The Impact of Unions on the Wage Distribution: Evidence from Higher Education[1]

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Abstract

We estimate the impact of unionization on the wage distribution of Canadian university faculty using longitudinal administrative data on salaries and exploiting the staggered rollout of unionization across institutions. We find that unionization compressed salaries: wages at the bottom of the unconditional distribution increased by roughly 10 percent while wages at the top were unaffected. Our evidence suggests that these distributional impacts were driven by the introduction of contractual salary floors. We also estimate little impact of unionization on faculty employment. Instead, our results suggest that the increase in universities' wage bills was financed by an increase in student enrollment.

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1. Introduction

Understanding the effect of unions on the distribution of wages has long been a central goal of economists. The rise in income inequality over the past several decades has often been linked to the decline in unionization (Farber et al. 2021). It has also been argued that cross-country differences in wage inequality can be explained by differences in the incidence of unionization (e.g., Mogstad et al. 2025).

Despite this interest, there exists limited empirical evidence to shed light on this relationship. Most studies of unions estimate the average causal effect on wages. While there are a small number of studies that examine the distributional impacts of unions using longitudinal micro data, they suffer from well-known limitations: first, they use research designs which potentially confound compositional effects (systematic differences between union and non-union firms) with causal effects (rent-sharing within a given firm); second, they rely on changes to union status over time which can be sensitive to measurement error (see, for example, Lewis 1990).

In this paper, we overcome these challenges using administrative matched employee-employer data linked to newly collected data on union certifications and first contracts and leveraging changes in the union status across different workplaces over time. Since we observe the first contract post-certification, we can precisely measure changes in union status at each workplace. Additionally, since we observe the same institution and worker pre and post unionization, we can fully control for both firm and worker composition. Our empirical strategy isolates within-worker changes in wages in response to an exogenous change in a workplace's union status allowing us to cleanly trace out the wage effects over time.

Our empirical setting is full-time faculty at Canadian universities which are a significant constituent of the public sector: 7 to 11 percent of total public sector employment in Canada was in the tertiary education sector over the period of our data.[2] Our administrative panel salary data capture the population of faculty in Canadian universities for the years 1970 through 2022, which we combine with newly collected records of unionization events. These records capture features of first contracts such as the presence of "salary floors", and establish the date of union certification, allowing us to investigate the impact of unions in their first years. Together these data begin in a period with no faculty unions and end with over 80 percent of faculty covered by union contracts. Thus, we can empirically examine the unionization of an entire sector of the economy over a 50-year period which is useful for understanding the impacts of unions over time. Finally, since we perfectly observe the union status of all workers in the data, we avoid misclassifying union status across workers in the workplace.[3]

Our empirical analysis leads to the following key findings. First, unions compressed faculty salaries. In the unconditional salary distribution, gains were concentrated at the lower percentiles. Six years post certification, they ranged from over 10 percent at the 10th percentile to close to 0 at the 75th and 90th percentiles. Consistent with this evidence, we find that the effect on salaries was concentrated locally around salary floors specified in the first union contracts, with little overall impact at the top of the distribution. Interestingly, the heterogeneity in salary gains

[2] See CANSIM table 10100025, for the years 1981 through 2012. https://open.canada.ca/data/en/dataset/b38895a5-eef9-43ad-bd3f-aa2525de8d24. In recent years, universities represent a combined $40 billion enterprise, employing over 400,000 workers (see https://univcan.ca/universities/facts-and-stats/#:~:text=Source%3A%20Universities%20Canada%20approximation%20based,Labour%20Force%20Survey%20data%2C%202022.&text=As%20a%20%2440%20billion%20enterprise,for%20close%20to%20410%2C000%20people).

[3] Occupation is unobserved in most administrative datasets. This is an important limitation since not all workers get unionized when a unionization event takes place. For instance, management typically remains non-union, which means that union status systematically depends on salary rank. The advantage of observing occupation in our data is that we can ensure workers are fully covered under the union contract when their workplace unionizes.

was not as pronounced by academic rank, indicating the compression occurred both within and across ranks.

Second, unionization increased salaries on average. In the first-year post unionization, the increase in average salary was roughly 2 percent, rising to 6 percent, 6 years after certification. These salary effects were primarily for union certifications in the first half of our sample period (1970-1995); we observe little impact in the second half (1996-2022). We consider several mechanisms that could explain the time pattern of our results.

Third, we do not find any effect of unionization on faculty employment. Given a downward-sloping labor demand curve, one might expect that the wage increase induced by unions would lead to a reduction in employment. However, we estimate no impact both on the stock of employment and the inflows and outflows of faculty.

Fourth, in the subperiod in which we observe salary gains, we find that unionization led to a significant increase in student enrollment but had no impact on student tuition or government transfers to universities. This suggests an increase in class size and/or greater workload per faculty post-unionization. Also, given that we find no impact on faculty employment, this indicates the salary gains were financed out of increased university revenues from higher enrollment.

Our paper contributes to the literature on the impact of unions on wages. Several influential papers use quasi-experimental designs to investigate the effect of recent union elections on wages. LaLonde, Marschke, and Troske (1996), Dinardo and Lee (2004) and Frandsen (2021) find no impact on wages on average, while Sojourner et al (2015) find negative effects although they are imprecisely estimated.[4] None of these papers examine whether unions

[4] Our analysis of salary floors relates to Card and Cardoso (2022) who examine the responsiveness of wages to changes in wage floors in collective bargaining agreements in Portugal.

compress wages at a given workplace. Our estimate of the mean impact of unionization is more in line with the “union wage premium” literature which finds a wage premium in the range of 10-20 percent (e.g., Freeman 1984, Card 1996, DiNardo et al. 1996, Lemieux 1998, Card 2001, Farber et al. 2021 and Fortin, Lemieux and Floyd 2021). Studies in this literature tend to rely on stronger identification assumptions: cross-sectional methods with parametric selection corrections for unobserved heterogeneity or “job mover” designs. Interestingly, Wang and Young (2024) point out that while the quasi-experiment literature considers union events post-1980, the union wage premium literature typically considers unionization earlier, when unions were believed to exert greater bargaining power. The temporal pattern of our quasi-experimental estimates is consistent with this narrative. In summary, our primary contribution is to use a quasi-experimental design to examine the impact of unions on the wage distribution within establishments using data in which we can precisely observe switches in union status.

The paper also contributes to the much smaller body of research on union effects in the public sector. The dearth of research is surprising because unionization rates are substantial in the public sector.[5] Freeman (2005) wrote that “If one were to analyze the impact of unionism by sector proportionate to collective bargaining coverage or membership today, nearly half of one’s research effort would be devoted to the public sector”. Early studies include Ashenfelter (1971) who finds a pay gap between 2-10 percent for fire fighters using cross-sectional methods. Robinson and Tomes (1984) present cross-sectional estimates of the union premium for private and public sector workers and find they are large and similar. Hoxby (1996) uses difference-in-

[5] Card, Lemieux and Riddell (2020) report unionization rates of 39% in the U.S. public sector versus 7% in the private sector and 76% versus 17% in Canada. This implies one-half of unionized workers in the U.S. and close to 60% in Canada are employed in the public sector even though that sector accounts for only 15% (U.S.) to 20% (Canada) of total in the public sector even though that sector accounts for only 15% (U.S.) to 20% (Canada) of total employment.

differences and reports a positive union salary effect of 2-5 percent for teachers based on district-level data, while Lovenheim (2009) following a similar approach finds no effect. Biasi (2021) found that salaries increased for high-quality teachers when a district-level reform in Wisconsin led to the expiration of preexisting collective bargaining agreements.

Closer to our setting, several studies have considered the effects of unions on wages in higher education. Rees, Kumar and Fisher (1995), Hosios and Siow (2004) and Martinello (2009) using Canadian data found little to no impact of unionization on faculty salaries. Hedrick et al. (2011) using data from the National Study of Postsecondary Faculty report average effects that are small and statistically insignificant in specifications with state fixed effects. By contrast, our estimates of the wage effects of unions in these public institutions are large and precisely estimated.

2. Faculty Unions in Canada

The rules for certifying unions are set by the provinces.[6] Certification begins with a membership drive through which employees sign union cards. Once the proportion of employees signing cards crosses a threshold value, the relevant provincial labor relations board either certifies the union, or conducts a vote amongst employees for certification. Unionized faculty are typically represented by standalone unions rather than larger unions which represent workers across institutions or sectors of the economy.[7] At most universities, they represent "academic staff", which almost always includes faculty and librarians, and in some cases also includes sessional instructors, archivists, counsellors and professional administrative officers.

[6] See Baker et al. (2024) for the historical context of the emergence of faculty unionism in the 1960s, and Axelrod (1982) and Whalley (1964) on the roles of economic and governance considerations, respectively.
[7] All are also affiliated with the Canadian Association of University Teachers (CAUT), the Fédération québecoise des professeurs d'université (FQPPU) or the Confédération des syndicats nationaux (CSN). Both unionized and non-unionized faculty are affiliated with the CAUT and the FQPPU. Only certified faculty unions (in Quebec) are affiliated with CSN. The organizations advocate for university teachers, as well as providing some collective bargaining assistance to unionized members. The CSN affiliation unions are autonomous organizations. See Ross and Savage (2020).

It is worth noting that many faculty unions in Canada grew out of faculty associations which were founded before the unionization drives of the 1970s. Faculty associations are common at universities that have not unionized. A key difference between faculty unions and faculty associations is the right to strike. Faculty associations do not have the right to strike, although they may have access to binding arbitration to settle disagreements. Another difference is the structure of compensation. As noted by Chant (2005), unionized faculty are much more likely to receive formulaic, lock step salary increments based on seniority, and face salary ceilings. Faculty who are not unionized are much more likely to receive a part or all their increments based on merit. Finally, the scope of discussions between faculty associations and universities is typically not protected by provincial labour relations law and instead governed by their historical relationship ("memorandums of agreement").

3. Data

Our data on faculty salaries come from Statistics Canada's University and College Academic Staff System (UCASS), for the years 1970 through 2022. This is an annual collection of population-level data on all full-time teaching staff at degree-granting Canadian universities and their affiliated colleges, as of October 1 in each year.[8] There are anonymized individual level identifiers in the data which allow us to track individuals within universities but not across them.[9] Our sample includes all individuals holding appointments at the rank of assistant, associate or full professor, and excludes full-time faculty at a rank below assistant professor because pay determination is less clear in this case. Our analysis sample also omits private, theological, and military institutions.

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[8] See Baker et al. (2023) and the Supplemental Appendix for further details on this dataset.
[9] While this is potentially a limitation, we estimate null impacts of unionization on the inflow or outflow of faculty at the university level.

Our primary measure of compensation is “base salary”. This is the annual rate of pay contractually negotiated between the employee and employer. It excludes other components of actual salary including unpaid leave (including maternity or parental leave) and stipend pay for senior administrative duties. It also excludes income paid out of research grants and other external funding sources. As a robustness check, we consider a measure of compensation corresponding to the actual salary which is available from 1985 onwards and includes these additional pay measures.

Our data on the dates of unionization and the date and terms of the first contract are based on direct contact with the faculty union at a given university. In most instances, we obtained a copy of the first contract which includes information on salary floors. In some cases, missing information was obtained from websites maintained by the faculty unions, as well as university newspapers which reported the dates and terms of the first agreements. For certain institutions, we were able to discover the date of unionization but no other details. Contract lengths typically range from 1 to 3 years with some applying retroactively to the previous salary year. A list of universities, including the union information we collected, is provided in Supplemental Appendix Table S1. Institutions that unionized, but without information on salary floors, are included in our analysis of salary but excluded from our analysis of salary floors. This change in sample has little effect on our estimates as we discuss below.

We also use data on universities’ enrollments and tuition levels for the period 1972-2022. The enrollment data is obtained from Statistics Canada’s University Student Information System for 1972-1994, and Postsecondary Student Information System for 1995-2022. The tuition data is obtained from Statistics Canada’s Tuition and Living Accommodations Costs survey. Tuition can vary by program, and we use the tuition for domestic students in Arts or Humanities as representative for the majority of students. Finally, we use data on the operating funds universities receive from provincial governments which is obtained from the Canadian Association of University Business Officers and Statistics Canada (2024). These data are available for the fiscal years 1979/80 through 2022/23.

4. Empirical Specification

We use the Difference-in-Differences (DID) framework of Callaway and Sant'Anna (2021) to estimate the causal effect of unionization on the distribution of salaries and other outcomes. This framework is designed for a setting with multiple time periods and staggered treatment and avoids the econometric challenges associated with standard two-way fixed effects (TWFE) regressions.[10] The proposed DID estimands identify group-time average treatment effects under the standard parallel trends and no anticipation assumptions. In our baseline specification, we use the "doubly-robust" DID estimator. For the reference period, the pre-treatment coefficients average "short-differences", i.e. comparisons of consecutive periods, and the post-treatment coefficients are "long-differences", i.e. comparisons relative to the period before treatment.[11] The control group is "never-treated" institutions and all cohort-specific treatment effects are aggregated using a group-size weighted average.

We define an individual as treated in a given year if, during that year, the individual works at a university at which a faculty union has been certified. In our primary specification, we include individual and year fixed effects and report standard errors clustered at the institution

[10] For the pitfalls of using TWFE regressions in DID setups, see de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), Sun and Abraham (2021), Athey and Imbens (2022) and Borusyak, Jaravel and Spiess (2023).

[11] Roth (2024) shows that the choice between "short differences" and "long differences" may matter for interpreting visual evidence of a particular violation of the parallel trends assumption. Estimates of the pre-treatment coefficients using "long differences", reported in Supplemental Appendix Figure S1, are similar to the ones from our baseline specification.

level. Since we cannot track individuals who move across institutions in our data, controlling for individual fixed effects absorbs institution fixed effects and province fixed effects and implies that the treatment effects are identified using changes in union status for incumbent workers due to the formation of a union at the institution. Finally, we limit the sample to the relative years $[-4, +6]$ where the coefficient estimate at -4 is normalized to 0 by construction and year 0 corresponds to the year of union certification.

To investigate whether salary floors increased salaries at the bottom of the salary distribution, we use the "bunching" framework of Autor, Manning and Smith (2016) and Cengiz et al. (2019). For this analysis, we restrict our sample of institutions to 1) universities that unionized with first contracts specifying salary floors and 2) universities that never unionized, and limit the sample to the relative years $[-4, +6]$. Our dependent variable is defined as the count of the total number of workers within institution-year-rank-$1,000 wide salary bin cells, ranging from $0 through the maximum salary observed across universities and years. For the institutions with salary floors, we define relative bins based on the distance between bins and the salary floor, where the bin for the salary floor is normalized to 0. For example, the first positive bin consists of individuals earning up to $1,000 more than the salary floor while the first negative bin consists of individuals earning up to $1,000 less than the salary floor. We next define post-treatment indicators for each relative bin which equal 1 in the first 6 years post unionization and 0 otherwise.[12] Finally, we regress the dependent variable on a full set of year-bin and institution-rank-bin fixed effects, and the relative bins interacted with the post-treatment indicator.

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[12] For institutions where salary floors vary within cell (e.g., by experience), the smallest salary floor is used. This will introduce some measurement error into our estimates and may lead to attenuation.

To understand how the treatment effect is identified in this setting, consider a simple setting with two periods 0 and 1 and two groups, a treatment group who introduces a salary floor in period 1 and a control group who does not. Focusing on a single wage bin, if the parallel trends assumption holds for untreated outcomes, the sharp DID estimand identifies the causal effect of the floor on employment in that wage bin. The dynamic event-study estimator that we implement generalizes this estimand in two ways. First, since institutions introduce different salary floors, by recentering the bins around the salary floors and defining relative bin indicators, one can pool these events, even among institutions that unionize within the same year. Second, since institutions introduce the floors at different points in time, one can recenter the events in time using relative time dummies to implement an event study.

5. Results

Panel A of Figure 1 shows the geographic dispersion of the union events in the first and second halves of our sample period, a temporal distinction we make in our analysis. While the certifications are spread out across Canada, universities in Ontario and Quebec are early movers while universities in British Columbia (BC) do not unionize until the 2010s. There are also more union events in the first half of the sample period. By the early 1980s, close to 50 percent of faculty and institutions had been unionized, rising to close to 80 percent by 2022.[13] There are also a limited number of never treated universities (our baseline control group) by the end of the sample period. As noted below, our estimates are robust to including not-yet-treated universities in the control group.

In panel B of Figure 1, we display the coefficients and 95-percent confidence intervals that correspond to separate regressions of observable characteristics on an ever-unionized

[13] See Supplemental Appendix Figure S2.

dummy. This plot reveals how these characteristics differ between faculties and institutions that ever unionize relative to those that never unionize over the sample period. In general, salaries tend to be lower at all ranks at institutions that unionize, and faculty at these institutions are less likely to have achieved the rank of Full Professor. We also see smaller differences in the highest degree attained.[14]

We next present our DID estimates of the impact of unionization on average faculty salaries in panel A of Figure 2.[15] In the pre-unionization period, the estimates are statistically insignificant and tightly centered around 0 demonstrating that faculty who unionized were not experiencing differential salary growth prior to certification relative to faculty who were not unionized. In the year after certification, there is a jump in the average salary of unionized faculty of 2.4 percent, which grows over time reaching 6.1 percent by year 6. This dynamic pattern suggests the estimates should be interpreted causally rather than a result of differential pre-trends.

One interpretation of the growth in the union premium over time is that certain details of the first contract took time to implement. Since the first contracts vary in length between 1-3 years, it is also possible that subsequent contracts achieved larger gains. Nevertheless, these results provide clear visual evidence that the unionization of faculty led to short-term relative salary growth.

In panel B of Figure 2 we report DID estimates by subperiods, 1970-1995 and 1996-2022. The results indicate that the wage effects of unionization are primarily concentrated in the first period. In the first period, the estimates are roughly one percentage point larger than their

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[14] Supplemental Appendix Table S2 presents descriptive statistics for the full sample and separately by union status. Supplemental Appendix Table S3 presents descriptive statistics by subperiod.

[15] The full set of regression estimates for this specification and the others presented in this section are provided in tables in the Supplemental Appendix.

counterparts panel A, while in the second period the estimates are mostly near zero and statistically insignificant.[16]

One explanation for the decline in the union premium over time is selection on gains into treatment: universities with the largest treatment effects were the first to unionize. This is related to the concept of "site selection bias" (Allcott 2015). Another explanation is that the bargaining power of unions has declined over time. A third explanation is that the threat effects of a growing union sector led to higher salaries in the shrinking group of universities that remained non-unionized. Although we cannot definitively distinguish between these three mechanisms, two pieces of evidence support the second one. First, there has been a sustained decline in the union premium in both the Canadian university sector and the broader Canadian labor market at a time when unionization was *declining* (which contrasts with our setting where unionization is increasing over time). Supplemental Appendix Figures S3 and S4 show using conventional OLS that the union premium has declined over time in both the university and public sectors. Second, markers of union militancy declined over the period, perhaps reflecting declining unionism in the broader labor market.[17]

We evaluate the sensitivity of our estimates in several ways. First, we implement the standard two-way fixed effects estimator and the inverse probability weighting (IPW) estimator from Callaway and Sant'Anna (2021) and find that the estimates are similar (see Supplemental Appendix Figure S1). Second, we find that the estimates are robust to including both never-treated and the later-treated institutions in the control group (see Supplemental Appendix Figure

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[16] An exception is the point estimate at +6 years. However, since the standard errors are considerably larger at longer time horizons, we do not interpret this as strong evidence of a true causal effect.

[17] Aggregate unionization fell from near 40 percent to under 30 percent over our sample period (https://www150.statcan.gc.ca/n1/daily-quotidien/170908/cg-a003-png-eng.htm), while the hours not worked due to strikes and lockouts per 1000 employees fell from over 100 in the late 1970s to less than 10 in 2021. (https://www150.statcan.gc.ca/n1/pub/14-28-0001/2020001/article/00017-eng.htm).

S5). Third, we consider a wider event-study window and find a similar pattern of treatment effect estimates although the post-treatment estimates get noisier at longer time horizons (see Supplemental Appendix Figure S6). Fourth, we find that the estimates remain stable when we include time-varying individual controls (see Supplemental Appendix Figure S7). This is potentially important because there are some differences in the characteristics of faculty, across the union and non-union sectors (see Figure 1, panel B). Fifth, we find that using an individual's actual salary as opposed to base salary reveals a slightly larger effect of unionization which provides suggestive evidence that unions are negotiating additionally on non-base pay margins such as stipends, although these differences are not statistically significant (see Supplemental Appendix Figure S8). Finally, while our results provide little evidence of a violation of our assumption of common pre-trends, to further assess this assumption, we construct robust confidence intervals following the method of Rambachan and Roth (2023) (see Supplemental Appendix Figure S9). This method involves constructing confidence intervals for specific estimated treatment effects (in this case period 1) that account for post treatment deviations in common trends as multiples of the maximum deviation in the pre-treatment period. The so-called "breakdown value", which is the multiple at which the estimated confidence interval spans 0, is around 0.4. With reference to panel A in Figure 2, this pattern might be expected since the original confidence interval of the estimate of the treatment effect in period 1 spans an interval very close to 0. We balance this evidence against the strong visual evidence in Figure 2 of post-treatment effects that are distinct from the estimated pre-trends.

We next present our quasi-experimental estimates of the impact of unionization on the distribution of salaries. Panel C of Figure 2 presents DID estimates at different percentiles of the unconditional faculty salary distribution using the re-centered influence function method of Firpo et al. (2009).[18] Similar to panels A and B of Figure 2, the estimated pre-trends are small and statistically insignificant. Post-unionization, the magnitude of estimated treatment effects are monotonically decreasing in the percentile—12.4 percent for the 10th percentile and indistinguishable from zero at the 90th percentile. These results indicate that the distribution of faculty salaries becomes more compressed when a faculty becomes unionized which is consistent with Freeman (1980).[19]

A natural question is whether this wage compression has implications for salary differences by academic rank. The point estimates in panel D of Figure 2 show some compression across ranks, although it takes time to emerge. This suggests that the salary compression occurs both within and across academic rank.[20]

While there are several mechanisms a union could pursue to compress the salary distribution, not all of them have immediate effects. For example, unionized workplaces often negotiate standardized salary ladders as a function of job class and experience which, as noted above, characterize unionized universities in Canada. Absent any allowance for "market adjustments" or merit, ladders might lead to compression across academic disciplines and ranks. Another possibility is to structure cost-of-living adjustments to advantage lower paid faculty.

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[18] This method helps overcome some of the challenges in estimating a regression of a dependent variable quantile while removing covariates not at that quantile. Although Firpo et al. (2009) examine re-centered influence function in a cross-sectional setting, its use in a DID setting was initially proposed by Havnes and Mogstad (2015) and subsequent papers have followed their approach.

[19] In Supplemental Appendix Figure S10, we consider a simpler measure of compression: a 0/1 indicator that a faculty member's salary is below the 25th percentile of the (inflation-adjusted) distribution of salaries for the treatment group in the pre-treatment period. The relative probability of being below this salary percentile declines rapidly post unionization: a decline of 4.9 percentage points in the year after certification and of 11.1 percentage points by year 6.

[20] Goolsbee and Syverson (2019) find that universities have significant labor market power over their tenure track faculty, greatest over full professors and smaller over associate and assistant professors. As Robinson (1933) noted, unions can substantially increase wages in the presence of monopsony. Our results are nominally at odds with this line of reasoning as they tend to suggest the opposite pattern.

While either of these options might undermine fledgling union solidarity, more importantly, it is hard to see how they would have a large impact in a short period of time.

A more promising explanation is the implementation of wage floors, especially if they are set to affect a non-trivial number of faculty salaries. These floors stipulate an overall minimum salary for all faculty, or floors that vary by rank, experience, and/or educational attainment. They are present in 85 percent of the union contracts we observe covering over 89 percent of union observations in our sample.

Figure 3 reports the estimated effects of unionization on employment in $1,000 salary bins defined relative to the salary floors specified in the first contracts using the bunching framework described above. In panel A, we report the estimates for the full sample period and in panels B and C the estimates for the two subperiods.[21] Panel A shows that salary floors push faculty up the salary distribution. First, the estimates below the floor are mostly negative, with larger reductions in bins further from the floor. Second, the estimates for the bins just above the salary floor are mostly positive, with the largest changes at $6,000 above the floor. Third, as expected, the effect fades higher up the salary distribution: the estimates are small and statistically insignificant by roughly $12,000 above the floor. The results in panels B and C are consistent with our earlier findings that the salary effects of unionization are concentrated in the first half of our sample period.

To underline the causal interpretation of these estimates, in Supplemental Appendix Figure S11 we report the results of a placebo exercise in which we randomly assign salary floors at the institution/rank level. The evidence shows that these placebo salary floors have little

[21] Our sample of unionized institutions for this analysis is restricted to those that have a salary floor. We verified that our main estimates reported in Figure 2 are largely unchanged when we estimate the event-study on this restricted sample. In the restricted sample, the event-time estimate for salaries in year 6 (see Figure 2, panel A) is 0.062 compared to 0.061 in the unrestricted sample. The other event-time estimates are similarly unaffected.

impact on the wage distribution, both in nearby relative salary bins and across the entire salary distribution. We also compare estimates when we redefine the post-treatment indicator to be one year post unionization (as opposed to six years post unionization) in Figure S12. As expected, we observe greater bunching of the positive changes in employment just above the salary floor under this alternative definition, while the negative employment changes below the floor are largely unchanged.

We have also investigated other dimensions of the compressing effects of unionization by markers of high- and low-paid faculty. On one hand, we find evidence that the salary gains of unionization are larger for low-paying academic departments, consistent with unionization reducing interdepartmental salary differences (see panel A of Supplemental Appendix Figure S13).[22] On the other hand, unionization has little effect on salary differences across Science, Technology, Engineering and Mathematics (STEM) and non-STEM fields (see panel B of Figure S13). Overall, our evidence indicates an increase in salaries at the bottom of the distribution with little change at the top.

Since an increase in salaries presumably moves universities up their labor demand schedule, some overall negative impact on employment might be expected. However, given the academic institution of tenure, the possibility of such an adjustment in the short term might be limited. Additionally, the effect on employment may be muted if universities have monopsony power as in Goolsbee and Syverson (2019).

In the lower right-hand corner of each panel of Figure 3, we report an estimate of the change in overall employment resulting from the bin-level changes. The estimates suggest that unionization has little effect on employment: for example, for the full sample period, we estimate

[22] Departments are assigned to be high paying or low paying based on whether their pay was below or above the median for all departments at event-time −4, respectively.

a statistically insignificant loss of just over 6 faculty, which corresponds roughly to a 3 percent change. In Figure 4, we provide a more direct analysis of the impact of unionization on the stock and flows of faculty. DID estimates of the impact of unionization on the stock of faculty employment are presented in panel A, broken down by sample period.[23] In panels B through D, we extend the analysis to union impacts on the number of new hires, promotions to higher ranks and separations, respectively. These results make the strong case that unionization did not affect faculty employment, both in the subperiod in which unionization raised salaries and in the subperiod in which it did not.[24]

Our finding of a null impact of unionization on employment begs the question of how universities pay for the wage increase that we document in the first half of our sample. In Figure 5, we investigate the impact of unionization on some possible mechanisms at the institution level: student enrollment, student tuition and provincial government transfers to universities.[25] The estimates in panel A reveal a statistically significant increase in enrollment of 13.6 percent 6 years post unionization in the first half of our sample period (when the salary gains are concentrated). In contrast, there is little evidence of any adjustment of tuition or government transfers for this period. This is perhaps not surprising as tuition fees for domestic students are typically regulated by provincial governments and transfers are standardized at the provincial level. It is unlikely a province would increase transfers solely for the universities that unionize. By contrast, enrollment is a lever that is relatively straightforward for each institution to adjust.

[23] For this specification, we collapse the micro data to institution-year cells and replace individual fixed effects with institution fixed effects.
[24] It is possible that unions affect workforce composition along other margins. Supplemental Appendix Figure S14 shows that unions have no impact on the observable composition of faculty according to age, sex, citizenship, and experience.
[25] To get a sense of the incidence of unionization born by universities, we aggregated our data to the institution level and examined the effect on the average salary (see Supplemental Appendix Figure S15). The estimates indicate that unionization increases the average faculty wage by 2.1 percent in year 1 and by 4.7 percent by year 6.

The implication is the salary gains for faculty were achieved at a cost of increases in class size and/or greater workload per faculty.

Of course, we cannot rule out that some of the wage increase is paid for through channels that are unobserved. For example, universities can cut back on their use of part-time faculty or staff. Additionally, fewer resources may be spent on capital expenditures, such as maintaining infrastructure.

6. Conclusion

We use a quasi-experimental DID framework to estimate the impact of unionization on the salaries of faculty at Canadian universities. On average, we uncover an initial positive impact of over 2 percent, which grows to 6 percent after 6 years. This impact is primarily for universities that unionized in the first half of our sample period, which suggests either selection into unionization on gains, or a secular change in the bargaining environment.

We also find that unionization leads to the compression of salaries with the effects concentrated at the bottom of the salary distribution. Salary floors, present in many of the first union contracts we study, are a natural mechanism driving the salary compression in the first years after unionization. We document how these floors push faculty up the salary distribution in the initial period post certification.

Finally, our evidence indicates that the wage gains due to unionization are primarily paid for through increased student enrollment. We find no evidence of a reduction in employment or an increase in tuition or government transfers. This evidence suggests that faculty may bear part of the cost of unionization if teaching demands increase and/or class sizes become larger.

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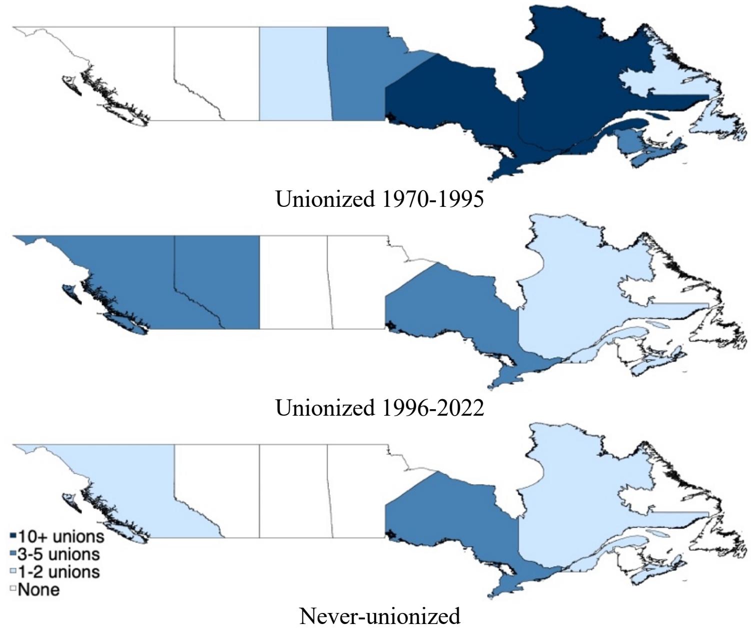
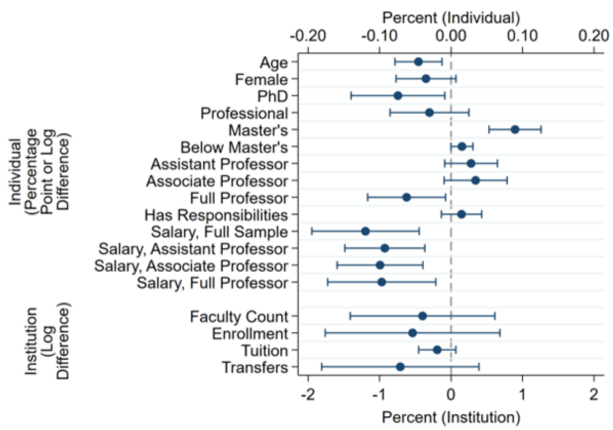
Figure 1: The Geographic and Temporal Dispersion of Union Events and Analysis Sample Balance



(A) Timing of Unionizations by Province

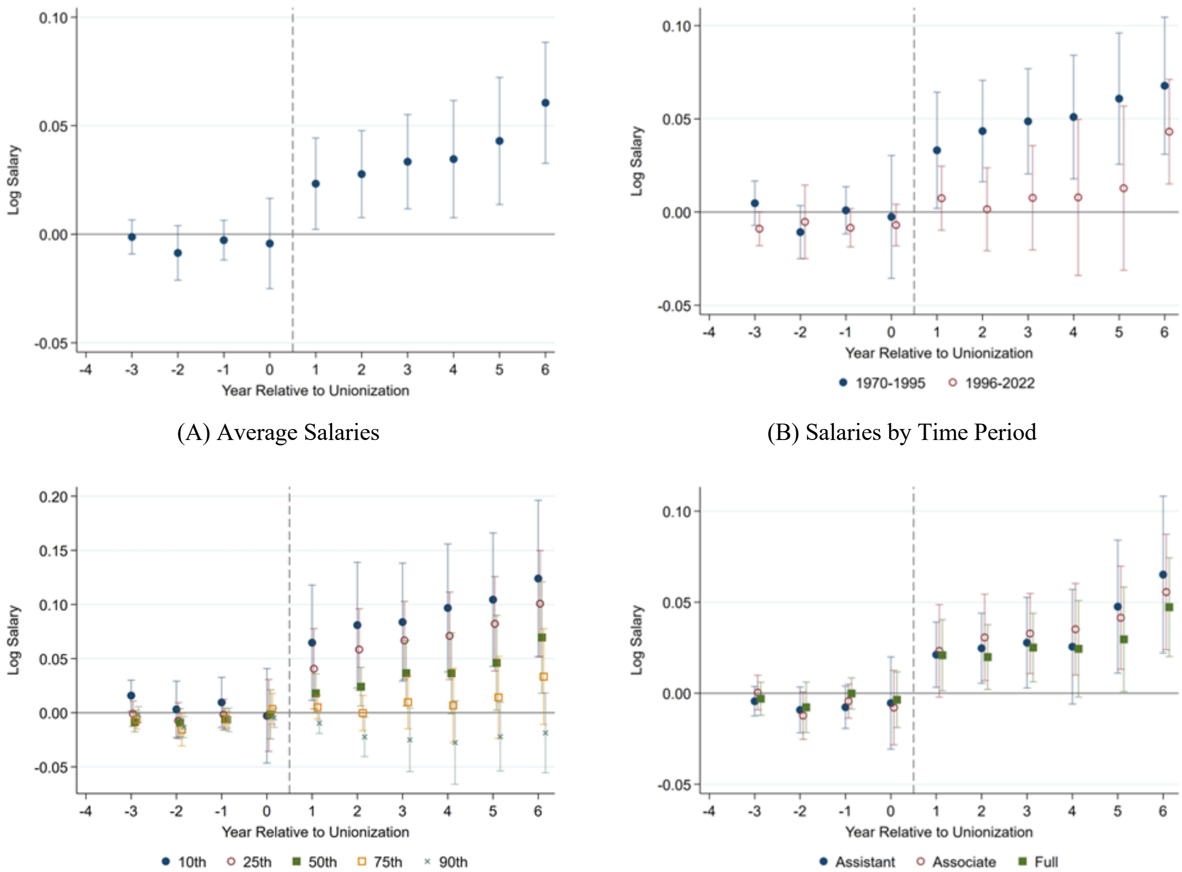


(B) Observable Characteristics of Workers and Institutions by Union Status

Notes: Panel A shows the geographic distributions of union formation events for the two main time periods in our analysis (first two maps) and never-unionized institutions (third map). The sample of universities included is equivalent to that used in the baseline event-study specification and is detailed in Supplemental Appendix Table S1. Panel B plots differences in characteristics between workers and institutions by unionization status. Point estimates (blue dots) and standard errors (horizontal bars) are based on separate regressions of the dependent variable on a dummy variable for ever being unionized. "Individual" results are obtained from the individual-level data and "institution" results are obtained from institution-level data. The sample is equivalent to that used in the baseline event-study specification. Age, salary, faculty count, enrollment, tuition and transfers are expressed in logs meaning regression estimates correspond approximately to percent differences between union and non-union groups. The remaining variables are indicators meaning regressions estimates are the percentage point differences between union and non-union groups. The categories for PhD, Professional, Master's or Below Master's indicate highest degree attained. The categories of Assistant, Associate and Full Professor indicate a worker's academic rank. The category "Has Responsibilities" indicates whether the individual has administrative responsibilities. The categories for administrative responsibilities are: none; Chairs/Heads/Directors; Associate/Vice Deans; and Deans. The categories "Salary, (rank) Professor" measure the difference in salary between union and non-union groups within the specified rank. The salary measure used is a base annual rate. Currency values are expressed in 2022 constant dollars. Salaries are winsorized at the 0.5th and 99.5th percentiles.

Sources: Statistics Canada, University and College Academic Staff System, 1970 to 2022; Statistics Canada, University Student Information System, 1972 to 1994, and Postsecondary Information System 1995 to 2022 (enrollment statistics); Statistics Canada, Tuition and Living Accommodation Costs, 1972 to 2022 (tuition statistics); Statistics Canada and Canadian Association of University Business Officers, Financial Information of Universities and Colleges, 1979 to 2022 (government transfers statistics); and self-collected union data.

Figure 2: The Effects of Unionization on Salaries



(A) Average Salaries

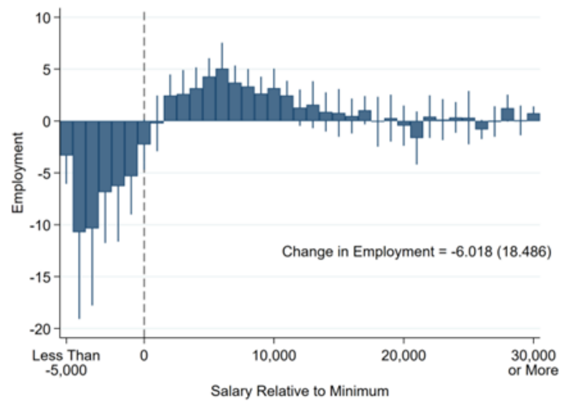
(B) Salaries by Time Period

(C) Percentiles of Salaries

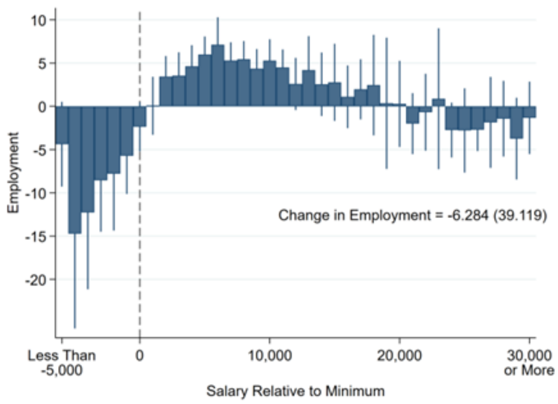
(D) Salaries by Academic Rank

Notes: The dependent variable uses base annual salary, which excludes additional pay such as stipends and reduced pay due to leave. Specifically, the dependent variable in panels A, B and D is the log of base salary. The dependent variable in panel C is the re-centered influence function (RIF) of earnings evaluated at each percentile of the salary distribution. The estimates are based on the "doubly robust" estimator from Callaway and Sant'Anna (2020). The pre-treatment coefficients average "short-differences," i.e., comparisons of consecutive periods, whereas the post-treatment coefficients average "long-differences," i.e., comparisons relative to the omitted reference period. The control group consists of never-treated institutions and all cohort-specific treatment effects are aggregated using a simple average. Individual and year fixed effects are included in all specifications. Standard errors are clustered by institution and the 95% confidence intervals are shown as vertical bars.
Source: Statistics Canada, University and College Academic Staff System, 1970 to 2022; and self-collected union data.

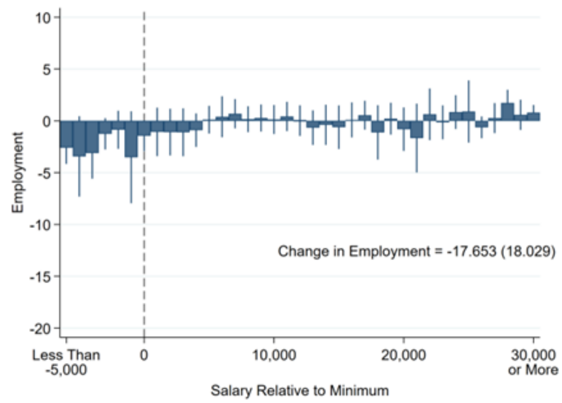
Figure 3: The Effect of Salary Floors at Unionization on Employment by Relative Salary and Time Period



(A) 1970-2022



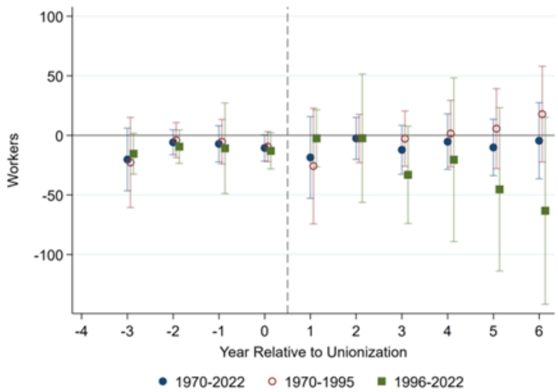
(B) 1970-1995



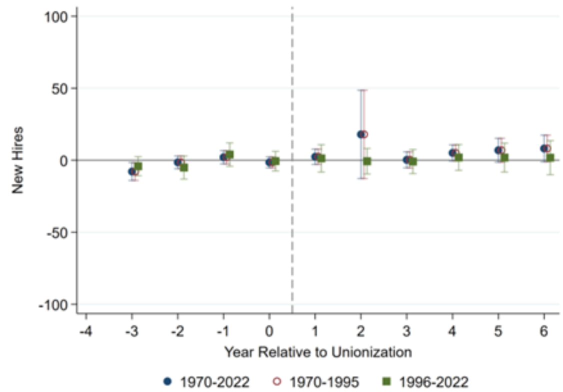
(C) 1996-2022

Notes: The estimates reported are based on the “bunching” estimator from Cengiz et al. (2019). Restricted to institutions that ever unionized and have salary floor information in the relevant time period as stated in the legend or that never unionized and to the years used in the event-study analysis, i.e., from event-time −4 to +6 for the treatment group and all years for institutions that never unionize. The model is estimated on data collapsed to institution-year-rank-salary bin cells. Salary bin widths of $1,000 are used, beginning at $0 and increasing to the maximum salary. The dependent variable is the total number of individuals within each cell. The dependent variable is regressed on a set of relative-bin indicators interacted with a post-treatment indicator that averages over the post-treatment years 0 to +6, while controlling for absolute bin-rank-institution and absolute bin-year fixed effects. Each relative-bin indicator takes the value of “1” if the salary in that bin is $$x$ distance from the salary floor that took effect in the year of unionization, and “0” otherwise, where x varies along the horizontal axis (also in bins of width $1,000). For institutions whose salary floors vary within cell (e.g., by experience), the smallest salary floor is used. The coefficients on the relative-bin indicators interacted with a post-treatment indicator are shown in the figure. Each bar is the effect of unionization on the change in the number of workers earning $$x$ from the salary floor. Standard errors are clustered by institution and the 95% confidence intervals are shown as vertical bars. The change in employment reported in each panel is the sum of all bars, with standard error in parentheses.
Source: Statistics Canada, University and College Academic Staff System, 1970 to 2022; and self-collected union data.

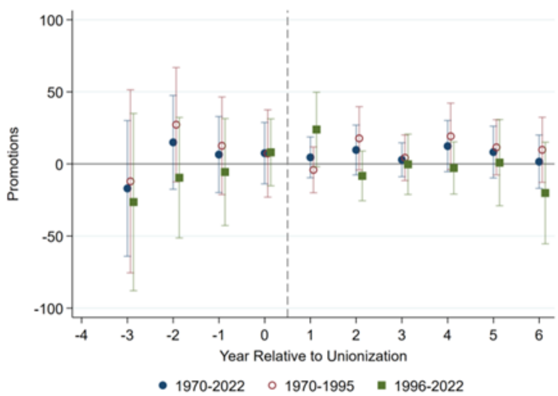
Figure 4: The Effects of Unionization on Employment



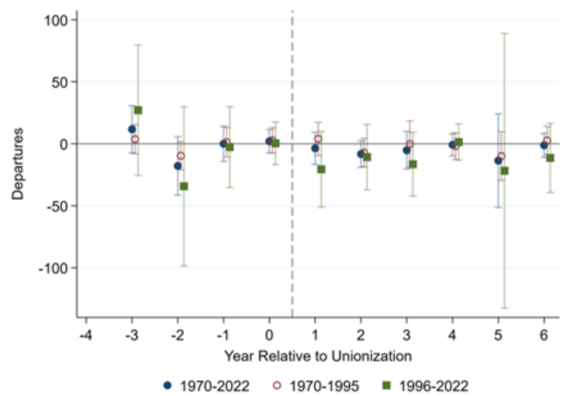
(A) Number of Workers



(B) Number of New Hires



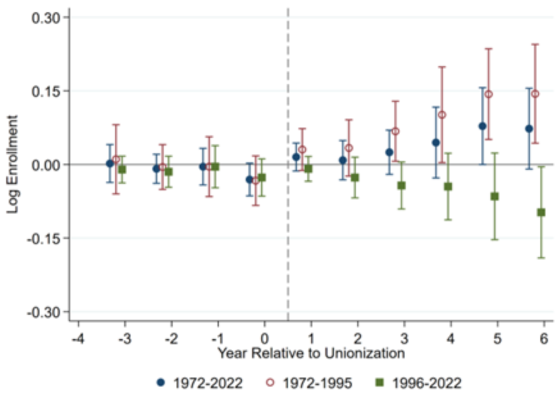
(C) Number of Promotions



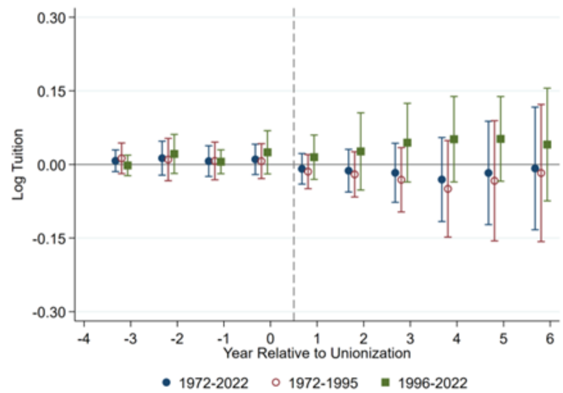
(D) Number of Early Departures

Notes: The dependent variable in panel A is the number of workers in a given instituition and year. The dependent variable in panel B is the number of new hires in a given institution and year. The dependent variable in panel C is the number of promotions to a higher rank, i.e., assistant to associate, or associate to full, in a given institution and year. The dependent variable in panel D is the number of early departures in a given institution and year. An early departure is defined as a worker who exits the sample before the age of 65. The estimates are based on the "doubly robust" estimator from Callaway and Sant'Anna (2020). The pre-treatment coefficients average "short-differences," i.e., comparisons of consecutive periods, whereas the post-treatment coefficients average "long-differences," i.e., comparisons relative to the omitted reference period. The control group consists of never-treated institutions and all cohort-specific treatment effects are aggregated using a simple average. The model is estimated on data collapsed to institution-year cells. Institution and year fixed effects are included in all specifications. Standard errors are clustered by institution and the 95% confidence intervals are shown as vertical bars.
Source: Statistics Canada, University and College Academic Staff System, 1970 to 2022; and self-collected union data.

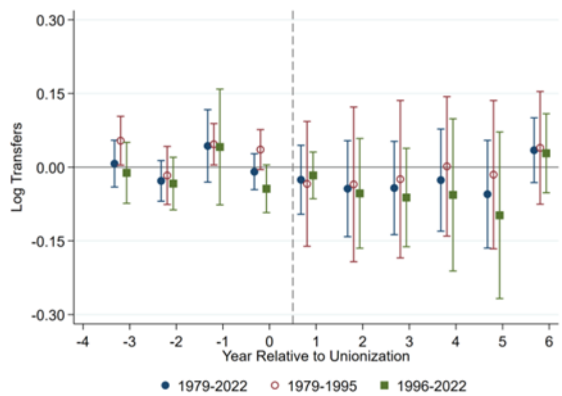
Figure 5: The Effects of Unionization on Enrollment, Tuition and Government Transfers



(A) Enrollment



(B) Tuition



(C) Government Transfers

Notes: The dependent variable in panel A is the log of total enrollment by institution and year. This includes full-time and part-time students who are in both undergraduate and graduate programs. It excludes students who are enrolled in courses but not seeking an academic degree, diploma or certificate. The dependent variable in panel B is the log of tuition by institution and year. The measure of tuition is the price paid for a Bachelor's degree in the Arts or Humanities by resident students, i.e., domestic or non-international. The dependent variable in panel C is the log of transfers from the provincial government to the university in the year. The estimates are based on the "doubly robust" estimator from Callaway and Sant'Anna (2020). The pre-treatment coefficients average "short-differences," i.e., comparisons of consecutive periods, whereas the post-treatment coefficients average "long-differences," i.e., comparisons relative to the omitted reference period. The control group consists of never-treated institutions and all cohort-specific treatment effects are aggregated using a simple average. The model is estimated on data collapsed to institution-year cells. Institution and year fixed effects are included in all specifications. Standard errors are clustered at the institution level and the 95% confidence intervals are shown as vertical bars.
Sources: Statistics Canada, University Student Information System, 1972 to 1994, and Postsecondary Information System 1995 to 2022 (Panel A); Statistics Canada, Tuition and Living Accommodation Costs, 1972 to 2022 (Panel B); Statistics Canada and Canadian Association of University Business Officers, Financial Information of Universities and Colleges, 1979 to 2022 (Panel C); and self-collected union data.