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Pay Transparency and the Gender Gap†

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We examine the impact of public sector salary disclosure laws on university faculty salaries in Canada. The laws, which enable public access to the salaries of individual faculty if they exceed specified thresholds, were introduced in different provinces at different times. Using detailed administrative data covering the majority of faculty in Canada, and an event-study research design that exploits within-province variation in exposure to the policy across institutions and academic departments, we find robust evidence that the laws reduced the gender pay gap between men and women by approximately 20–40 percent. (*JEL* I23, J16, J31, J44, K31)

One of the most persistent and salient features of labor markets around the world is that women earn less than men. For example, in the United States, a woman earns roughly $77 for every $100 earned by a man (Goldin 2014). A hypothesis gaining traction among academic researchers and policymakers is that the gender gap in earnings persists, in part, because it is hidden (Trotter et al. 2017). This belief is expressed in a series of policy reforms that mandate the disclosure of salaries broken down by gender.[1] For example, in 2016, President Barack Obama issued an executive order expanding pay disclosure requirements for employers with more than 100 employees.[2] There have also been calls in the private sector for more transparency

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[1] Throughout we will use the terms "pay transparency" and "salary disclosure" interchangeably.

[2] See http://wapo.st/2vMvIph?tid=ss_tw&utm_term=.a21256120472.

about pay differences between men and women. Technology firms, for example, are facing growing public pressure to disclose salaries by gender.[3]

Outside of the United States, transparency laws are increasingly considered as a policy to reduce the gender gap. Denmark introduced legislation in 2006 requiring large firms to report wage statistics by gender (Bennedsen et al. 2022). Starting in 2017, firms in the United Kingdom with more than 250 employees are required to report salaries and bonuses by gender.[4] Similar reforms are underway in Australia, France, and Germany. In Ontario, Canada, the Pay Transparency Act requires all publicly advertised job postings to include a salary range, prohibits employers from asking about past compensation, and mandates that employers report gender earning gaps to the Province.[5]

Despite the proliferation of pay transparency legislation as a tool to reduce pay inequalities and the gender gap, there is limited research that sheds light on its effectiveness. The objective of this paper is to provide new evidence on whether pay transparency laws, as implemented by policymakers, reduce the gender pay gap.

We examine the impact of the (staggered) introduction of pay disclosure laws in Canada on university faculty salaries. The laws, which cover public sector workers and apply to most university faculty in Canada, enable public access to the salaries of individual faculty if they exceed specified thresholds. In 1996, British Columbia, Manitoba, and Ontario were the first to introduce disclosure laws that required universities to report the salaries of each employee earning in excess of $50,000, $50,000 and $100,000, respectively. Disclosure laws in other provinces have passed more recently, and currently only four of the ten provinces do not legally require university faculty salaries to be publicized.

To evaluate the effect of these laws on faculty salaries, we leverage restricted-use Statistics Canada data, which contain the salaries, demographic characteristics, and job-related variables of full-time academic employees at Canadian universities since 1970. These data, which have close to universal coverage of full-time faculty at Canadian universities, allow us to identify faculty with salaries that meet the disclosure requirement within their province. Additionally, because the data contain an indicator for the academic unit of each individual faculty member, we are able to observe faculty with co-workers whose salaries are disclosed. This is one of the few datasets in Canada that jointly provides information on earnings and demographic characteristics of both employees and their co-workers for a comprehensive set of employers within a sector.

Our research design exploits variation in the incidence of the laws across university departments within provinces. Because salaries were only disclosed if they exceeded a legally determined threshold, lower paying departments, in contrast to higher paying departments, were not affected by the laws. Specifically, we define reference or peer groups within academic units, and consider a group as "exposed" to treatment when the salary of at least one faculty member was disclosed. This

[3] See https://www.bloomberg.com/news/articles/2017-04-13/tech-companies-tout-gender-pay-equity-but-balk-at-transparency but also https://www.nytimes.com/2019/03/07/opinion/google-pay-gap.html.

[4] See http://www.legislation.gov.uk/uksi/2017/172/pdfs/uksi_20170172_en.pdf.

[5] This law, called The Pay Transparency Act, 2018, came into effect on January 1, 2019.

definition provides a source of variation in exposure to the law within province. Thus, we can define treatment and control groups at the level of an academic unit and control for time-varying trends at the province level in a flexible manner. We consider both a broad and narrow definition of the reference group. The broad definition allows for vertical comparisons within departments across adjacent levels of academic rank, so, for example, assistant and associate professors compare themselves to each other, but assistant and full professors do not. The narrow definition is restricted to horizontal comparisons: individuals compare themselves only to other individuals of the same rank.[6]

We find that, on average, transparency laws significantly reduced the gender salary gap. Across our specifications for the reference group, we find that they led to a statistically significant 1.2–2 percentage point reduction in the gender gap. Our estimates are robust to controlling for a rich set of employer and individual characteristics, including individual fixed effects and time-varying individual-level observables, such as whether the individual has senior administrative responsibilities.

We can evaluate the magnitude of the change relative to the gender gap that prevailed at the time the first transparency reforms were introduced in the mid-1990s, which was roughly 6 percent. Using this benchmark, the change corresponds to an effect size of roughly 20–30 percent. Between the first reform and the end of the sample period in 2018, the overall gender gap fell in magnitude by 5 percentage points, from around 6 percent from the time of the initial reform to roughly 1 percent. Our estimates imply that approximately 25–40 percent of the overall reduction can be accounted for by disclosure. The estimates imply a large effect of pay transparency on the gender pay gap.

A natural question to ask is whether our results are driven by higher growth in women's salaries and/or slower growth in men's salaries, relative to untreated peers. While our estimate of the effect of transparency laws on the gender gap is similar across all specifications, our estimates of the effect of the laws on female and male salaries separately depend on the exact specification used. When individual fixed effects are included, the estimates suggest that slower relative growth in men's salaries contributed to the reduction in the gender gap.

We next explore heterogeneity in the gender pay gap effect along a number of dimensions. First, we find that the effect of salary disclosure laws on the gender pay gap is more pronounced in unionized workplaces. Second, the effects are primarily driven by changes in pay gaps among full professors. Third, we examine heterogeneity in exposure to the laws on the basis of the share of the peer group whose salaries are revealed and find that the gender pay gap closes even when only a subset of a faculty's salaries is revealed. Finally, our results indicate that the effects of disclosure are strongest when the gender pay gap prior to disclosure is smaller.

The rest of the paper is organized as follows: Section I summarizes the relevant literature; Section II provides an overview of public sector disclosure laws in Canada and discusses the mechanisms by which transparency laws might affect the gender

[6] In a previous version of the paper, we have also defined the reference group to be the academic unit, whereby everyone in the department is in the comparison set. Estimates using this definition are similar to those included here.

wage gap; Section III describes the data; Section IV considers the event-study specification; Section V contains the empirical analysis of pay transparency laws; and Section VI concludes.

I. Literature

Our paper contributes to a growing literature on pay transparency. Several studies have examined the effects of transparency on wages. For example, Gomez and Wald (2010) evaluate the impact of pay disclosure in the province of Ontario. They find that salaries of university presidents in the province increased relative to the average public sector salary, and that the policy also led to higher growth in average professorial salaries in Ontario relative to other provinces.[7] Mas (2017) considers the effects of a California mandate that required online disclosure of municipal salaries, and finds compression in salaries.

In the closest study to ours, Bennedsen et al. (2022) examine the impact of a law in Denmark that required private firms with more than 35 employees to provide salary statistics by gender to an employee representative.[8] The data are reported for groups that are large enough to protect the anonymity of individuals.[9] Using a difference-in-differences design that compares firms with 35–50 employees to firms with 20–34 employees, the authors report that the disclosure law led to a reduction in the gender wage gap in treated firms that was primarily driven by a slowing of males' wage growth. Compared to this study, in our setting, all public sector salaries above a specified threshold are not anonymized and are *individually* disclosed and accessible to the public. We discuss below how disclosure at the individual level permits an extra layer of treatment heterogeneity since individuals in some departments may effectively be more "exposed" to pay disclosure if relatively more faculty have salaries that exceed the specified threshold. Finally, it is worth noting that the transparency mandate in our setting does not explicitly target gender wage inequality, which contrasts with the policy examined in Bennedsen et al. (2022).

Several studies have examined the impacts of pay transparency on other labor market outcomes. Cullen and Perez-Truglia (2022) conducted a field experiment at a large corporation that revealed salaries of peers and managers. They find that a higher perceived peer salary lowers effort, output, and retention, but a higher perceived manager salary increases these outcomes. Relatedly, Breza, Kaur, and Shamdasani (2018) find that the ability of Indian manufacturing workers to learn about their peers' salaries led to lower productivity. Cullen and Pakzad-Hurson (2019) develop a dynamic bargaining model and test the equilibrium predictions regarding the introduction of pay transparency using data from an online labor market. They find that higher transparency lowers wages on average, but leads to less wage dispersion across workers.

[7] The latter conclusion is based on a difference in differences analysis using 1991, 1996, and 2001 census data.

[8] There was also an alternative choice available to employers that permitted them to replace the wage statistics broken down by gender with an internal report on equal pay.

[9] Anonymity is preserved by restricting disclosure to six-digit occupation codes that have at least ten employees of each gender at the firm level.

A number of studies examine the connection between pay transparency and well-being. Card et al. (2012) use a randomized information experiment to show that pay transparency reduced the well-being of university faculty in departments where they earned below median pay in California. Perez-Truglia (2020) finds a reduction in well-being following a reform in Norway that made the entire population's tax records publicly accessible online. Finally, Kim (2015) investigates the effect of US state-level laws that ban pay secrecy; that is, employer-level prohibitions on employees sharing salary information. Using a difference-in-differences design, the results indicate that in states with a law prohibiting pay secrecy, the wages of college-educated women were higher, leading to a lower gender pay gap.

II. Background

The first public sector salary disclosure laws were passed in 1996 in the provinces of British Columbia, Manitoba, and Ontario. In each case, the government mandated disclosure of all university salaries exceeding a certain threshold—\$50,000 in British Columbia, \$50,000 in Manitoba, and \$100,000 in Ontario.

In Table 1 we outline the timing, disclosure thresholds, and coverage of university faculty of the disclosure laws and legislation in provinces providing access to public salaries.[10] A number of additional features of these laws are noteworthy. First, most provinces with a salary disclosure law publish the salary data online.[11] The first publication of salaries online by the governments of Ontario, Nova Scotia, Alberta, and Newfoundland and Labrador was directly followed by news coverage in the media with wide dissemination. However, in other provinces disclosure laws do not require the province to make these data accessible online. In British Columbia, online access to faculty salaries was made possible in 2008, only after a freedom of information request by journalists from the Vancouver Sun. The provincial newspaper maintained an online, searchable data bank of public sector salaries from 2008 to 2015, including faculty salaries.

Second, the initial reporting threshold for disclosure has remained fixed throughout time in most provinces but has been adjusted for inflation in others. For example, in Alberta, several years following legislation on salary disclosure for government employees, a separate act that applied more broadly to the public sector, including university faculty, was passed in 2012 with a threshold of \$125,000 adjusted annually to Alberta's Consumer Price Index.

Finally, in some provinces legislation affecting salary disclosure was passed prior to the legislation cited in Table 1, but did not require public salary disclosure of university faculty whom we study. For example, preceding the legislation in Ontario, the salaries of government employees earning in excess of \$40,000 were published

[10] The laws covering salary disclosure in Saskatchewan are targeted at employees in crown corporations and have not been expanded to include other public employees, such as university faculty. However, the pressure of having some salaries disclosed in this province is leading the University of Saskatchewan to undertake its own transparency initiative. See https://thestarphoenix.com/news/local-news/u-of-s-online-salary-disclosure-a-step-in-the-right-direction-expert accessed March 6, 2019.

[11] A list of current websites providing online access to the salaries by province is provided in Table A1 of the online Appendix.

Table 1—Disclosure Laws

	Year of implementation (1)	Disclosure threshold (2)	Online government publication (3)
British Columbia	1996/2002	\$50,000/\$75,000	No
Manitoba	1996	\$50,000	No
Ontario	1996	\$100,000	Yes
Nova Scotia	2012	\$100,000	Yes
Alberta	2015	\$125,000	Yes
Newfoundland and Labrador	2016	\$100,000	Yes
New Brunswick	N/A		
Prince Edward Island	N/A		
Quebec	N/A		
Saskatchewan	N/A		

Notes: In British Columbia the initial salary reporting threshold of \$50,000 was amended to \$75,000 in 2002. Alberta's threshold is adjusted to the province's consumer price index. There are no pay transparency laws in Prince Edward Island, Quebec, New Brunswick, or Saskatchewan that require universities to disclose nonexecutive salaries to the province or respond to freedom of information requests for non-anonymized faculty salaries. N/A = Not applicable.

in the Public Accounts. This disclosure, however, did not cover university faculty, and access was limited as it required obtaining a hard copy of the Public Accounts document.[12]

To the best of our knowledge, these laws only imposed transparency and were not passed in conjunction with other reforms that would affect the gender salary gap in universities. Moreover, our empirical analysis controls very flexibly for any time-varying shocks at the province level since we exploit variation in the salary thresholds within provinces.

Why might pay transparency affect the gender pay gap? One effect of the provision of information on gender-based salary disparities within an organization is that it may lead individuals to privately demand higher pay from their employer. The case of Lilly Ledbetter is illustrative of this mechanism. Ledbetter, a supervisor at Goodyear Tire, an American manufacturing company, was unaware that her male counterparts in similar positions were being paid more than she was. Revelation of this fact through an anonymous letter led her to file an employment discrimination lawsuit against her employer. This case proceeded all the way to the US Supreme Court and subsequently led to the Lilly Ledbetter Fair Pay Act of 2009, which eased the burden of filing a discrimination lawsuit.[13]

The Ledbetter case emphasizes individual action by employees. It is also possible that broad salary disclosure reduces the gender pay gap as a result of an institutional response to wider public attention to pay disparities. In particular, organizations may take institutional action to make salary adjustments, in part to maintain public

[12] Starting in 1996 the Financial Information Act, which requires public bodies to prepare a statement documenting the salaries of employees making \$75,000 or more (threshold starting in 2002), was in force in British Columbia. We are unable to uncover any evidence that these statements were ever made public. Since 1996, public employees earning \$25,000 or more in Nova Scotia have been published in the Public Accounts, but university faculty are excluded. New Brunswick has a similar requirement starting in 2008, excluding university faculty and with a \$60,000 threshold.

[13] See https://www.congress.gov/bill/111th-congress/senate-bill/181.

relations.[14] A number of the universities in our sample undertook campus-wide studies of gender differences in compensation over our sample period. The analysis in these studies typically involves the use of regression analysis to estimate the gender pay gap, controlling for factors such as field and experience (years since highest degree and years at institution). In many of these cases, the studies have revealed evidence of a gender gap which has led the university to make a onetime across-the-board adjustment to women faculty member salaries. In other cases, a pool of money has been established to grant anomalies to faculty who fall below the regression line.[15] While we do not have direct evidence that these studies were in response to transparency laws, to our knowledge, they have all occurred within provinces after a law came into effect. These studies may be a mechanism by which disclosure affected compensation at the institution level.

It is also possible that the gender wage gap is unaffected by transparency laws, or perhaps, as a result, even further widens. For example, if there is taste-based discrimination or if the gender wage gap is due to monopsony whereby men and women have different labor supply elasticities, there may not be any impact of transparency. Similarly, learning about coworkers' wages might reveal something about the nature of firm-specific rents, and if men and women use this information in a symmetric fashion in bargaining, one should not expect to see any impact on the gender pay gap.[16] However, if men, but not women, use the information in bargaining, pay transparency could exacerbate the gap.[17]

III. Data

Our main estimates are based on an analysis of data from the Statistics Canada's University and College Academic Staff System (UCASS), for the years 1989 through 2018. This is an annual census survey that collects data on full-time teaching staff at degree-granting Canadian universities and their affiliated colleges, as of October 1 of each year. The survey includes all teachers within faculties, academic staff in teaching hospitals, visiting academic staff, and research staff who have academic rank and salary similar to teaching staff, for all those whose term of appointment is not less than 12 months. It excludes administrative and support staff, librarians, and research and teaching assistants.

UCASS is administered directly to institutions and participation is mandatory. The unit of observation in the data is the individual, but the survey unit is the institution, and information on the socioeconomic characteristics of staff—including pay—are obtained directly from payroll records. Statistics Canada works closely with institutions to maintain consistent reporting each year and to ensure the data

[14] For example, Mas (2017) finds that the disclosure of city manager salaries in California led to a reduction in average salaries, which is interpreted as an institutional response to public outcry over high levels of compensation.

[15] A list of these initiatives, their relevant dates, and the amount and timing of any resulting salary adjustment is presented in Table A2 of the online Appendix.

[16] As Cullen and Pakzad-Hurson (2019) show, however, this depends crucially on outside options. If women start out with lower outside options than men, then transparency could close the gap—even if men and women use the information in the same way.

[17] Leibbrandt and List (2014) present evidence that, in some circumstances, men are more likely to negotiate wages than women.

are comparable across institutions. Individuals are assigned (anonymized) internal identification numbers so they can be followed over time within institutions, but not across institutions.

A limitation of this dataset is that it was discontinued from 2011 to 2015. During this period, data were collected independently by participating institutions in association with the National Vice President's Academic Council, leading to the construction of the National Faculty Data Pool (NFPD). The goal of the NFPD was to emulate the UCASS as closely as possible for longitudinal consistency. Through a recent collaborative effort between Statistics Canada and the university consortium, the NFDP has been integrated with UCASS to fill in the missing years.

The NFDP has two limitations that are important to note. First, participation in the survey was voluntary. Between 2010 and 2012, the sample size decreased from approximately 34,475 workers to 26,700 and the number of institutions observed decreased from 85 to 50. The loss of institutions is proportionately larger, as the withdrawal of a given university from the survey will also lead to the loss of all of its (small) satellite colleges. Second, for confidentiality reasons or ease of reporting, several institutions did not maintain consistent reporting of their employees' personal identifiers moving from UCASS to the NFDP in 2011 and/or back to UCASS in 2016. To overcome this issue, we match on observables to generate longitudinally consistent identifiers for institutions where a break is observed. This is done by matching within institutions and departments based on year of birth, gender, year appointed to the institution, and year of highest degree. An assessment of the matching procedure for institutions and years where no break occurred, where we can assess whether the match was correct, indicates that the success rate exceeds 99 percent.

The following sample restrictions are imposed throughout our analysis. Individuals are included only if they hold appointments at the rank of assistant, associate or full professor, they are not employed in a faculty of medicine or dentistry, and they are assigned to a specific department.We make these restrictions since we have a clearer understanding of salary determination for the faculty that are included. For example, salary determination in medicine and dentistry may be affected by activities beyond research and teaching, including medical practice. We restrict to faculty with a non-missing department since our empirical specification requires assigning a peer group based on department, and this is not possible for those not assigned to a department.[18] Lastly, the sample is restricted to institutions that are not private, theological, or military and that are observed in the 2012 wave of the NFDP and that finalized their data with or submitted back information to Statistics Canada. This restriction on institutions is made to balance the panel around the years that the survey was discontinued.

Throughout the analysis, base salary is used as the earnings measure of interest. This measure effectively comprises the annual (12 month) rate of pay contractually negotiated and agreed upon between the employee and employer. Specifically, the data are collected toward the end of the calendar year (typically in October)

[18] Prior to 2008 the department variable is not well-reported. Thus, we proxy for department using a variable for subject taught, which uses the same classification system as the department variable.

and therefore provides a "snapshot" of salaries for the fiscal year as of this time. It excludes various factors that may influence pay which may be determined endogenously, such as 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.[19]

Although the dataset contains a variable for actual salary, the base measure of salary is used for several reasons. First, actual salary is not observed for all the relevant years. Second, Statistics Canada has worked closely with respondents to obtain a measure of base salary that is comparable across institutions and over time. Lastly, there is a close relationship between base and actual salary in practice; base salary accounts for 102.0 percent of actual salary (101.8 and 102.3 for men and women, respectively) on average within institutions and years for which actual salary is observed. Note that base salary exceeds actual salary because of the presence of unpaid leave.

In Table 2 we present descriptive statistics for the full sample used in this study and separately for men and women from 1989–2018. There are 50,332 individual university employees across Canada in our sample. On balance, individuals are approximately 49 years old and just over one-quarter of them are women. In more recent years, the share of faculty that are women has climbed to about 40 percent, which has been driven by a larger share of new hires that are women (see Figure 1, panel A). In addition, 84 percent of faculty hold a PhD, and about 62 percent belong to institutions that are unionized. Interestingly, women are about 7 percentage points more likely to be unionized than men and this is driven by two factors: (i) women are more likely to work at institutions represented by unions or faculty associations; and (ii) the proportion of women in the industry has risen over time alongside the gradual increase in unionization.

IV. Empirical Model

Our empirical model takes advantage of the fact that in the Canadian setting there are three separate sources of variation in pay transparency: provincial, temporal, and threshold salary. For example, as discussed above, salary disclosure in Ontario was introduced in 1996 but only individuals whose salaries were above the \$100,000 threshold were included.[20]

Our baseline definition of treatment takes advantage of all of these sources of variation. Specifically, we define an individual as treated in a given year if, during that year, she or he works in a province where there is a salary disclosure in place and in a department where a faculty member (excluding herself or himself) was revealed by the disclosure policy in the year of the reform.[21] Our baseline definition

[19] In the province of Ontario, salary disclosure is based on tax (calendar) year reporting, whereas the salary measure in the data is based on the university's fiscal year reporting. To better align these two measures, we construct two-year averaged salaries between years t and $t-1$ for Ontario and use this variable throughout the analysis. However, we also present estimates for which we do not make this adjustment.

[20] In Ontario, the median salary in 1996 was \$74,950, thus indicating that many faculty were not necessarily "treated" by the transparency law despite working in Ontario.

[21] According to our definition of treatment, an individual can be untreated if his or her salary is above the threshold but no peers have a salary above the threshold. Our results are virtually unchanged if we instead consider this individual as being treated.

Table 2—Descriptive Statistics

	Full sample		Men		Women	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Demographics						
Age (in years)	49.2	9.3	49.9	9.2	47.3	9.0
Women (percent)	27.5	44.6	0.0	0.0	100.0	0.0
Highest degree (percent)						
PhD	84.1	36.6	86.0	34.7	79.0	40.7
Professional	0.5	6.8	0.4	6.4	0.6	7.9
Master's	12.3	32.9	10.7	30.9	16.7	37.3
Below Master's	3.1	17.4	2.9	16.8	3.7	18.8
Rank (percent)						
Assistant professor	20.8	40.6	16.5	37.1	32.1	46.7
Associate professor	39.2	48.8	37.2	48.3	44.7	49.7
Full professor	40.0	49.0	46.4	49.9	23.2	42.2
Other job traits (percent)						
Unionized	62.3	48.5	60.5	48.9	67.3	46.9
Has responsibilities	13.1	33.7	13.6	34.3	11.5	32.0
Compensation						
Salary (dollars)						
Full sample	118,600	26,700	122,100	26,550	109,350	24,850
Assistant professor	90,200	16,950	90,900	17,550	89,250	16,050
Associate professor	112,900	17,700	113,950	17,700	110,500	17,500
Full professor	138,950	21,450	139,650	21,200	135,100	22,050
Salary growth (percent)						
Full sample	2.5	5.0	2.3	4.9	3.1	5.2
Assistant professor	3.4	4.7	3.3	4.6	3.4	4.8
Associate professor	2.7	5.0	2.5	4.9	3.1	5.1
Full professor	2.0	5.1	1.8	4.9	2.7	5.9
Individuals	50,332		35,079		15,253	
Observations	399,843		290,021		109,822	

Notes: The salary measure used is a base annual rate, which offers a consistent measure of employees' annual earnings both over time and across institutions. To control for outliers, observations with salaries below the 0.5th percentile or above the 99.5th percentile (in 2018 constant dollars) are dropped. The currency values are rounded to the nearest $50 and are expressed in 2018 constant dollars. The descriptive statistics refer to data for years used in the event study analysis and are computed using all years. More precisely, the reported averages are full-sample averages calculated over all observations. See the notes in Table 4 for more information.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018

of peer group consists of faculty in the same *Institution* and *Department* who are either in the same rank or in adjacent ranks. According to this definition, individuals compare themselves to peers as follows: (i) assistant professors compare themselves to other assistant and associate professors, (ii) associate professors compare themselves to all ranks, and (iii) full professors compare themselves to associate and other full professors. This specification allows for vertical comparisons within departments but does not allow assistant and full professors to compare themselves to each other. We also report results from another definition based on *Institution*, *Department*, and *Rank*, where individuals compare themselves only to other individuals of the same rank.

The two definitions of the treatment are conceptually distinct; the former captures "horizontal and vertical comparisons" whereas the latter definition is limited to "horizontal comparisons" (see Cullen and Perez-Truglia 2022). Allowing for

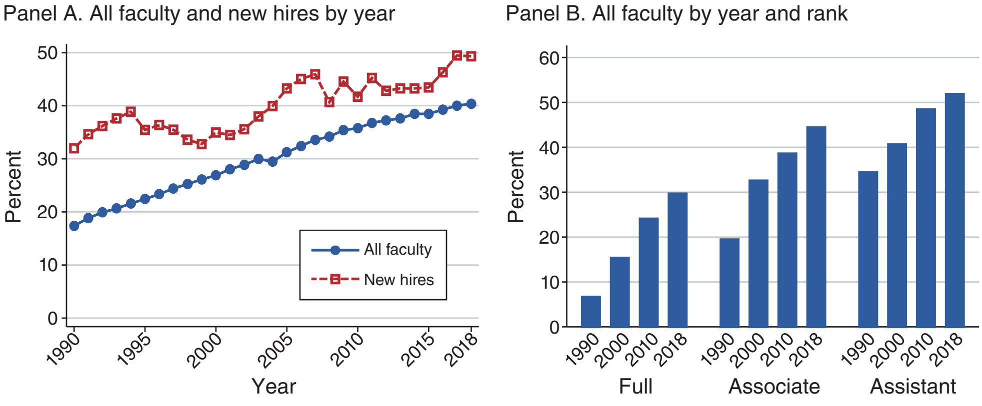

Figure 1. Percent of Women Faculty Members by Year

Notes: The analysis shown here is based on data for the institutions and years used in the event-study analysis. All faculty refers to assistant, associate, and full professors (individuals with rank below assistant professor are omitted). New hires are defined as individuals for whom the year is equal to the year appointed to institution.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018

vertical comparisons is potentially important since there are many cases where a worker is paid less than a peer in the same institution and department who has a lower rank—a situation commonly referred to as "salary inversion." This is shown in Figure A1 in the online Appendix.

To formalize our approach, we consider a panel of $i = 1, \ldots, N$ individuals in which salary Y_{it} is observed for $t = 1, \ldots, T$ years or for some, a subset thereof. We also observe a binary treatment variable $D_{it} \in \{0, 1\}$: $D_{it} = 0$ if i has not been treated by year t and $D_{it} = 1$ if i has been treated by year t. In our setting, treatment is an absorbing state, and the treatment path $\{D_{i,t}\}_{t=0}^{T}$ is a sequence of zeros and then ones. In this case, the treatment path is uniquely characterized by the time period of the initial treatment, which we denote by $E_i = \min\{t : D_{i,t} = 1\}$. This is typically referred to as the "event time," and we denote $K_{it} = t - E_i$ as the "relative time." We let F_i be an indicator variable that takes on a value of 1 if individual i is a woman. We consider the standard dynamic specification:

$$\text{(1)} \qquad \log(Y_{it}) = \alpha_i + \beta_{pt}^{M} + \beta_{pt}^{W} + \sum_{k=-A}^{B} \gamma_k \mathbf{1}\{K_{it} = k\} + \sum_{k=-A}^{B} \delta_k \mathbf{1}\{K_{it} = k\} \times F_i + \epsilon_{it},$$

where $A \geq 0$ leads of the treatment are included together with $B \geq 0$ terms that capture the short-run effects and a single parameter to capture longer-run effects. Our baseline specification limits the sample to relative years $A = 7$ and $B = 7$. Our model controls for an individual fixed effect (α_i) and province-by-year-by-gender fixed effects $(\beta_{pt}^{M}, \beta_{pt}^{W})$ (M = man, W = woman). The latter set of fixed effects control for time-varying, province-specific shocks that might differentially affect men's and women's salaries and are correlated with the event time.

Table 3—Prevalence of Treatment across Institutions, Peer Groups, and Workers

	Peer group specification			
	Horizontal and vertical comparisons		Horizontal comparisons	
	All provinces and years (1)	Adopting provinces in reform year (2)	All provinces and years (3)	Adopting provinces in reform year (4)
Percent of institutions treated	53.1	86.7	53.1	86.7
Percent of workers treated	24.4	75.2	19.0	58.8

Notes: This table reports the percent of institutions and workers treated as a fraction of the full sample used in the event-study analysis (columns 1 and 3) and as a fraction of the number of institutions or workers in the analysis and within provinces that adopted pay transparency in the year of the reform (columns 2 and 4). The difference between the percent of institutions treated relative to all versus adopting provinces arises because some provinces never adopted pay transparency, such as Quebec, which has a relatively large number of institutions.

Source: Statistics Canada, University and College Academic Staff System

Our identifying assumption is that there are no shocks correlated with the introduction of transparency laws that *differentially* affect the salaries of men and women within peer groups (i.e., same department or same rank in the department). The coefficients of interest are the parameters $\{\delta_k\}_{k=-A}^{B}$. These indicate the causal effect of transparency on the gender salary gap in the short- and long-run, respectively. We can also test for the presence of pre-trends by plotting the $\hat{\delta}_k$ for $k < 0$ and examining whether $\hat{\delta}_k = 0$. In all of our event-study figures, we normalize $\gamma_{-1} = 0$ and $\delta_{-1} = 0$ and otherwise estimate the full set of event dummies from -7 to $+7$.

Finally, to quantify the magnitude of the effect and to increase precision of our estimates, we adopt the "static" or canonical specification by setting $A = B = 0$:

$$\log(Y_{it}) = \alpha_i + \beta_{pt}^{M} + \beta_{pt}^{W} + \gamma_{0+} D_{it} + \delta_{0+} D_{it} \times F_i + \epsilon_{it}, \tag{2}$$

where γ_{0+} is the causal effect of transparency on average salaries for faculty who are men and $\gamma_{0+} + \delta_{0+}$ is the causal effect for faculty who are women. Compared to the dynamic model, this specification imposes no pre-trends and assumes constant treatment effects for all k. The standard errors are clustered at the level of institution, as this is typically the level at which pay scales are determined. We also report results that cluster standard errors by institution and department, which is the level at which treatment is defined in our baseline specification, and we consider the wild bootstrap.

Table 3 presents the fraction of institutions and workers treated, separately for each of the peer group specifications. Since not all provinces adopted salary disclosure laws, we report these statistics for both the full sample and the sample of adopting provinces in the reform year. The results in the first row, columns 1 and 3, show that roughly 53 percent of institutions had at least one employee who was treated by the reform, rising to roughly 87 percent in the sample of adopting provinces in columns 2 and 4. The proportion of workers treated (second row of table) varies by the specification of the peer group, ranging from roughly 59 through 75 percent in adopting provinces.

Figure A2 and Table A3 in the online Appendix provide further information about the peer groups. Figure A2 documents that, regardless of the specification of the peer group, in roughly 60 to 80 percent of cases, a peer group has either no worker with a salary above the disclosure threshold or all workers with salaries above the threshold. This fact provides some rationalization of our baseline specification where we specify the treatment as a dichotomous variable, but we also explore the robustness of our findings to an alternative specification that exploits the level of exposure across peer groups. In Table A3 we document the conditional gender pay gap by the level of exposure to disclosed salaries in the period before the reforms and find that there is a sizable pay gap (of at least 2.5 percent or greater) within treated provinces regardless of the level of exposure.

V. Results

A. *Trends in Hiring and Gender Salary Gap*

We begin our analysis by providing context for our results through evidence of trends in hiring and the gender salary gap among university faculty, and more generally in the Canadian labor market, over our sample period. Panel A of Figure 1 shows that representation of women in the university sector in Canada has increased significantly over the past few decades, from about 20 percent in the early-1990s to 40 percent (approximately double) in 2018. This trend has occurred alongside increased enrollment of women in university, including graduate studies that lead to academic positions. Among new hires, approximately half were men and half were women by 2018. In addition, panel B of Figure 1 shows these gains in the representation of women are observed across all faculty ranks; the effect appears largest among assistant professors likely because most new hires enter at this rank and changes in worker composition are gradual due to low turnover rates and the tenure process. These trends are expected to contribute to improvements in pay equity as women move into senior administrative roles at universities and oversee the salary determination of new hires.

The gender earnings gap in our sample of faculty is reported in Figure 2.[22] We present this gap over time both unconditionally and conditional on controls (fixed effects for institution-by-department and year of birth, and controls for the number of years since appointed to institution and years since highest degree obtained). The conditional gender gap was roughly 8 percent in 1990 and has closed to roughly 1 percent as of 2018, the last year in our sample.[23]

[22] While it has become commonplace to measure gender pay disparities with hourly wages in Canada, earnings are the norm in many other countries, and we focus on the annual earnings of faculty in our analysis. Using earnings to document gender differences of course may conflate both differences in hours worked (e.g., part-time versuss full-time) and differences in hourly wages. This is less of a concern in the present context, as we restrict our sample to full time appointments at the rank of assistant, associate or full professor, and faculty salaries in Canada are typically a fixed amount paid on a 12-month basis.

[23] This is consistent with Warman, Woolley, and Worswick (2010) who use similar data to document a narrowing in gender earnings differentials among university faculty between 1990 and 2001.

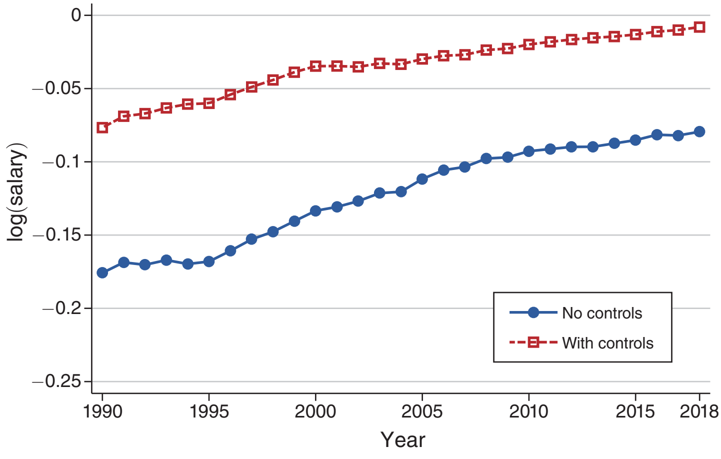

Figure 2. Gender Salary Gap with versus without Controls by Year

Notes: The estimates are based on a regression of the log of salary on year fixed effects and their interactions with an indicator for women. The salary measure used is a base annual rate, which offers a consistent measure of employees' annual earnings both over time and across institutions. To control for outliers, observations with salaries below the 0.5th percentile or above the 99.5th percentile (in 2018 constant dollars) are dropped. The coefficients of the interaction variables are reported, where 1989 serves as the reference year, after being scaled down by the estimated unconditional gender salary gap from the coefficient for the indicator for women. Control variables include institution by department fixed effects, year of birth, number of years since appointed to institution, and years since highest degree obtained.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018

For some broader context, in Figure A3 of the online Appendix we present evidence of the gender hourly wage ratio for full-time workers in the Canadian labor market from 1997 to 2018.[24] We report the ratio for all workers and for professional occupations within the educational services sector. The ratio for all workers rises from a low of just over 0.82 to almost 0.89 over the period. The ratio for education workers is more volatile, reflecting smaller sample sizes. It begins the period just over 0.88 and rises above 0.90, except for an abrupt decline in 2018. Throughout almost all of the period, academics faced a smaller gender salary gap than their counterparts in the wider labor market.

A potential concern of focusing on pay in the university sector is that salaries may be set according to a statutory formula; for example, they may be entirely determined on the basis of institution, department and rank. To gauge whether there is discretion in pay and scope for transparency laws to impact the gender salary gap, we predict salaries by regressing them on the interaction of institution-department-rank-tenure-year fixed effects, age fixed effects, and highest degree obtained fixed effects. If salaries are set in a formulaic way, then there should be very little residual variance between actual salaries and predicted salaries. Figure 3 shows that this is not the case, as we observe substantial residual variation for both men and women. The R^2 for men is roughly 83 percent and the R^2 for women is roughly 87 percent. Additionally, the

[24] See also Baker and Drolet (2010) and Morissette et al. (2013).

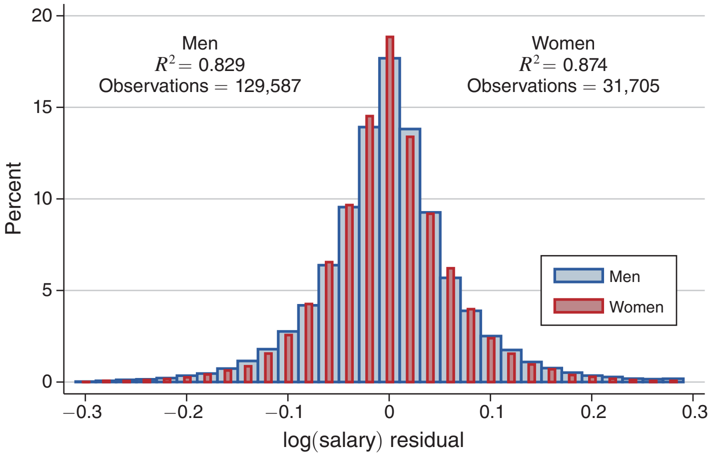

FIGURE 3. DISTRIBUTION OF THE RESIDUALS FROM SALARY REGRESSIONS, BY GENDER

Notes: The distributions are plotted of the residuals from regressions of the log of salary on fixed effects (FEs) for the interaction of institution, department, rank, years since appointed to institution, and year; and FEs for year of birth and highest educational attainment. The salary measure used is a base annual rate, which offers a consistent measure of employees' annual earnings both over time and across institutions. The analysis is carried out separately for men and women using the Stata command "reghdfe," by Correia (2014), which iteratively removes singleton groups. The number of observations reported reflects the number after dropping singleton groups. This analysis uses data for years used in the event study analysis. See the notes in Table 4 for more information.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018

fact that the conditional gender gap is roughly 6 percent at the time the first disclosure laws were introduced in the mid-1990s suggests that there is scope for disclosure to affect the gap.

B. *Main Results*

We start our formal analysis by presenting a series of nonparametric event-study plots to visually examine the effects of transparency on the gender salary gap. Next, we turn to regression models to quantify the precise impact.

Figure 4 contains our main event-study figure corresponding to equation (1) showing the impact of pay disclosure laws on the gender salary gap.[25] Panel A presents the graphs for men and women faculty's log salaries, separately. The dots for men correspond to γ_k while the squares for women correspond to $\gamma_k + \delta_k$. Year "0" is the reform year. All coefficient estimates are expressed relative to the event year $-$ 1 (year prior to the reform), which is normalized to 0.

In panel A, prior to the reform, the time profiles of both the solid round dots (men) and the hollow squares (women) are fairly congruent, with, at best, modest positive slopes up to the omitted year, indicating that the gender pay gap is approximately static. However, in the years after the reform, the salaries of men and women

[25] Treatment is defined based on the year the laws were implemented. Results using the year that the salaries were disclosed are very similar and are available upon request.

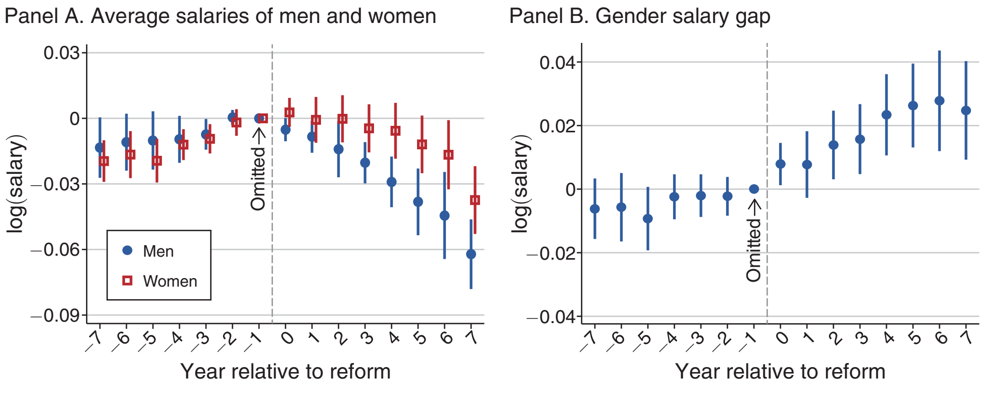

Figure 4. Event Study of the Effect of Pay Transparency on Average Salaries of Men and Women and Gender Salary Gap

Notes: The salary measure used is a base annual rate, which offers a consistent measure of employees' annual earnings both over time and across institutions. The analysis controls for fixed effects by individual and province-year-gender. The figure is based on the specification of peer groups, which permits comparisons both within rank and to adjacent ranks ("horizontal and vertical comparisons"). The coefficient for event time -1 is omitted to normalize the gender salary gap to zero in the year prior to the reform. The 95 percent confidence intervals shown are based on standard errors clustered by institution. See the notes in Table 4 for more information about the regression specifications.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018

move apart. In the first years post-reform the salaries of treated women remain on pace with the control group, while salaries of treated men lag behind.[26] The net impact is a reduction in the gender pay gap due to the disclosure laws, as can be seen in panel B (here we graph estimates of δ_k).

Panel B also documents that there is little movement in the gender gap in the years prior to the reforms and a clear and noticeable jump around the event year, providing some degree of confidence that we are not merely detecting differential pre-trends. Also, the law's impact on the gender pay gap appears to gradually increase over the first four years, and then stabilize subsequently. This gradual evolution would be consistent with the annual salary setting in most universities, and that it might take some time for information to disseminate and any institutional mechanisms for salary redress to play out. Finally, we note that the dynamic treatment effects on men's and women's log salaries after event time $+5$ are quite large. Given that these longer-run effects are estimated with much less precision, we do not emphasize them as much.

The regression results corresponding to equation (2) are presented in Table 4. In columns 1 and 2, we consider the peer group specification that allows for horizontal and vertical comparisons, while columns 3 and 4 consider the peer group specification that only allows for horizontal comparisons.[27] We present estimates

[26] We are cautious about interpreting the longer run impacts of the reforms on men's and women's salaries. Given the timing of the disclosure laws (Table 1) and the time span of our data, our identification strategy is strongest for the initial impact of the reforms.

[27] For the peer group specification by institution and department, we assume that individuals compare themselves to peers as follows: (i) assistant professors compare themselves to assistant and associate professors; (ii)

TABLE 4—EFFECT OF PAY TRANSPARENCY ON AVERAGE SALARIES AND THE GENDER SALARY GAP

	Peer group specification			
	Horizontal and vertical comparisons		Horizontal comparisons	
	(1)	(2)	(3)	(4)
Salaries of men	0.034	−0.017	0.052	−0.034
	(0.007)	(0.006)	(0.009)	(0.007)
Salaries of women	0.052	0.003	0.067	−0.022
	(0.007)	(0.010)	(0.010)	(0.006)
Gender salary gap	0.018	0.020	0.015	0.012
	(0.005)	(0.006)	(0.005)	(0.004)
R^2	0.645	0.938	0.646	0.939
Observations	384,519	378,890	384,519	378,890
Clusters	49	48	49	48
Institution FEs	✓		✓	
Department FEs	✓		✓	
Individual FEs		✓		✓
Province-Year-Gender FEs	✓	✓	✓	✓
Additional Controls	✓	✓	✓	✓

Notes: The estimates for men and the gender salary gap correspond to the coefficient estimates for the treatment effect (γ_{0+}) and its interaction with the indicator for women (δ_{0+}), respectively, from equation (1) described in the main text. The estimates for women are computed as the sum of these two effects ($\gamma_{0+} + \delta_{0+}$). The salary measure used is a base annual rate, which offers a consistent measure of employees' annual earnings both over time and across institutions. Additional controls include an indicator for having senior administrative responsibilities in all regressions, as well as experience measures for the number of years since appointed to institution and years since highest degree obtained in the regressions without individual fixed effects (FEs). When individual and year FEs are both included, experience is collinear notwithstanding cases in which individuals take unpaid time off or obtain a higher degree after being appointed to the institution. Individual FEs nest the institution and department FEs, so that institution and department FEs need not be explicitly included in the regressions when individual FEs are included. Responsibilities are defined as appointments to senior administrative roles, including: dean; assistant, associate, or vice dean; director whose responsibility and salary is equivalent to dean; department head or coordinator; and chairperson. Models are estimated using the Stata command "reghdfe," which calculates degrees of freedom lost due to FEs and iteratively removes singleton groups to avoid biasing standard errors. For the peer group specification with horizontal and vertical comparisons, individuals compare themselves to peers as follows: (1) assistant professors compare themselves to assistant and associate professors; (2) associate professors compare themselves to all ranks; and (3) full professors compare themselves to associate and full professors. This specification allows for vertical comparisons within departments but assistant and full professors do not compare themselves to each other. For the peer group specification with horizontal comparisons, individuals compare themselves only to other individuals of the same rank. In all regressions, the analysis includes data for years used in the event-study analysis, namely: 1989 to 2003 for British Columbia, Manitoba, and Ontario; 2005 to 2018 for Nova Scotia; 2008 to 2018 for Alberta; 2009 to 2018 for Newfoundland and Labrador; and 1989 to 2018 for New Brunswick, Prince Edward Island, Quebec, and Saskatchewan. The full time period is used for these latter provinces because no reform occurred, so it is not possible to center the data on a seven-year interval around the reform. Standard errors (in parentheses) are clustered by institution. ✓ denotes included in the regression.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018.

for the salaries of men and women separately, and for the gender salary gap. In all specifications, we control for province-by-year-by-gender fixed effects along with several controls (indicator for having senior administrative responsibilities, experience based on the number of years since appointed to institution and years since

associate professors compare themselves to all ranks; and (iii) full professors compare themselves to associate and full professors. This allows for vertical comparisons to the nearest rank (higher or lower) but prevents assistant professors from comparing themselves to full professors and vice versa.

highest degree obtained). Columns 1 and 3 control for institution and department fixed effects, while columns 2 and 4 include individual fixed effects.[28]

A consistent result across the columns of Table 4 is that the pay transparency laws reduced the gender gap. These estimates are statistically significant at the 1 percent level across all specifications and are robust to the inclusion of individual fixed effects, additional controls, and the definition of peer group. Conditional on the additional controls and the individual and province-year-gender fixed effects, the laws reduced the gender gap by 1.2 to 2 percentage points. Relative to a conditional gender gap of roughly 6 percent at the time of the initial reforms in 1996 (see Figure 2), this effect represents a roughly 20–30 percent reduction in the gender gap.

These inferences are based on standard errors clustered by institution. We also report standard errors clustered by institution and department in Table A4 in the online Appendix and find that our main point estimates remain statistically significant. In this case, the estimate for the gender pay gap in column 4 is significant at the 5 percent rather than 1 percent level. Table A5 reports 95 percent confidence intervals using the wild bootstrap.

The sign of the effects of the reforms on women's and men's salaries vary by specification. In the specifications that include institution and department fixed effects, the salaries of both women and men rise relative to their control groups post-reform (columns 1 and 3), while in the specifications with individual fixed effects, the salaries of men fall relative to control while the salaries of women either do not change (column 2) or fall (column 4). Note, however, that in either case the net effect is a similar relative decrease in the gender salary gap. The difference in estimates across specifications suggests that disclosure was associated with some compositional changes in the units, not just changes in pay setting.[29] To better understand the sources of the changes, in Table A6 of the online Appendix, we replicate Table 4 but vary the control variables used in the specifications without individual fixed effects. We find that the relative increases in the salary levels for women and men disappear once we control for the person's rank (see columns 2, 4, 7, and 9). A possible interpretation of this finding is that following disclosure, departments may have curtailed hiring at junior levels, possibly to pay for salary increases at more senior levels if they faced a budget constraint.

In Figure 5 we conduct a distributional analysis by presenting event-study estimates for the gender salary gap at the twenty-fifth, fiftieth, and seventy-fifth percentiles of the salary distribution estimated using unconditional quantile regressions. The figure shows that the new laws have a greater sustained impact on the gender gap at the twenty-fifth percentile and the median than at the seventy-fifth percentile of the salary distribution.

[28] When individual and year fixed effects are both included, experience is collinear notwithstanding cases in which individuals take unpaid time off or obtain a higher degree after being appointed to the institution. Individual fixed effects nest the institution and department fixed effects, so that institution and department fixed effects need not be explicitly included in the regressions when individual fixed effects are included.

[29] Figure A4 presents estimates of equation (1) corresponding to Figure 4 without the individual fixed effects. Consistent with Figure 4, there is stability in gender salary gap in the years leading up to the reform; further, we again see the relative growth of the salaries of women post reform, leading to a clear impact of the laws on the gender salary gap.

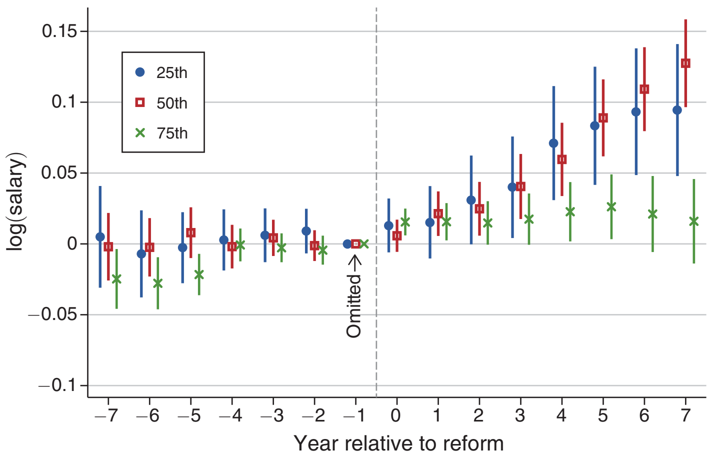

Figure 5. Event Study of the Implications of Pay Transparency for the Gender Salary Gap at Different Points of the Salary Distribution

Notes: This analysis replicates panel B of Figure 4 except that the effects are evaluated at various quantiles in salary, rather than mean salary, using unconditional quantile regressions. See the notes in Figure 4 for more information.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018

C. *Assessing Robustness*

We next investigate the sensitivity of our results to our choice of analysis sample and to specifying department-specific trends in equation (2). Recall that we restrict our primary analysis sample to institutions that appeared in the 2012 wave of the National Faculty Data Pool (NFDP). In Table 5 we present estimates dropping this restriction (columns 1 and 5), or further restricting to balanced samples of institutions (columns 2 and 6) or workers (columns 3 and 7). These changes in sample definitions have little to very modest impact on our estimates of the effect of disclosure on the gender pay gap, particularly in our preferred specification of the peer group, which involves both horizontal and vertical comparisons.[30]

The universities we study are publicly funded, and so salary and employment decisions can be directly affected by cyclical effects on provincial government budgets.[31] While our province-by-year-by-gender fixed effects can account for any effects of these developments by gender, these sorts of budgetary shocks will not necessarily play out similarly across departments. In Table 5 and Figure A8, we also present estimates using our baseline analysis sample and specifying department-specific linear trends to account for these sorts of effects (columns 4

[30] Figures A5-A7 show the event-study plots corresponding to the estimates in this table.

[31] For example, in the period surrounding the disclosure laws in British Columbia, Manitoba, and Ontario, the federal government dramatically reduced its budget deficit in part by cutting transfers to the provinces. The provinces in turn cut transfers to provincial entities including universities, leading to hiring freezes and reduced salary growth.

Table 5—Robustness Checks of the Effects of Pay Transparency Based on the Sample Composition and Model Specification

	Peer group specification							
	Horizontal and vertical comparisons				Horizontal comparisons			
	Full sample (1)	Balanced sample of institutions (2)	Balanced sample of workers (3)	Baseline sample with trends (4)	Full sample (5)	Balanced sample of institutions (6)	Balanced sample of workers (7)	Baseline sample with trends (8)
Salaries of men	−0.013	−0.024	−0.017	−0.023	−0.029	−0.044	−0.034	−0.040
	(0.006)	(0.005)	(0.006)	(0.001)	(0.006)	(0.005)	(0.007)	(0.001)
Salaries of women	0.009	−0.006	0.001	−0.010	−0.010	−0.036	−0.023	−0.030
	(0.009)	(0.010)	(0.010)	(0.002)	(0.007)	(0.008)	(0.007)	(0.002)
Gender salary gap	0.021	0.018	0.019	0.013	0.019	0.008	0.011	0.010
	(0.005)	(0.006)	(0.006)	(0.002)	(0.006)	(0.006)	(0.004)	(0.002)
R^2	0.933	0.932	0.927	0.943	0.933	0.932	0.927	0.943
Observations	501,737	287,617	314,738	379,657	501,737	287,617	314,738	379,657
Clusters	82	36	48		82	36	48	
Individual FEs	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year-Gender FEs	✓	✓	✓	✓	✓	✓	✓	✓
Department time trend				✓				✓
Additional controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This analysis replicates Table 4 except it modifies the sample restrictions or model specification as robustness checks for the baseline findings. As described in the main text, the primary analysis restricts to institutions that appeared in the 2012 wave of the National Faculty Data Pool (NFDP). Columns 1 and 5 in this table repeat the baseline analysis without this restriction of the NFDP. Columns 2 and 6 impose the 2012 NFDP restriction but further restrict to treated institutions that are observed for all 15 years centered on the reform year, or employed at untreated institutions that are observed for all years since 1989. Columns 3 and 7 drop workers at any institution that are not observed for at least 10 years. Since the unit of observation is at the institution-worker level, balancing on workers also implicitly balances on institutions. Standard errors (in parentheses) are clustered by institution except for columns 4 and 8 where robust standard errors are used, due to the larger number of coefficients being estimated relative to the number of clusters. See the notes in Table 4 for more information about the regression specifications. ✓ denotes included in the regression.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018.

and 8). The resulting estimates for the gender pay gap are modestly smaller, indicating a decrease in the gender pay gap of 1.0–1.3 percentage points.

In the online Appendix we also explore the sensitivity of our estimates to other changes in the specification and sample. Our main event-study estimates are based on a window of +/− 7 years around each reform. In Figure A9 and Table A7 we extend the window to +/− 14 years. Note that this requires temporally extending our sample back to 1982 to capture 14 years before the first reforms. Among other effects, adding more data to our sample potentially changes the estimates for any regressors not fully interacted with the year effects. The story in online Appendix Figure A9 is fairly consistent with the one in Figure 4, though there is clearly less stability in the gender salary gap in the early pre-event period, and the post-event impact on the gender salary gap is in excess of 2 percentage points. The regression estimates in online Appendix Table A7 are largely in line with the estimates in Table 4, though there is more consistent evidence that the men's salaries did not rise relative to the control group.

As sample sizes permit, in Table A8 of the online Appendix we present the effect of the transparency laws by province. For the gender salary gap, the majority of the point estimates by province are consistent with the estimates from the pooled specification, although the estimates vary more when we define the peer group based on horizontal comparisons only. Moreover, if we take a simple unweighted average across all of the columns, we find an average estimate that comes very close to our baseline estimate in Table 4 that uses all provinces in the pooled event-study specification. This, in part, addresses the Goodman-Bacon (2021) concern that heterogeneous treatment effects in a difference-in-difference framework may lead to bias if treatment effects vary over time. We note that, for Ontario, there is enough variation to estimate the effects with reasonable precision; given the large share of faculty at academic institutions in Ontario in our sample, are main effects are to a large extent driven by the effects in Ontario.

Lastly, in Table A9 of the online Appendix, we repeat the main analysis but not adjusting salaries to account for differences in calendar year versus fiscal year reporting. As shown, this salary adjustment does not impact our results.

D. *Heterogeneous Treatment Effects*

We next present a heterogeneity analysis, exploring the effects of pay transparency on the gender gap by rank, union status, size of prevailing wage gap in the year of the reform, the degree of exposure, and the calendar period in which the transparency law came into effect. We adopt the specification with individual fixed effects for this exercise to limit the number of estimates.

In the top panel of Table 6, we present the estimates by academic rank. They indicate that the largest effects of transparency on the gender gap are for full professors. This is likely explained at least in part by the fact that assistant professors tend to be hired at approximately the same salaries, and that discretion in pay emerges among senior faculty members. This is consistent with the dynamics of gender pay differentials in other contexts (e.g., Bertrand, Goldin, and Katz 2010; Goldin 2014). To explore this further, we replicate the pay dispersion analysis from Figure 3 separately by rank in Figure A10 of the online Appendix. We find that variation in pay is low among assistant and associate professors but higher among male full professors. This suggests that our pattern of result may potentially be due to the greater bargaining power of full professors.

An important institutional mediator in the Canadian higher education setting are unions, as a large share of faculty are unionized (see Table 2). Unions may play an important role in the response to disclosure, since universities must participate in, and respond to, the formal grievance procedures of unionized workplaces.[32] In contrast, the request for higher pay in a nonunionized environment is more likely to occur through an informal meeting with a department chair, which may be difficult absent an external competing offer from a peer institution. The availability of a formal grievance procedure might particularly benefit women in an environment in which the majority of chairs and senior faculty are men.

[32] Another possibility is that unions directly bargain for redress for women faculty, separate from the institutional responses we document in Table A2.

Table 6—Effects of Pay Transparency by Rank and by Union Status

	Peer group specification					
	Horizontal and vertical comparisons			Horizontal comparisons		
	Assistant (1)	Associate (2)	Full (3)	Assistant (4)	Associate (5)	Full (6)
Salaries of men	0.017	0.001	−0.006	−0.014	−0.001	−0.007
	(0.013)	(0.006)	(0.007)	(0.006)	(0.011)	(0.007)
Salaries of women	0.030	0.008	0.014	−0.009	−0.006	0.013
	(0.009)	(0.007)	(0.009)	(0.006)	(0.008)	(0.009)
Gender salary gap	0.013	0.007	0.020	0.005	−0.005	0.020
	(0.009)	(0.005)	(0.006)	(0.008)	(0.008)	(0.006)
R^2	0.956	0.925	0.917	0.956	0.925	0.917
Observations	74,459	148,736	151,810	74,459	148,736	151,810
Clusters	48	48	48	48	48	48
	Unionized (1)	Not unionized (2)	Difference (3)	Unionized (4)	Not unionized (5)	Difference (6)
Salaries of men	−0.009	−0.025	0.017	−0.026	−0.040	0.014
	(0.006)	(0.003)	(0.007)	(0.005)	(0.006)	(0.007)
Salaries of women	0.020	−0.018	0.038	−0.013	−0.029	0.016
	(0.012)	(0.007)	(0.014)	(0.007)	(0.006)	(0.009)
Gender salary gap	0.028	0.008	0.021	0.013	0.011	0.002
	(0.006)	(0.007)	(0.009)	(0.005)	(0.007)	(0.009)
R^2	0.942	0.946	0.944	0.942	0.947	0.945
Observations	238,959	139,295	378,254	238,959	139,295	378,254
Clusters	38	20	48	38	20	48
Individual FEs	✓	✓	✓	✓	✓	✓
Province-Year-Gender FEs	✓	✓	✓	✓	✓	✓
Additional controls	✓	✓	✓	✓	✓	✓

Notes: The estimates for men and the gender salary gap correspond to the coefficient estimates for the treatment effect (γ_{0+}) and its interaction with the indicator for women (δ_{0+}), respectively, from the econometric specification described in text. The estimates for women are computed as the sum of these two effects ($\gamma_{0+} + \delta_{0+}$). The salary measure used is a base annual rate, which offers a consistent measure of employees' annual earnings, both over time and across institutions. In the top panel, each column conditions on individuals during the years they were employed at the rank of assistant, associate, or full professor, as shown. The analysis excludes individuals with rank below assistant professor because, as described in the main text, pay determination is more heterogeneous for that group. In the bottom panel, union status is assigned to each institution on a yearly basis; most institutions that switched union status did so during the 1970s and 1980s before the time period used in this analysis. Standard errors (in parentheses) are clustered by institution. See the notes in Table 4 for more information about the regression specifications. ✓ denotes included in the regression.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018.

In the bottom panel of Table 6, we present estimates of the effect of the treatment separately based on whether faculty were unionized or nonunionized in the year of the reform. The estimates reveal that the primary effect of transparency laws on the gender pay gap is in unionized workplaces using our preferred peer group specification allowing for horizontal and vertical comparisons. The gender salary gap closed by almost 3 percentage points in response to the introduction of a disclosure law in unionized universities. In nonunion universities, the estimated effect on the gender salary gap is markedly smaller and statistically insignificant. We cannot be certain that this difference by sector is the result of the mechanisms unions provide (discussed above), but this does suggest that the efficacy of the transparency laws turns on something that is different across, rather than common among, union and nonunion universities.

In the top panel of Table 7, we consider the extent to which transparency affects the gender pay gap differently by the size of the pay gap within the peer group in years prior to the reform. To estimate this pay gap, we regress log salary on an indicator for women separately for each peer group and within provinces that enacted a reform. This analysis is implemented at event time -2 to obtain a measure of the pay gap prior to the reforms being implemented. Next, we sort universities on the estimates of the dummy variable for women. The departments with the smallest (most negative) coefficients are predicted to have the largest gender salary gaps in the reform year. All peer groups from provinces with no reform, or from provinces with reforms but that are not observed at event time -2, and peer groups for which a gender salary gap cannot be estimated (such as all-men or all-women faculty) are included in the regressions within the control group.

The expected difference in the effect of the laws by the initial pay gap is ambiguous. For example, a large initial gap may suggest there is significant scope for transparency to improve pay inequality, leading to the largest effect for this group. However, peer groups with a small initial gap may have greater bargaining power for equal pay, more equal distribution of men and women, greater likelihood of union representation, or other factors that correlate with a large effect of transparency in this case.

The results of this analysis are consistent with the latter hypothesis. The pay gap is reduced by roughly 2 percentage points in peer groups with a *small* initial gap (columns 1 and 3), whereas the effect is virtually zero for those with a large initial gap (columns 2 and 4). This finding is interesting because it suggests that, while transparency reduces gender pay differences in the aggregate, these laws are perhaps not sufficient to resolve pay inequality on their own where the problem is most pervasive.

In the bottom panel of Table 7, we examine the effect of disclosure by the degree of exposure within a reference group. Recall that most units either have no faculty whose pay is disclosed or all faculty's pay is disclosed, but that a smaller share of units have partial exposure, whereby some, but not all, faculty have salaries above the threshold, as shown in Figure A2 of the online Appendix. We estimate the impact of the reforms for partially and fully exposed groups relative to nonexposed groups. The results indicate that the impact of the reforms on the gender salary gap is fairly similar in units with full or partial exposure, relative to units with no exposure. When peer groups are defined by both horizontal and vertical comparisons, the estimates for the gender pay gap are the same and, for the peer group based on horizontal comparisons only, full exposure is associated with a reduction of the gender pay gap to a slightly greater extent than partial exposure, although this difference is not statistically significant.

Finally in Table 8, we investigate how the impact of pay transparency laws varies over calendar time. As documented in Table 1, the transparency laws came into effect first in 1996 in some provinces and then later in the period 2012–2016 in other provinces. This has two implications for our analysis. First, because our sample ends in 2018, in our specifications in which we pool all the law changes

Table 7—Effects of Pay Transparency by Size of the Initial Gender Salary Gap and by Level of Exposure

	Peer group specification			
	Horizontal and vertical comparisons		Horizontal comparisons	
	Small initial gender salary gap (1)	Large initial gender salary gap (2)	Small initial gender salary gap (3)	Large initial gender salary gap (4)
Salaries of men	−0.013	−0.016	−0.031	−0.033
	(0.008)	(0.006)	(0.006)	(0.007)
Salaries of women	0.010	−0.016	−0.011	−0.035
	(0.010)	(0.012)	(0.008)	(0.007)
Gender salary gap	0.023	0.000	0.020	−0.002
	(0.007)	(0.006)	(0.006)	(0.007)
R^2	0.930	0.933	0.930	0.933
Observations	248,984	292,006	248,984	292,006
Clusters	48	48	48	48
	Partial exposure (1)	Full exposure (2)	Partial exposure (3)	Full exposure (4)
Salaries of men	−0.019	−0.036	−0.035	−0.049
	(0.006)	(0.008)	(0.007)	(0.010)
Salaries of women	0.002	−0.016	−0.023	−0.031
	(0.010)	(0.012)	(0.007)	(0.008)
Gender salary gap	0.021	0.021	0.012	0.017
	(0.006)	(0.007)	(0.004)	(0.008)
R^2	0.938		0.939	
Observations	378,890		378,890	
Clusters	48		48	
Individual FEs	✓	✓	✓	✓
Province-Year-Gender FEs	✓	✓	✓	✓
Additional controls	✓	✓	✓	✓

Notes: In the top panel, each column is a separate regression and the estimates for men and the gender salary gap correspond to the coefficient estimates for the treatment effect (γ_{0+}) and its interaction with the indicator for women (δ_{0+}), respectively, from the econometric specification described in text. The estimates for women are computed as the sum of these two effects ($\gamma_{0+} + \delta_{0+}$). The salary measure used is a base annual rate, which offers a consistent measure of employees' annual earnings both over time and across institutions. The initial gender salary gap corresponds to the gap that prevailed within the department two years prior to the reform in that province. This gap is estimated as follows. Regress the log of salary on an indicator for women, separately for each department and within provinces that enacted a reform and at event time −2. Then, obtain the coefficient estimates for the indicator for women and sort these estimates. A negative coefficient indicates women earn less than men in the department, on average. The departments with the most negative coefficients are therefore determined to have the largest gender salary gaps in the year. All departments from provinces with no reform, from provinces with reforms but that are not observed at event time −2, or for which a gender salary gap cannot be estimated (such as all-men or all-women faculty) are included in the regressions within the control group. Columns 1 and 3 exclude departments with large initial gender salary gaps, and columns 2 and 4 exclude departments with small initial gender salary gaps. In the bottom panel, columns 1 and 2 are jointly obtained from a single regression and columns 3 and 4 are jointly obtained from another regression, and so the corresponding statistics are only reported once per regression. In this analysis the treatment variable is decomposed into two indicators for whether the percent of individuals whose salaries were revealed in the peer group is above zero but below 100 percent ("partial exposure") or equal to 100 percent ("full exposure"). The reference group is zero exposure. The base annual rate measure of salary is again used. Figure A2 shows the distributions in the percent of exposure in a worker's peer group. Standard errors (in parentheses) are clustered by institution. See the notes in Table 4 for more information about the regression specifications. ✓ denotes included in the regression.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018

Table 8—Effect of Pay Transparency for Early versus Late Adopters

	Peer group specification			
	Horizontal and vertical comparisons		Horizontal comparisons	
	Early (1)	Late (2)	Early (3)	Late (4)
Salaries of men	−0.017	−0.026	−0.035	−0.024
	(0.006)	(0.010)	(0.007)	(0.009)
Salaries of women	0.005	−0.015	−0.023	−0.020
	(0.011)	(0.012)	(0.008)	(0.008)
Gender salary gap	0.021	0.011	0.012	0.005
	(0.007)	(0.010)	(0.005)	(0.003)
R^2	0.933	0.927	0.934	0.927
Observations	343,438	190,756	343,438	190,756
Clusters	42	24	42	24
Individual FEs	✓	✓	✓	✓
Province-Year-Gender FEs	✓	✓	✓	✓
Additional controls	✓	✓	✓	✓

Notes: This analysis replicates Table 4 except that the regressions condition on early or late adopters. Specifically, columns 1 and 3 exclude Nova Scotia, Alberta, and Newfoundland and Labrador, whereas columns 2 and 4 exclude British Columbia, Manitoba, and Ontario. Observations from the provinces of New Brunswick, Prince Edward Island, Quebec, and Saskatchewan never adopted pay transparency and are always included in the control group. Standard errors (in parentheses) are clustered by institution. See the notes in Table 4 for more information about the regression specifications. ✓ denotes included in the regression.

Source: Statistics Canada, University and College Academic Staff System, 1989 to 2018

together, their short-term effects are estimated from a different sample of provinces than the long-term effects. Second, it is plausible that gender norms at Canadian universities change over time, so the impact of the laws in the mid-1990s might differ from their impact 15–20 years later. In Table 8 we present the estimates from a specification in which we allow the effect of the laws to be different between the early and late adopters. The results indicate that the effects are larger for the early adopters. This may be because, as documented in Figure 2, the 1990s was a period in which there were larger gender disparities in compensation. It may also be due to the fact that, for two of the three late adopters, we only have two or three years of post-reform observations, and so we can only estimate immediate impacts.

VI. Conclusion

This paper examines the effect of transparency laws on the gender pay gap. While we focus on public sector salaries, the ongoing efforts of governments around the world to increase transparency of wages in the private sector may allow researchers to determine if the effects we document hold in other sectors of the economy.

There are several directions for future research. First, our estimates are informative about the partial equilibrium impacts of transparency. It is possible that transparency laws have spillover effects through broader changes in social norms and, thus, the general equilibrium effects of these laws may be different. Second, transparency laws are complex and vary in their nature. One can distinguish between "active" disclosure whereby salaries are easily accessible online or "passive" disclosure in which salaries are only available upon request. These two forms of disclosure may not have the same equilibrium effects on salaries. For example, salaries that are available online may garner significantly more media attention and public pressure for adjustment. Additionally, the lower cost of access means that they are more likely to be used to a greater extent in bargaining with employers.

Finally, the similarity between our results and the results of Bennedsen et al. (2022) is perhaps surprising given the differences in the nature of the transparency laws. Those results show that transparency laws can reduce gender gaps—without identifying individuals. As Cullen and Perez-Truglia (2018) have shown, non-anonymous information may harm well-being for those who earn less. Indeed, these authors have shown that employees like anonymous transparency, as in Denmark, but exhibit a distaste for non-anonymous transparency, as in Canada, and are often willing to pay a significant amount of money to keep their salaries private. This may have implications for policymakers who are considering transparency laws as a way to reduce gender gaps.

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