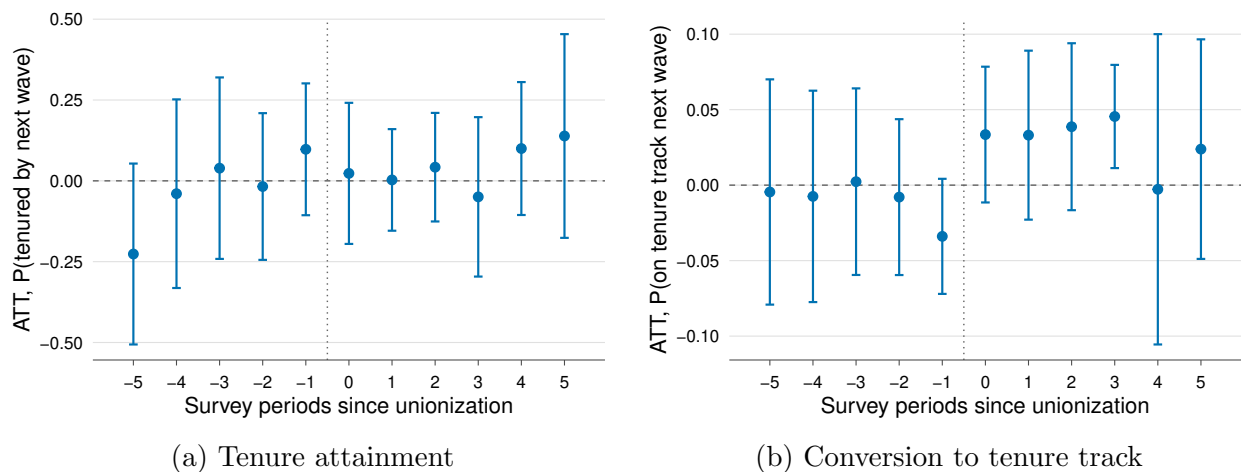


Figure 8: Career progression around unionization, SDR

Note: Stayer sample. Tenure attainment: among faculty on the tenure track, tenured by the next wave. Conversion: among faculty in non-tenure-track positions or at institutions without a tenure system, on the tenure track or tenured by the next wave. Bands as in Figure 4.

6.3 Comparing Institution- and Worker-Level Estimates

Table 8 sets the institution-level salary estimates beside the SDR estimates for faculty who stay at their institution, both by current rank. The two measure different objects, and need not agree if unionization changes who holds each rank (Section 3.5). The SDR estimates are positive at every academic rank, 3.3 log points overall and 7.3 for assistant professors,

while the institution estimates lie between -1.7 and -0.5 log points; at face value, the SDR assistant-professor estimate lies well outside the institution panel’s confidence interval. The face-value comparison overstates the disagreement. The institution estimates come with flat pre-unionization paths (Section 6.1.1), while for the SDR a cumulative drift before unionization cannot be ruled out (Section 6.2.1), so part of the SDR gains may reflect salary growth already under way at unionizing institutions.

Table 8: Salary effects in the two tiers, by rank group

	Institution average (IPEDS)	Observed stayers (SDR)
Overall	-0.015^* (0.009)	0.033^{**} (0.017)
Professor	-0.007 (0.010)	0.011 (0.027)
Assoc. Professor	-0.017^{**} (0.008)	0.034 (0.025)
Asst. Professor	-0.005 (0.009)	0.073^{**} (0.033)
Non-tenure-track	-0.005 (0.023)	0.194^* (0.105)
No rank	—	-0.010 (0.037)

Note: IPEDS: main specification of Table 3; the outcome is the log of salary outlay divided by headcount, over all full-time instructional faculty holding the rank in each period, with institutions weighted equally. SDR: current-rank stayer specification (first column of Table 6); the same individuals over time, conditional on remaining at their institution, research-doctorate holders only, with individuals weighted equally. The IPEDS no-academic-rank group is not estimated separately. The two columns differ in estimand, population, and weighting.

Three further differences between the tiers can produce faster stayer growth alongside flat institution averages. The SDR compares whoever is observed in each period (Section 6.2.1), while the institution panel follows the same institutions throughout. The SDR covers holders of research doctorates, concentrated at research universities, while the institution panel covers all full-time instructional faculty at institutions observed in every period since 1984, most of them long-established four-year institutions.

The associate-professor result is consistent with the institution panel rather than in ten-

sion with it. That gain arises beyond the associate rank (Section 6.2.2), and movement to a higher rank raises individuals' pay without raising the average pay of any rank, which is what the institution panel measures (Section 3.5).

The non-tenure-track estimates differ most. The average salary of non-tenure-track faculty in the institution panel does not change (-0.5 log points, standard error 2.3), while non-tenure-track faculty who stay in the SDR gain 19.4 log points (standard error 10.5), and the two confidence intervals overlap only between roughly zero and four log points. The populations also differ most here. The institution group pools all full-time instructors and lecturers, whereas the SDR group consists of non-tenure-track faculty with research doctorates who remain at their institution. A large gain for that group is compatible with a flat average only if it makes up a small share of the institution group, or if its gain is offset by changes in who else holds the positions.

The tiers therefore agree that unionization does not raise average pay at any tenure-line rank, and the SDR's within-rank gains for continuing faculty are not clearly attributable to it, with the possible exception of non-tenure-track faculty. Where they see movement, it is at margins only one tier observes: fewer assistant professors in the institution panel, and gains beyond the associate rank in the SDR.

7 Discussion

This paper asks what unions do for professional workers, using faculty unionization at U.S. colleges and universities between 1993 and 2023. Unionization does not raise what institutions pay their tenure-line faculty, but it reduces the number of assistant professors, after an expansion in the years before unionization. Faculty who stay at unionizing institutions see faster salary growth, concentrated below full professor and, for associate professors, among those who move beyond that rank, although a drift that predates unionization cannot be ruled out. Exits do not change.

The union wage premium is large in the settings where it is usually measured: for 1994–2018, the CPS union wage gap averages 0.16 log points for all wage and salary workers and 0.10 in the public sector (Blanchflower and Bryson, 2004; Hirsch, 2004; Kulkarni and Hirsch, 2021). After faculty unionize, average pay within tenure-line ranks does not rise, and the bound on any gain is tight under parallel trends. Where pay is set individually and anchored to outside offers (Ransom, 1993; Goolsbee and Syverson, 2023), bargaining acts on the terms and number of positions more than on the pay attached to them. For institutions and policymakers weighing faculty bargaining, the margin to watch is staffing rather than salaries.

Professional workers now organizing share the features that distinguish faculty: individualized pay, productivity-based advancement, and outside options in a broader market (Mirabello et al., 2024; Rooke-Ley et al., 2025; Cohen and de Peuter, 2020). The gains their unions deliver may therefore come less through pay levels than through careers and employment. The extrapolation has limits. Tenure already provides much of the job security that unions elsewhere negotiate for (Section 2.3), and faculty bargain under state public-sector law or, at private institutions, a federal law that excludes faculty with managerial authority (Section 2.2), so professionals organizing under other rules may meet different employer responses.

The results also carry a methodological lesson: in unbalanced panels, staggered difference-in-differences estimates that compare whoever is observed in each period can show effects that following the same units does not. In the institution panel, salary declines estimated on the unbalanced panel are two to five times those on the balanced panel at every tenure-line rank (Section 6.1.1).

Three caveats qualify these conclusions. Unionization is not randomly timed, and assistant-professor employment rises before unionization, so the employment estimate rests on an assumption about how that expansion would have continued (Section 5.4). The SDR stayer salary estimates may combine an effect of unionization with pre-existing growth and with

changes in who is observed. And the institution panel covers long-established institutions that report in every period, so its estimates may not extend to newer ones. Following the same individuals in the SDR would separate pay growth from changes in who is observed, and non-wage outcomes, such as benefits, workload, and part-time staffing, are the natural place to look for what faculty bargaining delivers.

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Appendix

A Institutional background

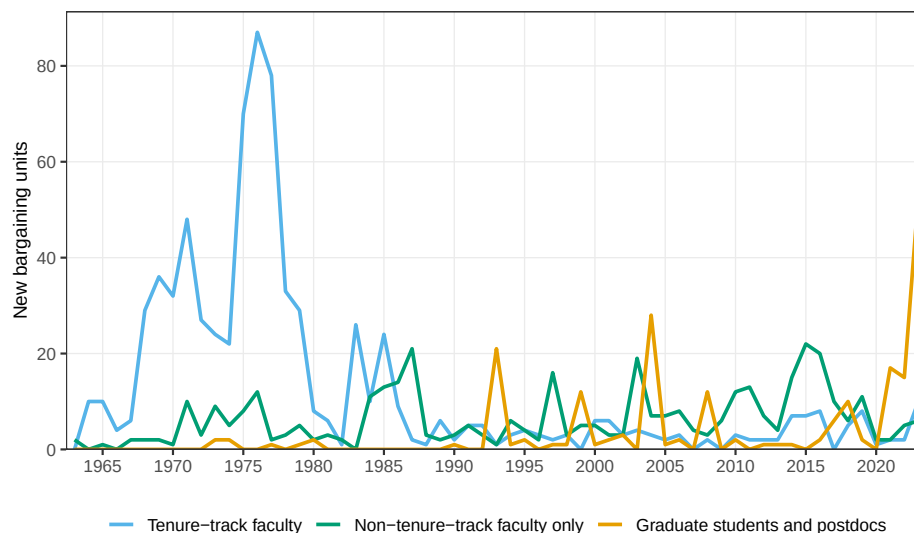


Figure A1: New bargaining units by unit type, 1963–2023

Note: Bargaining units in the 2024 Directory by year of recognition ([Herbert et al., 2024](#)). Tenure-track faculty: units covering tenured or tenure-track faculty, alone or with non-tenure-track colleagues. Non-tenure-track faculty only: units covering non-tenure-track faculty and no tenure-track faculty. Graduate students and postdocs: units covering graduate or undergraduate student employees or postdoctoral scholars and no faculty. Units of other employees, such as administrators, are omitted. Units still active in 2024 only.

Table A1: Bargaining units recognized 1993–2023, by type and institutional control

Unit type	Public	Private	Total
Faculty NTT only	145	92	237
Faculty TTT only	33	1	34
Faculty TTT+NTT	56	14	70
Grad/PDS/USE	141	56	197
Other	35	0	35
Total	410	163	573

Table A2: Institutions by portfolio of bargaining units recognized 1993–2023

Institution unit portfolio	N institutions
NTT only	154
Grad only	108
TTT + NTT	66
NTT + Grad	34
TTT only	17
Other only	14
TTT + NTT + Grad	13
TTT + Grad	1
Total	407

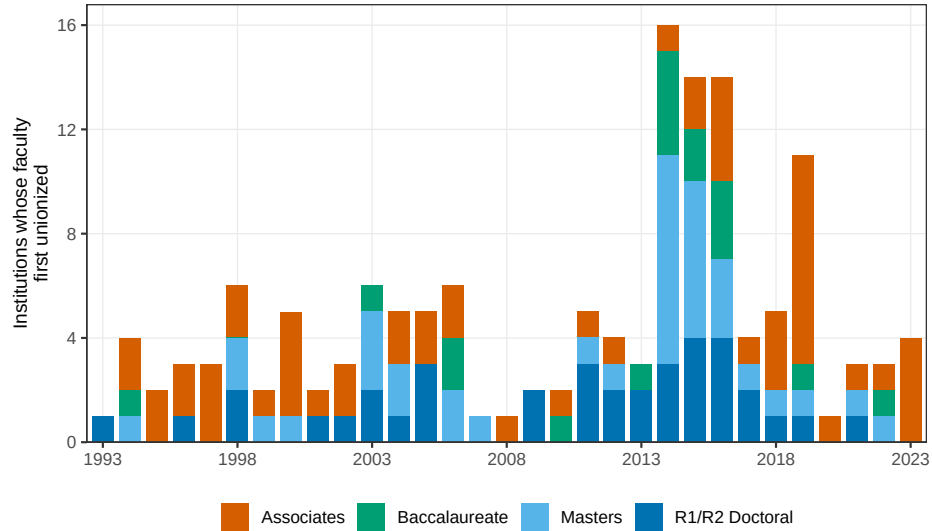


Figure A2: Institutions whose faculty first unionized, by Carnegie class, 1993–2023

Note: Same institutions and years as Figure 1, stacked by Carnegie classification (latest non-missing IPEDS HD record). Institutions in other or missing Carnegie classes are omitted.

B SDR sample and survey design

Variables. The sample includes respondents employed at a two- or four-year college or university, a medical school, a university-affiliated research institute, or another educational institution other than an elementary or secondary school. Salary is annual salary on the principal job, deflated to 2019 dollars. We set it to missing where the survey codes a skip or nonresponse, where it is reported as zero with no hours reported, where it is below \$500, and where it is below \$10,000 for respondents reporting 35 or more hours per week. Faculty rank takes five values: professor, associate professor, assistant professor, a non-tenure-track rank, or no rank system at the institution. The “no rank system” category includes doctorate holders in non-faculty posts, such as research staff. Tenure status is tenured, on the tenure track, not on the tenure track, or no tenure system. Seniority is the number of years since the respondent began working for the current employer.

Survey design. One feature of the survey design changes the sample without reflecting individual behavior: in 2015 the SDR drew a fresh sample and expanded its coverage ([National Center for Science and Engineering Statistics, 2025](#)). We do not count the change as an exit. The estimates are unweighted.

Samples. In the transition sample, each person-wave is classified by the individual’s status at the next survey wave: at the same institution, at a different institution, employed outside academia, unemployed, or out of the labor force. Person-waves whose next wave is not observed are dropped. Tables [A3](#) and [A4](#) split both samples into three treatment groups: individuals at institutions unionized in or before 1993 (always treated), during the sample window (treated in-window), and never (not treated).

Table A3: Summary statistics, SDR wage (stayers) panel

	Always treated	Not treated	Treated in-window
Person-waves	32,842	97,602	36,048
Persons	11,299	36,851	13,698
Log real salary	11.353	11.316	11.422
Age	48.885	48.464	46.878
Seniority (years with employer)	11.795	11.407	10.628
Share federally supported	0.378	0.370	0.529
Share U.S. citizen	0.883	0.878	0.870
<i>Faculty rank</i>			
Professor	11,748	32,468	9,998
Associate Professor	6,677	23,102	6,477
Assistant Professor	5,807	20,016	6,680
Non-tenure-track	2,401	5,218	2,382
No rank system	6,139	16,669	10,426
<i>Baseline faculty rank</i>			
Professor	8,774	23,833	10,265

	Always treated	Not treated	Treated in-window
Associate Professor	5,844	20,111	6,081
Assistant Professor	9,001	29,850	6,638
Non-tenure-track	2,585	5,619	2,503
No rank system	6,532	18,044	10,473
<i>Race</i>			
Asian	5,147	13,016	5,563
Black	1,866	6,260	1,812
Hispanic	2,672	7,032	2,304
White	22,423	69,265	25,551
Other	734	2,029	818
<i>Marital status</i>			
Formerly partnered	2,878	8,092	2,701
Never married	3,918	10,810	4,661
Partnered	26,046	78,700	28,686
<i>Institutional control</i>			
Missing	36	6,238	7
Private	2,020	29,390	21,614
Public	30,783	61,558	14,424
<i>Employer type</i>			
Carnegie missing	165	6,518	108
Liberal arts college	469	11,232	1,318
Master's university	9,427	19,472	1,769
Medical school	588	5,601	896
Research university	19,561	50,037	31,335
Specialized institution	149	1,316	48
Two-year college	2,480	3,010	571
<i>Log real salary by rank</i>			
Professor	11.658	11.599	11.826
Associate Professor	11.413	11.350	11.540
Assistant Professor	11.264	11.212	11.404
Non-tenure-track	10.703	10.708	10.982
No rank system	11.053	11.036	11.075
<i>Seniority by rank</i>			
Professor	19.479	19.278	20.642
Associate Professor	12.071	11.913	12.253
Assistant Professor	4.191	4.300	4.506
Non-tenure-track	7.795	6.651	6.611
No rank system	5.791	5.577	5.050

Note: Wage/stayers panel — people whose employing institution is constant across their whole observed panel. Counts are person-waves unless labelled otherwise. Continuous rows are means.

Table A4: Summary statistics, SDR transition panel

	Always treated	Not treated	Treated in-window
Person-waves	41,518	153,357	45,768
Persons	14,610	52,823	17,298

	Always treated	Not treated	Treated in-window
Log real salary	11.354	11.282	11.427
Age	48.232	48.057	46.294
Seniority (years with employer)	10.442	10.324	9.489
Share federally supported	0.393	0.348	0.541
Share U.S. citizen	0.891	0.801	0.879
<i>Transition state</i>			
Not in labor force	1,364	4,634	1,174
Unemployed	247	897	265
Employed outside academia	1,701	7,839	2,974
Moved to another institution	3,841	18,969	4,894
Stayed	29,303	85,885	30,759

Note: Transition panel — includes movers, with treatment anchored to the first-observed institution. Counts are person-waves unless labelled otherwise. Continuous rows are means.

C Descriptive salary and mobility patterns, SDR

Figure A3 plots mean log real salary by wave, treatment group, and institutional control among all SDR respondents employed in higher education, movers included. At public institutions, the three groups track one another closely throughout the period. At private institutions, faculty at institutions unionized during the window earn more than faculty at never-unionized institutions in nearly every wave, and the gap widens over the period. These are calendar-time means that pool observations from before and after unionization, and the never-unionized group is not the comparison group used in estimation, so the figure describes differences in levels rather than effects of unionization. Section 5.4 discusses why level differences of this kind do not threaten the design.

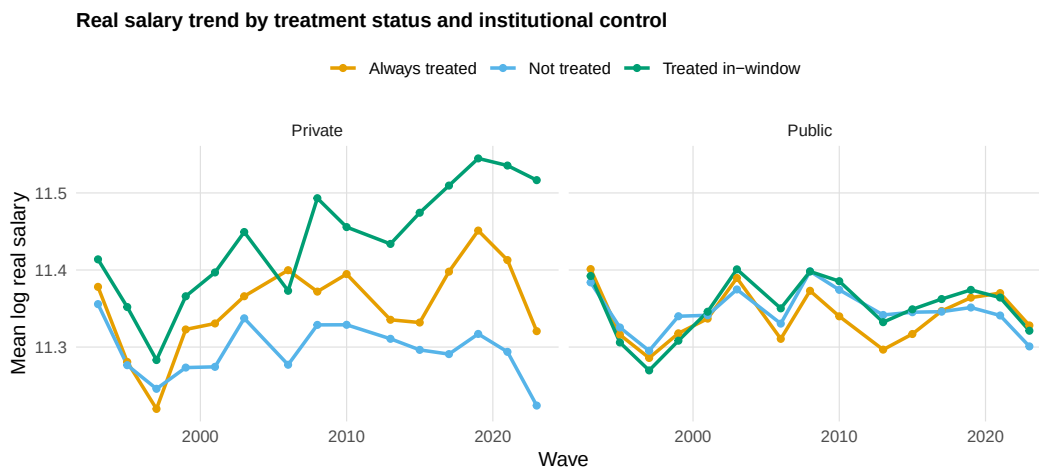


Figure A3: Mean log real salary by treatment group and institutional control, SDR

Note: Mean log real salary (2019 dollars) by survey wave, over all person-waves of SDR respondents employed in higher education, movers included. Treatment group and institutional control are those of the employing institution in each wave: unionized in or before 1993 (always treated), during the sample window (treated in-window), or never (not treated). Treatment groups date unionization by the institution's first bargaining unit of any kind, including units of graduate students, postdoctoral scholars, and other non-faculty employees. These are calendar-time means that pool observations from before and after unionization; the not-treated group is not the not-yet-treated comparison group used in estimation.

D Institution-level robustness

Table A5: Institution sample: treated and matched never-treated institutions

	Institutions	Unweighted share	Weighted share
<i>Treated (unionized 1993–2023)</i>			
Private NFP	68	0.456	0.456
Public	81	0.544	0.544
<i>Never-treated controls</i>			
Private NFP	759	0.517	0.456
Public	709	0.483	0.544

Note: Main sample for overall salary: balanced panel, exactly matched on sector, degree level, Carnegie class and faculty-size class measured before unionization. Exact matching with the ATT estimand reweights controls to the treated covariate distribution, so the weighted control share equals the treated share by construction — that is the quantity the CS estimator uses.

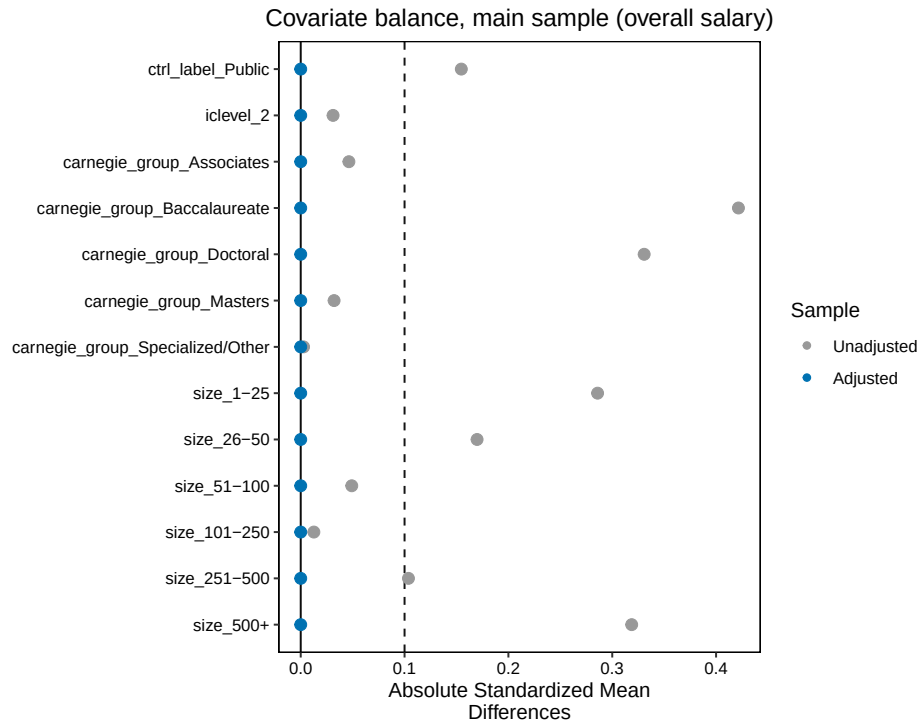


Figure A4: Covariate balance, matched post-1993 sample

Table A6: Institution-average salary effects by institutional control

	Public	Private NFP
Overall	−0.005 (0.013)	−0.016 (0.013)
<i>N</i> treated inst.	81	68
Professor	0.000 (0.015)	−0.007 (0.012)
<i>N</i> treated inst.	43	58
Assoc. Professor	−0.001 (0.012)	−0.023** (0.011)
<i>N</i> treated inst.	43	59
Asst. Professor	0.014 (0.010)	−0.016 (0.014)
<i>N</i> treated inst.	45	57
Non-tenure-track	0.002 (0.019)	−0.009 (0.056)
<i>N</i> treated inst.	36	23

Note: Callaway–Sant’Anna (2021) simple aggregate ATT. Treated group restricted to institutions unionizing (first faculty unit recognized) in 1993–2023; institutions unionized before 1993 are dropped entirely. Panel years 1984–2023. Rank structure mirrors the individual-level SDR analysis: Professor, Assoc. and Asst. Professor separate, Instructor and Lecturer pooled into non-tenure-track as combined salary outlay divided by combined headcount. Overall covers all full-time instructional faculty, including no academic rank. Years are binned onto the SDR survey waves (each year assigned to the first wave at or after it; 2-year bins before 1993), outcomes averaged within bin, and unionization assigned to the first wave at or after the unionization year. Unless noted: balanced panel (institutions observed in all 19 wave bins, separately for each outcome and rank group); controls are not-yet-treated institutions (never-treated plus those unionized after the period); controls exactly matched to the treated on sector $\times$ degree level $\times$ Carnegie class $\times$ faculty-size class, all measured before unionization, with the match re-run within each balanced sample. Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Main sample split by institutional *sector* (first observed, so before unionization for the treated).

Table A7: Institution-level effects by unionization era

	All cohorts	Unionized 1993–2003	Unionized 2004+
Salary, Overall	−0.015*	−0.011	−0.007
	(0.009)	(0.018)	(0.009)
<i>N</i> treated inst.	149	33	116
Salary, Professor	−0.007	0.018	−0.010
	(0.010)	(0.025)	(0.010)
<i>N</i> treated inst.	101	16	85
Salary, Assoc. Professor	−0.017**	−0.011	−0.011
	(0.008)	(0.018)	(0.009)
<i>N</i> treated inst.	102	16	86
Salary, Asst. Professor	−0.005	−0.004	−0.000
	(0.009)	(0.022)	(0.009)
<i>N</i> treated inst.	102	15	87
Salary, Non-tenure-track	−0.005	−0.057	0.025
	(0.023)	(0.046)	(0.024)
<i>N</i> treated inst.	59	10	49
Count, Overall	−0.032	−0.022	−0.030*
	(0.022)	(0.047)	(0.017)
<i>N</i> treated inst.	151	33	118
Count, Professor	−0.024	−0.045	−0.003
	(0.037)	(0.069)	(0.040)
<i>N</i> treated inst.	106	16	90
Count, Assoc. Professor	−0.003	−0.004	0.005
	(0.040)	(0.135)	(0.029)
<i>N</i> treated inst.	105	16	89
Count, Asst. Professor	−0.130***	−0.211**	−0.085**
	(0.038)	(0.087)	(0.040)
<i>N</i> treated inst.	103	15	88
Count, Non-tenure-track	0.023	0.025	0.027
	(0.078)	(0.178)	(0.075)
<i>N</i> treated inst.	76	13	63

Note: Callaway–Sant’Anna (2021) simple aggregate ATT. Treated group restricted to institutions unionizing (first faculty unit recognized) in 1993–2023; institutions unionized before 1993 are dropped entirely. Panel years 1984–2023. Rank structure mirrors the individual-level SDR analysis: Professor, Assoc. and Asst. Professor separate, Instructor and Lecturer pooled into non-tenure-track as combined salary outlay divided by combined headcount. Overall covers all full-time instructional faculty, including no academic rank. Years are binned onto the SDR survey waves (each year assigned to the first wave at or after it; 2-year bins before 1993), outcomes averaged within bin, and unionization assigned to the first wave at or after the unionization year. Unless noted: balanced panel (institutions observed in all 19 wave bins, separately for each outcome and rank group); controls are not-yet-treated institutions (never-treated plus those unionized after the period); controls exactly matched to the treated on sector $\times$ degree level $\times$ Carnegie class $\times$ faculty-size class, all measured before unionization, with the match re-run within each balanced sample. Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Era columns keep treated institutions unionized in that era only, with never-treated and not-yet-treated controls, re-matched.

E Individual-level robustness

Table A8: Salary and leaving effects by institutional control, SDR

	Log real salary (stayers)	Leaves within one wave
Public	0.033 (0.025)	0.019 (0.022)
Private	0.019 (0.022)	−0.015 (0.020)

Note: Callaway–Sant’Anna simple aggregate effects, not-yet-treated controls, conditional on age and seniority; standard errors clustered by institution. Log real salary: stayer sample (Table 5), split by the institutional control reported in each wave, so waves in which control is not reported fall in neither column. Leaving: transition sample (Figure 7), split by the control of the first institution at which each individual is observed.

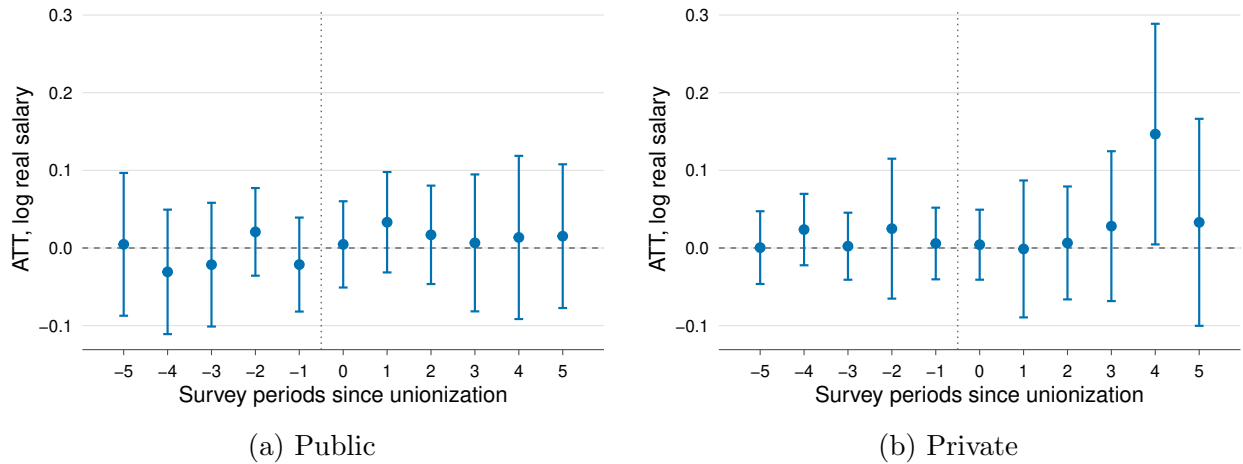
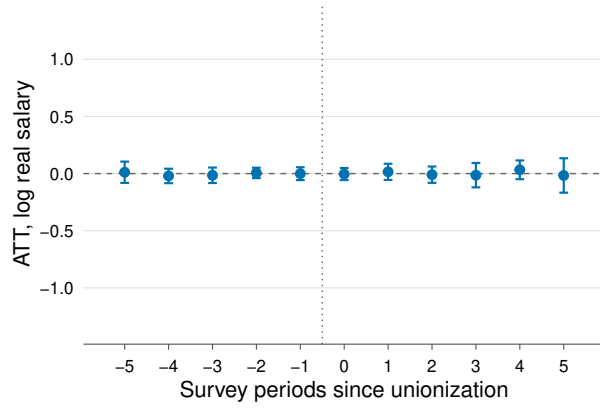
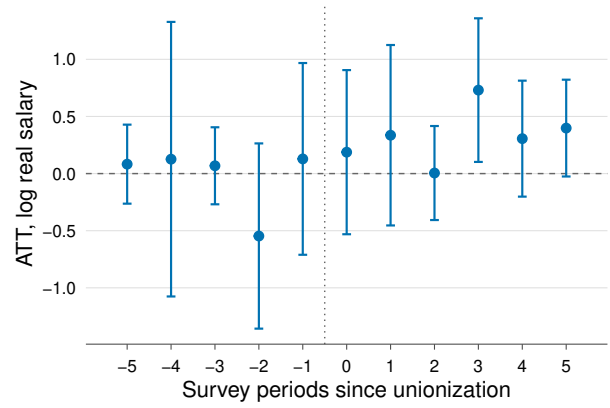


Figure A5: Log real salary around unionization, observed stayers, by institutional control

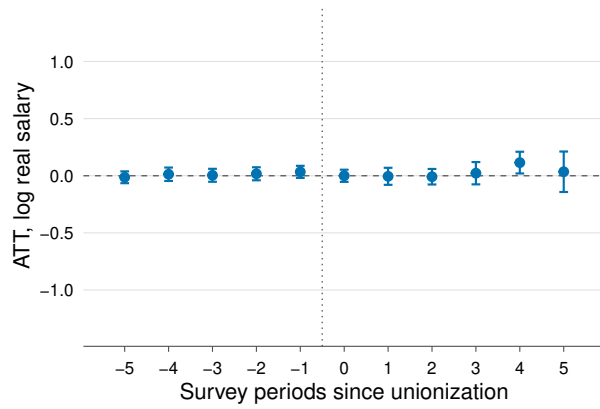
Note: Callaway–Sant’Anna dynamic aggregation, stayer sample split by the institutional control reported in each wave, as in Table A8. Not-yet-treated controls, conditional on age and seniority. Event time in SDR survey periods. Bands as in Figure 4. Panels share a vertical axis.



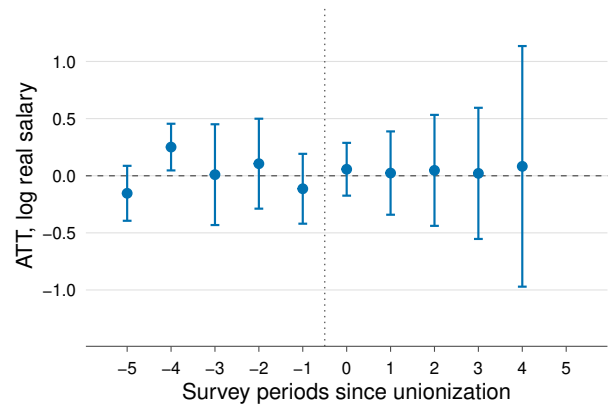
(a) Public, tenure line



(b) Public, NTT

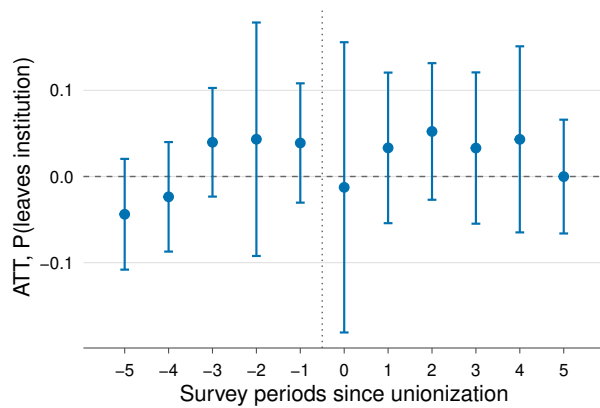


(c) Private, tenure line

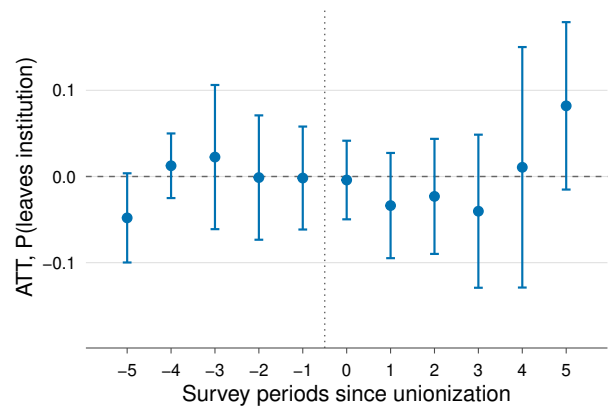


(d) Private, NTT

Figure A6: Log real salary around unionization, institutional control $\times$ tenure line



(a) Public



(b) Private

Figure A7: Probability of leaving within one wave, by institutional control