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Employment Effects Of Nominal-Wage Rigidity: An Examination Using Wage-Settlements Data

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Umar Ahmed Faruqui

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Umar Ahmed Faruqui

Research Department, Bank of Canada
Ottawa, Ontario, Canada K1A 0G9
ufaruqui@bank-banque-canada.ca

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Abstract

The argument advocating a moderate level of inflation based on the downward nominal-wage rigidity (DNWR) hypothesis rests on three factors: its presence, extent, and negative impact in the labour market. This paper focuses on the employment effect of DNWR. It reviews the evidence presented by Simpson, Cameron, and Hum (1998), in light of a potential bias problem associated with their reduced-form model. We describe modifications to their employment model that aim to better isolate the effects of DNWR on employment growth. Analysis shows that empirical evidence in Simpson, Cameron, and Hum (1998) is sensitive to model specification. In contrast to Simpson, Cameron, and Hum (1998), who found—economically and statistically—significant employment costs for DNWR, in most of our specifications DNWR has no significant effect on employment growth.

JEL classification: C23, J23, J30
Bank classification: Labour markets

Résumé

Ceux qui invoquent l'hypothèse de rigidité à la baisse des salaires nominaux pour préconiser le maintien d'un taux d'inflation modéré fondent leur argumentation sur la présence et le degré de rigidité des salaires nominaux ainsi que sur les effets négatifs d'une telle rigidité sur le marché du travail. L'auteur de l'étude résumée ici s'attache tout particulièrement à l'incidence de la rigidité des salaires nominaux sur l'emploi. Il examine les résultats présentés par Simpson, Cameron et Hum (1998) et cherche à établir si le modèle à forme réduite de ces derniers est entaché d'un biais. L'auteur décrit les modifications qu'il a apportés à leur modèle de l'emploi afin de mieux cerner les effets d'une rigidité à la baisse des salaires nominaux sur la croissance de l'emploi. Il constate que les résultats empiriques obtenus par Simpson, Cameron et Hum sont sensibles à la spécification du modèle. Alors que, d'après ces chercheurs, la rigidité des salaires nominaux a sur la croissance de l'emploi des effets négatifs qui sont significatifs sur les plans tant économique que statistique, l'auteur n'observe aucun effet significatif de cette nature dans la majorité des spécifications retenues.

Classification JEL : C23, J23, J30
Classification de la Banque : Marchés du travail

1. Introduction

The debate over the desirability of maintaining a low rate of inflation centres on inflation's perceived costs and benefits. Convention holds that the costs of reducing inflation are transitory while the benefits of maintaining low inflation are permanent.[1] But recent studies by Akerlof, Dickens, and Perry (1996) and Fortin (1996) assert that targeting and maintaining low inflation can have long-term costs stemming from permanently higher unemployment rates. The two main ways in which these costs could emerge are through downward nominal-wage rigidity (DNWR) and hysteresis. DNWR focuses on the costs of maintaining a low level of inflation, while the hysteresis theory asserts that the path followed in moving from moderate (or high) inflation to low inflation can have long-term effects. Hysteresis has been thoroughly investigated over the past two decades, and while there is considerable evidence of persistence in unemployment rates, the consensus is that the rates do not exhibit hysteresis. Research on DNWR and its implications for the desirability of low inflation is more recent, dating from Akerlof, Dickens, and Perry (1996).

A number of studies have examined the extent of DNWR in Canada.[2, 3] Much less work, however, has been done on the employment effects of DNWR. An important exception is Simpson, Cameron, and Hum (1998) (henceforth SCH), who use wage-settlements data to study the employment effects of DNWR. SCH find a significant adverse effect on employment from DNWR when they estimate a reduced-form employment equation using industry-level data. They claim that pursuing a policy to keep inflation low is not an optimal choice for the monetary authority.

We reconsider the evidence in SCH, particularly a potential bias problem associated with their reduced-form model. SCH's proxy for DNWR will pick up more than just the effects of DNWR on employment if their reduced-form equation fails to adequately control for demand and wage shocks.[4] In this paper we attempt to control for those shocks and re-estimate a modified employment equation to better ascertain the employment effects of DNWR.

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1. For a detailed overview of the perceived costs and benefits of inflation, see Laidler (1993), Howitt (1997), and Coletti and O'Reilly (1998).
2. See Fortin (1996), and Crawford and Harrison (1998).
3. Menu costs are those associated with negotiating and implementing wage changes, such as administrative costs and costs involved in bargaining. The wage change being considered could be so small that the adjustment costs would lead to a decision to leave the wage unchanged.
4. See Farès and Hogan (2000).

Section 2 reviews the literature on DNWR in a Canadian context. Section 3 describes the data used for the quantitative analysis. Section 4 describes the theoretical models used and the results of the regression analysis. Section 5 discusses the implications of the study and proposes avenues for further research.

2. Literature Review

The belief that some minimal level of inflation is required for the labour market to function efficiently has a long history in macroeconomics. Tobin's seminal work on the subject asserted that employers facing negative shocks can maintain employment levels only if they can reduce real wages. He claimed that it was easier to make real wage cuts with higher inflation than to reduce nominal wages, because workers suffer from some form of money illusion. Tobin postulated that, for psychological reasons, workers are reluctant to accept decreases in their nominal wages, but will accept real wage cuts if inflation erodes the value of a given nominal wage. When inflation is low, employers might not be able to reduce real wages as much as they want without cutting nominal wages, so employment will have to fall.

Tobin's views were at first discounted because there was no formal model to support his assertions. Moreover, earlier empirical studies on labour-market behaviour found limited evidence for DNWR.[5] Akerlof, Dickens, and Perry (1996) (henceforth ADP), using U.S. panel data, cast doubt on earlier studies that had found little empirical support for DNWR, and described a theoretical model that shows how DNWR might generate a negative long-term relationship between inflation and unemployment. Since that time, a number of U.S. and Canadian studies have focused on DNWR, to ascertain the presence and extent of wage rigidity using micro data.[6] Few papers, however, have addressed the link between wage rigidity and employment, with SCH being an exception.

SCH investigate the relationship between pay-cut resistance and employment growth. Using data on employment and output by sector combined with wage-settlements data, they estimate the effect of wage freezes on employment growth. Their a priori belief is that nominal-wage rigidity should have a negative effect on employment, based on much the same reasoning as that of ADP: firms that face negative shocks want to cut real wages to help absorb the shocks. In times of low inflation the desired cuts in real wages cannot be made

5. See Mitchell (1985), O'Brien (1989), McLaughlin (1994), and Hanes (1996).
6. See Kahn (1997), Groshen and Schweitzer (1996), and Crawford and Harrison (1998).

without cutting nominal wages; if workers resist these nominal pay cuts, then the firms will reduce employment instead.

SCH use the following models to estimate the effects of pay-cut resistance on employment:

$$\Delta \log E_{it} = \alpha + \beta' FrzCut_{it} + \gamma' \Delta \log Y_{it} + \mu_{it} , \tag{1}$$

$$\Delta \log E_{it} = \Gamma + \lambda' Frz_{it} + \rho' Cut_{it} + \Upsilon' \Delta \log Y_{it} + \upsilon_{it}, \tag{2}$$

where *Frz* measures the percentage of union contracts with wage freezes for industry group *i* in time period *t*, *Cut* measures the percentage of contracts with a wage cut, and *FrzCut* is the sum of the *Frz* and *Cut* variables for industry *i* in time *t.* The dependent variable in both equations is the growth in employment for industry *i* from time period *t-1* to *t*. The $\Delta \log Y_{it}$ regressor measures the growth in industry *i*'s output from time period *t-1* to *t,* and is included in the regressions to control for employment changes that might result from changes in demand factors. Equation (1) treats wage freezes and wage cuts as indicators of DNWR, whereas in equation (2) only the wage freezes indicate DNWR.

SCH estimate the models using weighted least squares for the private sector wage settlements over the sample period 1978–95, and find a negative coefficient on the wage-freeze variable (-0.029; t-ratio: 17.0) for equation (2). Their regression results also yield a negative coefficient on *FrzCut* (-0.026; t-ratio: 18.4) for equation (1). Using equation (2), they estimate that for the private sector a 10 per cent increase in the incidence of pay freezes is associated with a 0.3 per cent decline in employment growth, other things being constant (SCH 1998). SCH assert that, in the absence of the negative effects of pay freezes, employment would have been 0.33 per cent higher for 1978–95 in the private sector.

While the results of the reduced-form estimation fit nicely with the a priori assertions of SCH, their results should be regarded with caution. As Farès and Hogan (2000) show, the SCH results may overstate the effects of wage rigidity on employment. Because of the low explanatory power of the regressors in the SCH regressions, the coefficient on the wage-freeze variable is likely correlated with the error term, which would bias the point estimates. In equations (1) and (2), it is unclear whether SCH control effectively for labour-demand shocks, which would shift the demand curve in or out, and would produce a negative correlation between the wage-freeze variable and the error term (since wage changes and employment would be positively correlated). This in turn would bias the point estimates on the wage-freeze variable to show evidence of negative employment costs due to DNWR. Farès and Hogan show that the wage-change variable corrects for the bias in the SCH model by isolating the demand-shock effects associated with the wage-change variable.

Farès and Hogan re-estimate the reduced-form wage and employment equations using wage-settlements data after correcting for the bias in the SCH regressions. In their analysis (unlike that of SCH), they use firm-level instead of industry-level data for employment. Their reduced-form employment equation is as follows:

$$\Delta \log E_{it} = \theta^{c} + \theta^{x_1} X_{it} + \theta^{x_2} X_{it_{-1}} + \sum_{t=1}^{T} \theta_t^{y} DY_t + \sum_{a=1}^{A} \theta_a^{r} DY_a + \delta D0_{it} + \gamma \Delta \log W_{it} + \mu_{it} \text{ , (3)}$$

where X_{it} is the average annual growth in industry output in the industry to which firm i belongs over the period of the contract signed in period t; DY_t and DY_a are the year and regional dummies, respectively; $D0_{it}$ is a dummy variable that takes a value of 1 if the contract shows a wage freeze, and zero otherwise; and $\Delta \log W_{it}$ is the average annual wage change for the contract (defined using the lifetime definition).

The inclusion of the wage-change variable in Farès and Hogan's regression changes the sign of the coefficient on the wage-freeze variable from negative to positive.[7] However, these results are not reliable, given the low statistical significance of the coefficients and the limited explanatory power of the regressors. Nonetheless, their study does reveal an important limitation of the SCH methodology.

We use industry-level employment data, as in SCH, but follow Farès and Hogan (2000) in using yearly frequency. Unlike Farès and Hogan (2000), however, we do not use the wage-change variable to correct for the bias in SCH. Incorporating the wage-change variable into the reduced-form employment equation could introduce endogeneity problems into the model. The output gap is therefore used (in conjunction with the lagged output growth) to control for demand shocks and to allow for a better estimation of the relationship between DNWR and employment.

7. Farès and Hogan estimate one model in which they do not correct for the bias, and find a negative coefficient on the wage-freeze dummy.

3. The Data

3.1 Description of data and explanation of variables

The primary source of data for this paper is the wage-settlements file maintained by Human Resources Development Canada (HRDC). The data cover collective bargaining agreements in the Canadian unionized sector for contracts involving 500 or more employees, and are grouped by HRDC into separate files for the public and the private sector. Data are available for January 1978 to May 1999,[8] and include 4,350 observations for the private sector and 6,411 observations for the public sector, where each observation is a negotiated contract.

The wage-settlements data are published monthly and provide detailed information about contracts that have been settled since the last release, as well as revisions to previous data. Historical wage-settlements data are subject to revision from one release to another for two principal reasons. First, inaccuracies associated with the data collection process are corrected, as much of the data is collected through telephone surveys. Second, the method of calculating effective wage increases for contracts containing a cost-of-living adjustment (COLA) clause requires re-estimation of the average wage change for each contract as new information on actual inflation becomes available (HRDC 1997).

The information for each contract includes comprehensive coverage of most aspects of the agreement, but for this analysis the variables of interest are the date of settlement, the average wage increase over the duration of the contract, the yearly wage increases, and the standard industrial classification (SIC) code. Following Crawford and Harrison (1998), SCH, and Farès and Hogan (2000), the settlement date is used to sort the data by year and by inflation periods. The settlement date represents the date at which the agreement was signed or ratified, and may differ from the starting date of the contract, since a contract can be retroactive.

The wage change for the contracts can be calculated in three different ways: lifetime wage change, first-year wage change, and year-over-year (YOY) wage change.[9] We consider the YOY and the lifetime changes. The lifetime change takes the average of annual wage changes over the duration of the contract, whereas the YOY method treats the wage change in each year of the contract as a separate observation. The two methods give different

8. The data used by SCH cover from January 1978 to August 1995, while that used by Farès and Hogan (2000) spans January 1978 to December 1996.

9. Crawford and Harrison (1998) describe the relative merits of using the different definitions of wage change.

counts of contracts in a given time period (as shown in Table 1). For the lifetime method, each contract is counted once. For the YOY method, a three-year contract is counted thrice, since it has three wage-change observations.[10] The lifetime wage change offers ease of use and consistency with previous work on DNWR;[11] the YOY method is most appropriate for analyzing wage changes for industries on a yearly basis,[12] and is therefore used in the quantitative analysis of this paper.

The average wage-change variable for each contract in the wage-settlements data represents a simple arithmetic average of (actual or agreed upon) wage changes for contracts that last longer than one year (for contracts of one year's duration, it is the wage change for only that year). For contracts that include a COLA clause, the formula for calculating the average increase is more complicated. For *completed* contracts with COLA clauses, the average wage change includes increases based on actual inflation data. For *ongoing* contracts (i.e., contracts that have not been completed at the time of the release), the average wage increase is the agreed-upon increase in wages, including estimated COLA payments. Estimates of the yield of COLA clauses are obtained by quantifying the characteristics of those clauses in each agreement and applying a combination of actual consumer price index (CPI) increases available to date plus a specified projected rate of inflation[13] for the remainder of the contract (HRDC 1997). In subsequent quarters, these estimates are revised using actual CPI values as they become available.

The SIC codes provided in the data base are used to group the data by industry. However, the wage-settlements data, which use the 1970 SIC codes, had to be converted to the 1980 SIC codes, which the rest of the Statistics Canada series used for this study (Appendix 1 describes the methodology used).

3.2 Summary statistics

This paper's quantitative analysis is limited to private sector data for the following reasons. First, fiscal pressures in recent years have caused many imposed wage freezes in the public

10. The contract count has important implications on weighted estimation results, as undertaken in this paper.
11. See Farès and Hogan (2000) and Crawford and Harrrison (1998).
12. The YOY method of wage change covers all contracts in effect in that particular year, rather than only new contracts (which the other two measures would cover).
13. In the current data set, an inflation projection of 2 per cent is used when the actual rate is unknown (HRDC 1997).

sector, so the aggregate wage-settlements data could lead to inappropriate conclusions about the degree of downward wage rigidity in the total economy. Second, the wage-settlements data cover approximately 10 per cent of paid employees in the private non-agricultural sector, whereas this sector accounts for close to 80 per cent of total employment. Consequently, the public sector is overrepresented in the wage-settlements data base, accounting for 60 per cent of the settlements in the 1992–95 period, but for less than 20 per cent of total employment (Crawford and Harrison 1998). Third, the wage and employment models used in this paper assume profit-maximizing behaviour by firms, and therefore would be more applicable to the private sector.

Although statistics for the entire private sector sample period are summarized in Table 1, the data used in the regression analysis end in December 1996, because disaggregated employment and output data by two-digit SIC codes are available only until that date.[14] Table 1 shows that using the different definitions of wage change does not significantly affect the average wage-change statistic, but does have a marked effect on the contract count and the number of observations with wage freezes and wage cuts. In particular, it is clear that the YOY method picks up relatively more wage freezes and cuts than the lifetime method.

Based on the YOY method, the 1993–95 period shows the highest proportion of contracts with wage freezes and cuts, followed by the 1983–86 era. Interestingly, the two recession periods (as dated by SCH (1998)) show a smaller percentage of contracts with wage freezes and cuts than the years immediately following the recession—possibly because it takes time for shocks faced by firms to be reflected in wages.[15] The industrial breakdown of pooled private sector data for 1978–99 (Appendix 2) shows that the wood, construction, and wholesale-trade industries had the highest percentage of contracts with a wage freeze.

14. Output data are taken from Statistics Canada CANSIM Matrix 4673 (catalogue no. 15-001), and employment data are from Statistics Canada CANSIM Matrix 7916 (catalogue no. 15-204). Disaggregated employment data for the manufacturing sector from 1992 onwards are taken from the Annual Survey of Manufacturers (catalogue no. 31-203-XPB).

15. For example, firms may wait to see whether the shock they face is temporary or permanent. Alternatively, the wage-bargaining process may take time, so there may be a lag between the time that firms want to change wages to the time that the bargained wage changes take effect.

Table 1: Summary for private sector wage settlements (1978–95)

Method		1978–81	1982	1983–86	1987–90	1991–92	1993–95	1996–99.05
	Avg. total output growth (%)	3.2	-2.9	4.1	2.9	-0.5	3.3	2.9
	Avg. total employment growth (%)	3.4	-3.1	2.3	2.2	-1.2	1.7	2.1
	Avg. core inflation rate (%)	9.2	9.7	4.4	4.3	2.6	1.8	1.2
Lifetime definition[1]	Contracts	807	170	768	677	288	390	393
	Avg. wage change (%)	10.9	9.5	3.8	4.9	3.0	1.5	2.2
	% of contracts with a wage freeze (%)	0.0	1.2	7.7	1.6	9.4	10.0	7.6
	% of contracts with a wage cut (%)	0.0	0.0	0.4	0.0	1.7	3.1	1.3
YOY definition[1]	Contracts	1328	355	1559	1583	709	950	947
	Avg. wage change (%)	11.0	11.1	5.0	4.7	4.0	2.3	2.2
	% of contracts with a wage freeze (%)	0.5	2.2	12.8	4.4	6.9	17.4	9.3
	% of contracts with a wage cut (%)	0.2	0.6	0.3	0.0	0.3	1.0	0.2
SCH data:	Contracts	2615		3700	3835		942	
YOY definition[1]	% of contracts with a wage freeze (%)	1.5		18.0	5.4		36.9	
	% of contracts with a wage cut (%)	0.1		0.3	0.1		2.4	

Notes: The time frame covered by the data is divided into 7 periods; 1982 and 1991–92 represent recession periods. The output and employment growth figures represent average growth rates in the total economy. The core inflation rate is based on the total CPI excluding food, energy, and the effects of changes in indirect taxes. The last three rows represent figures from the processed data used in SCH and were provided by Professor Wayne Simpson of the University of Manitoba.

1. Our and SCH's calculations shown in the table exclude contracts from the community, business, and personal-service industries (SIC 1970: 801–899). Although those industries are part of the private sector wage-settlements file, both papers exclude them from the sample used in the regression analysis because of the lack of reliable employment and output data for them.

The regression analysis used in this paper, as well as in SCH, uses weighted ordinary least-squares (OLS) and some of the regressors are scaled by the contract count, so it is interesting that our two sets of calculations of contract count, wage freezes, and wage cuts differ considerably. The differences arise primarily from how the wage-settlements file is processed to get YOY-defined variables (i.e., total number of contracts, number of contracts with wage cuts, and the number of contracts with wage freezes). The contract counts (along with the count of wage freezes and wage cuts) using the YOY method must be derived from the wage-settlements file, and there are potential problems relating to double counting of contracts and overlapping contracts.[16] Our calculations (which control for these potential problems) of contract count in the private sector are supported by independent computations done by HRDC, whereas the contract counts reported by SCH are substantially higher.

HRDC (1999) calculations show that, for 1987–90, 831 contracts were signed in the private sector (including the community, business, and personal-service industries (CBPS), with an average duration of 2.43 years. Subtracting contracts signed in the CBPS industries[17] (163) from this period total 668, which is close to our estimate of 677. Using a crude method to obtain the YOY count of contracts from the HRDC numbers by multiplying the adjusted contract count with the average duration gives us a total of: 668 x 2.43 = 1,623 observations for 1987–90. Our calculations give 1,583 contracts. In contrast, SCH report 3,835[18] contracts for 1987–90. Similar results are found for the other time periods.

This study (which includes data up to May 1999) is more up-to-date than that done by SCH (which includes data up to August 1995). Even when restricted to the same time period as SCH, some minor differences may appear because of revisions that are commonplace with the wage-settlements data. Some differences may also show up for the 1993–95 period, because while the SCH data end in October 1995, we consider data until the end of 1995 (for the convenience of data processing).

16. Double counting means that contracts that are longer than 3 years should be either excluded from the sample or counted appropriately, since the wage-settlements file contains only wage-change data (from which we get the count of wage freezes and cuts) for the first 3 years of the contract. Since SCH exclude the 1982 and 1991–92 periods from their sample, there should be a clear methodology to ensure that contracts that overlap the excluded periods are counted only for those years that fall in the sample period considered. For example, a 3-year contract starting in 1980 should be counted only once, since the second and third years of the contract fall in the excluded period.
17. See footnote 1 in Table 1.
18. Calculations based on data provided by Professor Wayne Simpson.

4. Econometric Methodology and Empirical Results

4.1 Attempt to replicate the SCH results

Given the strong claims that SCH make based on their results, it is useful to check whether their quantitative analysis can be replicated using a similar methodology. Replicating the SCH results helps to ensure that any comparisons made are meaningful.

SCH use private sector wage-settlements data spanning January 1978 to August 1995. The processed data contain information on 26 industry groups over 4 time periods.[19] The reduced-form employment equations (equations (1) and (2) in Section 2) are underestimated using weighted OLS, and their reported results are reproduced in the first part of Table 2.

Table 2: Attempt to replicate the SCH results

	Simpson, Cameron, and Hum (1998)				Attempt at Replication			
	1978–95		1993–95		1978–95		1993–95	
Constant	1.494 (42.4)	1.495 (42.4)	1.134 (4.6)	0.334 (1.6)	1.265 (28.3)	1.287 (28.7)	1.671 (9.7)	1.472 (8.2)
% pay freezes	-0.029 (17.0)		-0.017 (3.0)		-0.047 (-15.5)		-0.07 (-10.6)	
% pay cuts	0.043 (2.0)		0.023 (6.5)		0.151 (5.3)		0.224 (7.0)	
% pay freezes or cuts		-0.026 (18.4)		0.010 (2.5)		-0.038 (-13.9)		-0.047 (-7.4)
Output growth	0.273 (46.3)	0.271 (46.3)	0.075 (3.1)	0.156 (7.6)	0.145 (19.4)	0.139 (18.6)	0.072 (4.1)	0.093 (5.0)
Adjusted R^2	0.180	0.180	0.095	0.059	0.099	0.092	0.213	0.151

Notes: Values in parentheses are corresponding “t” statistics. The dependent variable is employment growth by sector. Weights, and the pay cuts and freeze variables, are based on theYOY method of wage change. All regressions are weighted by the number of contracts for each industry for each time period. We use our own weights in the attempt at replication, and not those used by SCH.

19. SCH divide the sample into four time periods: 1978–81, 1983–86, 1987–90, and January 1993 to August 1995. Data are then aggregated over each time period for each industry; e.g., there are four observations for the fishing industry, corresponding to each of the four time periods.

Using our own processed data and imposing the same conditions as SCH did on the sample period and grouping of industries, the sign on most of the estimated coefficients is the same for both sets of results. In particular, the coefficient on the wage-freeze term is negative and statistically significant in both sets of results. However, we can not replicate the SCH results precisely (Table 2): the numerical values of the coefficients as well as the standard errors of the estimates differ.

The main cause of this discrepancy is likely the differences in the weights used in the regressions. The regressions are weighted by the number of contracts in each sector and time period that must be calculated from the source file. As Section 3.2 indicated, there are considerable differences in contract count between our calculations. Overall, however, these differences do not appear to affect the quantitative analysis. Both the SCH results and ours show that a rise in the incidence of wage freezes raises unemployment.

4.2 Modifying the SCH methodology

At least two potential corrections can be made to the SCH models (equations (1) and (2)). The first relates to the use of contemporaneous output in the SCH model to control for demand shocks. Second, allowance should be made for the temporal and spatial variations in the data; one way to do that is to incorporate industry dummies and a binary output-gap variable. In addition, we think it reasonable to include the 1982 and 1991–92 periods in the sample, to use a yearly frequency for observations (rather than aggregate data into time periods), and to consider alternative estimation methods to handle the two-dimensional nature of the data. This section gives an overview of the models that are considered in this paper, including the form and estimation methodologies.

SCH use contemporaneous output to control for demand shocks. However, lagged output is more appropriate in this case.[20] The relationship between lagged output and contemporaneous employment involves firms waiting to react to shifts in demand (to see whether the shocks are temporary or permanent) and lags in the wage-bargaining process in an institutional setting.

SCH do not allow for spatial variation in the employment–wage rigidity relationship in their models. However, this relationship could vary across different sectors as well as over time. Variation across industries could result from differences in trend productivity growth,

20. See Farès and Hogan (2000) for additional criticism of the inclusion of contemporaneous output by SCH in their regressions.

the composition of the work force for the industry, the availability of substitute labour for that industry, and the inherent volatility in demand for that industry's output, among other things. The industrial variation in the data can be captured by including industry dummies in the model.

Further, it can be argued that the employment–wage rigidity relationship depends on the position of the economy in the business cycle. The temporal variation in the employment–wage rigidity relationship is partially picked up by the output-growth variable. However, output growth may be an imperfect proxy for demand conditions; we therefore supplement output growth with the aggregate output-gap variable to better capture all of the temporal variance in the relationship. Using the output gap from the Bank of Canada's Quarterly Projection Model, we construct a binary output-gap[21] variable, which takes on a value of positive one if there is excess demand (output gap is positive), and minus one otherwise.[22] The employment relationship can thus be written as:

$$\Delta \log E_{it} = \beta' Frz_{it} + \gamma' \Delta \log Y_{it-1} + \vartheta' YGap_t + \sum_{i=1}^{I} \delta' DSIC_i + \mu_{it} . \quad (4)$$

For the reduced-form employment model, the cross-sectional units are the industry groups (26 in total), and the time period covers 19 years (1978–96).The dependent variable is the percentage growth in employment for industry *i* from year *t-1* to *t*. The explanatory variables include the percentage of wage freezes for industry *i* in time *t* (Frz_{it})[23] and the percentage growth in output for industry *i* from year *t-2* to *t-1* ($\Delta \log Y_{it-1}$).[24] A wage freeze is used as a (best-available) proxy for evidence of wage rigidity in the data. $YGap_t$ is the aggregate output gap in year t, while $DSIC_i$ are industry dummies.

Conceptually, we can think of employment being affected by movements along the labour demand curve (supply shocks, including wage-bargaining shocks) and by shifts of

21. Given the uncertainty associated with the level of potential output, we use a binary variable for the output gap to shift the emphasis from the magnitude of the gap to the direction of the gap (of which we can be more certain).
22. A priori we expect employment growth to be positively correlated with the output-gap variable; i.e., employment growth is higher in years that have excess demand.
23. We follow SCH in using the YOY method to calculate contract counts, the count of wage freezes, and the average wage change by industry and year.
24. A more elaborate structure might be needed to capture the dynamics of the demand side; i.e., incorporate higher-order lags. We did estimate models with higher-order lags, but their coefficients were statistically insignificant.

the demand curve (demand shocks), and if we control for both types of shocks, any residual effects on employment shown by the wage-freeze variable might appropriately be attributed to DNWR. In equation (4) we control for supply (and demand) shocks other than those due to nominal-wage rigidity by including the lagged industry output growth and binary output-gap variables. This should allow us to test for a causal relationship between wage rigidity (as measured by wage freezes) and employment.

The temporal and spatial properties of cross-sectional time-series (CSTS) data may make the use of OLS problematic. The problem is to specify a model that will adequately control for differences in behaviour over the cross-sections, as well as any differences in behaviour over time for a given cross-sectional unit. In addition, there is a possibility that CSTS data may have temporally and spatially correlated errors as well as panel heteroscedasticity. One way to incorporate flexibility in our model is to use dummy variables to capture those differences. The alternative is to use more sophisticated modelling methods, such as fixed- or random-effects estimation, or the approach proposed by Beck and Katz (1996).

Fixed- or random-effects models are used specifically to deal with panel data, and allow variations across cross-sections to be embodied in the error term. Both models assume that the error term in the employment-change equation has two components: one that is specific to the industry but unchanging over time, and the residual error that is time- and industry-specific. That is,

$$\mu_{it} = \phi_i + \upsilon_{it}. \tag{5}$$

The industry-specific error term, ϕ_i, captures any feature determining employment changes that is specific to the industry and not captured in any of the right-hand-side variables. The fixed-effects model differs from the random-effects model in that the time-invariant industry-specific effect, ϕ_i, is correlated with the right-hand-side explanatory variables for the fixed-effects model, whereas the opposite is assumed for the random-effects model. This makes the fixed-effects estimator robust to the omission of any relevant time-invariant regressors. We estimate both models. To determine which of the models is the most appropriate is difficult, but the process is aided by specification tests, such as the Hausman test.

Beck and Katz (1996) assert that CSTS estimation can be handled best by using simple OLS with panel-corrected standard errors (PCSE). The authors state that more econometrically complex techniques, such as those of Parks (1967) (feasible generalized least squares (GLS)) and Kmenta (1986) (a cross-sectionally heteroscedastic and time-wise autocorrelated model), offer no discernible gain in estimator efficiency over OLS (especially when the number of time periods is small relative to the number of panels, as in our case). Beck and Katz also argue that the empirical weights used by panel-weighted least squares can mislead investigators, and that there is typically little or no gain from weighting (Beck and Katz 1996).

In SCH, the regressions are weighted[25] by contract count (for each industry and time period). Is the use of weights in this case appropriate? If the data are themselves observed averages (as in our case),[26] then it is suitable to use weights in regression analysis and we follow SCH in weighting our regressions by contract count. We want to give more weight to sectors with more contracts, in the absence of which our regressors may provide misleading information. Whereas SCH do not make any adjustments to their estimates of standard errors, we apply Beck and Katz's PCSE method for comparison purposes; Appendix 3 provides the results of this alternative analysis.

4.3 Discussion of results

The results from the models in Table 3 show consistency in the signs of the coefficients on the lagged output-growth and the output-gap variable. The coefficient on the wage freeze (i.e., our proxy for wage rigidity), however, does show some variance, being negative in some models and positive in others.

25. SCH use frequency weights, which treat each observation as one or more real observations but assume that these expanded observations are identical to each other.
26. The fact that our data comprise observed averages makes it logical to use weights in the regressions. Beck and Katz's (1996) criticism of using weights in CSTS analysis is more appropriate for non-aggregated data.

Table 3: Regression results from reduced-form employment equation

	1.1	1.2	1.3	1.4	1.5	1.6*	1.7**
% pay freezes	-0.006 (0.19)	0.004 (0.42)	0.04 (0.00)	-0.02 (0.00)	0.03 (0.00)	0.02 (0.32)	0.02 (0.40)
Lagged (1) output growth		0.19 (0.00)	0.17 (0.00)	0.20 (0.00)	0.17 (0.00)	0.19 (0.00)	0.19 (0.00)
Output-gap binary variable			1.57 (0.00)		1.55 (0.00)	1.31 (0.00)	1.24 (0.00)
Industry dummy	No	No	No	**Yes**	**Yes**	No	No
Adjusted R^2	0.0001	0.09	0.177	0.155	0.236	0.118	0.117

Notes: Only Models 1.1–1.3 include a constant term. All models are weighted by the number of contracts in the industry and year. The value in brackets is the corresponding "P" value. All P-values show results from a two-sided hypothesis test. The joint F-test rejects the null hypothesis that the industry dummies are all zero for Models 1.4 and 1.5. Appendix 3 provides a table of results using Beck and Katz's (1996) methodology (unweighted OLS with PCSE).

*1.6: Random-effects model.

**1.7: Fixed-effects model.

The results of our base-case estimation (Model 1.1 in Table 3) using OLS[27] show an adjusted R^2 of close to zero and a negative coefficient on the wage-freeze term, though it is very small and statistically insignificant. Adding the lagged output-growth variable (Model 1.2) causes the wage-freeze coefficient to change signs, though it remains statistically insignificant at the 5 per cent confidence level. Integrating the binary output gap (Model 1.3) into our equation causes major changes in the results. The coefficient on the wage-freeze variable becomes positive and statistically significant. In addition, the adjusted R^2 increases significantly from the previous specification. When the industry dummies are added to the model and the output-gap variable is excluded (Model 1.4), the sign switches for the wage-freeze variable from Model 1.3. Including industry dummies and the output-gap variable

27. OLS is chosen over GLS because heteroscedasticity is not expected to be an issue. Since the variables are in percentage terms and not in levels, scale factors across industries are not a worry. We tested for autocorrelation informally and formally. Informal analysis involved plotting the residuals and looking for trends or runs for autocorrelation, and for large variations in the residuals (when scaled by the independent variable) for heteroscedasticity. Formal tests done for heteroscedasticity included the Goldfeld-Quandt test and the Breusch-Pagan test; autocorrelation used the Durbin-Watson test. No conclusive evidence of heteroscedasticity or autocorrelation was found. Contemporaneous correlation across the industries is a problem more likely to occur in the data being used. For example, an external shock to one industry will probably have an impact on other related industries. Seemingly unrelated regressions would control for this correlation, but technical difficulties did not allow this. The key problem is that our panels are unbalanced (it is relatively rare that an industry has an observation for each sample point, and in some cases an industry group may contain only one observation for the entire sample).

(Model 1.5) provides the strongest results amongst the models tested. The coefficients on all explanatory variables are statistically significant and the adjusted R^2 is further improved from Models 1.3 and 1.4. A point of interest is that the coefficient on the wage-freeze variable is positive and highly significant statistically.

The random-effects and the fixed-effects models (Models 1.6 and 1.7, respectively, in Table 3) show similar results, with the coefficient on the wage-freeze variable being positive and statistically insignificant. Using Hausman's specification test,[28] we cannot reject the hypothesis that the random-effects model is the appropriate model to use. Beck and Katz's method of unweighted OLS with PCSE (see Appendix 3) also shows the wage-freeze coefficient to be statistically insignificant in all but one specification. While the Beck and Katz method is a useful check for our results, using weighted OLS is important for our case and the primary results are those presented in Table 3.

SCH find a negative coefficient on their wage rigidity term, in line with their a priori beliefs. Their implicit reasoning is that after imposing wage rigidity in a simple labour supply-and-demand (downward-sloping) framework, employment effects would be greater in the face of any shocks than if the rigidity was not present.

The wage-freeze coefficient is statistically insignificant in most of the models shown in Table 3 (i.e., the coefficient is not statistically different from zero), which suggests that this variable should not be included in the regression. Reinforcing this idea is the variability in the wage-freeze coefficient across different specifications.[29] Last, but not least, the strongest result (as measured by explanatory power) from our regressions (Model 1.5) suggests a positive coefficient on the wage rigidity proxy, instead of a negative coefficient, which SCH found.

The coefficient on lagged output growth is consistently positive in our regressions, in line with our a priori expectations. The output-gap coefficient is also positive in models in which it is included, indicating that it is picking up the intended business cycle effects.[30]

28. Hausman's (1978) specification test is a formal test of the equality of the coefficients estimated by the fixed- and random-effects estimators. If the coefficients differ significantly, either the model is misspecified or the assumption that the random effects ϕ_i are uncorrelated with the regressors is incorrect. The null hypothesis tested is that the difference in the coefficients is not systematic, and we get a P-value of 0.13 (on a chi-squared test), so the null hypothesis cannot be rejected.

29. See Table 3 as well as Appendix 3.

30. Note that a positive output gap corresponds with excess demand, and vice versa.

5. Conclusion

The argument for a moderate level of inflation based on DNWR hinges on the impact of wage rigidity on the labour market. SCH find (economically and statistically) significant employment costs of wage rigidity, from which they conclude that maintaining a very low inflation rate may not be an optimal policy choice. This paper modifies and refines the SCH model.

We draw a number of conclusions from our analysis. The most important conclusion is that the SCH results do not seem to be robust: the results are sensitive to model specification. Furthermore, in most of our specifications we find no discernible effect of DNWR on employment growth. These conclusions weaken the case for advocating a higher inflation target for Canada based on the DNWR argument.

A number of studies could be done to further assess the relationship between wage rigidity and employment. Simultaneous estimation of the employment and wage-change equations to fully define the labour demand-and-supply curves might permit more robust conclusions about the effect of wage rigidity on employment. Using an industry-level output gap variable could improve the empirical analysis. Lastly, using a measure of total compensation, instead of only changes in base wage rates, might enrich our analysis of the employment effects of DNWR. These studies remain for future work.

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Appendix 1: Mapping 1970 and 1980 SIC codes

SIC major group (1980 SIC)	1980 SIC interval	1970 SIC interval	Group name
03	30–39	41–47	Fishing
04	40–59	31–39	Logging/forestry
06	60–99	51–87	Mining, quarrying, and oil
10	100–149	101–109	Food
15	150–159	162	Rubber
16	160–169	165	Plastic
17	170–179	172–179	Leather
18	180–190	181–189	Primary textiles
24	240–249	243–249	Clothing
25	250–259	251–259	Wood
26	260–269	261–268	Furniture
27	271–279	271–274	Paper
28	281–284	286–289	Printing
29	291–299	291–298	Primary metals
30	301–309	301–309	Fabricated metals
31	311–319	311–318	Machinery industries
32	321–329	321–329	Transportation equipment
33	331–339	331–339	Electrical and electronic products
35	351–359	351–359	Non-metallic mineral products
36	361–369	365–369	Petroleum
37	371–379	372–379	Chemical and chemical products
39	390–399	391–399	Other manufacturing
40	400–449	401–500	Construction
50	500–599	602–629	Wholesale trade
60	600–699	631–699	Retail trade
70	700–769	701–737	Finance, insurance, and real estate

Appendix 2: Wage freezes and cuts by industry (1978–99.05)

Group name	Contract count	No. of contracts with wage freezes *(%)*	No. of contracts with wage cuts *(%)*
Fishing	4	0 *(0.0%)*	0 *(0.0%)*
Logging/forestry	142	4 *(2.8%)*	0 *(0.0%)*
Mining, quarrying, and oil	448	56 *(12.5%)*	0 *(0.0%)*
Food	576	58 *(10.6%)*	0 *(0.0%)*
Rubber	130	1 *(0.8%)*	1 *(0.8%)*
Plastic	3	0 *(0.0%)*	0 *(0.0%)*
Leather	46	0 *(0.0%)*	0 *(0.0%)*
Primary textiles	219	20 *(9.1%)*	1 *(0.5%)*
Clothing	151	9 *(5.9%)*	0 *(0.0%)*
Wood	131	29 *(22.1%)*	0 *(0.0%)*
Furniture	16	0 *(0.0%)*	0 *(0.0%)*
Paper	810	55 *(6.8%)*	1 *(0.1%)*
Printing	111	2 *(1.8%)*	0 *(0.0%)*
Primary metals	413	16 *(3.9%)*	2 *(0.5%)*
Fabricated metals	144	2 *(1.4%)*	0 *(0.0%)*
Machinery industries	129	4 *(2.9%)*	1 *(0.8%)*
Transportation equipment	765	20 *(2.6%)*	2 *(0.3%)*
Electrical and electronic products	406	5 *(1.2%)*	0 *(0.0%)*
Non-metallic mineral products	152	9 *(5.9%)*	0 *(0.0%)*
Petroleum	16	0 *(0.0%)*	0 *(0.0%)*
Chemical and chemical products	119	0 *(0.0%)*	1 *(0.8%)*
Other manufacturing	56	2 *(3.6%)*	0 *(0.0%)*
Construction	1035	149 *(14.4%)*	8 (0.8%)
Wholesale trade	108	15 *(13.8%)*	1 *(0.9%)*
Retail trade	622	85 *(13.7%)*	7 *(1.1%)*
Finance, insurance, and real estate	16	0 *(0.0%)*	0 *(0.0%)*

Note: All calculations are based on the YOY definition of wage change.

Appendix 3: Unweighted OLS with panel-corrected standard errors

Model	1.1	1.2	1.3	1.4	1.5
% pay freezes	-0.002	-0.002	-0.002	0.0003	-0.000
	(0.07)	(0.09)	(0.05)	(0.86)	(0.97)
Lagged (1) output growth		0.22	0.20	0.21	0.19
		(0.00)	(0.00)	(0.00)	(0.00)
Output gap (binary variable)			1.22		1.21
			(0.00)		(0.00)
Industry dummy	No	No	No	**Yes**	**Yes**
Adjusted R^2	0.01	0.09	0.12	0.09	0.12

Notes: This appendix is similar to Table 3, but the regressions are not weighted and standard errors are panel-corrected. The value in brackets is the corresponding “P” value. All P-values show results from a two-sided hypothesis test. The joint F-test rejects the null hypothesis that the industry dummies are all zero for Models 1.4 and 1.5.

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