The Effect of Labor Relations Laws on Unionization Rates within the Labor Force: Evidence from the Canadian Provinces

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Abstract:

We provide evidence on the distributional effects of labor relations reforms by relating an index of the favorableness to unions of Canadian provincial labor relations laws to changes in industry, occupation, education, and gender-specific provincial unionization rates between 1981 and 2012. The results suggest that shifting every province's 2012 legal regime to the most union-favorable possible would raise the national unionization rate by no more than 3 percentage points in the long run. Moreover, while there is some evidence of larger gains among high-school educated workers, the differences across groups are small and in some cases suggest larger gains among professionals. Overall, the results suggest a limited potential for reforms in labor relations laws to mitigate growing labor market inequality.

Keywords: Labor relations legislation; union density rates; union-management relations
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1. Introduction

According to data from the OECD, union membership as a proportion of the workforce declined in all but five OECD countries between 1980 and 2010.[1] In Australia, New Zealand, the U.K., and the U.S. the declines were particularly dramatic. While there are sharply diverging views on whether a smaller role for unions in labor markets is desirable, there is little disagreement that it matters. On the one hand, unions have been shown to reduce corporate profits, investments and dampen employment growth. On the other hand, unions have clear beneficial impacts on the wages, fringe benefits and working conditions of unionized workers.[2] Moreover, recent evidence examining potential spillover effects of unions on nonunionized workers suggests that the decline of unions in the U.S. may have had much broader distributional effects than previously thought (Western and Rosenfeld 2011). Consistent with this evidence, the set of Anglo-Saxon countries that have experienced the largest declines in unionization internationally, have also experienced the largest increases in inequality. These developments are resulting in heightened interest in the potential for policies aimed at reversing deunionization trends to mitigate growing labor market inequality.[3]

Whether unionization can provide a policy lever to affect inequality depends critically on the extent to which deunionization has been a consequence of government policies (and can therefore potentially be reversed through policy), as opposed to an inevitable development driven by broad globalization and deindustrialization trends. The relative stability of unionization rates in Canada, despite its legal, political and cultural similarities and close economic ties to the U.S., suggests that the phenomenon was not inevitable. Comparing survey and opinion poll data, Riddell (1993) finds that the vast majority of the Canada-U.S. gap in unionization rates cannot be accounted for by structural economic differences or social attitudes and infers that the gap is most consistent with differences in legal regimes. Following on this evidence, there now exists a substantial Canadian empirical literature linking changes in provincial labor relations laws to administrative data on certification success rates (Martinello 1996; Martinello 2000; Johnson 2002; Riddell 2004, Bartkiw 2008), applications for certification (Johnson 2004), as well as successful negotiations of first contracts (Riddell 2013).[4] This research consistently finds a

[1] Exceptions are Belgium, Chile, Iceland, Norway and Spain. The data are from: http://stats.oecd.org/ and measure the proportion of the workforce that are union members.

[2] For reviews of the evidence on the economic effects of trade unions, see Addison and Hirsch (1989), Kuhn (1998) and Hirsch (2004a, 2004b).

[3] For a formal analysis of the link between deunionization and inequality trends across OECD countries see Jaumotte and Buitron (2015).

[4] Directly relating labor relations laws to unionization is more difficult in the U.S. and U.K. where labor law falls under the federal jurisdiction and, therefore, provides no cross-sectional variation. Consequently, one has to rely exclusively on time-series variation to identify the effects of laws. This is the approach of Freeman and Pelletier (1990) and Farber and Western (2002). An exception is at the local government level within the U.S., where laws

significant effect of the labor relations regime on the ability of unions to organize new bargaining units. Of particular importance appears to be rules for certification and for insuring that a first contract is successfully negotiated.[5]

In establishing that labor relations laws matter for union formation, the current literature is both extensive and highly compelling. However, in informing the potential for legal reforms to not only reverse deunionization trends, but also mitigate inequality trends, it falls short in two key respects. First, changes in unionization rates at the aggregate level depend not only on the rate of organizing new union members, but also on relative changes in employment levels within the union and nonunion sectors, including those resulting from expansions and contractions of existing bargaining units and firm closures (Farber and Western 2001). The current literature has, however, largely overlooked the effect that labor relations laws have on employment levels. For example, in examining the impact of mandatory certification votes on the Canada-U.S. union density gap, Johnson (2004) explicitly assumes that the law has no impact on employment. One would, however, expect such effects to be important as a more union-friendly legal environment, for example, affects employers' perceived threats of unionization or their relative bargaining power and, in turn, investment, capital utilization, scale, and locational decisions.

To identify the general equilibrium effects of labor relations reforms, including employment effects, one has to relate the cross-sectional and/or time-series variation in laws directly to unionization rates. We are aware of four studies that do so: one using Canadian data (Martinello and Meng 1992); one British (Freeman and Pelletier 1990); and two from the U.S. (Freeman and Valletta 1998; Farber 2005). However, the evidence provided in these studies falls short in another key respect. By restricting the effect of legal reforms to be identical across workers within the labor force, these papers tell us nothing about where in the earnings distribution unionization rates are expected to increase most.[6] However, from a standard model of rational union organizing activity (Pencavel 1971), we expect that legal reforms will primarily affect workplaces where the net marginal benefit of organizing a new bargaining unit is close to zero. The reason is that where the net benefits of unionization are large, workers will already have incentive to unionize regardless of small changes in legislation. Where unionization is very costly, on the other hand, small reductions in the marginal cost

vary across occupation groups (e.g., firefighters, police and teachers). This variation is exploited by Freeman and Valletta (1998) and Farber (2005). For an earlier review of the evidence, see Godard (2003).

[5] For evidence of the alternative view that deunionization trends in Canada and the U.S. are primarily driven by broader economic factors beyond the influence of public policy and therefore unlikely to be reversed through labor relations reforms, see Troy (2000, 2001).

[6] There is, of course, evidence on how rates of deunionization have varied across worker types. For example, we know that deunionization has been particularly dramatic among men employed in manufacturing. But, this does not necessarily tell us anything about how legal reforms affect workers differentially. There is also evidence that the existence of unions serves to reduce earnings inequality among men, but have little impact on inequality among women (Lemieux 1993; Card 1996). But again, this does not tell us anything about the effects of legal reform, which are likely to affect the unionization rates of some types of workers more than others.

of unionization resulting from legal reforms will be insufficient to alter unionization decisions. It is where the net benefit of unionization is close to zero and becomes more positive as the result of legal reforms that changes in unionization will occur. The question is where are these workplaces? And, in informing the potential for legal reforms to address inequality, are these workplaces employing workers who fall in the lower end of the earnings distribution?[7]

In this study we provide evidence on the distributional effects of labor relations reforms by relating an index of the favorableness to unions of each Canadian province's labor relations regime to its unionization rates estimated within industry, occupation, education, and gender groups over the 1981-2012 period. To estimate these rates we rely on nationally-representative survey data, as opposed to the administrative data that currently predominates the literature. The advantage of the Canadian setting in doing this analysis is that the legislative jurisdiction primarily lies at the provincial level, rather than the national level, as it does in the U.K. and U.S., thereby allowing us to disentangle policy effects from the effects of broader unobserved economic fluctuations correlated with the timing of legal changes. Moreover, given the contentiousness of these laws, changes in governing provincial parties has resulted in significant historical swings across Canadian provinces and over time in the favorableness of provincial laws to unions, thereby providing substantial policy variation to identify effects.

To identify the distributional effects of legal reforms, we use a dynamic feasible generalized least squares (FGLS) estimator that conditions on a full set of province and year fixed effects, as well as provincial-level measures of unemployment, inflation, the manufacturing share of employment, and public opinion of unions. The aggregate results suggest that shifting every Canadian province's current legal regime to the most union-favorable possible (within the set of laws considered) would raise the national unionization rate in the long-run by no more than 3 percentage points, from its current value of 30%. Moreover, distinguishing the effects across industries, occupations, education, and gender groups, we find little evidence that legal reforms will have a greater impact among workers who in the absence of unions would fall at the lower end of the earnings distribution. In fact, comparing across occupation groups, the results, if anything, point to largest gains among professionals. Overall, our findings suggest a limited potential for reforms in labor relations laws to counter growing wage inequality.

The remainder of the paper is organized as follows. In the following section we describe our empirical methodology for estimating the effects of legal reforms on provincial-level unionization rates. In the third section, we describe the data we use to estimate the model and

[7] The only evidence we have found on distributional effects in the existing literature is from Farber and Western (2002), who examine the effects of the U.S. air-traffic controllers' strike in 1981 and the Reagan NLRB appointment of 1983 on the number of certification applications (but not unionization rates more generally) separately by industry and occupation groups.

in the fourth section we discuss our findings. The paper concludes with a discussion about the practical policy relevance of our findings.

2. Methodology

Modelling the decision of a union to invest the resources necessary to organize a new bargaining unit involves an optimization problem in which unions compare the relative marginal costs and benefits of additional membership (Pencavel 1971). By influencing these costs and benefits, small changes in the legal environment can potentially alter optimal behaviour, thereby initiating organizing activities in a particular workplace and, in turn, the per-period flow of workers transitioning from the nonunion to union sector.[8] Ideally, we could estimate the effect of legal changes directly on these worker-level flows across different types of workers. However, this requires large samples of longitudinal microdata with information on workers' union status and either demographic characteristics or earnings going back to at least the early 1990s, when the key historical variation in laws began. Such data for Canada do not exist.[9] We can, however, estimate provincial unionization rates for particular types of workers using repeated cross-sections of nationally-representative household survey data. But this requires that we think carefully about how changes in the per-period flows of workers in and out of the union sector resulting from changes in labor relations laws affect unionization rates in the long-run.

Assuming for simplicity a two-state national labor market in which all workers are employed in either the union or nonunion sector, the unionization rate in any year t can be expressed as:

$$U_t = (1 - p_{un})U_{t-1} + p_{nu}(1 - U_{t-1}) \tag{1.1}$$

where p_{un} and p_{nu} are the union-to-nonunion and nonunion-to-union transition probabilities, respectively. That is, in a world with no possibility of non-employment, the unionization rate is equal to the proportion of the previous year's union members that maintain their union status into the next year plus the proportion of the previous year's nonunion members that switch to the union sector. Rearranging terms, equation (1.1) can be rewritten as the first-order Markov process:

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[8] Similarly, legal changes could influence the marginal cost of decertifying an existing bargaining unit, which would instead increase union-to-nonunion transitions. However, since decertifications are relatively rare, we focus our discussion on certifications.

[9] A possible exception is the Longitudinal Administrative Databank (LAD), which links T1 income tax returns of individuals going back to the early 1980s.However, unlike the survey data we employ, the LAD do not provide any information on workers' education levels or occupations.

$$U_t = (1 - p_{un} - p_{nu})U_{t-1} + p_{nu}\,. \tag{1.2}$$

Assuming the per-period flows p_{un} and p_{nu} are constant over time and sufficiently small so that $1 - p_{un} - p_{nu} > 0$, this process implies a steady-state unionization rate given by:

$$U^* = \frac{p_{nu}}{p_{un} + p_{nu}}, \tag{1.3}$$

which is strictly increasing in the nonunion-to-union transition rate p_{nu} and strictly decreasing in the union-to-nonunion transition rate p_{un}.[10]

Equation (1.2) implies that one can recover the underlying transition probabilities by regressing aggregate unionization rates on their own lagged values. The intercept in the model identifies the numerator in equation (1.3); the coefficient on the lagged dependent variable identifies the denominator; and together this provides two equations to solve for p_{un} and p_{nu}. Moreover, assuming that legal reforms favorable to unions raise unionization rates by permanently increasing the nonunion-to-union transition rate p_{nu}, one could identify this effect on the long-run unionization rate by allowing the legal reform variable to interact with both the overall intercept and the lagged dependent variable (since p_{nu} appears in both the intercept and the lagged dependent variable terms in equation (1.2)).

Of course, changes in unionization rates over time are driven by numerous factors, some of which may be correlated with the timing of provincial changes to labor relations laws. The key empirical challenge is, therefore, to separately identify the effects of the laws from other factors. To do so, we extend the model implied by equation (1.2) by controlling for province and year fixed effects, as well as a set of province-level covariates intended to capture province-specific trends in unionization rates that may be correlated with legislative changes. Specifically, we estimate the linear model:

$$U_{pt} = \alpha U_{p,t-1} + \delta R_{pt} + \theta(U_{p,t-1} \cdot R_{pt}) + x'_{pt}\beta + c_p + y_t + \varepsilon_{pt} \tag{1.4}$$

where R_{pt} is an index of the favorableness to unions of the provincial labor relations regime that exists in province p in year t; x_{pt} is a vector of control variables intended to capture underlying province-specific trends in unionization, which includes the inflation rate (based on the all-items CPI), the unemployment rate (age 25 and over), the manufacturing share of employment, and an estimate of public opinion of trade unions based on opinion poll data; c_p and y_t are province and year fixed effects, respectively; and ε_{pt} is an error term with an expected value of 0, but potentially non-spherical variance-covariance matrix.[11] Given variation over time in R_{pt} within at least one

[10] This can be derived by either solving the infinite geometric series obtained by substituting in for U_{t-1} or from simply equating $U_t=U_{t-1}$.

[11] See Section 3.3 for detailed descriptions of each of the control variables.

province, all the parameters of equation (1.4) are identified. Equating U_{pt} and $U_{p,t-1}$, the estimates of equation (1.4) imply an expected steady-state unionization rate U_p^*, which depends on all the parameters of the model.[12] Moreover, using unionization rates estimated for different subgroups of the labor force, such as more or less educated workers, we obtain evidence of the distributional effects of legal reforms.

It turns out that the term containing the interaction of the lagged dependent variable and the legal index is poorly identified in our data. To address this problem, we compare our estimates of the long-run policy effect at the provincial level to those obtained when we impose the restriction $\theta = 0$, so that legislation only affects the intercept through δ.[13] Having shown that the implied steady-state effects are similar whether the interaction term effect θ is estimated or not, we estimate the effects for subgroups of the population using the restricted model.

It is well known that a consequence of including the lagged unionization rate in equation (1.4) is that the ordinary least squares (OLS) estimates are biased. They are, however, consistent if the error term ε_{pt} contains no serial correlation. Using a Breusch-Godfrey test of autocorrelation based on the OLS fitted errors from estimating equation (1.4) we are unable to reject the null hypothesis of no serial correlation.[14] However, efficiency gains can be made using a feasible generalized least squares (FGLS) estimator that estimates the structure of the variance-covariance matrix of the error term. We therefore begin by comparing the estimates across four estimators: OLS; FGLS with province-specific heteroskedasticty; FGLS with province-specific heteroskedasticity and spatial correlation; and FGLS with province-specific heteroskedasticity, spatial correlation, and province-specific autocorrelation.[15] Reporting separate results for the

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[12] Equating U_{pt} and $U_{p,t-1}$ in equation (1.4), we obtain the expected steady-state unionization rate:

$$U_p^* = \frac{\delta R + W}{(1-\alpha-\theta R)}$$

where $W = x'_{pt}\beta + c_p + y_t$. Taking the derivative of this term with respect to the legal index R implies an effect on the steady-state unionization rate given by:

$$\frac{\partial U^*}{\partial R} = \frac{\delta(1-\alpha)+\theta W}{(1-\alpha-\theta R)^2}.$$

[13] In this case, the effect of a marginal change in the legal index on the steady-state unionization rate is simply $\partial U^* / \partial R = \delta / (1-\alpha)$.

[14] We also performed tests of: (i) the poolability of the parameters across provinces; (ii) heteroskedasticity; and (iii) stationarity. The results are discussed in the notes of Table 4.

[15] If the variance-covariance matrix of the error term ε_{pt} is given by Ω, then in the most flexible case we estimate:

$$\Omega = \begin{bmatrix} \sigma_1^2\Omega_1 & \sigma_{1,2}I & \cdots & \sigma_{1,10}I \\ \sigma_{2,1}I & \sigma_2^2\Omega_2 & \cdots & \sigma_{2,10}I \\ \vdots & \vdots & \ddots & \vdots \\ \sigma_{10,1}I & \sigma_{10,2}I & \ldots & \sigma_{10}^2\Omega_{10} \end{bmatrix}.$$

models with and without the θ interaction term discussed above, we obtain eight sets of estimates. As it turns out, the estimated steady-state effects of policy reform are remarkably robust across specifications. Given the statistical challenge of identifying these effects for particular subgroups of the population, we take as our preferred specification the estimator with a smallest variance and then examine the robustness of the estimates to: (i) including province-specific linear time trends to capture any possible remaining latent provincial trends correlated with legal reforms; (ii) sample weights based on the underlying number of observations used to estimate the provincial unionization rates; and (iii) an alternative source of data on unionization rates based on administrative data on union membership. We conclude our analysis by estimating the distributional effects of legal reform by comparing the magnitude of the long-run estimated effects across industry, occupation, education and gender groups.

3. Data

3.1. Unionization rates

Obtaining estimates of the legal regime effects in equation (1.4) with enough statistical precision to be informative requires time-series data on provincial unionization rates going back to at least the early 1990s. Moreover, to identify variation in the magnitude of these effects across different types of workers requires estimation of conditional unionization rates within each province. To obtain these rates, we patch together a series of nationally-representative household surveys spanning the 1981-2012 period. Specifically, we use the Survey of Work History (SWH) for 1981; the Survey of Union Membership (SUM) for 1984; the Labor Market Activities Survey (LMAS) from 1986 through 1990; the Survey of Work Arrangements (SWA) for 1991 and 1995; the Survey of Labor and Income Dynamics (SLID) for 1993, 1994 and 1996; and the monthly Labor Force Survey (LFS) from 1997 through to 2012. With the exception of the 1981 SWH, all these files identify not only union membership, but also nonunionized workers whose terms of employment are covered by union contracts.[16] In addition, these data allow us to consistently distinguish four industries of employment: primary, manufacturing, private services, and public services (where public services are defined broadly, including provincial and municipal government employees, education and healthcare sector workers, and workers employed in electric power, gas and water utilities); three occupations: blue-collar, administrative and professionals; three education levels:

Not allowing province-specific serial correlation imposes that the diagonal matrices Ω_j are all equal to a $T \times T$ identity matrix; not allowing spatial correlation imposes that all the off-diagonal elements $\sigma_{i,j}$ are zero; and not allowing for heteroskedasticity imposes that σ_j^2 is a constant equal to σ^2.

[16] See Table A.1 in the appendix for detailed descriptions of the household survey data sources. For the 1981 SWH rates we use the 1984 SUM to estimate the difference between unionization rates using the coverage and membership definitions and add this difference to the 1981 rates.

high school or less, post-secondary diploma/certificate and university degree; and gender.[17] Employees of the federal government are dropped for all years (except 1991 and 1996 when they are not separately identified), as labor relations for these workers is governed by a separate federal statute. This complication is discussed in more detail in the following subsection.

In Table 1 we consider long-term declines in unionization rates across provinces and worker types by comparing rates in 1981 and 2012. The estimates point to relatively large declines in New Brunswick, British Columbia and Alberta; in manufacturing and private services; and among men. In most cases, the three-decade decline in unionization is more than twice as large for men as women, whether measured in terms of the change in the level of the rate or the proportionate change. There appears relatively little difference in deunionization trends across broad occupation groups, although in the two western-most provinces – Alberta and British Columbia – the overall declines have clearly been much larger among blue-collar workers. The ubiquity of these trends across provinces, as well as the large gender difference, emphasizes that an important part of the deunionization trends are driven by factors beyond labor relations laws. The empirical challenge is to determine to what extent the declines in Table 1 reflect changes in provincial labor relations laws.

The are two significant limitations of the household survey data that we employ: (i) missing years (specifically 1982, 1983, 1985, and 1992); and (ii) substantial sampling biases in the estimation of unionization rates arising from the limited sample sizes, particularly prior to 1997 when the Canada's monthly Labor Force Survey (LFS) first introduced a question identifying union status. To provide ourselves with some confidence in the accuracy of our estimated provincial time-series prior to 1997, we compare our estimates to those obtained using comparable provincial time-series data based on mandatory union filings under the *Corporations and Labor Unions Returns Act* (CALURA). Specifically, prior to 1996 all unions with members in Canada were required to file an annual return in December of each year reporting the total number of union members within each union local. These counts were then aggregated at the provincial level and published annually by Statistics Canada. To obtain provincial unionization rates we divide these membership levels by estimates of provincial employment from the LFS. This provides us with unionization rates from 1976 to 1995, which can be combined with the 1997 to 2012 LFS data to produce a complete series. However, to make the LFS series consistent with the CALURA, for this comparison series we exclude from the LFS data employees who are covered by union contracts, but not union members.[18]

[17] Detailed definitions are available from the authors on request.

[18] There are two significant complications in comparing the LFS and CALURA rates. First, unions with less than 100 members did not have to provide information in the CALURA. This will tend to underestimate unionization rates in the CALURA relative to the LFS. On the other hand, CALURA membership counts include union members who are not currently employed, such as workers on temporary layoff, and are recorded as of December 31 of each year,

The resulting provincial time-series of unionization rates using both the household survey data (labeled HS-LFS) and CALURA (labeled CALURA-LFS) are plotted in Figure 1.[19] Consistent with Table 1, both data sources point to larger declines in New Brunswick, Alberta and British Columbia. However, in all provinces the long-term declines are smaller in the CALURA-LFS series. In fact, in Prince Edward Island, Nova Scotia, Quebec, Manitoba and Saskatchewan there is little or no evidence of a long-term secular decline in unionization in the administrative data. One possible explanation is that deunionization has occurred primarily through a decline in workers covered by union contracts, as opposed to union membership. Indeed, to some extent, this has been the experience in Australia, the United Kingdom and New Zealand, where declines in union coverage rates since the early 1980s have exceeded declines in union membership rates (Schmitt and Mitukiewicz 2011).[20]

The key advantage of the survey data is that it allows us to estimate unionization rates for particular subgroups of the population. Before considering the role of labor relations laws, we examine to what extent Canadian deunionization trends can be accounted for by compositional shifts in employment across provinces, industries, occupations, education groups and gender. For example, unionization rates have always been higher in the manufacturing sector than in private services. Consequently, employment shifts away from manufacturing towards services, will push aggregate unionization rates downwards for reasons unrelated to labor relations laws. To quantify the role of these compositional shifts more generally, we compare the estimates from two different regressions, the results of which are reported in Table 2. In the first, we pool the aggregate provincial-level HS-LFS unionization rates plotted in Figure 1 and regress them on linear (specification 1) or quadratic (specification 2) time trends. In the second, we do the same thing using unionization rates estimated at the level of a particular province-industry-occupation-education-gender group. With 32 years of data this gives us 320 observations in the first case (32 x 10 provinces) and 23,040 in the second (32 x 10 provinces x 4 industries x 3 occupations x 3 education groups x 2 genders). Estimating the unionization rates at this detailed level compromises the precision of the estimates significantly. However, since there is no reason to believe that the expected value of this measurement error is correlated with a trend (although

when seasonal layoffs are typically highest. Consequently, dividing by December employment levels tends to overestimate unionization rates, particularly for the Atlantic Provinces where seasonal layoffs are most prevalent. To limit this measurement error, we instead use employment levels estimated using the July LFS files. For detailed information on the comparability of the CALURA and LFS data, see Table A.2.

[19] Note that we are missing some years in both time series. The CALURA are missing 1996 and with the series based on survey data are missing 1982, 1983, 1985, and 1992. To fill in these gaps we use a simple linear interpolation of the neighbouring years. For 1985, 1992, and 1996, this is simply an average of the values for the years on either side of the missing year. For 1982 and 1983 we use a weighted average (e.g. 1982 is two-thirds of the 1981 value and one-third of the 1984 value).

[20] Another difference with the CALURA data series is that professional organizations certified as unions, such as teachers federations and nurses associations, were not included prior to 1983 (Mainville and Olinek 1999). This will tend to understate unionization rates in the early 1980s, resulting in flatter profiles over time.

its variance is decreasing due to larger sample sizes beginning with the LFS in 1997), it should not bias our estimates.

The first two columns of Table 2 point to a downward trend in unionization when the rates from all provinces are pooled. The linear specification points to an annual decrease of 0.37 percentage points, while the quadratic specification suggests that the rate of decline is decreasing such that by the end of our sample period, rates have stabilized (the slope of the time trend is -0.0065 x 0.0002*$time$, where $time$ is equal to 32 in 2012). To the extent that this declining trend reflects employment shifts across groups, it should not be evident within groups. However, the third and fourth columns of Table 2 suggest only slightly smaller rates of decline when we use the group-specific unionization rates. The linear specification now suggests an annual decline of 0.31 percentage points, while the quadratic specification suggests rates stabilized by 2009. These results imply that something more than structural economic shifts are responsible for decreasing Canadian unionization rates over the past three decades.

3.2. Labor relations legal index

The current literature has taken one of three approaches to empirically identifying the effects of labor relations laws on unionization rates. The first is to focus on the effects of particular types of regulations, such as automatic certification or first-contract arbitration. While focusing on a particular regulation makes interpreting estimates relatively straightforward, new regulations are seldom introduced in isolation, so that the estimates potentially capture the effects of concomitant legal changes. To identify the independent effect of particular regulations, other features of the legal regime need to be controlled for, but knowing what these features should be is unclear. Moreover, because the legal changes are highly collinear, disentangling their independent effects with meaningful statistical precision becomes a challenge. An alternative strategy is to focus on the effects of political regime changes where there has been a clear and significant shift in the favorableness of legal regime to unions. Martinello (2000), using data from the Canadian province of Ontario, and Farber and Western (2002), for the U.S., provide examples of this strategy. Unfortunately, these types of regime switches are rare. A third approach, which we follow in this paper, is to exploit variation across a broad set of regulations, but combine the variation into an overall index capturing the favorableness to unions of the law. This is the approach of Freeman and Valletta (1988) and Farber (2005), who examine unionization rates of U.S. public sector workers, and Freeman and Pelletier (1990), who examine long-term changes in the U.K. national union density rate.

The advantage for us in employing an index is twofold. First, the primary objective of our analysis is to identify the potential for broad shifts in provincial labor relations regime, as opposed to specific types of regulations, to differentially affect the unionization rates of different groups of workers. By using an index, we obtain estimates of a *single* coefficient, the magnitude of which can be compared in a straightforward way across different samples of workers to obtain evidence on where legal changes are likely to have their biggest impact. Second, by pooling all the variation in a single variable, we estimate these effects with greater statistical precision, so that differences in the magnitudes of the estimates across groups are less likely to reflect random sampling error. This efficiency gain, however, comes at a cost. In constructing the index, one has to arbitrarily set weights on the relative contributions of the individual regulations to the index. To the extent that the weights chosen are incorrect, the resulting index will provide an inaccurate measure of the favorableness to unions of a province's legal regime. However, as Freeman and Pelletier (1990) emphasize, the effect of this measurement error should be to attenuate the estimated effects. Since we are primarily concerned with the relative differences in the magnitude of the estimated effects, as opposed to their overall levels, this bias is of secondary importance in our analysis.

In constructing our index, we consider three broad areas of labor relations laws that have been shown in the existing literature to impact unionization rates: regulations of (i) union recognition; (ii) first-contract negotiations; and (iii) strikes. With respect to (i), the key policy variation in the Canadian setting is whether new bargaining units can be certified solely through union card checks or whether secret-ballot certification elections are required (Johnson 2002). However, as emphasized by Ferguson (2008), certification is only the first hurdle in union formation as only one-seventh of newly certified bargaining units successfully negotiate a first contract within a year of certification. In recent work, Riddell (2013) shows that providing unions with automatic and timely access to first-contract arbitration, of which there is also considerable variation both within and across provinces over time, significantly increases the likelihood that newly certified units obtain first contracts. Finally, with respect to (iii), we follow Budd (2000) and distinguish between jurisdictions that: (i) ban all replacement workers; (ii) ban permanent replacement workers (but allow temporary); (iii) ban only professional replacement workers; and (iv) have no ban on replacement workers. In addition, we exploit variation on whether a conciliation process, cooling-off period, and/or strike vote is required before a union is in a legal strike position.

The data on each of these regulations across Canada's 10 provinces is presented in Table 3 separately by laws that are favorable and unfavorable to unions. Since our earliest data begins in 1976, we indicate with "76" instances where the regulation was already effective in 1976. Otherwise, the dates in the table indicate years in which the regulation became effective. If no second date is given, the regulation was effective in 2012, the end of our sample period. To construct the index, we assume each of the 3 main areas described above additively contribute a value between 0 and 1 (so the index falls between 0 and 3, with larger values indicating more union-favorable regimes). These values are determined as follows:

i. **recognition**=1 if certification through card check is possible; =0 if a secret-ballot certification elections are required
ii. **bargaining**=1 if union has right to first-contract arbitration ; =0 otherwise
iii. **strikes**=(replacement+wait+vote)/3, where replacement=1 if ban on all replacement workers, =2/3 if ban on permanent, =1/3 if ban on professionals, =0 if no bans; wait=1 if no conciliation or cooling-off, =1/2 if conciliation or cooling-off, =0 if conciliation and cooling-off; and vote=1 if no strike vote required, =1/2 if strike vote required only if demanded by employer, =0 if mandatory vote. [21]

Finally, in order to make the estimated effect of a marginal change in the index easier to interpret, we standardize the index by subtracting the average across the 370 values of the index in the data (37 years times 10 provinces) and dividing by the sample standard deviation.

In addition to the ambiguity about what the relative weightings of the regulations should be within the index, there is another possible source of measurement error in our index. The rules governing the formation, operation and destruction of union bargaining units in Canada is normally specified by the labor relations code of the province in which an employee works. However, not all workplaces within a province are governed by these provincial statutes. For example, labor relations for employees of the federal government are governed by the *Public Service Labor Relations Act (PSLRA)*, while employees in federally-regulated industries, such as air transportation and uranium mining, are regulated by the *Canada Labor Code*. Provincial civil servants, police, firefighters, teachers, and hospital workers on the other hand are, in some cases, but not all, governed by separate statutes. Ideally, one could separately identify each of these exceptional cases in the data in order to relate the relevant legislation to unionization rates of each employee group. However, with the exception of the federal government employees, the level of industry and occupation detail provided in the data is inadequate.

However, as we have emphasized, our primary objective is to identify the effect of legal environment broadly defined. When governments change provincial statutes, the effects are likely to not only have spillover effects on workers falling under separate statutes, but are also likely to be correlated with other legal decisions that affect the broad legal environment and, in turn, the unionization rates of excluded groups. For example, special statutes typically exist primarily to regulate the right to strike where employees are providing services deemed essential. Consequently, key regulations affecting unionization rates, such as rules for certifying new bargaining units, are taken from the overriding provincial statutes on which are index is based. Moreover, in some cases amendments to provincial statues coincide with comparable changes in the special statutes. And third, it may be that political swings that result in legislative

[21] In years in which a law is introduced, the law instead contributes a fraction (as opposed to 0 or 1), where the fraction represents the proportion of the year that the law was effective.

changes lead to broad changes in the labor relations environment within a province. To take a particular example, a change in government to a relatively labor-friendly administration, may lead to both a more union-friendly legal regime and an increasing reluctance of the government to force, through legislation, public sector workers who are in a legal strike back to work, which could influence subsequent employment growth and thereby membership. The key point is that in not excluding public-sector employees (with the exception of federal civil servants) from our analysis, we potentially capture the effect of broader changes in the labor relations climate within a province. Given that we are primarily interested in the distributional effects of the labor relations reforms and that changes in labor relations laws rarely happen in isolation, we think that this broad scope is most relevant.

In Figure 1, we plot our labor relations index for each province alongside the provincial unionization rate estimated using both the household (HS-LFS) and administrative-household (CALURA-LFS) data. Three features of the index stand out. First, and most important, there is variation in the legal index both across provinces and within provinces over time (except New Brunswick), allowing us to identify the effect of the labor relations legal regime on provincial unionization, while conditioning on both year and province fixed effects, as well as a set of province-level controls intended to capture province-specific trends. Second, provinces that have historically had legislation more favorable to unions – Quebec, Manitoba and British Columbia – have tended to have higher rates of unionization, whereas provinces with historically unfavorable laws -- Nova Scotia and Alberta – have had lower rates. On the other hand, the relationship is complicated, as Manitoba and Saskatchewan have historically had very similar rates of unionization, despite Manitoba having a more union-friendly labor relations environment throughout the 1980s and the first half of the 1990s. Third, and perhaps most striking, Canadian labor relations laws have not tended to become less favorable to unions over time, despite secular downward trends in unionization rates in nearly all provinces (at least based on the survey data that counts workers covered by union contracts). This, of course, does not mean that laws do not matter; but, it does suggest that something beyond laws is primarily responsible for the Canadian deunionization experience of the past three decades.

3.3 Control variables:

To control for the broader trends that are common across provinces we include a full set of year fixed effects. However, as is evident in Figure 1 and Table 1, deunionization has clearly been stronger in some provinces – New Brunswick, Alberta and British Columbia – than in others – Newfoundland, Manitoba and Saskatchewan. We, therefore, also include a set of control variables that employ province-specific data, as well as examine the robustness of the estimates to including province-specific linear trends. Below we justify our choice of controls and describe the data we employ.

Inflation rate:

In periods of high inflation workers' real wages are often eroded. An important benefit of unionization is that unions typically negotiate clauses in collective agreements providing members with automatic cost of living wage adjustments. Since the demand for these COLA clauses, and therefore unionization, is expected to be higher in situations where inflation is high and the legal regime itself may be influenced by levels of inflation, we control for provincial-level inflation throughout our analysis. To do this, we use the all-items Consumer Price Index (Basket 2009, Year=2002). Note, that we use the inflation rate (year-over-year change in CPI), and not the *level* of the CPI.[22]

Unemployment rate:

Another key benefit of unionization is that it provides its members with increased job security, through seniority rules and restrictions on employers' use of technology to replace workers. Therefore, we would expect the demand for unionization to be increasing in provincial unemployment rates. In addition, job destruction during a recession may occur differentially in unionized workplaces, due primarily to higher fixed labor costs and therefore greater incentives for labor hoarding. Since provincial government initiatives to augment the labor relations environment may itself be influenced by business cycle fluctuations, it is important to condition on the unemployment rate. To do this we include the provincial unemployment rate among individuals aged 25 and over in all the estimated regressions.

Manufacturing share of employment:

There is considerable evidence that an important component of the long-term secular decline of unions in Canada and other OECD countries has been driven by structural economic shifts, in particular the shift from manufacturing to service-producing employment beginning in the 1980s. Since these trends are likely to have occurred differentially across provinces, and may be themselves correlated with changes in labor laws, we follow Bartkiw (2008) and Freeman and Pelletier (1990) and control for the manufacturing share of paid employment. These annual shares are estimated using the industry codes in the 1976 through 2012 Labor Force Survey (LFS) microdata files.

Popular preferences for unions:

[22] Provincial CPI series begin in 1979, so for the regressions using the CALURA-LFS data series, which begins in 1976, we use the national CPI for 1976-1978.

Changes in unionization rates are driven by individual preferences for unionization in the population, but these preferences are, in turn, likely to be correlated with political preferences and the decisions of politicians to augment labor relations laws. To capture changes in preferences that may be correlated with both unionization rates and our legal index, we exploit two sources of public opinion poll data – the Canadian Gallup Poll and the Canadian Election Study. The Canadian Gallup Poll surveyed individuals about their perceptions of unions between 1976 and 1989, and again between 1991 and 2000, while the Canadian Election Study contained questions about perceptions of unions between 1993 and 2008. Given the changes in the exact wording of poll questions over time and missing years, a separate model is estimated to obtain consistent provincial time-series measuring popular tastes for unions.[23]

4. Results

We begin by examining the results from estimating the lagged dependent variable (LDV) model defined in equation (1.4) of Section 2. In Table 4, we compare the results with and without the interaction of the LDV and legal index and across 4 alternative specifications of the error variance-covariance matrix. We then choose our preferred estimator and in Table 5 examine the sensitivity of the estimates to: (i) using the administrative CALURA-LFS data based on union membership counts; (ii) including province-specific quadratic trends[24]; and (iii) weighting observations by the underlying sample sizes used to estimate the unionization rates. Finally, in Table 6 and 7, we provide evidence on the distributional effects of the legal reforms by comparing the estimates across industry, occupation, education and gender groups.

In the absence of the LDV-labor relations index interaction (columns “a”), the coefficients on the LDV vary between 0.65 and 0.71. In terms of the underlying dynamics defined by equation (1.2), this implies considerable annual job flows in and out of the union sector and a gradual adjustment of unionization rates in response to legal changes (or changes in any of the control variables in the model). The interaction terms (columns “b”) are generally not well identified, although the point estimates are negative in all cases. This is consistent with our expectation that a shift towards a legal environment more favorable to unions will serve to increase the nonunion-to-union transition rate p_{nu} (note that the coefficient on the LDV in equation (1.2) is $1 - p_{un} - p_{nu}$). Similarly, the positive and statistically significant coefficients on the legal index itself across all

[23] Specifically, we map the categorical responses in each poll regarding support for unions into a binary variable: one for a favorable perception of unions and zero for a neutral or negative opinion. We then estimate a probit regression of this variable on a quadratic time trend; a set of province dummies; a set of province dummies interacted with both time and time-squared; and survey indicators to control for survey effects (in particular, changes in exact wording of questions). We then use the parameters from the probit to fit the model between 1976 and 2012 by province, thereby generating the “tastes” variable used to estimate equation (1.4).

[24] We restrict the quadratic term across provinces, but allow the linear term in the polynomial to vary across provinces.

specifications are, in terms of the structure given by equation (1.2), consistent with more favorable laws increasing nonunion-to-union transitions. To obtain an estimate of the long-run effect of legal reform, we predict the effect of increasing the (standardized) labor relations index from the average provincial value observed in 2012 (weighted by the population of each province) to the (standardized) value corresponding to the most union-friendly regime possible (a value of 3 for the non-standardized index). Given the dynamic structure implied by equation (1.3), the estimates in Table 4 imply a long-run increase in the national unionization rate ranging from 2.4 to 2.7 percentage points. The results are, therefore, remarkably robust to the assumed error structure, as well as the inclusion of the LDV-index interaction term.

With regard to the control variables, the unemployment rate effect estimates imply a countercyclical relationship with unionization rates, which is consistent with evidence elsewhere (Freeman and Pelletier 1990) and the belief that unionized jobs provide greater job security, so that in recessions they comprise a larger proportion of all jobs. All the point estimates also suggest that unionization rates are increasing in inflation, consistent with the demand for unionization and COLA clauses rising with inflation, although this effect is estimated much less precisely. As for the manufacturing share of employment, all the estimates are positive and in 6 of the 8 cases statistically different from zero at the 10% level. However, to some extent deindustrialization trends have been common across provinces, in which case their influence on unionization will be captured by the year fixed effects. Finally, and most surprisingly, we find no evidence that popular perceptions of unions captured in opinion poll data have a direct impact on unionization rates; all the estimates are insignificant at the 5% level. One interpretation is that public opinion impacts unionization rates both directly, through demand for unionization, but also indirectly through the political process and in turn the legal environment that elected governments impose. Alternatively, it may be that the public opinion variable is itself partially determined by unionization rates, in the sense that more union-friendly laws that lead to a greater union presence and power result in a more negative view of unions among the general public.

Given the similarity of the estimated long-run effects in Table 4, we subsequently restrict our attention to the estimator with the lowest variance – the FGLS estimator allowing for province-specific heteroskedasticity and autocorrelation, as well as contemporaneous spatial correlation. In addition, we restrict the interaction effect θ to be zero. The results from this case are reported in column (4a) of Table 4 and are replicated in the first column of Table 5 to enable comparison with the results using the same estimator and specification, but with the CALURA-LFS unionization rates (see the fifth column of Table 5). The additional specifications in Table 5 add province-specific trends (2); or sample weights (3); or both (4).

The estimated long-run effects of legal reform are in all cases larger using the CALURA-LFS data based on union membership. However, in none of the cases are the differences statistically significant. The larger estimates primarily reflect a longer adjustment process. That is, the coefficient on the LDV in the CALURA-LFS estimates is substantially larger in all cases, whereas the coefficient on the legal index is only larger when province-specific trends are included. The structural interpretation of this result, based on equation (1.2), is that transition rates in and out of union coverage exceed the transitions in and out of union membership; as one would expect. However, it is likely also the case that the difference reflects greater measurement error in the HS-LFS data. The greater noise in the unionization rates estimated using survey data is evident in Figure 1. Given that this measurement error is random, we know it will serve to attenuate the estimated LDV effect, which in turn will bias (or "smear") all the estimates in the model. Fortunately, the similarity of the long-run effects provides us with some assurance that the bias using the HS-LFS is modest, and if anything tends to underestimate the true effects.

Including province-specific trends and sample weights results in smaller estimated long-run effects of legal reform in both the HS-LFS and CALURA-LFS. However, including province-specific trends seems to matter more than sampling weights; the long-run estimate declines from 2.7 percentage points to 2.5 in the former case, but to 1.3 percentage points in the latter case. The difference appears to primarily reflect a decrease in the coefficient on the LDV, which is now less than 0.5 suggesting that the sum of the union-to-nonunion and nonunion-to-union annual transition rates is about one-half, which is clearly implausibly large. A possible explanation is that including province trends means that more of the remaining variation in the data to be explained is sampling error, which once again attenuates the estimated coefficient on the LDV. When we include the province trends and the sampling weights in specification (4), the long-run estimate is 1.1 percentage points; less than half the magnitude of the original estimate, but still statistically different from zero.

In Table 6 we report the results using the industry-, occupation-, education-, and gender-specific unionization rates. Once again, we present the results from the preferred specification in Table 4 (column 4(a)) and do not include provincial trends or sampling weights. These results should, therefore, be thought of as upper bound estimates; although of primary interest are the relative magnitudes of the estimates across groups in the labour force.

The industry estimates suggest a relatively small effect of legal reform in primary industries (1.6 percentage points), followed by manufacturing (3.1) and public services (3.4), and little or no effect in private services. Given that an important part of public services are governed by separate statutes not included in our labor relations index, the relatively large impact for this group is perhaps the most unexpected result. To obtain some perspective on this finding, we examined administrative data on the number of employees in bargaining units newly certified under the *Ontario Labor Relations Act* between 1985 and 2011.[25] The most striking feature of the data is a clear shift away from manufacturing towards private services. However, in every year up to 2005, more than one-quarter of employees in newly certified bargaining units were employed in health services, education services, local government or utilities (with the vast majority in health and education). Moreover, through the latter half of the 1990s and early 2000s, this proportion always exceeded 40%. This emphasizes that the provincial labor relations statutes on which we base our labor relations index apply to large proportions of public (or parapublic) sector workers within provinces.

Given that public services tend to employ workers with higher average skill levels than private services, an implication of the industry results is that increases in unionization resulting from legal reforms will be concentrated among relatively skilled workers. The occupation results in following columns of Table 6 appear consistent with this. Although the differences are small, the point estimates suggest larger gains among professionals (3.2 percentage point increase) than among blue-collar (2.0) or administrative (1.9) workers. The estimates across education groups are, however, more mixed. In this case, the largest gains appear to be for individuals with a high-school diploma or less (3.6 percentage points), followed by university graduates (2.7) and workers with a college credential (2.0). However, the large long-run estimate for high-school workers entirely reflects the estimated dynamics as the coefficient on the legal index itself is larger for the university educated. Finally, given that female employment is relatively concentrated in public services, it is not surprising that the estimate for women exceeds that for men; in this case the difference is almost two-fold (3.1 percentage points for women compared to 1.8 for men).

The obvious question is why would legal reforms favorable to unions have its largest impact on the unionization rates of professional women employed in health and education services where wages, benefits and working conditions are advantaged even in the absence of unions? One possible explanation is that this result reflects the optimizing decisions of unions and their organizers, as they seek to secure union dues, as opposed to the behaviour of workers. An alternative explanation is that legal changes will primarily affect workplaces where the net marginal benefit of unionization is close to zero, since this is where legal changes are most likely to alter optimal behavior. From this perspective, what the results seem to suggest is that legal reforms, at least within the scope of laws in the Canadian experience, are insufficient to affect unionization for many unskilled workers employed in the private services, where the risks inherent in organizing unions are too great. In contrast, in public services where profit incentives

[25] These data can be found in the Annual Reports of the Ontario Labor Relations Board between 1985 and 2011.

tend to be weaker, it is more likely that small changes in the costs of union organizing brought about by legal reforms are sufficient to alter organizing efforts.

Richer insight into the types of workplaces where legal reforms are expected to be most influential could be obtained by estimating the effects within the 72 industry-occupation-education-gender cells. For example, the long-run effect of legal reforms could be estimated separately for university-educated women employed in professional public-sector jobs. Unfortunately, in the vast majority of cases the sample sizes in the survey data are too small to estimate provincial unionization rates at this level of detail with sufficient precision. Alternatively, in Table 7 we report the results from the largest 10 of these 72 cells, in terms of the total provincial sample sizes provided in the HS-LFS data. The point estimates point to the largest long-run gains in unionization among unskilled (high-school and blue-collar) women and men employed in private services and manufacturing, respectively (columns 3 and 4). However, neither estimate is statistically distinguishable from the long-run effect for university-educated men or women employed as professionals in public services (columns 6 and 10). Moreover, both estimates are almost identical in magnitude to that of college-educated women employed as professionals in public services (column 5). The results also continue to suggest small gains among other unskilled groups, such as high-school educated men employed in private services in either blue-collar (column 1) or administrative (column (9) jobs, as well as high-school educated women employed as administrators in private services (column 2). Given the rising importance of private services in overall employment, these results suggest a limited potential for reforms in labor relations laws to mitigate rising inequality trends.

5. Conclusions

Using provincial-level unionization rates, we identify modest impacts of reforms in labor relations legislation on unionization rates. Specifically, our preferred estimates suggest that making legislation fully supportive of unions could raise Canada's current national unionization rate of 30% by no more than 3 percentage points in the long run. While shifting labor relations statutes towards these types of laws is clearly insufficient to reverse long-term deunionization trends, which have been relatively modest in Canada, it is worth emphasizing that the range of Canadian laws that we examine are also limited when compared to laws seen elsewhere. In particular, unlike the U.S., all Canadian statutes permit unions to negotiate union security clauses requiring employers to deduct union dues from employees' pay. Consequently, we are unable to examine the effect of introducing right-to-work laws. Nonetheless, the effects we identify within the relatively narrow range of Canadian laws, both contributes to the existing evidence on the importance of legal structures in determining unionization rates and emphasizes the limited potential of further legislation reforms in countries like Canada, where labor relations laws are already broadly supportive of unions, to mitigate rising inequality trends.

A key advantage of the survey data we employ is that it allows us to obtain evidence on what types of workplaces and workers are most likely to be affected by legal reforms. Our results indicate that the benefits of shifting to a more union-friendly legal environment are likely to benefit professional men and women employed in public services more blue-collar and administrative workers employed in private services. However, when estimated at finer levels of detail, the estimates also point to significant unionization gains among relatively uneducated men and women employed in private service-producing industries. The social welfare implications of these findings are mixed. The large gains among professional women who, in the absence of unions, enjoy relatively high wages and benefits and better job security, does not suggest that labor relations laws are an effective policy instrument addressing labor market inequality concerns. However, it may also be the case that the spillover effects of union outcomes on nonunion workers are more important in labor markets where unionization rates are lower, such as the unskilled service sector. A better understanding of these externalities of unions would be a fruitful area of future research.

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Table 1: Provincial unionization rates, 1981 and 2012

		NL	PE	NS	NB	QC	ON	MB	SK	AB	BC
All Workers	1981	0.45	0.40	0.36	0.41	0.49	0.35	0.40	0.40	0.32	0.44
	2012	0.38	0.30	0.29	0.28	0.39	0.27	0.35	0.35	0.23	0.30
Industry											
primary	1981	0.51	0.06	0.35	0.37	0.48	0.31	0.34	0.31	0.16	0.60
	2012	0.38	0.06	0.19	0.21	0.23	0.17	0.20	0.27	0.11	0.29
manufacturing	1981	0.69	0.39	0.46	0.43	0.57	0.47	0.45	0.42	0.40	0.63
	2012	0.43	0.26	0.17	0.24	0.36	0.21	0.31	0.25	0.17	0.25
private services	1981	0.25	0.25	0.22	0.28	0.38	0.22	0.27	0.27	0.23	0.30
	2012	0.19	0.10	0.12	0.10	0.26	0.14	0.18	0.18	0.12	0.18
public services[a]	1981	0.73	0.82	0.72	0.78	0.89	0.67	0.77	0.79	0.73	0.78
	2012	0.67	0.69	0.64	0.62	0.70	0.59	0.68	0.68	0.56	0.63
Occupation											
blue collar	1981	0.50	0.35	0.41	0.44	0.60	0.46	0.45	0.42	0.38	0.58
	2012	0.37	0.23	0.26	0.25	0.44	0.30	0.33	0.31	0.20	0.31
administrative	1981	0.26	0.28	0.25	0.35	0.40	0.26	0.33	0.32	0.26	0.29
	2012	0.25	0.20	0.17	0.17	0.26	0.15	0.23	0.24	0.16	0.20
professionals	1981	0.62	0.73	0.58	0.57	0.64	0.41	0.53	0.63	0.44	0.51
	2012	0.47	0.46	0.41	0.41	0.44	0.31	0.46	0.48	0.31	0.38
Education											
high school or less	1981	0.46	0.35	0.36	0.4	0.53	0.38	0.4	0.4	0.32	0.46
	2012	0.25	0.17	0.18	0.18	0.33	0.22	0.27	0.26	0.17	0.23
post-secondary degree	1981	0.46	0.6	0.5	0.56	0.59	0.44	0.52	0.59	0.46	0.55
	2012	0.43	0.36	0.34	0.31	0.43	0.3	0.39	0.4	0.25	0.36
university degree	1981	0.63	0.79	0.58	0.61	0.68	0.41	0.61	0.58	0.42	0.52
	2012	0.48	0.46	0.37	0.43	0.41	0.28	0.45	0.45	0.31	0.34
Gender											
male	1981	0.51	0.40	0.43	0.46	0.59	0.45	0.47	0.46	0.38	0.55
	2012	0.37	0.24	0.25	0.26	0.40	0.26	0.32	0.29	0.20	0.28
female	1981	0.43	0.46	0.37	0.43	0.50	0.32	0.39	0.42	0.34	0.38
	2012	0.38	0.36	0.32	0.30	0.38	0.27	0.38	0.40	0.26	0.32

Notes: Unionization rates are from the HS-LFS series and therefore exclude federal government employees. All other relevant sample restrictions are described in Table A1. The definition of unionization includes those who are covered by a collective agreement, but who are not a member of the union. Sources: SWH (1981), LFS(2012).

[a] Public services is broadly defined including provincial and municipal government employees, education and related services, health and welfare services and utilities.

Table 2: Unionization rates regressed on linear and quadratic time trends

	Unionization rates:			
	Provincial-level		Province-industry-occupation-education-gender-level	
Independent variables	(1)	(2)	(1)	(2)
time	-0.0037***	-0.0065***	-0.0031***	-0.0056***
	(0.0003)	(0.0006)	(0.0003)	(0.0005)
time squared		0.0001***		0.0001***
		(0.0000)		(0.0000)
constant	0.4011***	0.4150***	0.3924***	0.4052***
	(0.0220)	(0.0236)	(0.0188)	(0.0186)
observations	320	320	23040	23040
R^2	0.284	0.296	0.014	0.014

Standard errors in parentheses
* p < 0.10, ** p < 0.05, *** p < 0.01
Note: All linear regressions are weighted by sample sizes of underlying survey data. Standard errors are clustered; (1) and (2) at province level, (3) and (4) at unit level.

Table 3: Changes in Canadian labor relations legislation, 1976-2012

	NL	PE	NS	NB	QC	ON	MB	SK	AB	BC
1. Laws favorable to unions										
First-contract arbitration[26]	85:06		11:12[27]		77:12	86:05	82:02	94:10		76
Anti-temporary replacement workers					78:02	93:01-95:11				93:01
Ban on permanent replacement		87:05					85:01			
Re-instatement rights		87:05			78:02	76[28]	85:01	94:10	88:11	
Ban on professional strike-breakers						83:06	85:01			76
2. Laws unfavorable to unions										
Secret-ballot certification election[29]	94:02-12:06[30]		77:05			95:11[31]	97:02-00:09[32]	08:05[33]	88:11	84:06-93:01, 01:08[34]
Compulsory conciliation	76	76	76	76	76-78:01	76[35]			76-81:02, 88:12	
Cool-off period[36]	76	76	76	76	77:12	76		83:07	76-88:11	76
Mandatory strike vote		76	76	76	78:04	95:11	85:01	76	76	76
Employer-initiated strike vote			94:05		02:11	80:07	97:02-00:10	83:07	88:12	87:08

Notes: All dates are from Johnson (2010) unless otherwise noted by a footnote. Date specifies when law comes into effect (may be different from royal assent date). “76” indicates law was in effect in January 1976, the beginning of our sample period. A blank value indicates the law was never in effect during our sample period. All ten laws above are used to calculate our index of the collective favorableness to unions; see Section 3.2 for the formulation of our index.

[26] Update since Johnson (2002). PEI did not implement first contract arbitration in 95:05; never received Royal Assent.
[27] Bill 102: An Act to Prevent Unnecessary Labor Disruptions and Protect the Economy by Amending Chapter 475 of the Revised Statutes, 1989, the Trade Union Act, Chapter 71; Royal Assent: December 15, 2011.
[28] Different from Johnson (2010); change due to discussions with Ontario Ministry of Labour.
[29] Dates are from Johnson (2002) unless otherwise noted by a reference in this row.
[30] Bill 37: An Act to amend The Labor Relations Act, Chapter 30; Royal Assent: June 27, 2012.
[31] Bill 144: An Act to amend certain statutes relating to Labor Relations; Royal Assent June 13, 2005. Remove mandatory vote below 55% support for construction workers only. Note: we do not exclude construction workers in HS-LFS series.
[32] Highlights of Major Developments in Labor Legislation (1999-2000)
[33] Bill 6: An Act to amend The Trade Union Act, Chapter 26; Royal Assent: May 14, 2008.
[34] Highlights of Major Developments in Labor Legislation (2000-2001)
[35] Different from Johnson (2010); change due to discussions with Ontario Ministry of Labour.
[36] We do not specify the number of days of cool-off period in this table – see Johnson (2010) for more detail.

Table 4: Estimates of the effect of provincial labor relations index on unionization rates

	Dependent variable: HS-LFS unionization rates							
Independent var.	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
lagged unionization	0.6475***	0.6356***	0.6924***	0.6864***	0.7123***	0.7094***	0.6959***	0.6803***
	(0.0449)	(0.0461)	(0.0401)	(0.0404)	(0.0400)	(0.0403)	(0.0389)	(0.0399)
legal index	0.0051***	0.0122*	0.0044***	0.0093*	0.0041***	0.0102**	0.0044***	0.0138***
	(0.0015)	(0.0063)	(0.0013)	(0.0051)	(0.0011)	(0.0043)	(0.0008)	(0.0040)
interaction term		-0.0198		-0.0142		-0.0172		-0.0275**
		(0.0170)		(0.0141)		(0.0117)		(0.0115)
unemployment rate	0.2094***	0.2108***	0.1938***	0.1992***	0.1282**	0.1305**	0.0765	0.0659
	(0.0740)	(0.0739)	(0.0629)	(0.0632)	(0.0580)	(0.0581)	(0.0546)	(0.0547)
inflation rate	0.1283	0.1532	0.0598	0.0768	0.0552	0.0588	0.0691	0.0667
	(0.1281)	(0.1298)	(0.1064)	(0.1086)	(0.0360)	(0.0372)	(0.0808)	(0.0814)
manufacturing share	0.1040*	0.1171*	0.0948*	0.1041**	0.0681	0.0736	0.0696*	0.0747*
	(0.0613)	(0.0623)	(0.0500)	(0.0504)	(0.0487)	(0.0485)	(0.0401)	(0.0401)
tastes	-0.0221	-0.0219	-0.0224	-0.0204	-0.0074	-0.0054	-0.0133	-0.0128
	(0.0238)	(0.0237)	(0.0175)	(0.0176)	(0.0156)	(0.0159)	(0.0201)	(0.0203)
constant	0.1298***	0.1330***	0.1163***	0.1160***	0.1090***	0.1091***	0.1241***	0.1340***
	(0.0274)	(0.0275)	(0.0250)	(0.0250)	(0.0266)	(0.0265)	(0.0269)	(0.0272)
Error Terms:								
Var[$\epsilon_{p,t}$]=	σ^2	σ^2	σ_p^2	σ_p^2	σ_p^2	σ_p^2	σ_p^2	σ_p^2
Cov[$\epsilon_{p,t}$, $\epsilon_{q,s}$]=	0	0	0	0	$\sigma_{p,q}$	$\sigma_{p,q}$	$\sigma_{p,q}$	$\sigma_{p,q}$
Cov[$\epsilon_{p,t}$, $\epsilon_{p,t-1}$]=	0	0	0	0	0	0	ρ_p	ρ_p
observations	310	310	310	310	310	310	310	310
R^2	0.969	0.969						
long run effect	0.0272	0.0264	0.0268	0.0257	0.0271	0.0265	0.0273	0.0238
	(0.0085)	(0.0077)	(0.0080)	(0.0073)	(0.0070)	(0.0071)	(0.0048)	(0.0067)

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Year dummies and province dummies are included in all regressions. The variable *tastes* varies between (0,1) with 1 being most supportive of unions. The following tests are performed on specification (1a): (i) Poolability: Using the gr (2008, p.57) for full poolability (we need to exclude year dummies to do the test), we reject the null of poolability of all parameters. Using the Beck (2001) test for poolability of a single parameter of interest, we fail to reject the null of poolability of the legal index parameter H_0: $\delta_i = \delta$ *for all i* at the 10%-level. (ii) Heteroskedasticity: Using the Wald Test proposed in Greene (2003, p.323) we reject the null of no groupwise (panel) heteroskedasticity H_0: $\sigma_i = \sigma$ *for all i* at the 1%-level. (iii) Serial Correlation: Using the Lagrange multiplier test for serial correlation in time-series-cross-section data as described in Beck and Katz (1996), we fail to reject the null of no serial correlation H_0: $\rho=0$ at the 5%-level. (iv) Stationarity: Using the Levin, Lin, Chu (2002) test for stationarity of time-series-cross-section data, we reject the null that the unionization rate contains unit roots (crosssectionally-demeaned stationary) at the 1%-level. The "long run effect" is the difference between the long run value of $U_{p,t}$ evaluated at R_t=1.88 (the maximum possible value of our standardized index) and evaluated at R_t=R_{2012}; where R_{2012} is the average of all provincial values of R in 2012, weighted by provincial populations.

Table 5: Robustness analysis of effect of legislative index on unionization rates

	Dependent Variable: unionization rates							
	HS-LFS				CALURA-LFS			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
lagged unionization	0.6959***	0.7101***	0.4768***	0.4537***	0.8314***	0.7695***	0.6315***	0.5794***
	(0.0389)	(0.0352)	(0.0498)	(0.0465)	(0.0227)	(0.0273)	(0.0385)	(0.0407)
legal index	0.0044***	0.0038***	0.0037***	0.0031***	0.0042***	0.0037***	0.0047***	0.0041***
	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0006)	(0.0007)	(0.0007)	(0.0008)
unemployment rate	0.0765	0.0971*	-0.0218	-0.0487	0.0490	0.0181	0.0478	0.0687
	(0.0546)	(0.0499)	(0.0599)	(0.0599)	(0.0352)	(0.0382)	(0.0408)	(0.0450)
inflation rate	0.0691	-0.0117	0.0127	-0.0769	0.0181	0.0163	-0.0550	-0.0139
	(0.0808)	(0.0765)	(0.0848)	(0.0814)	(0.0607)	(0.0469)	(0.0603)	(0.0504)
manufacturing share	0.0696*	0.0955***	-0.1294	-0.0746	0.0838***	0.0773***	-0.0469	0.0786*
	(0.0401)	(0.0369)	(0.0792)	(0.0646)	(0.0266)	(0.0261)	(0.0504)	(0.0461)
tastes	-0.0133	-0.0310**	0.0476	0.0082	0.0089	0.0179	-0.0164	0.0380
	(0.0201)	(0.0149)	(0.0502)	(0.0456)	(0.0101)	(0.0126)	(0.0190)	(0.0253)
constant	0.1241***	0.1271***	0.2484***	0.2843***	0.0266***	0.0503***	0.1447***	0.0955***
	(0.0269)	(0.0219)	(0.0488)	(0.0441)	(0.0073)	(0.0100)	(0.0231)	(0.0243)
province trends	No	No	Yes	Yes	No	No	Yes	Yes
sample size weights	No	Yes	No	Yes	No	Yes	No	Yes
observations	310	310	310	310	360	360	360	360
long run effect	0.0273	0.0247	0.0134	0.0106	0.0470	0.0304	0.0238	0.0183
	(0.0048)	(0.0054)	(0.0034)	(0.0032)	(0.0072)	(0.0056)	(0.0040)	(0.0037)

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Year dummies and province dummies are included in all regressions. The variable tastes is between [0,1] with 1 being most supportive of unions. All specifications use the same form of GLS as columns 7 and 8 in Table 4, $\mathrm{Var}[\epsilon_{p,t}]=\sigma_p^2$, $\mathrm{Cov}[\epsilon_{p,t}, \epsilon_{q,s}]=\sigma_{p,q}$, $\mathrm{Cov}[\epsilon_{p,t}, \epsilon_{p,t-1}]=\rho_p$. Sample size weights refer to total cell counts of micro data underlying the data.

Table 6: Estimates of legislative effect by industry, occupation, education and gender

	Dependent Variable: unionization rates											
	Industry				Occupation			Education			Gender	
	primary	priv good	priv serv	public	blue	admin	profes	HS	PS	university	male	female
lag un rate	0.6122***	0.7016***	0.5216***	0.6020***	0.6391***	0.5031***	0.6659***	0.7509***	0.4573***	0.5953***	0.6756***	0.6269***
	(0.0447)	(0.0425)	(0.0483)	(0.0447)	(0.0422)	(0.0483)	(0.0378)	(0.0371)	(0.0466)	(0.0391)	(0.0415)	(0.0364)
legal index	0.0033	0.0049*	0.0006	0.0071***	0.0038***	0.0050***	0.0056***	0.0047***	0.0058***	0.0058***	0.0030***	0.0061***
	(0.0032)	(0.0026)	(0.0009)	(0.0016)	(0.0011)	(0.0015)	(0.0015)	(0.0012)	(0.0018)	(0.0013)	(0.0009)	(0.0011)
unem rate	0.1374	-0.1067	0.0192	0.2458***	0.1595**	0.1961**	0.0896	0.0972	0.2965***	0.1767**	0.1020	0.1791***
	(0.1430)	(0.1220)	(0.0580)	(0.0939)	(0.0637)	(0.0854)	(0.0910)	(0.0787)	(0.0854)	(0.0692)	(0.0694)	(0.0572)
inflation rate	0.1855	-0.5800***	-0.0196	0.3669***	0.0375	0.0450	0.1918	0.2477**	-0.0112	0.1132	0.0106	0.1147
	(0.2434)	(0.2153)	(0.0857)	(0.1329)	(0.1019)	(0.1302)	(0.1493)	(0.1176)	(0.1325)	(0.1202)	(0.0949)	(0.0962)
manuf share	0.3213**	0.2251**	-0.1227***	0.0792	0.1482***	-0.0086	-0.1035	0.1071*	-0.0065	0.1381***	0.0798	0.1049***
	(0.1327)	(0.1141)	(0.0373)	(0.0708)	(0.0507)	(0.0787)	(0.0651)	(0.0547)	(0.0591)	(0.0513)	(0.0494)	(0.0366)
tastes	0.0541	-0.0316	-0.0060	-0.0494*	-0.0092	-0.1462***	-0.0151	0.0039	0.0099	-0.0711**	0.0089	-0.0257
	(0.0599)	(0.0381)	(0.0154)	(0.0294)	(0.0183)	(0.0339)	(0.0295)	(0.0221)	(0.0262)	(0.0355)	(0.0205)	(0.0196)
constant	0.0088	0.1669***	0.1796***	0.2780***	0.1219***	0.3442***	0.1875***	0.0791***	0.1448***	0.2071***	0.1375***	0.1204***
	(0.0419)	(0.0458)	(0.0234)	(0.0445)	(0.0277)	(0.0432)	(0.0353)	(0.0297)	(0.0243)	(0.0340)	(0.0284)	(0.0211)
observations	310	310	310	310	310	310	310	310	310	310	310	310
long run effect	0.0160	0.0308	0.0025	0.0335	0.0199	0.0188	0.0318	0.0355	0.0200	0.0269	0.0175	0.0310
	(0.0158)	(0.0159)	(0.0035)	(0.0068)	(0.0056)	(0.0056)	(0.0082)	(0.0091)	(0.0063)	(0.0059)	(0.0052)	(0.0051)

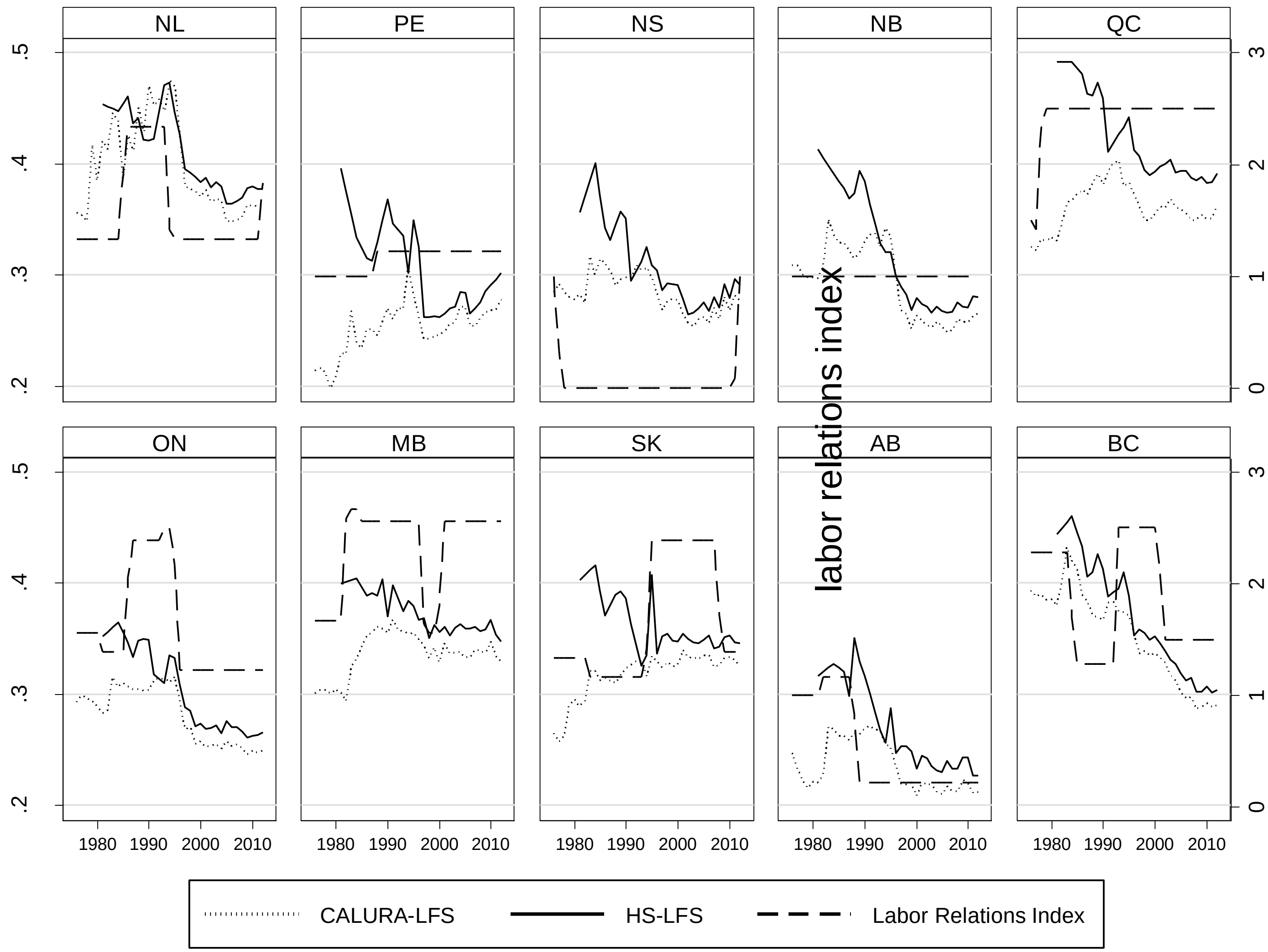
Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Year dummies and province dummies are included in all regressions. The variable tastes is between (0,1) with 1 being most supportive of unions. The estimator used for all 12 regressions above is the same is in Column (4a) of Table 4.

Table 7: Estimates of legislative effect for 10 largest industry-education-occupation-gender cells

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
lag un rate	0.4941***	0.4359***	0.4290***	0.5787***	0.4043***	0.3412***	0.4585***	0.4201***	0.3863***	0.4833***
	(0.0486)	(0.0493)	(0.0528)	(0.0443)	(0.0536)	(0.0524)	(0.0531)	(0.0469)	(0.0502)	(0.0455)
legal index	-0.0004	0.0038**	0.0093*	0.0075***	0.0084**	0.0062**	0.0057*	0.0037*	-0.0008	0.0055*
	(0.0019)	(0.0018)	(0.0051)	(0.0021)	(0.0039)	(0.0025)	(0.0034)	(0.0022)	(0.0031)	(0.0033)
unem rate	0.0268	-0.0002	0.1630	0.2167***	0.4712**	0.2746*	-0.0039	-0.1192	0.0784	0.4960**
	(0.1237)	(0.0973)	(0.2327)	(0.0832)	(0.1830)	(0.1550)	(0.1865)	(0.1301)	(0.1590)	(0.1954)
inflation rate	0.2729	-0.2949**	0.4229	0.2792*	0.0512	-0.0704	-0.0651	0.2361	0.4467**	0.1612
	(0.1973)	(0.1502)	(0.3635)	(0.1582)	(0.2753)	(0.2511)	(0.3051)	(0.2151)	(0.2204)	(0.3273)
manuf share	-0.1657**	-0.1054*	0.3968*	0.0142	0.3488**	-0.1376	-0.9054***	-0.0797	-0.0668	0.0303
	(0.0777)	(0.0610)	(0.2209)	(0.0608)	(0.1457)	(0.0969)	(0.1688)	(0.0860)	(0.1431)	(0.1296)
tastes	0.0313	0.0363*	-0.0197	-0.0786***	-0.2023***	-0.0286	-0.1128	-0.0430	0.0010	-0.1156**
	(0.0365)	(0.0210)	(0.0679)	(0.0251)	(0.0771)	(0.0454)	(0.0802)	(0.0347)	(0.0426)	(0.0484)
constant	0.2562***	0.1241***	0.2869***	0.0770***	0.5151***	0.5425***	0.5779***	0.1640***	0.1939***	0.4104***
	(0.0387)	(0.0270)	(0.0817)	(0.0227)	(0.0733)	(0.0620)	(0.0827)	(0.0357)	(0.0511)	(0.0648)
sector	services	services	manuf	services	public	public	services	services	services	public
education	high school	high school	high school	high school	college	university	college	college	high school	university
occupation	blue	admin	blue	blue	profes	profes	blue	admin	admin	profes
gender	male	female	male	female	female	female	male	female	male	male
observations	310	310	310	310	310	310	310	310	310	310
long run effect	-0.0007	0.0067	0.0164	0.0179	0.0141	0.0094	0.0105	0.0064	-0.0013	0.0107
	(0.0037)	(0.0033)	(0.0088)	(0.0050)	(0.0065)	(0.0039)	(0.0063)	(0.0037)	(0.0051)	(0.0064)

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Year dummies and province dummies are included in all regressions. The variable *tastes* is between (0,1) with 1 being most supportive of unions. The specification used for all 12 regressions above is the same is in Column (4a) of Table 4.

Figure 1: Unionization rate and labor relations index by province, 1976-2012



Appendix: Data source descriptions

Table A.1: Household survey descriptions

Survey	**1981 SWH**	**1984 SUM**	**1986-1990 LMAS**	**1991 SWA**	**1993, 1994, 1996 SLID**	**1995 SWA**	**1997-2012 LFS**
Format	Person file	Person File	Person file	Person file	Person (1993,1996), Job (1994)	Person file	Person file
Frequency	One Time (annual)	One Time (annual)	Annual	Two years	Annually	Two years	Monthly
Union status	Monthly	Annually	Weekly	Annually	Monthly	Annually	Monthly
Reference period	Week of 15th of each month	December 1984	Each week	November	Monthly	November	Week of 15th of each month
Variable definitions:							
Class of worker	claswkr: paid worker	clwsker: paid worker	q15cow: paid worker; no distinction of private/public	f05q76: paid worker	clwkr9 (1993,1994), clwkr1 (1996)	cowmain: paid worker	cowmain: public or private
Labor force status	q13: employed.	lfstatus: employed. q11: 'paid worker last week' in reference to reference week	clfs_: employed in week 2 of month	lfstatus: employed q10: 'paid worker last week'	mtwrk1 (1993); mtwr1c (1994); ml*v28 (1996)	lfsstat: employed	lfsstat: employed (at work or absent from work)
Union membership	q26: member only	q13_20; q14_21: member or covered	q112; q113: member or covered	q29: member and covered are combined in one variable	uncoll1 (1993, 1996); uncol1c (1994)	swaq29; swaq30: member or covered	union: member or covered
Industry	siccode: exclude fed gov't employees	sic1_: exclude fed gov't employees	sic`i': exclude fed gov't employees	f05q7374: no way to distinguish federal	sigc3g10 (1993, 1994); nai3g10, no way to distinguish	ind30: exclude fed gov't employees	naics_43: exclude fed gov't employees

				government employees	federal government employees (1996)		
Age	age: < 70 years old	age: < 70 years old	agegrp: < 70 years old	f03q33: < 70 years old	yobg21 (1993); eage26c (1994, 1996)	ageg: < 70 years old	age_12: < 70 years old
Main job	q21 & q22: calculated from data on hours worked per week	Identified by Statistics Canada based on most weekly hours worked	hrs,day: calculated from data on hours worked per week	Job information applies to 'main job'; survey was supplement to LFS. See SWA 1995 codebook	awh (1993, 1994); refers to job #1, no concept of main job in public-use data file (1996)	Job information applies to 'main job'; survey was supplement to LFS	Identified by Statistics Canada based on most weekly hours worked

Table A.2: Comparability of CALURA and LFS unionization rates

Issue	CALURA	LFS	COMMENT	SOURCE
100+ members	Only unions (national or international) with 100+ members in Canada reported their union members.	Conditional on being employed, the respondent can answer whether she is in a union or not.	CALURA understates relative to LFS; numerator is smaller.	Mainville/Olinek (1999 p. 11 Table 2). Akyeampong (1998 p. 30.)
Retired / Unemployed	Seasonally unemployed workers with recall rights may be included. Retired very unlikely to be included.	Union question asked conditional on employment. Must be paid worker.	CALURA overstates relative to LFS.	Galarneau (1996 p. 44,46). Table 1 (1970 CALURA report). Mainville/Olinek (1999 p.14). Bill Murnighan (CAW) email July 25, 2013.
Age	All union members. No age limit.	Age ranges from 15 to 70+, each of which has union members in LFS.	CALURA overstates relative to LFS.	Galarneau (1996 p. 44).
\`Employees' denominator	From Dec LFS for each year; conditional on employee.	Data are available for all months of year.	CALURA overstates relative to LFS due to seasonal unemployment in Atlantic Canada. We use July LFS to correct.	Galarneau (1996 p. 44)
Multiple jobholders	Would be counted twice in CALURA.	LFS only asks about main job.	CALURA overstates relative to LFS. LFS only allows main job per respondent so will not double-count.	Akyeampong (1997 p. 45). Historical CALURA data on CANSIM: a note to users.
Union members numerator – report date	Date unions report is as of Dec 31st.	Date report is as of Dec 15th.	No issue.	Galarneau (1996 p. 44). Mainville/Olinek (1999 p. 17 table footnotes). "Historical CALURA data on CANSIM: a note to users".
Union members numerator – new profession	In 1983, teachers, nurses, doctors added based on 1981 legislation.	N/A – these professions included.	CALURA understates relative to LFS (and itself) for pre-1983 SWH.	Mainville/Olinek (1999 p. 3-4, 9). Akyeampong (1998 p.31)
Self-employed	CALURA may include self-employed in (mostly) construction industry	LFS identifies self-employed and we exclude.	CALURA overstates relative to LFS.	"Historical CALURA data on CANSIM: a note to users".