Faculty Unions, Faculty Salaries, and Tuition

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Abstract

Faculty unionization has expanded substantially in U.S. higher education over the past two decades. Using IPEDS data from 2004 to 2024 and a staggered difference-in-differences design, we estimate the effect of first collective bargaining agreements on faculty salaries, the composition of the instructional workforce, tuition prices, and institutional spending. We find unionization increases wages for the unionized group, and institutions adjust for these expenses by substituting labor demand towards lower-paid positions. We find no detectable change in overall spending or net tuition.

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1 Introduction

The average real salary for full-time instructional faculty in the United States has been essentially constant since 1970.[1] At the same time, measures of research and instructional productivity have increased (Hather et al., 2010, Miles et al., 2018), as well as measures of institutional financial health (Lucianelli and Citro, 2017), while some evidence suggests that shared governance has become less prominent in the U.S. academic system (Bowen and Tobin, 2015).

Amidst this backdrop, faculty unionization efforts have gained momentum over the past two decades. According to Herbert et al. (2024), union representation of faculty increased roughly 10% between 2012 and 2024. Much of this growth occurred through the formation of new collective bargaining units. A natural question emerges: to what extent does the formation of a faculty union and accompanying collective bargaining processes affect the compensation of faculty and the overall financial position of the university? Furthermore, to the extent that universities behave like other firms and set prices as a function of marginal costs, it is reasonable to believe that increases in faculty salaries may be passed-through to students in the form of higher tuition.

We investigate these questions using a staggered difference-in-differences approach (Callaway and Sant'Anna, 2021) and data obtained from the Integrated Post-secondary Education Data System (IPEDS) for the period 2004–2024, which we combine with data on faculty unions (Herbert et al., 2024). We find that unionization raises salaries for the unionized group: approximately 5 percent for tenured / tenure-track faculty across the professor, associate, and assistant ranks, and 7–8 percent for non-tenure-track instructors and lecturers. We find no evidence of salary spillover across bargaining groups: at institutions where only non-tenure-track faculty unionize, tenure-track salaries do not move.

The pricing response differs sharply between public and private non-profit institutions. Privates raise published sticker tuition by roughly 4 percent following unionization, but their net price — the average price students actually pay after grant and scholarship aid

[1] According to the Digest of Education Statistics maintained by the National Center for Education Statistics, the average salary of full-time instructional faculty was $95,539 during the 1970–1971 academic year and $97,716 during the 2022–2023 academic year.

— is essentially unchanged, suggesting that any nominal pass-through is offset by increased institutional aid. Public institutions do not raise sticker tuition and may experience a slight decline in net price.

Instead, institutions appear to absorb the salary increase primarily by reshaping the composition of their full-time instructional workforce. Following a first collective bargaining agreement, the share of full-time instructional staff in tenured and tenure-track ranks falls by 3–4 percentage points relative to controls, while the share in instructor and lecturer ranks rises by 5–7 percentage points. The substitution holds under both varieties of unionization — when tenure-track faculty unionize, and when only non-tenure-track faculty do — suggesting that the composition response is the primary margin on which institutions adjust to the labor-cost shock.

At research-intensive institutions, the composition response has measurable consequences for academic output. Restricting attention to institutions classified as R1 or R2 under the Carnegie Basic Classification, we estimate that a first collective bargaining agreement covering tenured and tenure-track faculty is followed by a 22 percent decline in research doctorates awarded and a 15 percent decline in research expenditures per tenured / tenure-track faculty member. The labor-input margin responsible is concentrated at the assistant-professor rank: assistant-professor headcount falls by 18 percent, associate-professor headcount falls by 12 percent, and full-professor headcount is essentially unchanged. The tenured and tenure-track pool ages in place under unionization, in a pattern consistent with hiring frozen at the entry rank rather than retirement induced at the senior end. Only ten R1 / R2 institutions are cleanly treated under this specification, so these estimates are imprecise — but they survive leave-one-out, dropping the dominant Florida-system institutions, and replication by an augmented synthetic control estimator.

Our work contributes to a long literature on the effects of unionization on workers and the firms that employ them. The general private-sector union wage premium has been documented across multiple data sources and identification strategies (Lewis, 1986, Card, 1996, DiNardo and Lee, 2004, Hirsch, 2004, Sojourner et al., 2015). A smaller higher-education-specific literature (Barbezat, 1989, Ashraf, 1997, Hedrick et al., 2011) has produced consistently smaller and often statistically null estimates of the faculty union wage premium

— substantially below the 10–15 percent typical of private-sector estimates. Within this literature, the closest work to our own is Baker et al. (2026), who use data from the Canadian higher education system to test for changes in the wage distribution among unionized faculty; they find that unionization leads to compression of the wage distribution. We contribute to this literature by analyzing the U.S. higher education system with administrative data and studying a number of potential impacts of unionization on higher education beyond faculty salaries alone.

Our work also contributes to a literature on the changing composition of the academic labor force — specifically, the rising share of non-tenure-track and contingent faculty over the past three decades. The descriptive trend is comprehensively documented by Schuster and Finkelstein (2006) and updated annually by the AAUP's *Annual Report on the Economic Status of the Profession*. The economic consequences of the contingent-labor shift have been examined primarily through its effect on student outcomes: Ehrenberg and Zhang (2005), Bettinger and Long (2010), and Figlio et al. (2015) report a mix of effects on retention, graduation, and instructional quality, with adjunct and non-tenure-track exposure sometimes positive and sometimes negative depending on the institutional context and the academic field. We contribute to this literature by documenting a channel through which institutions substitute away from tenure-related faculty towards more contingent classifications.

We proceed in Section 2 with a brief overview of unionization in higher education. We describe our data and our empirical strategy in Section 3 and discuss our results in Section 4. We conclude in Section 5.

2 Background

Faculty unionization in U.S. higher education is a substantial but legally fragmented practice. Roughly one in four full-time instructional faculty in the country is covered by a collective bargaining agreement (Herbert et al., 2024), and bargaining units exist at about 700 distinct institutions in our analysis sample. Coverage is concentrated at public colleges and universities — private non-profit faculty bargaining is comparatively rare for reasons we discuss below. Higher-ed faculty unions differ in important ways from K–12 teacher bargain-

ing and from private-sector bargaining more generally, both because much of its activity is governed by state public-employee law and because the unionized workforce itself may span tenure-track faculty, non-tenure-track instructors, adjuncts, librarians, and graduate student employees (not to mention non-academic staff and administrators) — groups whose interests and bargaining power often diverge.

The legal framework that produces this fragmentation has two key features. First, in *NLRB v. Yeshiva University* (1980) the Supreme Court held that tenured and tenure-track faculty at private universities exercise sufficient "managerial" authority — through participation in shared governance, curriculum decisions, and personnel committees — to fall outside the NLRA's protection of employees' right to organize.[2] The practical consequence is that the principal organizing channel for tenured and tenure-track faculty at private institutions is closed, and private-university bargaining units in our data are overwhelmingly NTT-only. Second, public institutions are governed by state public-employee labor law, which varies dramatically: some states (California, New York, New Jersey, Pennsylvania, Ohio, Michigan, Illinois, Massachusetts) affirmatively permit and statutorily structure public-employee collective bargaining; others (most southern states and several mountain-west states) prohibit it altogether for public employees. Right-to-work laws — which prohibit union security clauses that require non-members to pay agency fees — add a further layer in 28 states.[3] Taken together, these legal features produce the geographic concentration of faculty unionization visible in our data: in-panel first CBAs cluster heavily in states with permissive public-employee bargaining law.

The unionized workforce in higher education has also changed compositionally over the panel period. Faculty unionization is not a new phenomenon — the City University of New York, the State University of New York, and the California State University systems organized in the 1960s and 1970s — but the past decade has seen a distinct acceleration concentrated in non-tenure-track faculty. The Service Employees International Union's "Faculty Forward" campaign, launched in 2013, organized full-time and part-time adjunct faculty at

[2]The Yeshiva ruling does not extend to non-tenure-track faculty, who typically lack the governance role on which the managerial exception turns; this asymmetry has shaped the post-2010 NTT-focused organizing wave we describe below.

[3]For public-sector employees the federal *Janus v. AFSCME* (2018) ruling functionally extended right-to-work to all states by holding mandatory agency fees unconstitutional for public-sector unions.

dozens of institutions on a metropolitan-region basis. The American Federation of Teachers and the National Education Association — the two largest affiliations among the 736 institutional units in our data, with 401 and 338 institution-level pairings respectively[4] — have similarly emphasized non-tenure-track organizing. The National Labor Relations Board's 2016 *Columbia University* decision, which reversed a 2004 precedent and held that graduate student teaching and research assistants at private universities are statutory employees, opened a parallel front in graduate-student organizing that lies outside the scope of our data but is relevant context for institutions' bargaining environments. The substantive implication for our design is that approximately two-thirds of in-panel unionization covers only non-tenure-track or part-time faculty, with tenured / tenure-track faculty unaffected.

A bargaining unit becomes binding through a multi-step process that takes typically one to three years from initial petition to first ratified contract.[5] Faculty interested in unionizing first identify a proposed bargaining unit — the set of employees the union would represent — and file a petition with the relevant labor board (the NLRB for private institutions, the state public-employee labor relations board for publics). The board determines whether the proposed unit is appropriate and conducts a secret-ballot election. If a majority of voting employees vote in favor, the union is certified as the exclusive bargaining representative. The certified union and the institution then negotiate the first collective bargaining agreement, a process that typically lasts six months to two years for newly certified units. We use the first CBA date, rather than the certification or recognition date, as our treatment variable: the recognition decision establishes the bargaining relationship, but no compensation terms are binding on the institution until the first CBA is ratified. The Herbert et al. (2024) directory reports both dates separately, and recognition consistently precedes first CBA by a median of approximately two years in our sample.

We note here that the composition of bargaining units is heterogeneous: some bargaining units feature only tenure-related faculty, others only non-tenure-related faculty, and some

[4]Counts are at the institution level (one institution can have multiple bargaining units of different compositions). The American Association of University Professors, historically the dominant tenured-faculty union, accounts for 128 institutions; the SEIU is more recently active and accounts for fewer institution-pairs in the directory but a larger share of adjunct-faculty coverage.

[5]Recognition can also occur through voluntary card-check agreements with the employer, bypassing the election step; this is uncommon but not unheard of in the higher-education context.

include both. Institutions may have multiple bargaining units covering different employee types. We return to these issues when we discuss our data.

Finally, we view the university for purposes of this paper as a firm that sets wages and prices that is subject to a labor-cost shock when unionization occurs. Standard union-wage-premium logic predicts that a CBA raises compensation for the covered group through some combination of bargaining power, productivity, and institutional reservation-wage shifts; the magnitude is an empirical question. Because both private and public institutions may set tuition, a natural hypothesis is that an upward shock to labor costs is partially passed through to tuition. The university's non-profit objective function complicates this prediction — a revenue-constrained but not profit-maximizing institution may absorb labor-cost increases by re-allocating spending across categories, by changing the composition of the workforce, or by reducing the scale of operations.

3 Data and empirical strategy

We collect data on faculty salaries and institutional characteristics from the Integrated Post-Secondary Education Data System (IPEDS) maintained by the U.S. Department of Education. IPEDS surveys all colleges, universities, technical and vocational institutions that participate in federal financial aid programs annually. Our panel spans 2004–2024 and covers 9,919 institutions (excluding institutions with collective bargaining agreements already in place before 2004). For compensation, we construct a 9-month salary equivalent for each of the six reported harmonized ranks (professor, associate professor, assistant professor, instructor, lecturer, and no rank).[6] We classify ranks into tenure-related (TTT) and non-tenure related (NTT). We also collect several institutional characteristics including enrollment, demographics, endowment, and indicators for public status and institutional type.

We collect unionization data from the 2024 *Directory of Bargaining Agents and Contracts*

[6]IPEDS restructured its Human Resources salary tables in the 2012 reporting year, replacing the earlier CONTRACT-dimensioned table with a flat table reporting contract months and outlays directly, and (from 2016 onward) a pre-computed 9-month equivalent. We construct the 9-month equivalent so that the measure is conceptually consistent across these changes, but the raw measure has a roughly 10% level shift at the 2012 boundary that reflects the survey restructuring rather than a real salary change. Year fixed effects absorb this level shift in all DiD specifications.

in Institutions of Higher Education (Herbert et al. 2024). These data describe individual bargaining units at the institution level, including the date of recognition, date of first contract, bargaining unit composition (tenure-track, non-tenure-track, part-time, etc.), and affiliation with national unions. We match these data to IPEDS institutions and transform the observations into institution-year form, retaining per-bargaining-group indicators and CBA dates that capture which employee groups are covered at each institution.

Of the 971 bargaining unit records covering the 736 institutions in the source directory, the four most common compositions are: full-time tenured / tenure-track plus full-time and part-time non-tenure-track combined ("FT-PT TTT-NTT," 292 units, the most comprehensive coverage); part-time non-tenure-track only ("PT NTT," 202 units, typically adjunct-only campaigns); full-time tenured / tenure-track plus full-time non-tenure-track combined ("FT TTT-NTT," 153 units); and full-time tenured / tenure-track only ("FT TTT," 150 units). An additional 78 units cover full-time non-tenure-track only.

Table 1 reports summary statistics for the primary analyses, split by several overlapping cuts: all institutions, ever-4-year institutions, 2-year-only institutions, institutions first treated during the panel, and never-treated institutions. Treated institutions are systematically larger (roughly 12,000 students vs. 3,000 at never-treated), more likely to be public (49% vs. 23%), and pay higher salaries across every rank. These selection patterns motivate the use of synthetic control methods as a check on our primary staggered-DiD design.

Figure 1 shows the distribution of first-CBA years across treated institutions, colored by whether the bargaining unit is affiliated with the AAUP. Treatment adoption is staggered across cohorts between 2005 and 2023. This staggered structure, combined with evidence of heterogeneity across cohorts and subgroups, motivates our use of the Callaway and Sant'Anna (2021) estimator rather than a two-way fixed-effects specification, which is known to produce biased average effects in staggered settings with heterogeneous treatment effects.

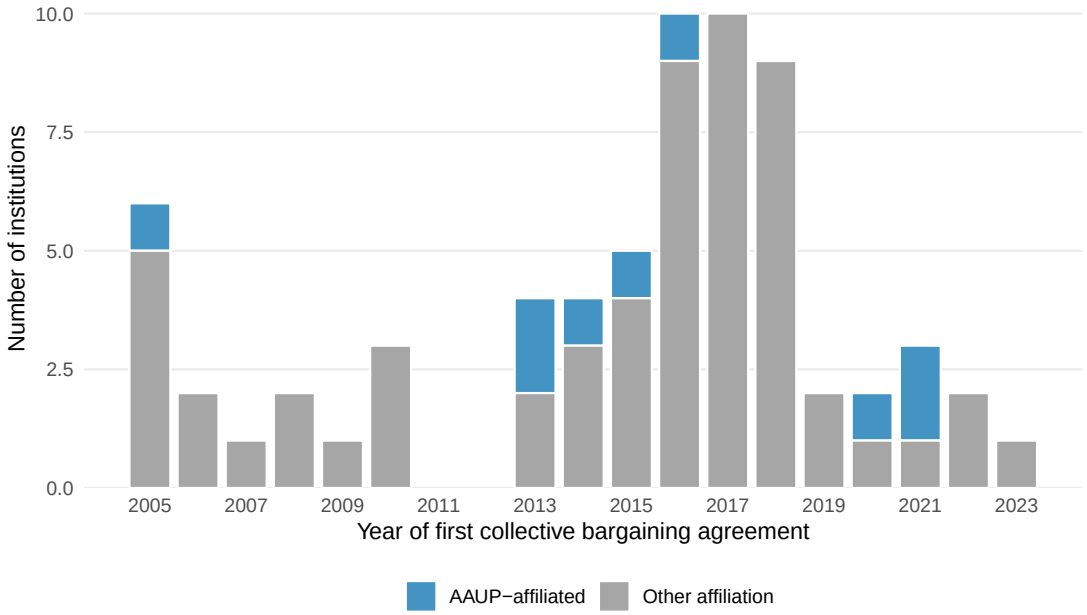
Our empirical strategy applies the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator separately to each outcome. A key design choice is the treatment variable. Bargaining units in U.S. higher education typically cover specific employee groups, and about two-thirds of in-panel unionization covers only non-tenure-track faculty. Estimating the effect of "any union" on a group-specific outcome such as professor salary therefore

Table 1: Summary Statistics

	All	4-year	2-year	Treated	Never-treated	Always-treated
Panel A: Faculty salary (9-month equivalent, $000s)						
Professor	76.9	78.7	66.1	98.2	74.1	87.5
Associate professor	64.1	65.1	57.7	75.1	62.3	71.0
Assistant professor	55.8	56.5	51.4	64.6	54.4	61.0
Instructor	45.5	45.4	45.7	51.7	43.9	54.7
Lecturer	50.4	50.8	45.5	55.8	49.0	54.1
All instructional staff	55.3	58.1	49.0	72.4	52.9	68.5
Panel B: Faculty tenure composition (share of full-time instructional, 2012–2024)						
Tenured	33.8%	34.9%	29.4%	30.0%	34.4%	32.2%
Tenure-track	14.8%	15.9%	10.7%	11.5%	16.1%	10.8%
Non-tenure-track	51.4%	49.2%	59.9%	58.5%	49.5%	56.9%
Panel C: Instructional staff counts (headcount)						
Professors	87	94	46	187	75	133
All instructional staff	196	237	102	526	168	322
Panel D: Tuition and pricing ($)						
In-state sticker tuition	13,707	17,015	6,948	20,722	14,390	8,046
Out-of-state sticker tuition	16,173	19,580	9,211	24,841	16,388	13,165
Tuition revenue per student	6,750	9,173	2,749	10,458	7,093	4,053
In-state net price	9,519	11,436	8,067	10,376	9,555	9,325
Panel E: Spending per student ($)						
Instructional spending	7,575	8,409	6,197	9,827	7,950	4,976
Total expenses	23,545	28,462	12,980	29,973	25,037	14,258
Panel F: Institutional characteristics						
Total enrollment	4,127	6,379	2,355	11,985	3,244	12,348
Undergraduate enrollment	3,571	5,117	2,355	9,109	2,763	11,426
Endowment ($M)	195.7	237.1	58.1	472.5	205.2	90.3
Public institution	28.8%	26.8%	30.5%	48.6%	23.4%	87.6%
Institutions	10,483	4,062	6,421	108	9,811	564
Institution-years	147,769	67,398	80,371	2,249	133,862	11,658

Notes: Cell entries are institution-year means pooled across 2004–2024. The first three columns describe the full panel: "4-year" indicates institutions classified as 4-year in at least one panel year, and "2-year" indicates institutions that are never 4-year. The last three columns partition the same institutions by treatment status: "Treated" comprises institutions whose first collective bargaining agreement is recorded between 2005 and 2023; "Never-treated" are institutions with no recorded union of any kind, plus the 2024 cohort whose post-treatment period is unobserved; "Always-treated" are institutions with a CBA already in effect by 2004. Always-treated institutions are reported here for completeness but are excluded from the difference-in-differences analysis because they have no untreated period available for identification. Tenure composition is reported for 2012–2024 due to data availability. Community colleges often do not employ faculty at the professor/associate/assistant ranks, so those cells are based on the subset that does. Dollar values are nominal.

Figure 1: Timing of first collective bargaining agreement, by affiliation



Notes: Number of treated institutions by the year of their first collective bargaining agreement (any bargaining unit). Sample excludes always-treated institutions (CBA pre-2004); the 2024 cohort is reclassified as never-treated because its post-treatment period cannot be observed within the panel.

combines genuinely treated institutions with institutions where the targeted group is unaffected, attenuating any causal estimate toward zero. Thus, when applicable, we define treatment as the establishment of a collective bargaining agreement *for the relevant group.* In these group-specific specifications, institutions that are unionized but for a different group are dropped from the sample: they are neither cleanly treated (the targeted group is not covered) nor cleanly control (they are unionized for a different group, which may contaminate outcomes). Controls in all specifications are not-yet-treated institutions. We allow an unbalanced panel; a balanced-panel sensitivity check is reported in Appendix B.

For some outcomes, this distinction is not necessary and so we define treatment as the presence of any faculty-related collective bargaining agreement.

We complement the CS DiD analysis with an estimate from the augmented synthetic control estimator of Ben-Michael et al. (2021), implemented via the `augsynth` package and reported in Appendix A. Multisynth constructs a separate synthetic-control counterfactual for each treated institution from the never-treated pool and is independent of the `did` ma-

chinery. The synthetic-control approach imposes a balanced-panel requirement that shrinks the effective treated sample materially, so we view it as a complement to rather than a replacement for the CS DiD estimates.

4 Results and discussion

4.1 Employee-level effects

Table 2 reports the overall average treatment effect on the treated (ATT) from the Callaway and Sant'Anna estimator for thirteen employee-level outcomes, organized into four panels. The central finding: unionization raises salaries for the unionized group by 4–5 percent across the three tenured / tenure-track ranks (Panel A, FT-TTT treatment) and by 7–8 percent for non-tenure-track instructors and lecturers at four-year institutions (Panel B, FT-NTT treatment), each significant at the 2% level or better. The separate community-college instructor row in Panel B is precise about being close to zero (+1.5%, not significant) but the minimum detectable effect is 13 percentage points and cannot distinguish the point estimate from the +7% four-year result; only fifteen 2-year-only institutions are cleanly treated under the FT-NTT spec, against a 2,500-unit never-treated control pool. Panel C reports the effect of unionization on the rank composition of the full-time instructional workforce as a two-by-two "own" versus "cross" table: each rank share is estimated against both the matched CBA (the union that covers that rank directly) and the cross CBA (the union that covers the other rank group). All four estimates are significant and the picture is a clean substitution story. FT-TTT unionization is associated with a 4.1 percentage-point decline in the professorial (TTT) rank share — the own effect — and with a 6.7 percentage-point rise in the instructor / lecturer (NTT) rank share — the cross effect. FT-NTT unionization is associated with a 5.3 percentage-point rise in the NTT-rank share (own) and a 2.9 percentage-point decline in the TTT-rank share (cross). Because these are shares of the full-time instructional headcount, they do not by themselves capture substitution into part-time or adjunct labor; Panel D tests that margin directly using the IPEDS occupation headcounts. Unionization raises the part-time share of instructional staff by about 4 percentage points under the any-union treatment

(significant at the 1% level), with a flat pre-trend (Figure 3), so the shift toward contingent labor extends past the full-time margin documented in Panel C. Because the underlying occupation cells begin in 2012, Panel D identifies only cohorts first treated in 2013 or later and should be read as restricted-cohort corroboration rather than a stand-alone estimate.

Table 2: Effect of Faculty Unionization on Salaries and Rank Composition

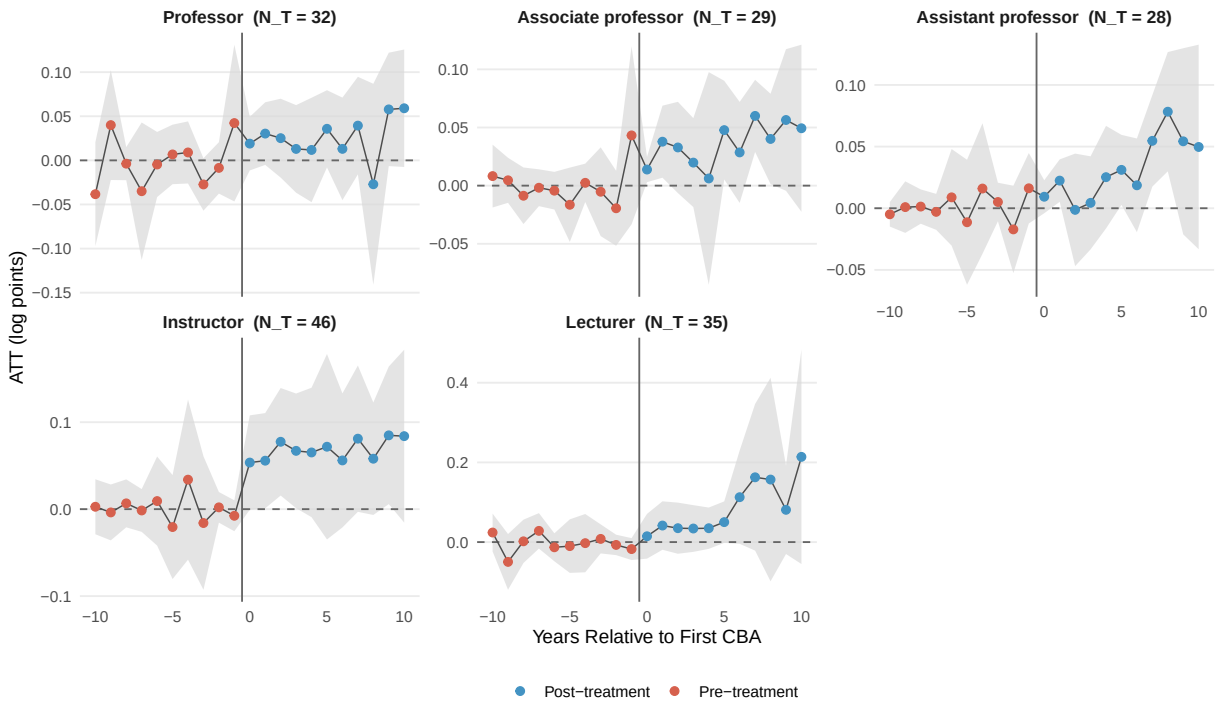
Outcome	ATT	SE	p-value	MDE_{80}	N_T	Sample/Spec
Panel A: Tenured/tenure-track faculty salary by rank (FT-TTT CBA; 4-year institutions)						
Professor	0.047	(0.019)	0.012	0.053	32	4-yr/FT-TTT
Associate professor	0.051	(0.016)	0.002	0.045	29	4-yr/FT-TTT
Assistant professor	0.049	(0.017)	0.003	0.047	28	4-yr/FT-TTT
Panel B: Non-tenure-track faculty salary by rank (FT-NTT CBA)						
Instructor	0.074	(0.029)	0.010	0.081	46	4-yr/FT-NTT
Lecturer	0.083	(0.033)	0.012	0.093	35	4-yr/FT-NTT
Instructor (2-year)	0.015	(0.044)	0.735	0.124	15	2-yr/FT-NTT
Panel C: Faculty rank composition: own and cross effects (4-year institutions)						
TTT-rank share (own)	−0.041	(0.015)	0.005	0.041	28	4-yr/FT-TTT
NTT-rank share (cross)	0.067	(0.021)	0.001	0.059	16	4-yr/FT-TTT
NTT-rank share (own)	0.053	(0.015)	<0.001	0.043	31	4-yr/FT-NTT
TTT-rank share (cross)	−0.029	(0.013)	0.025	0.036	44	4-yr/FT-NTT
Panel D: Part-time/adjunct substitution: PT share of instructional staff (4-year institutions, 2013+ cohorts)						
Part-time share (any-union)	0.040	(0.014)	0.005	0.040	89	4-yr/Any
Part-time share (FT-TTT)	0.020	(0.010)	0.051	0.029	35	4-yr/FT-TTT
Part-time share (FT-NTT)	0.008	(0.011)	0.472	0.031	52	4-yr/FT-NTT

Notes: Each row is the overall ATT from a separate Callaway and Sant'Anna (2021) DiD with not-yet-treated controls. Treatment is matched to outcome: Panel A uses the FT-TTT CBA, Panel B uses the FT-NTT CBA, Panel C pairs each rank share with both ("own" = matched, "cross" = the other group's CBA), and Panel D reports the part-time share of instructional staff under each treatment definition. Panels A, C, and D are estimated on ever-4-year institutions; Panel B reports the 4-year sample for both Instructor and Lecturer ranks and a separate 2-year-only Instructor row (Lecturer rank is essentially absent at community colleges). Institutions unionized for a different group than the spec are dropped in the group-specific rows. Always-treated institutions are excluded; the 2024 cohort is reclassified as never-treated. **Panel D:** the part-time share is part-time instructional headcount divided by total (full- plus part-time) instructional headcount from the IPEDS EAP survey (occupation code 210), reported in share points; the EAP occupation cells begin in 2012, so only cohorts first treated in 2013 or later are identified and N_T is correspondingly smaller. Log outcomes approximate percentage changes. $\text{MDE}_{80} = 2.8\times$ SE; N_T is the number of treated institutions.

Figure 2 shows the dynamic decomposition of all five rank-specific salary outcomes on the four-year sample. The three tenured / tenure-track ranks (top row) all display similar dynamics: pre-treatment estimates hover near zero with no visible trend, and post-treatment effects build gradually over the first several years into the +4–7% range, consistent with unions negotiating multi-year contracts with compounding raises. Instructor salary (bottom-left) shows a cleaner break at treatment and a sustained +5–9% effect through year 10. Lecturer salary

(bottom-center) has a flat pre-trend and large but noisy post-treatment estimates: the late-cohort point estimates climb to +20% or more by year 7, but the confidence intervals are wide because only a small subset of treated institutions contribute to the longest event-time horizons.

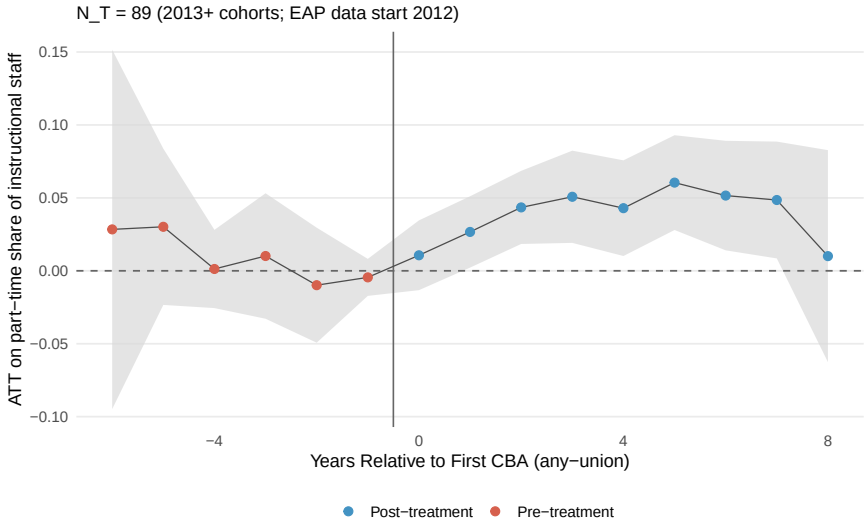
Figure 2: CS DiD event study: salaries by faculty rank



Notes: Dynamic aggregation of the Callaway and Sant'Anna (2021) difference-in-differences estimator for log salary at each rank. Top row uses the first FT-TTT CBA as treatment; bottom row uses the first FT-NTT CBA. Sample: ever-4-year institutions; institutions unionized for a different group than the panel's spec are dropped. Shaded bands are 95% pointwise confidence intervals. Event-time estimates are trimmed to $[-10, +10]$. The y-axis is scaled per panel because effect magnitudes differ substantially across ranks.

Figure 3 shows the dynamic part-time-share estimates behind Table 2, Panel D: pre-treatment effects are flat and the part-time share of instructional staff steps up following the first CBA. The pre-treatment window is short because the underlying IPEDS occupation cells begin in 2012.

Figure 3: CS DiD event study: part-time share of instructional staff



Notes: Dynamic aggregation of the Callaway and Sant'Anna (2021) estimator for the part-time share of instructional staff (part-time instructional headcount divided by total full- plus part-time instructional headcount, IPEDS EAP occupation code 210) under the any-union first CBA. Sample: ever-4-year institutions. Because the EAP occupation cells begin in 2012, only cohorts first treated in 2013 or later are identified, so the pre-treatment window is short and N_T is smaller than in Figure 2. Shaded bands are 95% pointwise confidence intervals.

4.2 Tuition pricing by sector

The pricing response to unionization differs substantially by sector, so we report pricing decomposed by institution type rather than pooled. Table 3 pairs four pricing measures — published sticker tuition (reported separately as in-state and out-of-state for publics; a single published price for privates, which do not price-discriminate by residency), in-state net price (the average price actually paid by Title IV undergraduates after grant and scholarship aid), and tuition revenue per student (the realized per-FTE price the institution actually receives, combining sticker, payer mix, and institutional discounting) — across three sector panels.

At four-year publics (Panel A), none of the four pricing measures moves significantly: sticker is essentially flat (in-state −0.6%, out-of-state −2.9%), net price drifts negative (−5.5%, marginally significant at $p = 0.10$), and tuition revenue per student is null with a wide confidence interval. The two-year publics (Panel B) show a similar but noisier picture from only seventeen treated institutions: in-state sticker null, out-of-state sticker −9.5% (significant at the 5% level), net price −7.2% (ns). At private non-profits (Panel C), by contrast, published sticker rises by 4.4% ($p = 0.03$), but the average net price is essentially

unchanged (+0.6%, ns) and tuition revenue per student actually falls modestly (−3.1%, $p = 0.07$). Whatever sticker increases unionized privates post are more than offset by institutional aid: students at unionized privates do not pay more on average, and the institution does not collect more per student.

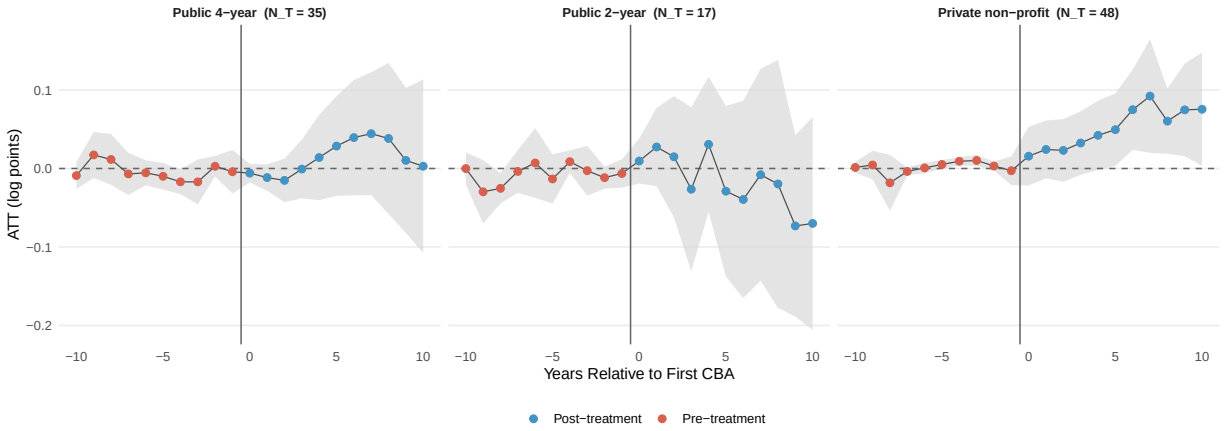
Table 3: Effect of Faculty Unionization on Tuition Pricing by Sector

Outcome	ATT	SE	p-value	MDE_{80}	N_T
Panel A: Public 4-year institutions					
In-state sticker tuition	−0.006	(0.029)	0.843	0.081	35
Out-of-state sticker tuition	−0.029	(0.047)	0.539	0.131	35
In-state net price	−0.055	(0.034)	0.104	0.095	35
Tuition revenue per student	−0.069	(0.093)	0.455	0.260	34
Panel B: Public 2-year institutions					
In-state sticker tuition	−0.029	(0.037)	0.432	0.102	17
Out-of-state sticker tuition	−0.095	(0.046)	0.039	0.128	17
In-state net price	−0.072	(0.047)	0.129	0.132	17
Tuition revenue per student	0.077	(0.063)	0.227	0.177	17
Panel C: Private non-profit institutions					
Published sticker tuition	0.044	(0.020)	0.025	0.055	48
Average net price	0.006	(0.018)	0.729	0.049	48
Tuition revenue per student	−0.031	(0.017)	0.070	0.048	48

Notes: Each row reports the overall ATT from a separate Callaway and Sant'Anna (2021) difference-in-differences estimation with not-yet-treated controls, restricted to the sector indicated by the panel. Treatment is the first collective bargaining agreement of any unit ("any-union" spec). Sticker tuition is the published price reported in the IC survey; for publics this is reported separately for in-state and out-of-state students, while privates publish a single price (no residency discrimination). Net price is the average price actually paid by first-time full-time Title IV undergraduates after grant and scholarship aid, from the Student Financial Aid survey (available 2009–onward): in-state residents for publics and all Title IV undergraduates for privates. Tuition revenue per student is institution-reported tuition and fee revenue divided by total enrollment (the realized per-FTE price the institution actually receives, combining sticker, payer mix, and institutional discounting). All outcomes are in logs, so ATTs approximate percentage changes. $\text{MDE}_{80} = 2.8\times$ SE.

Figure 4 shows the event-study dynamics for sticker tuition in each sector, confirming the table's static picture. Public 4-year and public 2-year sticker trajectories drift sideways or slightly downward through the post-treatment period with no visible break. The private sticker series shows a clean break at treatment and a sustained climb to +7–9% by years 5–10. Taken together, these results do not support a simple wage-pass-through-to-tuition mechanism. Students at unionized institutions are not paying more on average.

Figure 4: CS DiD event study: sticker tuition by sector



Notes: Dynamic aggregation of the Callaway and Sant'Anna (2021) estimator for log sticker tuition, separately by sector. For publics, sticker is the in-state published price; for privates, the single published price (no residency discrimination). Treatment is the first collective bargaining agreement of any unit ("any-union" spec). Shaded bands are 95% pointwise confidence intervals. Event-time estimates are trimmed to $[-10, +10]$. The y-axis is scaled per panel.

4.3 Institutional finances

Table 4 reports the institution-level non-pricing finance consequences of unionization. At the total-dollar level (Panel A) the picture is one of essentially flat institutional spending. Total expenses rise an estimated 1.0% with a wide confidence interval ($p = 0.75$); total revenue rises 3.9% but is also statistically indistinguishable from zero ($p = 0.11$). The institution does not appear to be expanding its budget to accommodate the bargaining-induced salary increase, suggesting that the workforce-composition shift documented in Table 2, Panel C — a substitution from higher-cost tenured / tenure-track positions toward lower-cost non-tenure-track ranks — is the dominant margin through which the new salary cost is absorbed.

Panel B decomposes total expenses into eight functional categories. None of the eight reaches conventional statistical significance and the point estimates are mixed in sign: instructional, research, academic support, and student services all move modestly upward (point estimates between +3% and +9%), while public service, auxiliary expenses, and scholarships have negative point estimates. The largest single magnitude is public service (-31%, marginally significant at $p = 0.09$), but the standard error on this outcome is large and we treat it as an informatively imprecise null rather than a substantive finding.

Table 4: Effect of Faculty Unionization on Institutional Finances

Outcome	ATT	SE	p-value	MDE_{80}	N_T
Panel A: Aggregate revenue, expense, and enrollment (log totals)					
Total revenue	0.039	(0.024)	0.114	0.068	103
Total expenses	0.010	(0.031)	0.753	0.087	103
Total enrollment	−0.048	(0.022)	0.027	0.061	104
Panel B: Expense decomposition (log totals)					
Instructional	0.034	(0.026)	0.201	0.074	103
Research	0.089	(0.226)	0.694	0.632	66
Public service	−0.311	(0.184)	0.091	0.515	75
Academic support	0.071	(0.045)	0.118	0.127	103
Student services	0.033	(0.033)	0.317	0.094	103
Institutional support	0.020	(0.027)	0.452	0.075	103
Auxiliary expenses	−0.050	(0.046)	0.274	0.129	100
Scholarships	−0.007	(0.143)	0.963	0.400	67

Notes: Each row reports the overall ATT from a separate Callaway and Sant'Anna (2021) difference-in-differences estimation with not-yet-treated controls. Treatment is the first collective bargaining agreement of any unit ("any-union"). Sample: all institutions in the panel, always-treated institutions (any CBA in effect before 2004) excluded, institutions first treated in 2024 reclassified as never-treated. No balanced-panel requirement; CS DiD's unbalanced-panel option handles missing cells. All revenue and expense outcomes are log totals in nominal dollars (*not* per-student normalizations): because total enrollment is itself an outcome that responds to unionization, per-student measures mechanically combine the numerator response with the denominator response and so understate or overstate the total-dollar effect. Total enrollment is reported separately as log(total enrollment). Tuition pricing outcomes (sticker, net price, tuition revenue per student) are reported by sector in Table 3. Revenue sub-category outcomes (gifts, investment income, total grants, state appropriations, auxiliary revenue) are not reported here: event-study diagnostics on per-student versions of these outcomes either showed pre-treatment trend continuation (grants, investment income) or had confidence intervals too wide to support a specific claim (gifts, state appropriations, auxiliary revenue). All log outcomes approximate percentage changes. MDE_{80} is the minimum detectable effect at a two-sided 5% level with 80% power (MDE = 2.8× SE). N_T is the number of treated institutions contributing to each outcome.

4.4 Cross-group spillover

If unionization raises salaries for the unionized group, one might ask whether it also raises salaries for non-unionized colleagues at the same institution, either because the institution has a compressed internal pay structure or because anticipated organizing pressure from the newly unionized group spills over to the non-unionized group's wages. Table 5 tests both directions. Panel A reports the effect on tenured / tenure-track salaries at institutions where only non-tenure-track faculty unionize: all three rank-specific estimates are close to zero and statistically indistinguishable from zero, with minimum detectable effects around 5–7 percentage points — so we can rule out spillovers as large as the own-group unionization effect (5%), but not smaller (2–3%) effects.

Table 5: Cross-Group Salary Spillovers

Outcome	ATT	SE	p-value	MDE_{80}	N_T
Panel A: TTT salary at NTT-only-union institutions					
Professor	0.015	(0.017)	0.381	0.047	22
Associate professor	0.012	(0.018)	0.515	0.051	22
Assistant professor	−0.021	(0.026)	0.419	0.074	22
Panel B: NTT salary at TTT-only-union institutions					
Instructor	−0.025	(0.069)	0.721	0.192	6

Notes: Each row reports the overall ATT from a separate Callaway and Sant'Anna (2021) difference-in-differences estimation with not-yet-treated controls. Sample: ever-4-year institutions; controls are institutions with no union coverage of any kind. Panel A tests whether tenured / tenure-track (TTT) faculty salaries move when only NTT faculty at the institution unionize (treated = first FT-NTT CBA during 2005–2023, no TTT union of any kind; institutions with any TTT union are dropped). Panel B is the reverse: whether Instructor salaries move when only TTT faculty unionize (treated = first FT-TTT CBA during 2005–2023, no NTT union of any kind; institutions with any NTT union are dropped). Lecturer is omitted from Panel B because most treated units have no lecturer-salary data. The small Panel B treated count (six institutions) means the estimate is precise about being non-zero only if the effect is large; MDE_{80} indicates the smallest effect we could distinguish from zero with 80% power. $\text{MDE}_{80} = 2.8\times$ SE.

Panel B reports the reverse direction — instructor salaries at institutions where only tenure-track faculty unionize. Only six ever-4-year institutions in the panel satisfy that condition, so the test has limited power. The point estimate is close to zero (−2.5%) but the standard error is large enough that the minimum detectable effect is 19 percentage points, uninformative about any spillover smaller than the largest own-group effect we observe. We include the row for completeness; the substantive result is that we can rule out large reverse-direction spillovers, but the symmetric small-effect question remains open.

4.5 Heterogeneity

Table 6 reports the salary ATT for professor salary (Panel A, FT-TTT treatment) and instructor salary (Panel B, FT-NTT treatment) across six subgroups each: public / private, large / small by baseline enrollment, and AAUP-affiliated / non-AAUP. Three patterns stand out. First, for tenure-track salaries, AAUP-affiliated unions produce a substantially larger effect (+7.0%, $p = 0.002$) than non-AAUP unions (+3.9%, $p = 0.10$). For instructor salaries this AAUP premium disappears — non-AAUP unions actually produce a somewhat larger point estimate. The AAUP advantage is therefore specific to tenure-track bargaining, consistent with the AAUP's long history of tenure-track-focused advocacy. Second, the salary effect is concentrated at large institutions for both outcomes; small-institution subsamples are too thinly populated (N_T of 3–5) to support meaningful inference. Third, public and private point estimates are of similar magnitude for each outcome, but private subsamples are noisy (especially for tenure-track salaries, where only seven private institutions enter the estimation).

4.6 Faculty productivity at research universities

The compositional substitution toward non-tenure-track ranks documented in Table 2, Panel C raises a natural question: does the institution's research and teaching output change as a consequence? Output is most informative where it is non-trivial, so we restrict the sample for this exercise to ever-R1 / R2 institutions (Carnegie Basic Classification 15 or 16). The treatment is the same FT-TTT CBA used elsewhere; always-treated and wrong-union institutions are dropped. Ten R1 / R2 institutions are cleanly treated.

Table 7 reports two panels. Panel A measures research output and intensity. Research doctorates (PhDs) awarded fall by 21.7% ($p = 0.003$). Master's-degree counts fall by a smaller 9.9% ($p = 0.11$, marginally significant), and research expenditures per tenured / tenure-track (TTT) faculty fall by 14.7% ($p = 0.004$). The PhD decline is therefore roughly twice the magnitude of the master's decline, which is informative: with total graduate enrollment essentially unchanged at these institutions (an explicit check; ATT = −0.6%, $p = 0.92$), the gap between the two suggests an enrollment composition shift toward master's course-

Table 6: Heterogeneity in Salary Effects

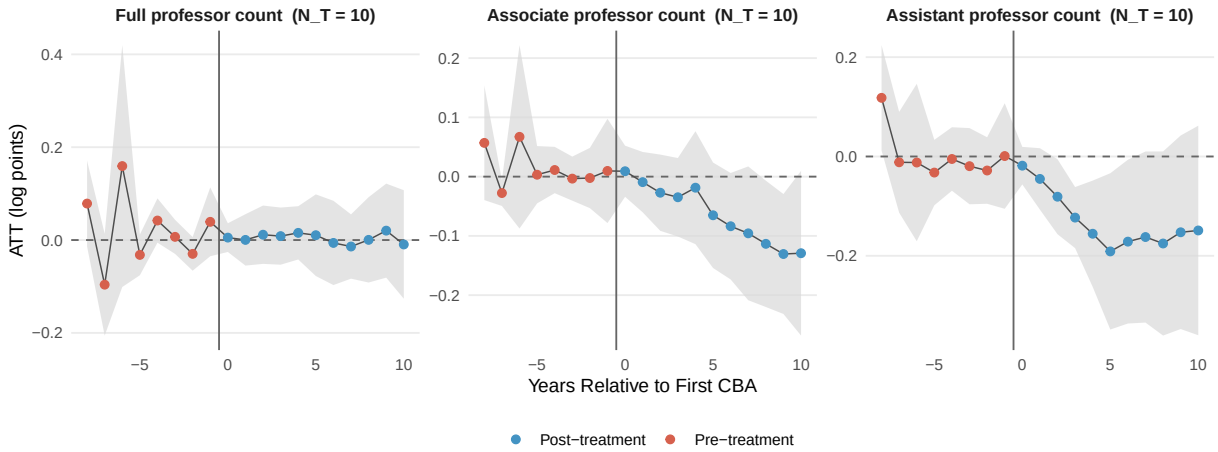
Subgroup	ATT	SE	p-value	MDE_{80}	N_T
Panel A: Professor salary (FT-TTT treatment)					
Public institutions	0.043	(0.020)	0.032	0.056	26
Private non-profit institutions	0.059	(0.060)	0.319	0.167	7
Large (baseline enroll. $\geq$ med.)	0.046	(0.019)	0.019	0.054	26
Small (baseline enroll. $<$ med.)	0.008	(0.054)	0.883	0.150	5
AAUP-affiliated	0.070	(0.022)	0.002	0.062	9
Non-AAUP	0.039	(0.024)	0.104	0.067	23
Panel B: Instructor salary (FT-NTT treatment)					
Public institutions	0.046	(0.032)	0.146	0.089	30
Private non-profit institutions	0.127	(0.084)	0.132	0.236	17
Large (baseline enroll. $\geq$ med.)	0.068	(0.029)	0.018	0.080	41
Small (baseline enroll. $<$ med.)	-0.045	(0.026)	0.080	0.072	3
AAUP-affiliated	0.056	(0.040)	0.166	0.112	12
Non-AAUP	0.080	(0.034)	0.018	0.095	34

Notes: Each row reports the overall ATT from a separate Callaway and Sant'Anna (2021) difference-in-differences estimation on a subgroup. Sample in both panels: ever-4-year institutions, unbalanced panel. Always-treated institutions (CBA $\leq$ 2004) and "wrong-union" institutions (unionized for a different group than the treatment definition) are excluded. Panel A uses FT-TTT treatment for tenured / tenure-track salaries; Panel B uses FT-NTT treatment for instructor salaries. Sector and size cuts restrict both the treated and the control group to the subgroup (apples-to-apples comparison). Size is determined by baseline total enrollment relative to the sample median. The AAUP cut restricts the treated group to institutions affiliated (or not) with the AAUP; the control group is the full never-treated pool, since there is no "AAUP never-treated" comparison institution. $\text{MDE}_{80} = 2.8\times$ SE.

work programs together with a slower PhD completion rate, rather than a uniform contraction of graduate education. A decomposition of research expenditures into salary and non-salary components attributes the per-TTT decline almost entirely to non-salary research dollars (grants and indirect cost recovery), with the salary share within research expenditures rising modestly.

Panel B traces the labor-input margin that is doing the work. Full-professor headcount is essentially unchanged (-1.4%, $p = 0.75$); associate professor count falls 11.8% ($p = 0.06$); assistant professor count falls 18.2% ($p < 0.001$). The TTT pool at unionized R1 / R2 institutions is not contracting overall (consistent with the Panel C result in Table 2 that the headline rank-share shift is across the TTT-NTT boundary); rather, the composition within TTT shifts toward seniority. Figure 5 shows the event-study trajectories. The full-professor series is flat across the post-treatment window, ruling out a retirement wave. The associate-professor series declines gradually from CBA year zero, consistent with attrition without timely replacement (associates promoting to full, but few new associate hires or promotions in). The assistant-professor series exhibits the sharpest break: a kink at year zero leading directly into an -15 to -20% level by year five, the shape of a hiring freeze rather than gradual attrition. Pre-treatment estimates in all three panels hover near zero.

Figure 5: CS DiD event study: TTT-rank counts at R1 / R2 institutions



Notes: Dynamic aggregation of the Callaway and Sant'Anna (2021) difference-in-differences estimator for the log full-time instructional staff count at each professorial rank. Sample: ever-R1 / R2 institutions. Treatment is the first FT-TTT CBA. Shaded bands are 95% pointwise confidence intervals. Event-time estimates are trimmed to $[-8, +10]$. The y-axis is scaled per panel; the magnitudes are not directly comparable across the three ranks. Each panel uses the same ten cleanly-treated institutions.

Table 7: Faculty Productivity at R1 / R2 Institutions

Outcome	ATT	SE	p-value	MDE_{80}	N_T
Panel A: Research output and intensity					
PhDs (research doctorate)	−0.217	(0.074)	0.003	0.208	10
Master's degrees	−0.099	(0.066)	0.131	0.183	10
Research $ per TTT faculty	−0.147	(0.050)	0.003	0.141	10
Panel B: Tenured / tenure-track labor input by rank					
Full professor count	−0.014	(0.042)	0.744	0.117	10
Associate professor count	−0.118	(0.064)	0.066	0.180	10
Assistant professor count	−0.182	(0.044)	<0.001	0.123	10

Notes: Each row reports the overall ATT from a separate Callaway and Sant'Anna (2021) difference-in-differences estimation with not-yet-treated controls. Sample: ever-R1 / R2 institutions, defined by Carnegie Basic Classification code 15 (R1, very high research) or 16 (R2, high research) using the 2021 vintage (fallback 2018). Treatment is the first FT-TTT collective bargaining agreement; always-treated institutions (CBA before the panel) and wrong-union institutions (unionized for some other group than FT-TTT) are dropped, and the 2024 cohort is reclassified as never-treated. Panel A outcomes use $\log(1 + y)$ to retain institutions with zero degrees in some years; Panel B outcomes use $\log y$ on the full-time instructional staff headcount at each professorial rank (IPEDS SAL / EAP). The MDE_{80} column reports the minimum detectable effect at 80% power ($2.8 \times \text{SE}$). Both Panel A and Panel B should be read against the headline rank-composition table (Table 2, Panel C), which shows that across all 4-year institutions, FT-TTT unionization shifts the rank-share mix toward non-tenure-track instructional ranks.

The picture that emerges is a structural-aging story: existing tenured faculty are protected by unionization, but the administration responds by freezing the assistant-professor hiring pipeline. The aging TTT pool that results is consistent with both observed downstream effects in Panel A. Research expenditures per TTT head fall because fresh assistant professors — who must build grant pipelines to make tenure — are not coming in. PhD output falls because there are fewer junior dissertation chairs, who at research universities typically carry the bulk of new doctoral student supervision. Graduate enrollment looks stable in aggregate because master's-coursework students plausibly substitute for doctoral students in the headcount, even though the labor-intensive PhD pipeline is shrinking.

Because only ten R1 / R2 institutions are cleanly treated, we explicitly check robustness. The PhD result survives leave-one-out (all ten leave-one-out estimates are negative and significant at $p < 0.05$; the range is -0.18 to -0.27), survives dropping the five Florida-system institutions that contribute the largest share of treated mass (-0.25, $p = 0.02$ on the remaining five), and is replicated by the augmented synthetic control estimator (`augsynth::multisynth`: -0.24 average ATT; all five cleanly-treated post-2010 units nega-

tive).[7] Standard errors widen substantially in multisynth because of the small treated sample under the synthetic-control balanced-panel requirement, but every individual treated unit's estimate is negative. The same pattern of robustness holds for the research-expenditure-per-TTT result. The assistant-professor count decline, by contrast, is statistically tight ($p < 0.001$) and visually clean in the event study.

5 Conclusion

Faculty bargaining units in U.S. higher education typically cover specific employee groups, but the prior empirical literature has predominantly defined treatment as "any union." That choice combines genuinely treated institutions with institutions where the targeted group is unaffected and mechanically attenuates the effect of unionization on group-specific outcomes. We argue that the appropriate treatment for a given outcome is the first collective bargaining agreement covering the group whose compensation that outcome measures. Under this construct, the salary effects of unionization in higher education are unambiguous and economically substantial: tenured and tenure-track salaries rise by roughly 5 percent across the professor, associate, and assistant ranks, and non-tenure-track instructor and lecturer salaries rise by 7–8 percent at four-year institutions, with pre-trends that are flat enough to support a causal reading. The same construct produces clean nulls in the cross-group spillover test: at institutions where only non-tenure-track faculty unionize, tenured-faculty salaries do not move detectably. The previously reported null on average faculty salary at unionized institutions is therefore largely a measurement artifact: averaging across bargaining-unit compositions dilutes group-specific effects toward zero.

Institutions absorb the salary shock primarily through a shift in the composition of their instructional workforce. Both varieties of unionization are associated with a multi-percentage-point decline in the tenured / tenure-track share of the full-time instructional headcount and a roughly five-percentage-point increase in the non-tenure-track share. Total

[7] All numerical claims in this subsection that do not appear directly in Table 7 — the graduate-enrollment ATT, the research-expenditure salary / non-salary decomposition, the leave-one-out range, the Florida-system drop estimate, and the multisynth average — are produced by `code/union/paper1_productivity_robustness.R` and written to `output/union_salaries/paper/productivity_robustness.csv`.

dollar spending at treated institutions does not change detectably and no individual expense category shows a statistically significant rise; the workforce-composition margin — replacing a higher-cost rank with a lower-cost rank — accounts for the salary-cost increase directly, without expanding the overall budget. The most striking expression of this margin appears at research universities, where unionization is followed by a hiring freeze at the assistant-professor rank — the labor input most easily compressed because it is at the entry point of the tenure pipeline. The downstream consequences at R1 / R2 institutions are visible in slower PhD production and falling per-faculty research expenditures, though these results rest on only ten cleanly-treated research universities and should be read cautiously.

The pricing response that the standard firm-pricing-of-marginal-cost story would predict does not materialize in the form one expects. Sticker tuition rises at private non-profits by about 4 percent following unionization, but the average net price paid by Title IV undergraduates does not move; institutional aid offsets the published increase. Public institutions do not raise sticker tuition at all and exhibit a marginally negative net-price response. The simple "unions raise costs, students pay more" narrative thus finds no support in our data — students at unionized institutions are not paying more on average. Yet total enrollment at treated institutions falls by approximately 5 percent. Because this enrollment decline is not a demand response to higher prices, its mechanism remains an open question. It may reflect a supply-side composition or capacity choice, a reputational response to organizing activity itself, or selection of organizing institutions on unobserved factors that also predict enrollment trajectories; the pre-trend on enrollment, while not large, is not flat enough to rule the last channel out.

Several limitations of our design point to next steps. The composition results report shares of the full-time instructional headcount only and are silent on substitution into part-time and adjunct categories; an extension that incorporates the IPEDS Fall Staff part-time tables would let us test whether the apparent shift toward non-tenure-track full-time positions extends or reverses when adjuncts are added. Our budget-side finding leaves unidentified the specific non-tuition revenue channel through which institutions absorb the salary shock — gifts, grants, investment income, state appropriations, and auxiliary revenue all fail pre-trend screens in our data and we report only the total. And although the rank-specific salary

results survive a synthetic-control cross-check, the in-panel treated sample of 108 institutions provides limited statistical power for finer subgroup analyses, particularly for private non-profits and for small institutions, where the salary effects are imprecisely estimated. Each of these is a constraint of the available data rather than of the design, and each becomes addressable as the directory of bargaining agreements is updated and as supplementary IPEDS extractions accumulate.

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A Augmented synthetic control

As an additional robustness check we re-estimate the effect of faculty unionization on rank-specific salaries using the augmented synthetic control estimator of Ben-Michael, Feller, and Rothstein (2021, 2022) as implemented in the `augsynth` package. Multisynth constructs a separate synthetic-control counterfactual for each treated institution from the never-treated pool and then aggregates the resulting unit-level ATTs. Because multisynth does not rely on the `did` package, it provides a genuinely independent check on our main results.

Table 8 reports the overall multisynth ATT alongside the CS DiD ATT from Table 2 for each of the five rank-specific salary outcomes. All five multisynth estimates are positive and directionally consistent with the CS DiD results. For tenure-track ranks, multisynth point estimates are about half the CS DiD magnitudes (professor +2.6% vs. +4.7% CS; associate +2.7% vs. +5.1%; assistant +2.3% vs. +4.9%); for non-tenure-track ranks the two estimators agree more closely (instructor +5.0% vs. +7.4%; lecturer +8.2% vs. +8.3%). None of the multisynth estimates is individually statistically significant: the balanced-panel requirement imposed by the synthetic-control approach shrinks the effective treated sample to nine–nineteen institutions per outcome, and standard errors from the multisynth jackknife are correspondingly four to five times larger than CS DiD. The attenuation in point estimates for tenure-track ranks is typical of synthetic-control matching, which absorbs pre-treatment level differences that CS DiD does not. Figure 6 displays the multisynth event-study trajectories for all five outcomes: post-treatment effects are consistently positive; pre-trends are flat (max $|t| \leq 0.60$ across the five outcomes).

Table 8: Augmented Synthetic Control (Multisynth) vs. CS DiD

	CS DiD			Multisynth		
Rank	ATT	SE	N_T	ATT	SE	N_T
Professor	0.047	(0.019)	32	0.026	(0.094)	17
Associate	0.051	(0.016)	29	0.027	(0.106)	14
Assistant	0.049	(0.017)	28	0.023	(0.083)	14
Instructor	0.074	(0.029)	46	0.050	(0.079)	19
Lecturer	0.083	(0.034)	35	0.082	(0.160)	9

Notes: The CS DiD columns reproduce the rank-specific salary rows of Table 2 (Callaway and Sant'Anna 2021 estimator, not-yet-treated controls, outcome-matched treatment, unbalanced panel). The multisynth columns report the average post-treatment ATT across unit-level synthetic-control effects as implemented by `augsynth::multisynth` (Ben-Michael, Feller, and Rothstein 2021) with $\nu = 0$ (no cross-unit pooling), unit fixed effects, five pre-treatment lags, and adaptive post-treatment leads. Multisynth requires a rectangular panel, which shrinks the effective treated sample relative to CS DiD. Cohorts first treated in the initial five years of the panel (before 2009) are reclassified as never-treated so that every treated cohort contributes at least five pre-treatment observations. Standard errors use the multisynth jackknife. Both estimators drop always-treated institutions and wrong-union institutions (unionized but not for the targeted group).

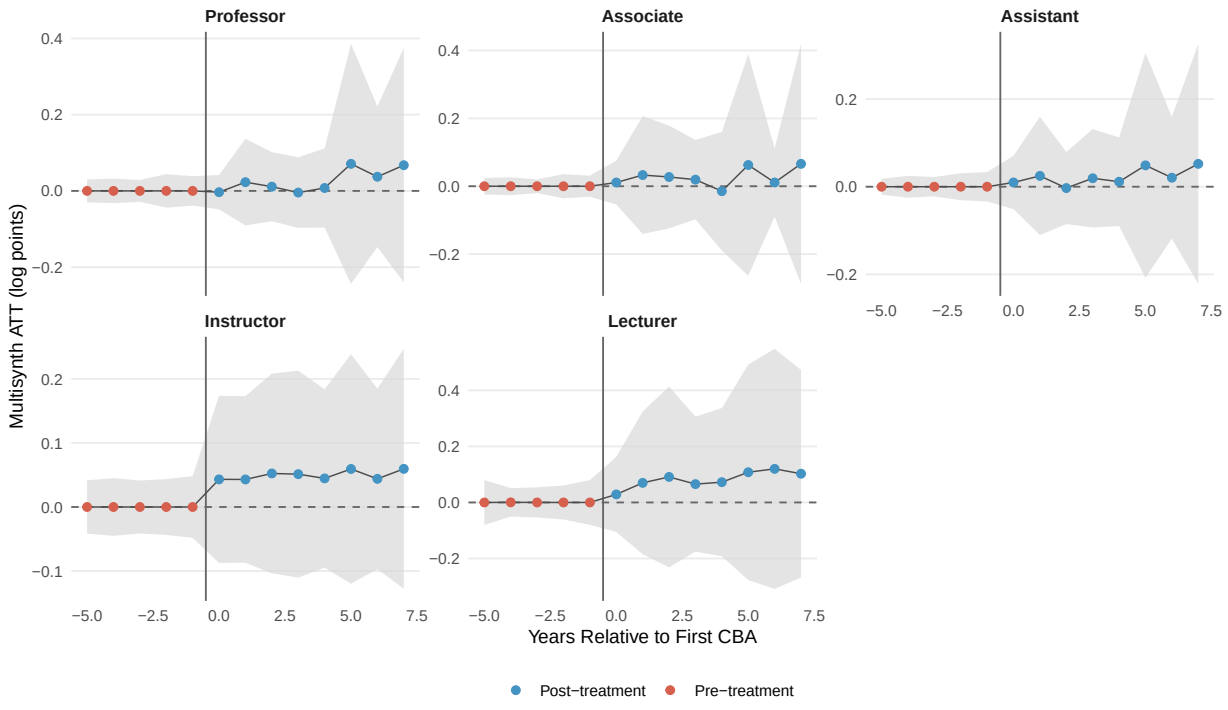

Figure 6: Multisynth event studies for the five rank-specific salary outcomes

Notes: Each panel plots the average post-treatment ATT across unit-level synthetic-control effects from `augsynth::multisynth` ($\nu = 0$, unit fixed effects, five pre-treatment lags, adaptive post-treatment leads), as a function of years relative to the first CBA. Shaded bands are 95% pointwise confidence intervals from the multisynth jackknife. The treatment definition is matched to the outcome (FT-TTT for professor / associate / assistant salary; FT-NTT for instructor / lecturer salary); always-treated and wrong-union institutions are excluded, and cohorts with fewer than five pre-treatment panel years (pre-2009) are reclassified as never-treated.

B Balanced-panel sensitivity

Our primary specification does not impose a balanced-panel requirement: institutions with missing outcome observations in some years remain in the sample. An alternative approach requires each institution to report the outcome in every year of that outcome's coverage range. Table 9 reports CS DiD estimates under both the primary unbalanced specification and a balanced-panel restriction for five representative outcomes: professor salary, NTT-rank share, in-state net price, instructional spending per student, and in-state sticker tuition. We also report estimates on the unbalanced sample after excluding the institutions that the balance requirement would have removed, to isolate their contribution to any difference.

For professor salary, in-state net price, and instructional spending per student, the balanced and unbalanced estimates are close: point estimates move by one percentage point or less, and the sign and statistical significance are preserved. For two outcomes the balance requirement moves the point estimate materially. The NTT-rank share effect drops from +5.3 percentage points (significant) to +2.6 percentage points (null) under the balanced restriction — the balance requirement shrinks N_T from 31 to 10 treated institutions, losing much of the power that drives the primary result. In-state sticker tuition moves from +4.7% (significant) to +1.4% (null): the aggregate sticker finding is sensitive to the inclusion of institutions with incomplete tuition records. The two-column "Excl. diff-set" estimates match the balanced column for every outcome, confirming that the difference between the unbalanced and balanced rows is entirely attributable to the institutions the balance requirement removes rather than to the estimator re-weighting the units it retains.

Table 9: Sensitivity to the Balanced-Panel Restriction

Outcome	Unbalanced (primary)	Balanced	Unbalanced, excl. diff-set
Professor salary	0.047	0.042	0.042
	(0.019)	(0.020)	(0.020)
N_T / N	32 / 2432	25 / 1382	25 / 1382
Share at NTT ranks	0.053	0.026	0.026
	(0.016)	(0.018)	(0.017)
N_T / N	31 / 933	10 / 116	10 / 116
In-state net price	−0.053	−0.047	−0.047
	(0.029)	(0.030)	(0.029)
N_T / N	52 / 1621	51 / 1035	51 / 1035
Instructional spending	0.073	0.058	0.058
	(0.021)	(0.019)	(0.018)
N_T / N	99 / 3709	95 / 2384	95 / 2384
In-state sticker tuition	0.047	0.014	0.014
	(0.015)	(0.013)	(0.014)
N_T / N	100 / 5006	97 / 2315	97 / 2315

Notes: Each cell reports the overall ATT (standard error below in parentheses) from Callaway and Sant'Anna (2021) DiD with not-yet-treated controls and the outcome-matched treatment specification used in Table 2. N_T is the number of treated institutions and N the total sample size. The "Unbalanced" column is the primary specification reproduced from Table 2. The "Balanced" column restricts the sample to institutions with non-missing outcome values in every year of that outcome's coverage range (2004–2024 for salary and spending outcomes, 2009–2024 for in-state net price, 2012–2024 for the tenure-share outcomes). The "Excl. diff-set" column drops, from the unbalanced sample, the institutions that the balance requirement would have removed, isolating their contribution to any difference between the first two columns. Always-treated institutions (CBA pre-2004) are excluded throughout, and the 2024 cohort is reclassified as never-treated. For the professor-salary outcome, the balance requirement drops 1050 institutions (see `appendix_balanced_diff_institutions.csv` for the list).