The Impact of Unions on Wages in the Public Sector: Evidence from Higher Education[1]

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Abstract

This paper studies the effects of unionization on the salaries of Canadian university faculty between 1970-2022. The evidence indicates that unionization increases salaries on average by 2 percent in the first year post-unionization and by 6 percent after 6 years. These gains are driven largely by wage increases in the bottom half of the salary distribution, and stem from the introduction of salary floors. The results further reveal that these effects are primarily concentrated between 1970 and 1995 and they are financed by an increase in student enrollment. We do not find any impacts on employment, tuition or government transfers.

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1. Introduction

Understanding the effects of unions on the distribution of income has long been a central goal of economists. In a seminal contribution, Freeman (1980) challenged the prevailing view at the time, showing empirically that unions reduce income inequality. The subsequent publication of "*What Do Unions Do?"* (WDUD, Freeman and Medoff 1984) triggered a substantial body of research on how unions affect workers' and firms' outcomes.

While historically most union jobs were in the private sector, coverage in the public sector has risen. Card, Lemieux, and Riddell (2020) report that by 2015 coverage rates were roughly 5 times higher in the public sector in both Canada and the United States, and the public sector accounted for half of all unionized workers.[2]

There is good reason to expect that the effects of unions on wages in the public sector are different than in the private sector. For example, while unions can do little to increase the demand for a firm's product in the private sector, in the public sector they may be able to extract greater resources for education, policing and other types of services. Hence, unions may face more resistance in the private sector than in the public sector.

Despite the growing relative importance of unions in the public sector, little is known about their causal impact on the wage structure (Card, Lemieux, and Riddell 2020). Freeman (2005) writes 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". In fact, Freeman laments that the omission of the public sector is one of three serious omissions in WDUD.

[2] 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 employment.

In this paper, we take a step towards addressing this deficit by estimating the causal effect of unions on the salary distribution of full-time faculty at Canadian universities. Our focus on higher education is motivated by several considerations. First, most Canadian universities are public. In recent years, they represent a combined $40 billion enterprise, employing over 400,000 workers.[3] Over much of our period of study, 7 to 11 percent of total public sector employment in Canada was in the tertiary education sector.[4] Therefore, higher education represents a sizeable share of the public sector. Second, there are administrative panel salary data for the population of faculty in Canadian universities for the years 1970 through 2022. We combine these data with newly collected records of unionization events, and features of first contracts such as the presence of "salary floors". This original data collection establishes the date of union certification, mitigating concerns over the mismeasurement of union status which has plagued earlier studies of the public sector (see Lewis 1990), and allows us to investigate the impact of unions in their first years. Third, a key feature of these data is that they begin in a period with no faculty unions and end with over 80 percent of faculty covered by union contracts. Thus, these data allow us to empirically examine the unionization of an entire sector of the economy over a 50-year period which is useful for understanding the effectiveness of unions over time.

We use difference-in-differences (DID) to estimate the causal effect of unionization on the distribution of salaries. We also leverage information on salary floors contained in the first union contracts to directly examine whether they contribute to compression of the salary

[3] 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.

[4] See CANSIM table 10100025, for the years 1981 through 2012. https://open.canada.ca/data/en/dataset/b38895a5-eef9-43ad-bd3f-aa2525de8d24.

distribution. Finally, we examine the effects of unionization on employment, student enrollment, tuition and government transfers to understand how any salary increases are financed.

Our empirical analysis leads to three key findings. First, unionization increases salaries, on average. In the first year post-unionization, the increase in average salary is around 2 percent, rising to 6 percent, 6 years after certification. These salary effects are primarily for union certifications in the first half 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.

Second, unionization compresses faculty salaries. In the unconditional salary distribution, gains are concentrated at the lower percentiles. Six years post-certification, the gains range from over 10 percent at the 10^{th} percentile to close to 0 at the 75^{th} and 90^{th} percentiles. Consistent with this evidence, the effect on salaries is concentrated locally around salary floors with little overall impact at the top of the distribution. Interestingly, the heterogeneity in salary gains is not as pronounced by academic rank, indicating the compression occurs both within and across ranks.

Third, in the subperiod where we observe salary gains, unionization led to a significant increase in student enrollment. In contrast, we do not find any impacts of unionization on faculty employment, tuition, or government transfers, both overall and by subperiod. This suggests that wage increases were financed out of increased university revenues.

Our paper contributes to a large literature on the effects of unions on the wage structure, most of which focuses on the private sector. Key studies include Freeman (1984), Card (1996), DiNardo, Fortin and Lemieux (1996), Lemieux (1998), Card (2001), DiNardo and Lee (2004), Sojourner et al (2015), Frandsen (2021), Fortin, Lemieux and Floyd (2021), Farber et al (2021)

and Dodini et al. (2023). These studies generally find a positive impact of unions on wages.[5] 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.

While studies of the union wage effect in the wider public sector have a long history (e.g., Ashenfelter 1971, Robinson and Tomes 1984), they are much fewer in number. A useful summary is Lewis (1990) who concludes that the public-sector union wage gap is between 8-12 percent. In their reflections on the impact of WDUD, Blanchflower and Bryson (2004) present evidence of public sector union wage effects in the late 1990s comparable to those in the private sector (15-17%). Finally, Card, Lemieux and Riddell (2020) report that the impact of unions on wage inequality is much larger in the public sector than in the private in both the U.S. and Canada.

Our paper also relates to a smaller literature which considers the impact of unionization on Canadian faculty compensation. Key papers include Rees, Kumar and Fisher (1995), Hosios and Siow (2004) and Martinello (2009).[6] These studies find small to no impact of unionization on salaries. More generally, studies of the impact of unionization on faculty salaries have yielded mixed results. Hedrick et al. (2011) conclude previous studies for the U.S. have produced positive, zero and negative estimates of the union salary difference.

[5] Farber et al (2021) report a positive family income union premium of between 10 and 20 percent over a 9-decade period.

[6] Rees, Kumar and Fisher (1995) and Hosios and Siow (2004) are noteworthy as they are based on somewhat similar data. There are some key differences, however. First, these studies use aggregate wage and employment data for each institution. The sample in Rees, Kumar and Fisher, which spans 1972-1991, is at the institution, rank level, covering 56 institutions (although many institutions are missing earnings data for some years). Hosios and Siow's sample, for the period 1973-1990, is at the institution, field of study, rank level for 45 universities. Our analysis is based on micro data which allows us to include individual fixed effects in our specifications to control for composition bias. Second, since we have micro data, our setting is better suited to estimating the effects of unions on the salary distribution. Third, our data covers 60 universities (after sample restrictions and availability of the union contracts) and the years 1970-2022, allowing us to investigate more unionization events and heterogeneity in the effects over time. Fourth, these studies use a static two-way fixed effects model whereas we focus on estimating the dynamic effects of unionization. Finally, we collected details of the first contract post-unionization which allows us to additionally understand the role of salary floors in increasing salaries.

A feature of many of these studies is that they are based on cross-sectional comparisons between union and non-union workers (controlling for observable differences between union and non-union members). Thus, the estimates may be confounded by selection on unobservables. Other studies employ parametric corrections to address the selection problem. Few studies (e.g., Hoxby 1996, Lovenheim 2009) estimate the wage gap using a quasi-experimental design. Against this background, we use DID in a unique setting which spans the entire period over which all unionization effects occur in an entire sector. We also note most previous estimates of the union wage effects are from samples dominated by "mature" unionized workplaces.[7] In contrast, our estimates are for newly unionized workers.

The rest of the paper is organized as follows. Section 2 describes the institutional context and rise of faculty unions in Canada. Section 3 discusses the data. Section 4 discusses our empirical specification and results. Section 5 concludes.

2. Faculty Unions in Canada

The union movement at Canadian universities began in the decade preceding the start of our sample period in 1970 where no faculty was unionized. The literature suggests that governance, rather than economic concerns, were initially at the forefront of this movement (e.g., Savage 1994).[8] The Canadian Association of University Teachers (CAUT) was one catalyst of the focus on governance, as was the publication *A Place of Liberty: Essays on the Government of Canadian Universities* (Whalley 1964) in 1964 (Horn 1994). Commentators on the state of universities at the time use terms like "autocratic", "oligarchic", and "paternalistic" to describe the rule of university presidents and boards of governors (Heron 2015). However, faculty were

[7] Analogous to the sampling of unemployed workers at a point in time, firms in long spells of unionization will be more likely to be captured.

[8] See Table S1 in the Supplemental Appendix for the complete list of institutions included in the analysis, dates of union formation and details about salary floors included in contracts.

split over unionization as the solution to governance issues. There was debate whether unions, which codified the employer/employee relationship, might rule out the possibility of collegial management (Horn 1994, Heron 2015).[9]

Economic considerations to some extent also played a role. Government funding of post-secondary typically followed enrolment and the budget balance, and consequently so did faculty grievance over compensation. The 1960s were a period of rising enrolments, university expansion and relative plenty. However, this trend reversed in the 1970s with dips in enrolment, the withdrawal of direct federal government funding, and macroeconomic stagnation which adversely impacted provincial budgets. The Ontario Minister of Colleges and Universities' stating that the province sought "more scholar for the dollar" in 1971 (Axelrod 1982) encapsulates the economic threat that faculty faced in this period. As a result, faculty may have come to accept the economic argument to unionize while still uncertain if it addressed questions related to governance. While these "structural" factors operated broadly at the national and provincial levels, we argue that within this context, idiosyncratic factors led some institutions to unionize before others. It is this variation in the *timing of unionization* that we exploit for our DID analysis. We investigate the validity of this assumption by testing for pre-trends.

The rules for certifying unions are set by provinces. It 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

[9] See also Mackinnon (2015).

institutions or sectors of the economy.[10] At most universities, they represent "academic staff", which almost always includes faculty and librarians, but in some cases also sessional instructors, archivists, counsellors and professional administrative officers.

It is worth noting that many faculty unions in Canada grew out of faculty associations which were founded long 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 a 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 of 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.[11] Our sample includes all individuals holding appointments at the rank of assistant, associate or full professor, and excludes full-time

[10]All are also affiliated with the 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).

[11] See Baker et al. (2023) and the Supplemental Appendix for further details on this dataset.

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.

Our primary measure of compensation is "base salary". This is the annual (12 month) rate of pay contractually negotiated between the employee and employer. It excludes other components/factors 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 also consider a measure of compensation corresponding to the actual salary which is available from 1985 onwards.

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 is the source of information on the salary floors we examine. 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 Table S1 of the Supplemental Appendix. Institutions that unionized but without information on salary floors are included in our analyses of salary, but excluded for our analysis of salary floors. This change in sample has little effect on our estimates.[12]

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

[12] For example, in the smaller sample the event-time estimate for salaries in year 6 (Figure 2, panel A) is 0.062 compared to 0.061 in the larger sample. The other event-time estimates are similarly unaffected.

(USIS) for 1972-1994, and Postsecondary Student Information System (PSIS) for 1995-2022. The tuition data are obtained from Statistics Canada's Tuition and Living Accommodations Costs (TLAC) survey. Tuition can vary by program, and we use the tuition for domestic students in Arts or Humanities as representative for the greater majority of students. Finally, we use data on the operating funds universities receive from provincial governments (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 and Results

We use the DID framework in Callaway and Sant'Anna (2021) (CS) to estimate the causal effect of unionization on 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.[13] CS show that their DID estimators identify group-time average treatment effects under the standard parallel trends and no anticipation assumptions. In our baseline specification, we use the "doubly-robust" (DR) 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.[14] The control group is

[13] For the pitalls 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).

[14] 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.

"never-treated" institutions and all cohort-specific treatment effects are aggregated using a simple average.[15]

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 level. 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. We also explore robustness to including time-varying controls for rank, administrative responsibilities, years of experience (cubic), and dummy variables for sabbaticals or unpaid leave. 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.[16]

We also investigate whether salary floors increase wages at the bottom of the salary distribution using the framework of Autor et al. (2006) and Cengiz et al. (2019). Starting with the sample of universities with first contracts specifying salary floors and universities that never unionized, we create counts of the total number of workers within institution-year-rank-$1000 wide salary bin cells, ranging from $0 through the maximum salary observed across universities and years. This dependent variable is regressed on a set of relative-bin indicators, their interactions with a post-treatment indicator, as well as year-bin and institution-bin fixed effects.

[15] We explore robustness of results to including the eventually-treated (i.e., not-yet-treated) group (Supplemental Appendix Figure S2) and other specifications, namely ordinary least squares and aggregating group-specific treatment effects using cohort weights (Figure S1).

[16] In Supplemental Appendix Figure S3, we consider a wider event-study window and a find similar pattern of treatment effect estimates although the post-treatment estimates get noisier at longer time horizons.

[17] Each relative-bin indicator takes the value of "1" if the salary in that bin is within $\$x$ of the salary floor that took effect in the year of unionization, and "0" otherwise, where x varies in $1,000 increments.[18] We report the average of the relative-bin indicator/ post-treatment indicator interactions over the first six years post-unionization. This indicates the effect of unionization on employment in that bin over this period.

Panel A of Table 1 shows the number and timing of certifications of faculty unions over the sample period. 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. Panel A of Figure 1 shows that the percentages of faculty and institutions unionized move in tandem, reaching around 60 percent by the end of the first half of our sample period in 1995 and close to 80 percent by the end of our sample period in 2022.

Panel B of Table 1 contains the descriptive statistics for faculty at institutions that never unionized during our sample period (columns 3-4), our control group, and institutions that ever unionized (columns 5-6). Faculty at unionized workplaces are slightly younger, more likely to be male, less likely to hold a PhD, hold a lower rank position, and have lower salaries.

To provide additional context for our results, we first present naïve estimates of the union salary premium in our sample, by year, using a standard OLS regression, in panel B of Figure 1. In the early 1970s, the premium hovered just above of 5 percent. Starting in the late 1980s, there was a secular downward trend and by the late 1990s, the premium disappeared and even became negative in some later years. While these estimates rely on the strong selection-on-observables

[17] Inclusion of year-bin and institution-bin FEs requires treatment effects to be expressed relative to at least one pre-treatment relative-bin indicator. The highest relative-bin indicator is used, as it is the furthest from the salary floor where direct effects of the floors should be negligible.
[18] For institutions where salary floors vary within cell (e.g., by experience), the smallest salary floor is used.

assumption, they are consistent with the causal impact of unions on salaries becoming weaker over time.

Our DID estimates of the impact of unionization on faculty salaries using the CS DR estimator are presented in panel A of Figure 2.[19] 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 in the first period. In the first period the estimates are roughly one percentage point larger than their counterparts panel A, while in the second period the estimates are mostly near zero and statistically insignificant.[20] Interestingly, this evidence which is based on an entirely different research design corroborates the evidence based on conventional OLS estimates in Figure 1.

[19] The full set of regression estimates for this specification and the others presented in this section are provided in tables in the Supplemental Appendix.
[20] 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.

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. Although we cannot definitively distinguish between these two mechanisms, two pieces of evidence support the latter one. First, there has been a sustained decline in the union premium in 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 Figure S4 shows that the union premium has declined over time in the public sector. Second, markers of union militancy declined over the period, perhaps reflecting declining unionism in the broader labor market .[21]

We have evaluated the robustness of our estimates in several ways. First, we find they are not sensitive to including time-varying individual controls (see Supplemental Appendix Figure S5). This is potentially important because there are some differences in the characteristics of faculty, across the union and non-union sectors (Table 1, panel B). Second, we consider a different measure of pay, an individual's actual salary (see Supplemental Figure S6). Interestingly, the estimates reveal larger effects of unionization: 4.9 percent salary increase in year 1 which grows to 11.6 percent in year 6, indicating that unions are negotiating additionally on non-base pay margins, such as stipends. Finally, while our evidence provides 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 S7). The so called "breakdown value" is around 0.4. With

[21] 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).

reference to panel A in Figure 2, this pattern might be expected since the original confidence interval of the estimate of the treatment effect estimate 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 examine the impact of unionization on wage inequality. Panel C of Figure 2 presents DID estimates at different percentiles of the unconditional faculty salary distribution using the re-centered influence function (RIF) method of Firpo et al. (2009).[22] 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).[23] One reason why unions may not increase salaries at the top is due to outside options. High-paying faculty at non-unionized workplaces may be more successful at securing wage increases through outside offers. This mechanism for salary growth may not be possible when salaries are determined by unions.

A natural question is whether this wage compression has implications for salary differences by academic rank. The point estimates in panel A 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.[24]

[22] Although Firpo et al. (2009) examine RIF 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.
[23] In Supplemental Apendix Figure S8, 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.
[24] 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,

While there are a number of 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, and as noted above they characterize unionized universities in Canada. Absent any allowance for "market adjustments" or merit, these ladders might lead to compression across academic disciplines and ranks. Another possibility is to structure COLAs to advantage lower paid faculty. 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 and/or experience. They are present in 85 percent of the union contracts we observe covering over 89 percent of union observations in our sample.

Our estimates of the effects of these floors are reported in Figure 3. Each bar in the figure reveals the average employment change in the indicated bin over the 6 years following certification, relative to universities that never unionized. In panel A, we report the estimates for the full sample period and in panels B and C the estimates for the two sub-periods. 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 $6000 above the floor. Third, as might be expected, the effect fades higher up the salary distribution: the estimates are small and statistically insignificant by roughly $12,000 above the

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.

floor. Finally, the estimates indicate a statistically insignificant impact on overall employment of just over 11 faculty. The results in panels B and C are consistent with our earlier findings that the salary effects of unionization are concentrated primarily in the first half of our sample period.

In the Supplemental Appendix, we further characterize the compressing effects of unionization by estimating the salary effects by other markers of high and low paid faculty. We find that the gains are concentrated in low-paying academic departments suggesting that unionization reduces interdepartmental salary differences (see Figure S9, Panel A).[25] In contrast, unionization has little effect on salary differences across STEM (Science, Technology, Engineering and Mathematics) and non-STEM fields (see Figure S9, Panel B).

Overall, our evidence indicates an increase in salaries at the bottom of the distribution with little change at the top. To get a sense of the incidence of unionization born by universities, we have aggregated our data to the institution level and examined the effect on the average salary (see Supplemental Appendix Figure S10). 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.

How do universities pay for this wage increase? Since the rise 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).

The evidence in Figure 3 suggests an economically small and statistically insignificant effect of unions on overall employment. This finding is underlined in Figure 4, panel A which

[25] 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.

presents the corresponding DID estimates of unionization on total faculty employment, broken down by sample period.[26] In Supplemental Appendix Figure S11, we also examine union impacts on the number of new hires, promotions to higher ranks and separations. For all these outcomes, we do not find any statistically significant effects of unionization.[27]

If the increase in salaries is a not offset by a reduction in employment, what other margins can universities adjust? Figure 4 presents evidence for enrollment (panel B), tuition (panel C), and government transfers (panel D) at the institution level. The estimates in panel B 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. This is perhaps not surprising as tuition fees for domestic students are typically regulated by provincial governments either in level or in the rate of increase. Similarly, transfers are standardized at the provincial level, and it is unlikely the province would increase them solely for the universities that unionize. By contrast, enrollment is a lever that is relatively straightforward for each institution to adjust and does not necessarily require provincial government involvement.

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.

5. Conclusion

[26] For this specification, we collapse the micro data to institution-year cells and replace individual fixed effects with institution fixed effects.

[27] It is possible that unions affect workforce composition along margins. Supplemental Appendix Figure S12 shows that unions have no impact on the observable composition of faculty according to age, sex, citizenship, and experience.

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

We also find that unionization leads to compression of salaries with the effects concentrated at the bottom of the salary distribution. This effect is evident in the percentiles of the unconditional distribution of faculty salaries. 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 greater enrollment if teaching demands increase and/or class sizes become larger. Given the effects we document in our paper, interesting directions for future research are to consider the effects of unions on faculty productivity, working conditions, and student performance.

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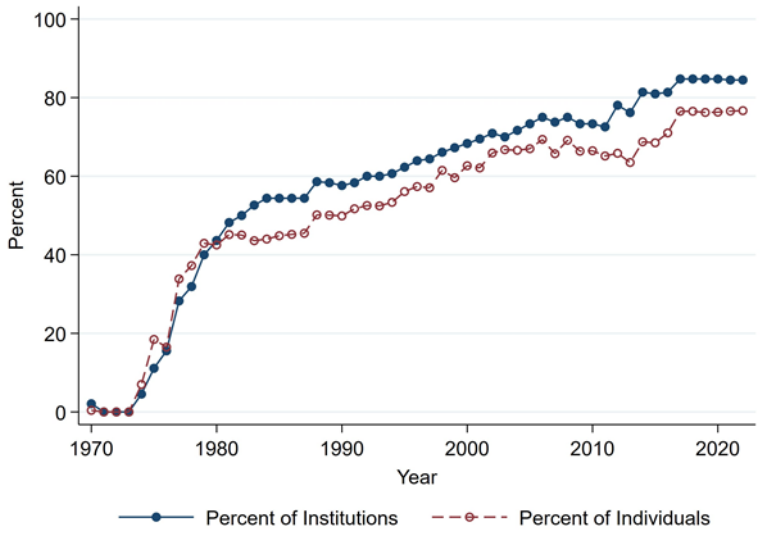
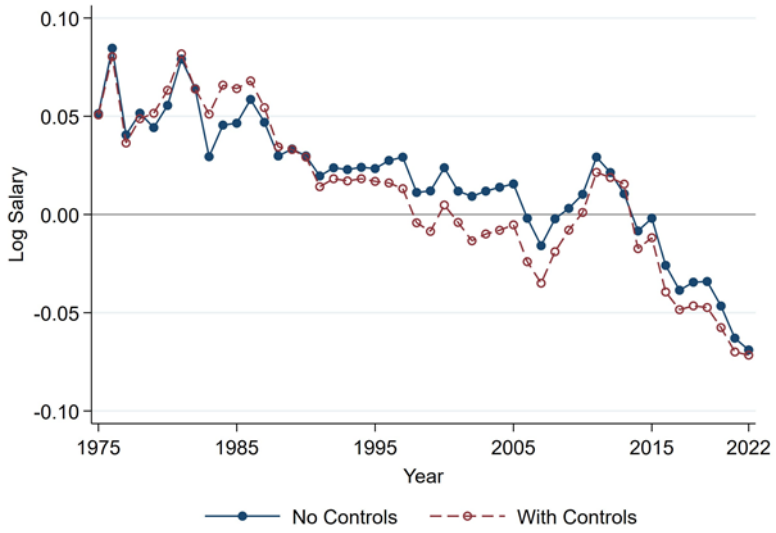
Figure 1: Union Rates and the Union Earnings Premium by Year



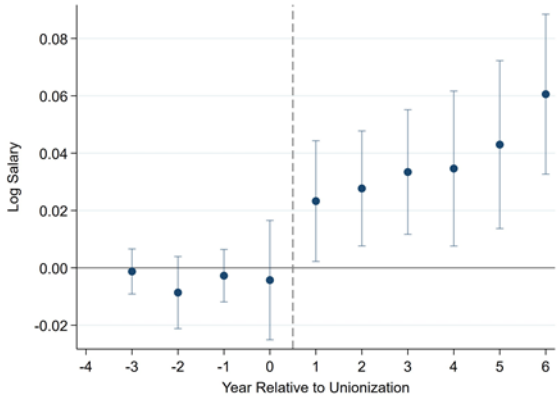
(A) Percent of Institutions and Individuals Unionized



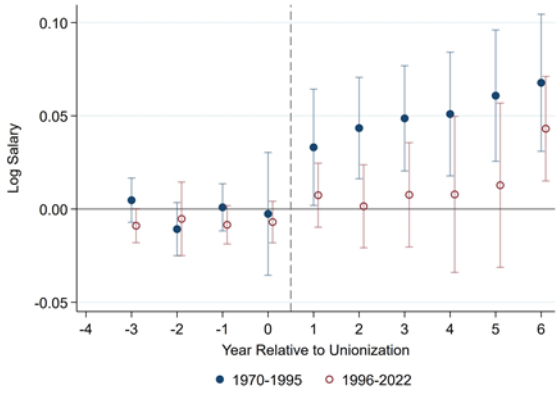
(B) Union Earnings Premium

Notes: Panel A shows the percent of institutions with unions relative to total number of institutions as well as the percent of faculty members at institutions with a union relative to the total number of faculty members. Panel B shows the estimated union earnings premium by year without controls and with controls. Base annual salary is used, which excludes additional pay such as stipends and reduced pay due to leave, reflecting a consistent measure of earnings over time. The premium is estimated by regressing the log of salary on a union status dummy interacted with year fixed effects, and including fixed effects for institution and department. The control variables include fixed effects for age (in years), gender, citizenship, rank, administrative responsibilities, sabbatical leave and unpaid leave, and a cubic polynomial for years of experience. The categories for administrative responsibilities are: none; Chairs/Heads/Directors; Associate/Vice Deans; and Deans.
Source: Statistics Canada, University and College Academic Staff System, 1970 to 2022; and self-collected union data.

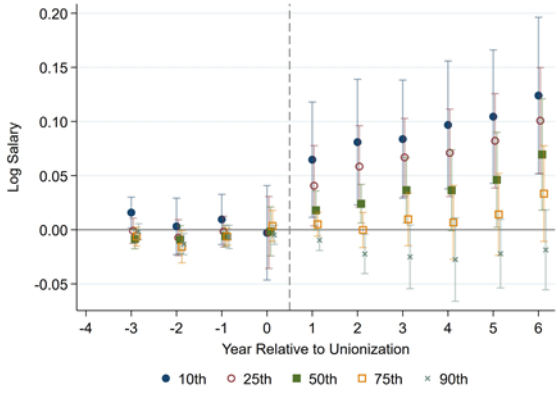
Figure 2: Effect of Unionization on Salaries



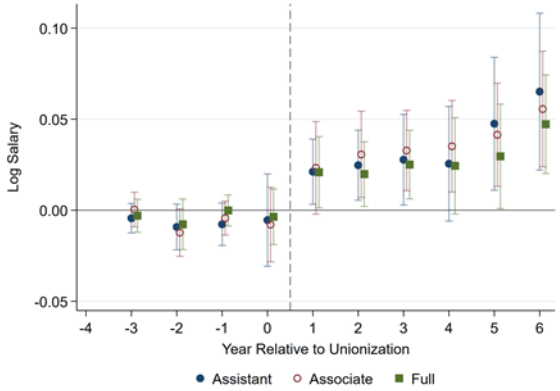
(A) Average Salaries



(B) Salaries by Time Period



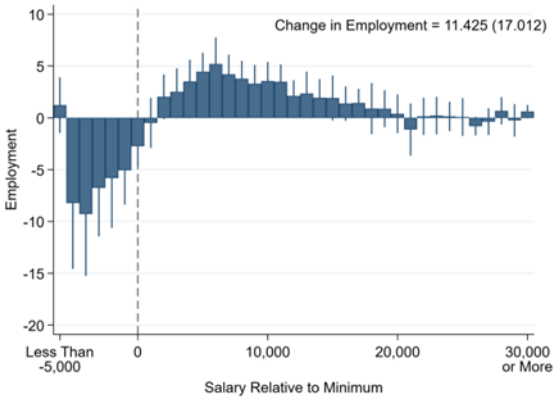
(C) Percentiles of Salaries



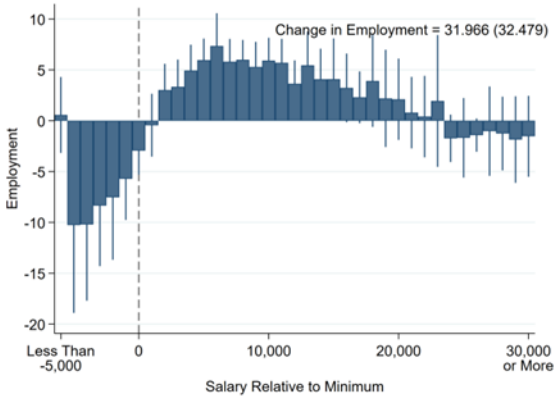
(D) Salaries by Academic Rank

Notes: The Callaway and Sant'Anna estimator is used in all panels. The default options (i.e., control group is never-treated only; and "short gaps" for periods before treatment) are selected. See the "csdid.ado" Stata command help file for further information. In all panels, the dependent variable uses base annual salary, which excludes additional pay such as stipends and reduced pay due to leave, reflecting a consistent measure of earnings over time. 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 shown in the event-study sample. The model specification includes individual and year fixed effects. Panel B is restricted to institutions that unionized in the relevant time period as stated in the legend or that never unionized. The corresponding regression results for panel A are shown in column (1) of Table S2, for panel B in Table S3, for panel C in Table S4, and for panel D in Table S5 of the Supplemental Appendix. The 95% confidence intervals are shown as vertical bars, with standard errors clustered by institution.
Source: Statistics Canada, University and College Academic Staff System, 1970 to 2022; and self-collected union data.

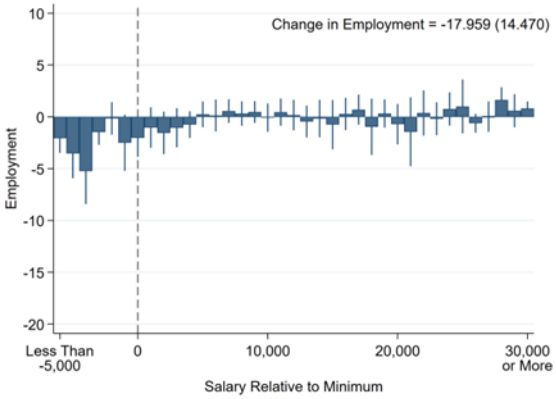
Figure 3: Effect of Salary Floors at Unionization on Employment by Relative Salary and by Time Period



(A) 1970-2022



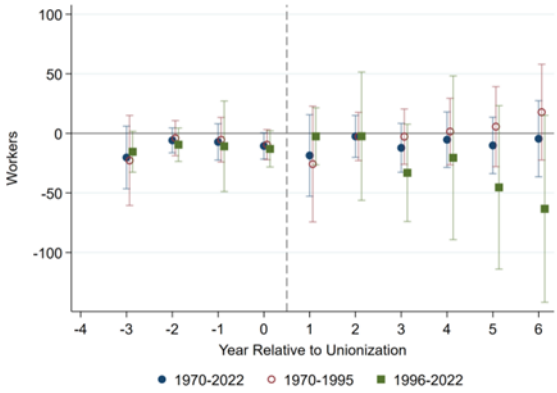
(B) 1970-1995



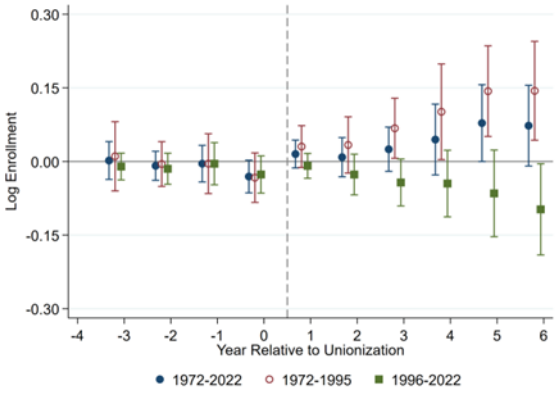
(C) 1996-2022

Notes: Based on the estimator by 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. 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 as well as a set of relative-bin indicators interacted with a post-treatment indicator. Year-bin and institution-bin fixed effects (FE) are included. 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. Inclusion of year-bin and institution-bin FEs requires treatment effects to be expressed relative to at least one pre-treatment relative-bin indicator. The highest relative-bin indicator is used, as it is the furthest from the salary floor where direct effects of the floors should be negligible. Each bar is the effect of unionization on the change in the percent of workers earning $\$x$ from the salary floor. The change in employment reported in the top-right is the sum of all bars, with standard error in parentheses. The 95% confidence intervals are shown as vertical bars, with standard errors clustered by institution.
Source: Statistics Canada, University and College Academic Staff System, 1970 to 2022; and self-collected union data.

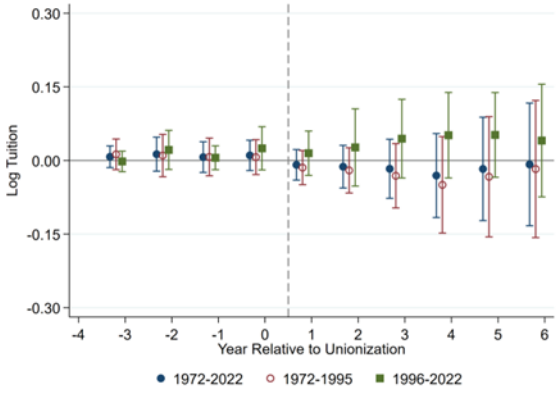
Figure 4: Effects of Unionization on Revenue-Generating Outcomes



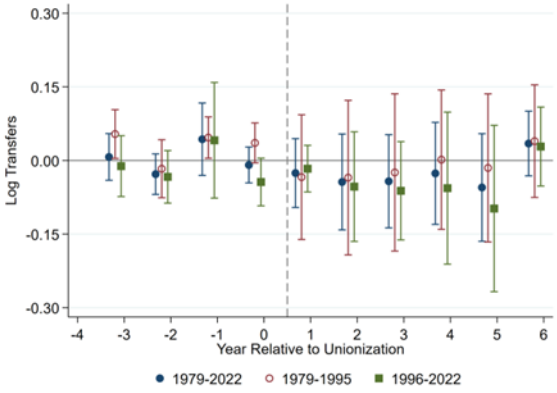
(A) Number of Workers



(B) Enrollment



(C) Tuition



(D) Government Transfers

Notes: Restricted to institutions that unionized in the relevant time period as stated in the legend or that never unionized. The Callaway and Sant'Anna estimator is used in all panels. The default options (i.e., control group is never-treated only; and "short gaps" for periods before treatment) are selected. See the "csdid.ado" Stata command help file for further information. The model is estimated on data collapsed to institution-year cells. The dependent variable in panel A is the number of faculty by institution and year. The dependent variable in panel B 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 C 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 (i.e., domestic or non-international) students. The dependent variable in panel D is the log of transfers from the provincial government to the university in the year. The model specification includes institution and year fixed effects. The corresponding regression results for Panel A are shown in column (1) of Table S6 for 1972-2022. For the estimates for the subperiods, see columns (1) and (2) of Table S7. The results in panels C, D and E in Table S8 of the Supplemental Appendix. The 95% confidence intervals are shown as vertical bars, with standard errors clustered by institution.
Sources: Statistics Canada, University and College Academic Staff System, 1970 to 2022 (Panel A); Statistics Canada, University Student Information System, 1972 to 1994, and Postsecondary Information System 1995 to 2022 (Panel B); Statistics Canada, Tuition and Living Accommodation Costs, 1972 to 2022 (Panel C); Statistics Canada and Canadian Association of University Business Officers, Financial Information of Universities and Colleges, 1979 to 2022 (Panel D); and self-collected union data.

Table 1: Characteristics of Institutions and Data Sample

Panel A: Unionization Events

	1970s		1980s		1990s		2000s		2010 or Later	
	Count (1)	Ratio (2)	Count (3)	Ratio (4)	Count (5)	Ratio (6)	Count (7)	Ratio (8)	Count (9)	Ratio (10)
Canada	23	41.8	11	56.7	6	65.6	4	71.0	7	82.3
By Region										
Atlantic	4	36.4	3	63.6	0	63.6	2	81.8	0	81.8
Quebec	8	53.3	4	70.6	2	82.4	0	82.4	1	88.2
Ontario	7	38.9	2	45.0	4	65.0	2	71.4	0	71.4
Western	4	50.0	2	66.7	0	66.7	0	66.7	3	100.0
British Columbia	0	0.0	0	0.0	0	0.0	0	0.0	3	75.0

Panel B: Data Sample Characteristics

	Full Sample		Never Unionized		Unionized	
	Mean (1)	Standard Deviation (2)	Mean (3)	Standard Deviation (4)	Mean (5)	Standard Deviation (6)
Demographics						
Age (Years)	47.2	9.9	48.0	10.0	45.9	9.6
Women (Percent)	21.8	41.3	23.1	42.2	19.6	39.7
Degree (Percent)						
PhD	78.0	41.4	80.7	39.5	73.3	44.3
Professional	7.2	25.8	8.3	27.5	5.2	22.3
Master's	11.7	32.1	8.4	27.7	17.3	37.8
Below Master's	3.2	17.6	2.7	16.1	4.2	20.0
Rank (Percent)						
Assistant Professor	24.4	42.9	23.4	42.3	26.2	43.9
Associate Professor	37.3	48.4	36.0	48.0	39.4	48.9
Full Professor	38.3	48.6	40.6	49.1	34.4	47.5
Job Traits (Percent)						
Unionized	20.4	40.3	0.0	0.0	55.3	49.7
Has Responsibilities	10.1	30.1	9.5	29.4	11.0	31.3
Salary (Dollars)						
Full Sample	142,900	46,300	149,700	48,650	131,350	39,400
Assistant Professor	106,850	31,200	108,100	32,650	101,500	23,600
Associate Professor	132,950	32,150	135,150	33,600	125,150	24,850
Full Professor	175,600	44,450	178,700	45,700	162,600	36,150
Institutional						
Faculty Size (Count)	600	600	750	700	400	350
Enrollment (Count)	16,850	16,950	24,350	20,700	10,750	9,500
Tuition (Dollars)	3,800	1,800	4,200	1,800	3,550	1,700
Transfers (Thousands of Dollars)	239,700	277,000	325,200	317,700	149,800	188,850
Number of Individuals	62,493		30,389		32,104	
Number of Observations	539,812		340,763		199,049	

Notes: Panel A reports the number of union formation events per decade and by region. The "Ratio" column refers to the cumulative percent of institutions that were unionized by the end of the corresponding decade in that region expressed relative to the total number of institutions in that region up to that time. The Atlantic region comprises Newfoundland and Labrador, Prince Edward Island, Nova Scotia and New Brunswick. The Western region comprises Manitoba, Saskatchewan and Alberta. Panel B reports descriptive statistics of the sample of faculty included in the analysis. The reported values are averages over all observations (i.e., averages both across individuals and within individuals over time) used in the event-study analysis. The salary measure used is a base annual rate, which offers a consistent measure of employees' annual earnings over time and across institutions. Currency values, faculty size and enrollment are rounded to the nearest 50. Currency values are expressed in 2022 constant dollars. To control for outliers, salaries are winsorized at the 0.5th and 99.5th percentiles. In Panel B, columns (3) and (4) pertain to individuals at institutions that never unionize and columns (5) and (6) pertain to the remaining institutions that eventually unionize, using observations from both before and after unionization.

Source: 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.