WAGE RIGIDITY IN CANADIAN COLLECTIVE BARGAINING AGREEMENTS

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The authors search information on the provisions of 10,947 wage contracts signed in the Canadian unionized sector between 1976 and 1999 for evidence of downward nominal wage rigidity (the disinclination of wages to fall, in nominal dollars, below their established level). Over the sample period, real wage reductions were common, but nominal wage reductions were rare. The probability of downward nominal wage rigidity increased substantially during low-inflation periods. During such periods, apparently there was no reduction in the incidence of real wage cuts, but the magnitude of those cuts was modest, suggesting a lesser ability of wages to adjust to labor market conditions than at other times.

The wage determination process is one of the most studied areas of empirical labor economics. Despite continuing work in this area, long-standing questions concerning the extent of downward nominal rigidity, an issue of fundamental importance to labor economics and industrial relations, remain. If pervasive, such rigidity would interfere with the functioning of the labor market, preventing the efficient re-allocation of labor from low- to high-demand areas and inducing quantity adjustments and unemployment. Should nominal rigidity be more prevalent in some sectors than in others, similar shocks will have different price and quantity effects. For instance, if the union sector is more resistant than the non-union sector to wage cuts, real wage realignment may be more difficult to achieve in the union sector. This mechanism will be particularly in evidence at times when inflation—which can serve to "grease" the wheels of the labor market—is low (Shultze 1959; Samuelson

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Inquiries concerning the electronic file on wage contracts from which our data were constructed should be addressed to Human Resources Development Canada, Ottawa. Basic calculations in the paper were done using TSP. The symmetry test of Ahmad and Li (1997) was obtained from Qi Li at Texas A&M University and a modified form of it was used for the D test described in the appendix. It is available from the first author at the Department of Economics, University of Cyprus, Kallipoleos 75, P.O. Box 20537, 1678 Nicosia, CYPRUS.

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and Solow 1960; Tobin 1972). Thus, recently achieved, exceptionally low levels of inflation in countries such as Canada may have been attained at the expense of higher unemployment. Under these circumstances, low-inflation regimes may inject new sources of stress into industrial relations. These arguments suggest that more information on the extent and pattern of downward nominal rigidity would be valuable.

In conventional Keynesian models, downward rigidity is "effective" when the real wage is too high, employment is on the labor demand curve, and unemployment prevails. Then shocks that raise the price level and lower the real wage increase employment. Thus, early attempts to gauge the severity of downward nominal rigidity were macroeconomic in nature and investigated whether the real wage is countercyclical. Papers from Dunlop (1938) and Tarshis (1939) to Solon, Barsky, and Parker (1994) and Abraham and Haltiwanger (1995) are in this tradition, and varying results are available.

However, a new literature stemming partly from the availability of data at the micro level has emerged.[1] These studies typically start by constructing the cross-sectional nominal wage-change distributions from data such as the Panel Study of Income Dynamics or the Current Population Survey. Annual histograms are then used to study features of interest, such as whether the mass to the left of zero is deficient relative to a no downward nominal wage rigidity (DNWR) counterfactual, whether spikes at zero can be identified, the extent to which holes around zero may suggest the presence of "menu costs," and whether wage-change distributions may be different in periods of high and low inflation. Central issues of concern are the extent to which periods of sufficiently low inflation have been examined, the role of recall, measurement, timing, and rounding errors inherent in these surveys, the extent to which the visual evidence presented amounts to statistical tests, and whether such tests are best conducted using parametric or non-parametric techniques.

Parallel with this literature has been work seeking the reasons for nominal rigidity by interviewing the individuals who ought to know, such as executives and labor leaders (Blinder and Choi 1990; Agell and Lundborg 1995; Campell and Kamlani 1997; Bewley 1999). Bewley (1999) suggested that nominal wage cuts are shunned because of their likely impact on morale and that this is all the more likely where information flows are good. He found that new employees are more likely to be hired at rates comparable to those of existing employees in the "primary" sector[2] than in the "secondary" sector, where short-term employees, often part-time, abound. This work suggests that the incidence of downward nominal rigidity should be most apparent in situations where long-term relations between a firm and its employees exist, where workers are organized into bargaining units in which "bad news travels fast," and particularly where the bargaining unit is a union whose very existence and *modus operandi,* as in Oswald (1993), place less emphasis on employment than on wages and the prevention of outcomes such as nominal pay cuts. Some studies based on survey

[1]The U.S. literature includes, *inter alia,* McLaughlin (1994), Lebow, Stocton, and Wascher (1995), Akerlof, Dickens, and Perry (1996), Card and Hyslop (1997), Kahn (1997), Altonji and Devereux (1999), Groshen and Schweitzer (1999), Lebow, Sacks, and Wilson (1999,) and McLaughlin (1999a,b). Smith (1999) studied experience in the United Kingdom, Beissinger and Knoppik (2000) that in West Germany, and Fehr and Gotte (2000) that in Switzerland. The extant Canadian literature is reviewed below.

[2]"Primary-sector personnel include most factory, clerical, and secretarial workers, technical, professional, and managerial employees with permanent positions, and salespeople in stores and restaurants with regular customers whom the staff should know on a first-name basis." Bewley (1999:18).

data have distinguished between the behavior of union and non-union workers, and some evidence has been provided that more rigidity exists in the union sector.[3]

A good source of information on outcomes in the union sector is collective bargaining agreements themselves. Detailed data on the provisions of Canadian collective bargaining agreements are compiled by Human Resources Development Canada, the federal agency in charge of industrial relations. These are legally binding documents whose provisions are recorded and distributed electronically by federal authorities. We refer to information from this source as the "contract data."

One of the controversies surrounding survey data is the extent to which recall, measurement, rounding, and timing errors may exist.[4] These concerns apply to a far lesser extent to the contract data because of the regulatory environment under which this information is collected. In addition, the Canadian data are available over a long time period that includes periods of high inflation, a period of substantially reduced inflation, and a period during which inflation was exceptionally low—much lower than in the United States. Thus, the issue of whether periods of exceptionally low inflation have been available for study does not arise either. Contract data also make it possible to examine the role of Cost-of-Living-Allowance (COLA) clauses as a means of built-in nominal wage flexibility. Survey data cannot differentiate contingent from non-contingent wage change.

Qualifying these advantages of contract data are some drawbacks. There are no reporting requirements for the formal and informal agreements reached in the non-union sector, nor can that information be inferred from other sources. Where labor is supplied on the basis of informal arrangements, nominal wages may be adjusted at intervals that are less rigid than is the case in the union sector. Moreover, worker duties may easily be re-assigned, thereby securing nominal wage flexibility that would be more difficult, or impossible, to attain in the union sector. Finally, the contract data do not cover small bargaining units, and they refer to bargaining units rather than the earnings of particular individuals. Given these problems, while it is important to examine the extent of downward nominal wage rigidity in contract data, our findings may not generalize to the labor market as a whole.

In this paper we use a recently released version of the Canadian contract file to study the implied distributions of nominal wage change over the period 1976-99. To that end, we use a variety of parametric and non-parametric techniques and statistical tests. Some other studies also have used the Canadian contract data in this general context. Fortin (1996) argued that the Canadian recession of the early 1990s was deeper in Canada than in the United States because of the conjunction of lower inflation and downward nominal rigidity. Fortin's examination of downward nominal rigidity was based on 1992-94 histograms of only the *first* year of wage settlements, a procedure criticized by Freedman and Maclem (1998). Simpson, Cameron, and Hum (1998) estimated the increase in the unemployment rate that would be needed to moderate wage inflation by the amount attributed to wage rigidity. Their conclusion that this could be as high as 2% was questioned by Fares and Hogan (2000). Fares and Lemieux (2000) also focused on the macroeconomic consequences of nominal rigidity. Crawford and Harrison (1998) presented histograms of nominal wage change in private and public sector union contracts. They calculated the skewness coefficients at times of high, medium, and low inflation. Surprisingly, these coefficients become more negative at times of low inflation. The authors also applied hazard methods to their data and investi-

[3]For instance, McLaughlin (1999a:129) found that "the skewness of union workers' wage changes is all attributable to nominal rigidity."

[4]For the significance of these issues for the size of the spike at zero in the context of British data, see Smith (1999).

gated whether the wage-change hazard depends negatively on the rate of inflation.

Data Sources

Wage agreements in the unionized sector are monitored by Human Resources Development Canada (HRDC), which made available to us detailed, monthly files containing information on provisions for 10,947 wage contracts signed in the Canadian unionized sector, both public and private, between 1976 and 1999.[5] Because reporting requirements apply, this information is very accurate. We detected inconsistencies in only two contracts, and these were excluded from the sample. The raw, monthly file was processed to extract the information needed for the purposes of this study, including the unique identifying code number for each contract, relevant dates,[6] wage change that was due to a COLA clause and wage adjustment that was not contingent, and the duration and sector of each agreement.[7]

The resulting data base covers settlements ranging in duration from a few months to several years, and bargaining units with between 200 and nearly 80,000 employees.[8] The average base wage rate paid to entry-level workers is $12.40 at the beginning and $13.49 at the end of these agreements, implying a rate of change of 8.79%. Since mean duration is approximately two years, the annual rate of wage adjustment is approximately 4.4%. The increase in the base wage rate is, on average, $1.09, and it consists of a $0.97 non-contingent increase and a $0.12 contingent increase through a COLA clause. Very few contracts contain COLA clauses.[9]

We pursue our analysis using two definitions of wage adjustment—one that includes COLA adjustments[10] and one that does not. Clearly, less rigidity will be displayed by the former series. It should be noted, however, that because the incidence of COLA clauses is limited and the extent of indexation they provide is usually modest, the results are

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[5]We are indebted to Michel Legault of HRDC for providing us with the raw, monthly data file.

[6]These include the settlement date, effective date, and expiry date of each contract. The effective date is used to date contracts in the histograms below. Of the 2,743 contracts settled before the effective date, 2,337 (or 85.2%) were signed within three months of that date. Of the 8,202 contracts settled after the effective date, 3,220 (or 39.3%) were signed within three months of it. Thus, most contracts are signed within a window of three months around the effective date. Contracts settled after the effective date include ones involving disputes, or even strikes, and in some cases the difference between the settlement and effective dates can be quite long.

[7]*Duration:* contract duration is defined as the expiry date minus the effective date. Average duration increases gently throughout the period under study, and there is no tendency for wage flexibility to be attained via more frequent contract negotiations. *Sector:* the private-public sector distinction is based on a code in the employer file supplied to us. The public sector includes contracts in public administration, health, education, and utilities.

[8]In 1999, union membership as a proportion to non-agricultural paid workers was 0.32. The *Workplace Gazette* reports that agreements signed in 1998 covered 916,900 employees and those signed in 1999 covered 797,600 employees—see Human Resources Development Canada (2000:23). When these two numbers are summed (to account for the fact that average contract duration is just over two years), the result as a ratio to the Canadian labor force (15,570,000 is the average for 1998 and 1999) is equal to 0.11. This number constitutes a lower bound on the proportion of the labor force covered by similar agreements, because the data set used does not include agreements involving fewer than 200 employees.

[9]The strength and incidence of COLA clauses, and their implications, particularly for modeling wage adjustment, are analyzed in, *inter alia,* Card (1983, 1986), Christofides (1987, 1990), Cousineau, Lacroix, and Bilodeau (1983), Ehrenbrerg, Danziger, and San (1984), Hendricks and Kahn (1985), Kaufman and Woglom (1984), Mitchell (1980), and Vroman (1984).

[10]For most contracts, the yield on COLA clauses is calculated by quantifying the detailed provisions of each clause using actual CPI information. These clauses are complex and involve a variety of formulas—for example, cents per point change in the CPI, percentage changes in wages following percentage changes in the CPI, combinations of these two, triggers, and caps. In the case of contracts that were still in effect when the data base was constructed, the CPI was projected to increase at the rate of 2% per annum—a fairly realistic assumption, as it happened.

Table 1. Number of Contracts by Year, Sector, Contract Length, and $\dot{W}$ Sign.

		Sector		*Duration*[a]			*Sign of* $\dot{W}$ *(incl. COLA)*		
Year	*Total*	*Private*	*Public*	*Long*	*Short*	*C$\dot{P}$I*	$\dot{W} < 0$	$\dot{W} = 0$	$\dot{W} > 0$
1977[b]	226	94	132	125	101	7.55		2	224
1978	673	310	363	373	300	8.01			673
1979	569	228	341	415	154	8.95			569
1980	520	242	278	407	113	9.13			520
1981	450	195	255	309	141	10.16		1	449
1982	562	206	356	282	280	12.43	1	3	558
1983	643	207	436	296	347	10.8	4	26	613
1984	676	284	392	425	251	5.86	1	61	614
1985	519	201	318	394	125	4.3	1	26	492
1986	551	247	304	449	102	3.96	2	24	525
1987	557	254	303	450	107	4.18		17	540
1988	556	264	292	484	72	4.34		4	552
1989	493	174	319	426	67	4.05			493
1990	547	278	269	462	85	4.99		14	533
1991	530	201	329	386	144	4.76	2	57	471
1992	632	213	419	450	182	5.62	7	82	543
1993	516	204	312	445	71	1.49	18	263	235
1994	471	186	285	399	72	1.86	53	186	232
1995	460	178	282	390	70	0.16	9	162	289
1996	448	194	254	382	66	2.16	3	164	281
1997[c]	346	207	139	292	54	1.62	1	50	295
Total	10,945	4,567	6,378	8,041	2,904		102	1,142	9,701

[a]Long contracts have duration longer than one year.
[b]Includes 1976 contracts.
[c]Includes 1998 and 1999 contracts.

not very sensitive to this distinction. To conserve space, the histograms in Figure 1 below refer only to the series that includes COLA adjustment, as this is likely to provide a more complete characterization, but histograms based on the alternative definition are very similar and are available on request. In this paper, wage change refers to what the negotiating parties implement over the whole contract *at annual rates* and appears in our sample as one observation for each contract.[11] Contract re-openers, lump-sum payments,[12] and profit-sharing are very rare and are not taken into account.

Table 1 below contains, for each year, the number of all contracts, as well as the number of contracts by sector and contract duration.[13] A total of 10,945 contracts are

[11]An alternative approach involves defining subperiods of the contract and establishing wage adjustment over each of these. For a discussion of this issue, see Fortin (1996) and Freedman and Macklem (1998). We prefer the current specification because it summarizes the overall intentions of the contract. See the information in footnote 14.

[12]A possible concern is that lump-sum payments may circumvent apparent nominal rigidities, particularly where nominal wage freezes or reductions are involved. This is not the case: among the 102 contracts involving nominal wage reductions, only 0.98% involved a lump-sum increase. This percentage was 4.82% and 6.11% in the case of freezes and wage increases, respectively. There is no information in the data base regarding the *size* of lump-sum payments.

[13]Because of the smaller number of contracts, the first two and the last three years in the sample are considered together in everything that follows.

spread over the 21 "years" in Table 1, with a low of 226 contracts in 1977 and a high of 676 contracts in 1984. Because of the broad definition of the public sector, it includes more agreements than does the private sector. Considerably more contracts are long than short, and the modal length is two years. The last four columns in Table 1 provide the annual $C\dot{P}I$ and a breakdown of $\dot{W}$ by its sign. In most years, the number of agreements involving negative wage change over the life of a contract is negligible.[14] The number of freezes reaches its maximum in the low inflation year of 1993, when 51% of the agreements signed entailed no wage adjustment. Further descriptive statistics, including the rate of price inflation as well as mean and median wage adjustment, appear in Table 2 below. Figure 1 and Tables 1 and 2 are considered in detail in the next section.

Features of Wage-Change Distributions

Figure 1 presents wage-adjustment histograms for each of the 21 year groups in the sample. In constructing these, we took care to center the bins on zero.[15] For the high-inflation years of 1977 through around 1983, the histograms are centered well to the right of zero. They are reasonably symmetric and display no pronounced spikes at zero.

A substantial portion of the wage settlements in each year imply negative *real* adjustments that could be quantitatively important. For instance, in 1978, when the annual $C\dot{P}I$ inflation rate was 8.01% (column 6, Table 1), the average change in the CPI over the life of all contracts that became effective in that year ($\dot{P}$ in column 1, Table 2) was 9.93%, and the average wage change including COLA was 8.16%, most contracts entailed real wage reductions, some of which were as high as about 10%. The average real wage change for contracts that became effective in 1978 (*MRW* in column 2, Table 2) was –1.81%, with a standard deviation (*SDRW* in column 3, Table 2) of 2.7%. Only 102 of the 10,945 contracts studied involved *nominal* wage reductions. Between 1980 and 1983, the real wage change over the life of effective contracts was, on average, positive, but, as Figure 1 in combination with column (1) of Table 2 shows, many contracts entailed negative real adjustments. It is noteworthy that the standard deviation of the real wage rate SDRW was very high (for example, 3.38% in 1981) during this period. This reflects the wide domain over which real wage change will range during high-inflation periods.

The general appearance of the histograms changes noticeably for the period between 1982, when the annual $C\dot{P}I$ (column 6, Table 1)[16] began to abate, and the late 1980s, when average real wages (*MRW* in Table 2) began to increase again: in that period, they are generally characterized by noteworthy mass and censoring at zero, no nominal wage decreases, and strong asymmetries. In 1987, MRW was –0.75% and SDRW was 1.76%, both lower than in 1978, a year of much higher price inflation. This pattern of downward rigidity combined with a reduced scope for real wage reductions

[14]The intra-contract behavior of the nominal wage rate is fairly regular, with most agreements being front-loaded. For instance, across one-year contracts involving positive wage change, 88.6% of all nominal wage adjustment occurs in the first month of the agreement. The percentages for two-year contracts are 39.7% in the first and 53.5% in the thirteenth month of the contract, respectively. However, some agreements display surprising intra-contract variation: for instance, one-year agreements involving a nominal wage cut have, on average, deeper cuts in the first month than in the contract as a whole. This suggests that (a) survey data that must reflect the monthly remuneration may contain noise, that is, nominal wage changes which are later reversed, and (b) characterizing the wage change in a contract using arbitrary subperiods may not accurately reflect the overall intentions of the agreement.

[15]That is, the zero interval is -0.5 to 0.49999. Intervals increase and decrease in 1% units.

[16]Note that since contract duration spans a number of years, P in column 1, Table 2, begins to decline earlier than *CPI* in Table 1.

achieved through nominal freezes or small increases will be seen even more clearly in the low inflation period at the end of the sample.

During 1988–90, average wage adjustment (column 5, Table 2) increased, and in 1989–90 it actually exceeded $\dot{P}$ on average (Table 2). Histograms for these three years are quite symmetric and the descent to zero is reasonably smooth. Despite the fact that wage and price inflation was considerably lower during 1988–90 than during 1977–82, these histograms are similar in general appearance to that for 1978, for example, and seem to have been substantially influenced by the easing of labor market conditions. This experience suggests a capacity for this labor market to operate smoothly at inflation rates in the region of 4%.

After 1990, wage and price inflation declined to levels that are unprecedented in recent decades and much lower than those in the United States for the same years. It is histograms like those for 1991 and 1992 (but based on first-year wage change only) that led Fortin (1996) to argue that extensive nominal wage rigidity was present in the Canadian economy. These histograms display considerable mass and very strong censoring at zero. The concentration of mass at zero is so pronounced that even though $\dot{P}$ was extremely low, a very substantial proportion of contracts experienced real wage declines. Indeed, during 1993–96, the average real wage change, MRW, was negative.

Naturally, the extent, as opposed to the incidence, of real wage reductions was limited by the fact that CPI inflation was exceptionally low; most real wage reductions were of the order of 1–2%, and SDRW declined to its minimum of 1.2% in 1995. Nominal wage reductions were the exception rather than the rule, though it should be noted that, in the exceptional year of 1994, 53 contracts involved nominal wage reductions (column 7, Table 1). It is noteworthy that 38 of these were in Alberta and 37 were in Alberta's public sector, reflecting the province's political outlook at the time. The remaining 15 contracts were distributed over four other provinces, but only 3 of these were in the private sector. Thus, to the extent that contracts involving wage cuts exemplify the absence of downward nominal rigidity, this was not achieved in the private sector.

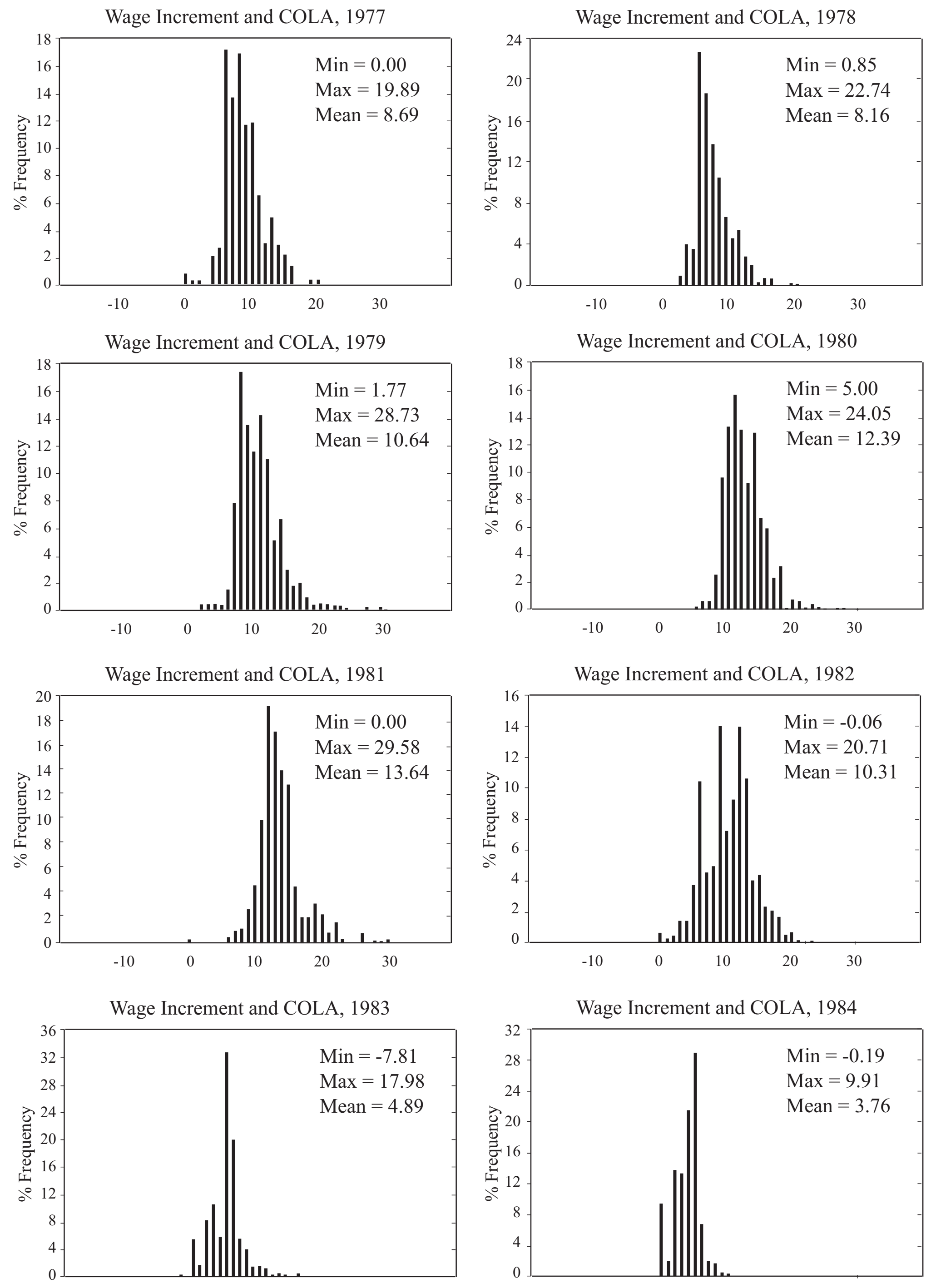
It is also noteworthy that some of the cuts experienced during 1994 were "undone" in the next contract signed by these pairs. The average wage increase in the next contract signed by the 51 (out of the 53) extant bargaining pairs was 1.74%, higher than the average of 0.43% achieved in the 178 contracts that had freezes in 1994, and higher too than the 1.65% achieved in the 200 contracts that had positive wage adjustment in 1994. (The sum of 51, 178, and 200 is smaller than the 471 contracts shown for 1994 in Table 1 because some of the latter did not have a subsequent contract.) A similar analysis for 1993 reveals that of the 18 contracts with a wage cut, the 17 extant pairs achieved an average wage increase in their next contract equal to 0.93%, a figure higher than the 0.28% agreed to by the 254 extant (out of 263) bargaining pairs that imposed a wage freeze. However, the pairs that had wage increases during 1993 had the highest average settlement during the next contract.

One would expect that nominal wage rigidity would be stronger when W is defined, as in the rightmost part of Table 2, to exclude COLA adjustments. MRW is algebraically larger with COLA than without. However, the effect of indexation is very small. In 1994, for example, there is only one more observation (54) involving a wage cut when COLA is excluded, and the number of wage freezes is 186 with COLA included and 196 with COLA excluded.

The apparent absence of nominal wage reductions from the histograms of Figure 1 may raise the concern that the zero bin (that is, the –0.5 to 0.4999 bin) may hide a substantial number of very small nominal wage reductions. This is not the case. Most of the mass in the zero bin is at zero itself (see columns 7–9, Table 1), and there are few wage cuts in this or in lower bins, for that matter.

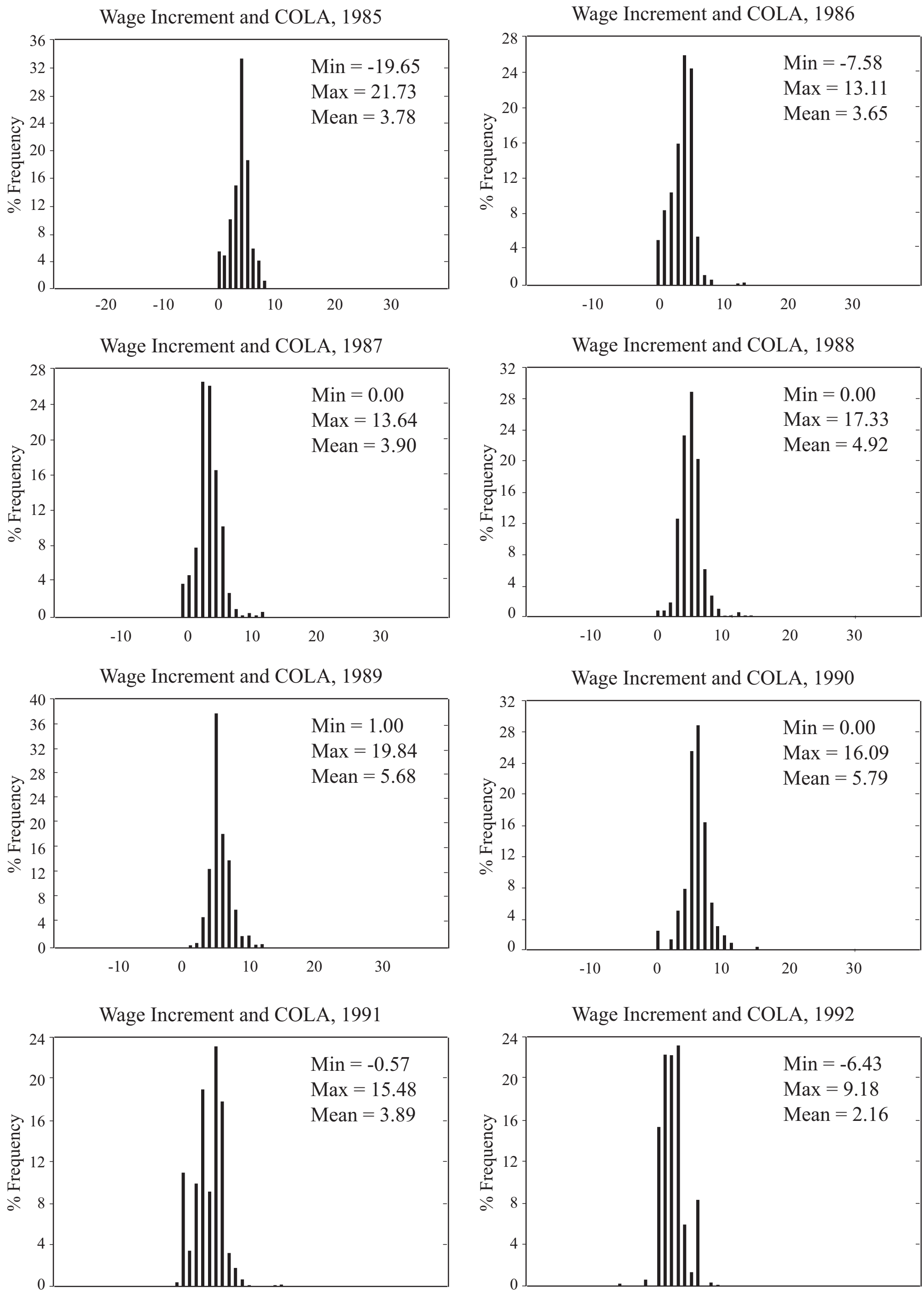
The review of the evidence above shows clearly that *nominal* wage rates in wage con-

Figure 1. Nominal Wage Change Distributions, 1977-1997.



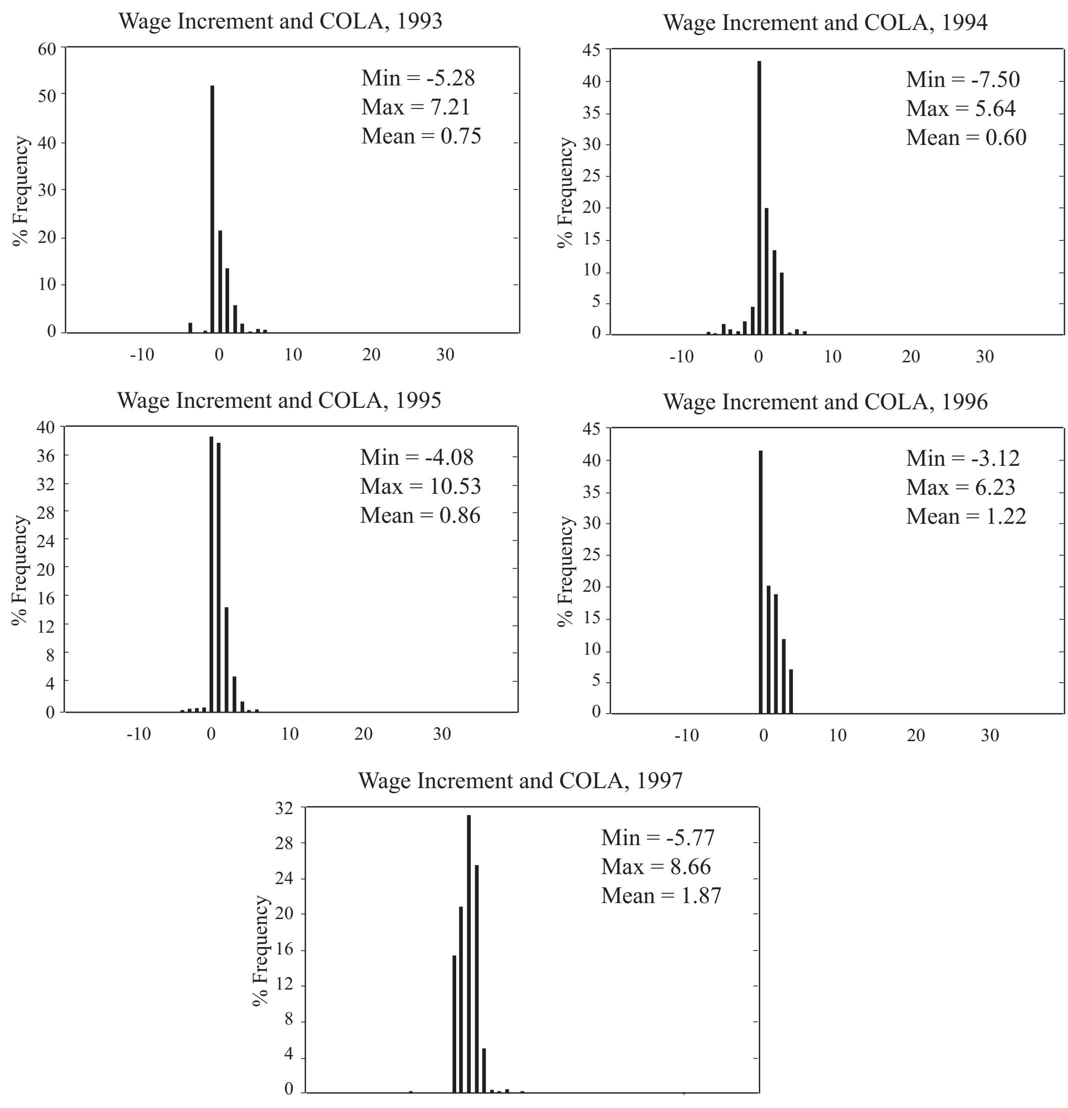
(continued)

Figure 1. Nominal Wage Change Distributions, 1977-1997 (cont'd).



(continued)

Figure 1. Nominal Wage Change Distributions, 1977-1997 (cont'd).



tracts are downwardly rigid and that when wage cuts occurred, they were concentrated in the public sector and tended to be undone in subsequent contracts. Our discussion also suggests that much more substantial *real* wage reductions can be achieved by default (that is, through nominal freezes) during high-inflation periods than during low-inflation periods. One suspects that, unless a case can be made that the need for real wage realignment is lower during periods of low inflation, this reduced scope for real wage reductions may have some impact on the smooth functioning of the labor market. It is important, therefore, to turn to some more formal statistical tests of features of interest in the nominal wage-change distributions.

Test Statistics and Econometric Results

Introduction and Descriptive Statistics

One feature of the new literature dealing with nominal wage rigidity is its concern with the symmetry of the wage-change distribution. During a period of high inflation, the nominal wage change distribution may be symmetric around some measure of inflation plus average productivity growth. By contrast, when for given average productivity growth inflation is low, some sectoral shocks may require decreases in nominal wage rates. *If* there is downward nominal wage rigidity, then wage-change distributions may display considerable mass at zero and be more asymmetric than in periods of high inflation.

Columns (4)–(6) and (10)–(12) of Table 2 present descriptive statistics that have been used to address this issue. In a right-skewed distribution, the mean will exceed the median. This is actually the case in most periods, a fact that accords with McLaughlin's (1999a) conclusions. Similarly, the skewness coefficients ("Skew") in Table 2 tend to be positive. Though they show no clear tendency to increase as inflation abates, more formal, non-parametric symmetry test results based on Ahmad and Li (1997) show a clear trend toward asymmetry as inflation subsides.[17]

Despite this evidence, symmetry tests may not provide a conclusive indication of the presence of DNWR. While at times of low inflation DNWR is likely to induce asymmetries, DNWR need not be the only cause of asymmetries. It is conceivable that the distribution of sectoral productivity shocks is systematically altered by inflation so that it is symmetric during high-inflation periods and asymmetric during low-inflation periods. No theory or empirical evidence exists on this point. A second possible complication is that some high-inflation distributions may themselves be asymmetric.[18] A possible source of asymmetry in the nominal wage-change distribution even in high-inflation periods is downward *real* wage rigidity. If present, it would censor the nominal wage-change distribution at the rate of price inflation. However, as already seen, there appears to be no evidence of downward *real* wage rigidity in the histograms of Figure 1. Thus, while indications of increased asymmetries in wage-change distributions do not prove the existence of DNWR, they are very suggestive and, in combination with other arguments, symmetry tests may be convincing.

In the next subsections, we focus on specific areas of and points on the wage-change distribution. In particular, we examine the area at and below zero, particularly as it relates to a no rigidity counterfactual.

Tail Behavior

A feature of nominal wage-change distributions that is of considerable interest is the extent to which mass at and below zero is unusual relative to some benchmark. Card and Hyslop (1997) assumed that the area above the median may be used as the no-rigidity counterfactual for the area below the median. They measured the extent of nominal wage rigidity by subtracting an appropriate integral of this counterfactual area from that of the actual density function to the left of the median. McLaughlin's (1999a) symmetrically differenced histograms are similar in spirit, except that they refer to the entire range of the distribution. Lebow, Stocton, and Wascher (1995:13) and Lebow, Stockton, and Wilson (1999:5) proposed tests measuring the difference between the left-hand and right-

[17]These results are available on request. Christofides and Stengos (2001) confirmed this for paid workers using U.S. data from the Panel Study of Income Dynamics.

[18]McLaughlin (1999a:130) provided possible reasons for asymmetries. On the other hand, Card and Hyslop (1997:86) noted that "most conventional models of wage determination imply symmetry."

Table 2. Descriptive and Test Statistics.

		Wage Change Includes COLA						Wage Change Excludes COLA					
Year	$\dot{P}$	*MRW*	*SDRW*	*Median*	*Mean*	*Skew*	$\hat{D}_n$	*MRW*	*SDRW*	*Median*	*Mean*	*Skew*	$\hat{D}_n$
1977[a]	9.58	–0.90	2.75	8.20	8.69	0.57	–0.09	–3.10	2.60	6.70	6.48	0.21	–0.80
1978	9.93	–1.81	2.70	7.42	8.16	1.30	1.02	–2.82	2.75	6.78	7.12	1.11	0.62
1979	11.53	–0.91	3.01	10.09	10.64	1.26	0.45	–3.11	3.74	8.63	8.41	0.21	0.36
1980	12.23	0.17	3.03	11.94	12.39	0.71	0.13	–1.04	3.35	11.03	11.15	–0.06	0.30
1981	9.51	4.14	3.38	13.09	13.64	1.03	0.12	3.27	3.98	12.87	12.76	–0.16	–0.09
1982	5.93	4.39	3.01	10.63	10.31	–0.01	–0.44	3.93	3.47	10.02	9.85	–0.25	–1.35
1983	4.46	0.44	2.67	5.00	4.89	0.60	–1.67	0.02	2.81	5.00	4.47	0.16	–4.83
1984	4.16	–0.40	1.87	4.00	3.76	–0.17	–4.44	–0.69	2.03	3.82	3.45	0.02	–6.03
1985	4.35	–0.56	2.17	4.04	3.78	–1.44	–2.34	–0.90	2.26	3.79	3.44	–1.12	–3.43
1986	4.41	–0.76	1.84	4.09	3.65	–0.07	–2.48	–0.97	1.90	3.76	3.44	0.08	–2.88
1987	4.65	–0.75	1.76	3.82	3.90	0.83	–1.29	–1.09	1.92	3.40	3.56	0.83	–2.33
1988	5.16	–0.24	1.78	4.89	4.92	1.44	0.03	–0.56	1.99	4.68	4.61	1.00	–0.79
1989	5.01	0.68	1.87	5.22	5.68	1.84	0.54	0.42	1.95	5.12	5.41	1.39	0.53
1990	3.90	1.88	2.16	5.77	5.79	0.47	–0.90	1.53	2.34	5.65	5.43	0.29	–1.58
1991	1.50	2.39	2.19	4.82	3.89	0.15	–5.20	2.20	2.20	3.90	3.69	0.28	–5.76
1992	1.50	0.66	1.68	2.00	2.16	0.40	–5.07	0.61	1.70	1.97	2.11	0.37	–5.69
1993	1.14	–0.39	1.41	0.00	0.75	1.00	n/a	–0.49	1.30	0.00	0.65	0.90	n/a
1994	1.80	–1.20	1.75	0.00	0.60	–0.79	n/a	–1.29	1.71	0.00	0.51	–0.77	n/a
1995	1.56	–0.69	1.20	0.68	0.86	1.98	–11.72	–0.74	1.15	0.68	0.82	1.99	–12.91
1996	1.62	–0.42	1.32	0.86	1.22	0.66	–10.20	–0.51	1.27	0.76	1.14	0.78	–10.55
1997	1.72	0.14	1.33	1.87	1.87	0.16	–4.94	0.03	1.29	1.71	1.76	0.07	–4.96

Notes: $\hat{D}_n = [1 - F_n(2 \times Median)] - F(0)$ is asymptotically normal. F is the empirical distribution function. Only values for $\alpha = 8$ are reported; those for $\alpha = 10$ are similar. MRW is the mean and SDRW the standard deviation of real wage change.

[a]Contracts for 1976 and 1977 have been merged into "1977" and those for 1997 to 1999 into "1997."

hand tails of the wage-change distribution.

Along similar lines, our own empirical measure, $\hat{D}_n$ (see the appendix for details), is used as a statistical, non-parametric test of the extent to which the left-hand tail, inclusive of zero and as measured by $F(0)$, contains more mass than the equivalent right-hand tail measured by $[1 - F(2 \times Median)]$.[19] To intuitively appreciate this measure and its use, imagine a symmetric distribution such as the normal. The median, which coincides with the mean in the case of a symmetric distribution, divides the area under this distribution in two. Since, for present purposes, the median is a positive number, it is equidistant between zero and the point $2 \times Median$. This latter point identifies a tail to its right that is exactly equal to the area below zero. Since, in a continuous symmetric distribution, density at any one point is zero, $[1 - F(2 \times Median)]$ should be approximately equal to $F(0)$ and $D = [1 - F(2 \times Median)] - F(0)$ should be close to zero. As inflation decreases, if DNWR is not an issue, the wage-change distribution shifts to the left without a change in shape, the similarity between the two tails still holds, and D continues to be close to zero.

However, if DNWR is prevalent, lower inflation will coincide with more mass piling up at zero, and there will be a deficit in the area *below* zero relative to the area *to the right of* the point $2 \times Median$. We refer to the thinning of the tail below zero as r*elative* DNWR, to be distinguished from *absolute* DNWR, which is indicated by a spike at zero.

The test-statistic $\hat{D}_n$ can be set up so as to identify both relative and absolute DNWR by excluding zero and by focusing only on zero, respectively. However, since there are virtually no wage cuts in the contract data,[20] a statistic that excludes zero would consistently indicate relative rigidity and would add little to what we have already shown in the previous sections. Instead, we use $\hat{D}_n$ as specified in the appendix to include the points 0 and $2 \times Median$. As already noted, in a symmetric context and in the absence of DNWR, $\hat{D}_n$ so specified should be close to zero and would be insensitive to shifts of the wage-change distribution to the left, following decreases in the rate of price inflation. Since, in the present context, there is essentially no mass below zero, a value of $\hat{D}_n$ close to zero would reflect the existence of substantial mass *at the point zero itself* and would be a measure of absolute rigidity. A negative value of $\hat{D}_n$ would suggest even more concentration of mass at zero relative to the right tail. This can occur as a right-skewed distribution shifts to the left when inflation abates.[21] Thus, in what follows, we will be looking for indications of absolute DNWR in small or negative values of $\hat{D}_n$, while statistically significant positive values would suggest a rejection of absolute DNWR.

Columns (7) and (13) of Table 2 indicate that the values of the calculated test-statistic $\hat{D}_n$ reflect very closely the visual evidence in Figure 1; these columns provide a statistical basis for how $\hat{D}_n$ changes with inflation. To begin with, in the high-inflation years, $\hat{D}_n$ tends to have positive values, suggesting that mass essentially at zero is small relative to the right-hand tail. During the low-inflation years, beginning with the years of moderating inflation, $\hat{D}_n$

[19]Note that $[1 - F(2 \times Median)]$ excludes the point $2 \times Median$, while $F(0)$ includes zero. In a continuous distribution this is innocuous, as mass at any one point is zero. However, given that in the present empirical context mass at zero can be substantial, the inclusion of the point zero in $F(0)$ requires due care. The exclusion of the point $2 \times Median$ from $[1 - F(2 \times Median)]$ is of less consequence, as mass at this point is negligible.

[20]Histograms, such as those in Figure 1, blur the distinction between relative and absolute DNWR, because the zero bin (–0.5 to 0.4999) includes modest wage cuts. However, as seen at the end of the section "Features of Wage-Change Distributions," most of the mass in the zero bin is at zero itself, and there are virtually no wage cuts in most years.

[21]As already noted, the wage-change distributions in these data tend to be skewed to the right.

becomes negative and is usually significantly negative. That is, the concentration of mass at zero is significantly larger than that in the right-hand tail.[22] By 1995, for instance, the distribution that includes COLA indicates that the left-hand side contains 11.72 percentage points more mass than the counterfactual, right-hand, tail. As can be seen from Table 1, most of this mass (162 observations) is at zero and only 9 contracts entail nominal wage cuts.

The tendency for $\hat{D}_n$ to decline algebraically as inflation decreases is shown clearly in Figure 2. The top line in Figure 2 plots the average rate of inflation prevailing over contracts (*P,* in Table 2), using the 21 observations for 1977–97. The two bottom lines plot $\hat{D}_n$ for the same period when COLA is included and excluded (columns 7 and 13, respectively, in Table 2). A strong positive relation between inflation and the test-statistics is suggested. This can be confirmed using an OLS regression of $\hat{D}_n$ on a constant and $\dot{P}$.

We ran this regression for the whole sample, including and excluding COLA adjustments, and for subsamples defined over the private and public sectors and long and short contracts. Interest in the public/private sector split arises because of the vast literature dealing with differences across the two sectors in the wage-determination process and because of the finding, noted earlier, that wage cuts were more prevalent in the public sector. The split between short and long contracts is useful because Fortin (1996) and others have examined first-year wage change only, which, in the case of short contracts, is essentially the same as our own definition. Thus, the sample split along duration lines provides a link with other studies in the area.

The slope coefficient in each of these regression equations is positive and almost always significantly different from zero, suggesting that, as inflation moderates, $\hat{D}_n$ declines.[23] We conclude, therefore, that the accumulation of mass at zero is statistically significant and that this is all the more so when inflation subsides. As already noted, this is an indication of absolute, rather than relative, rigidity.

In the next subsection we analyze in a more tractable way, using limited dependent variable models, the probability of wage cuts and freezes and its dependence on the rate of inflation. In light of the virtual absence of wage cuts from this sample, we do not attempt to estimate multinomial models, and our results continue to deal with absolute rigidity.

Probability of Non-Positive Wage Adjustment

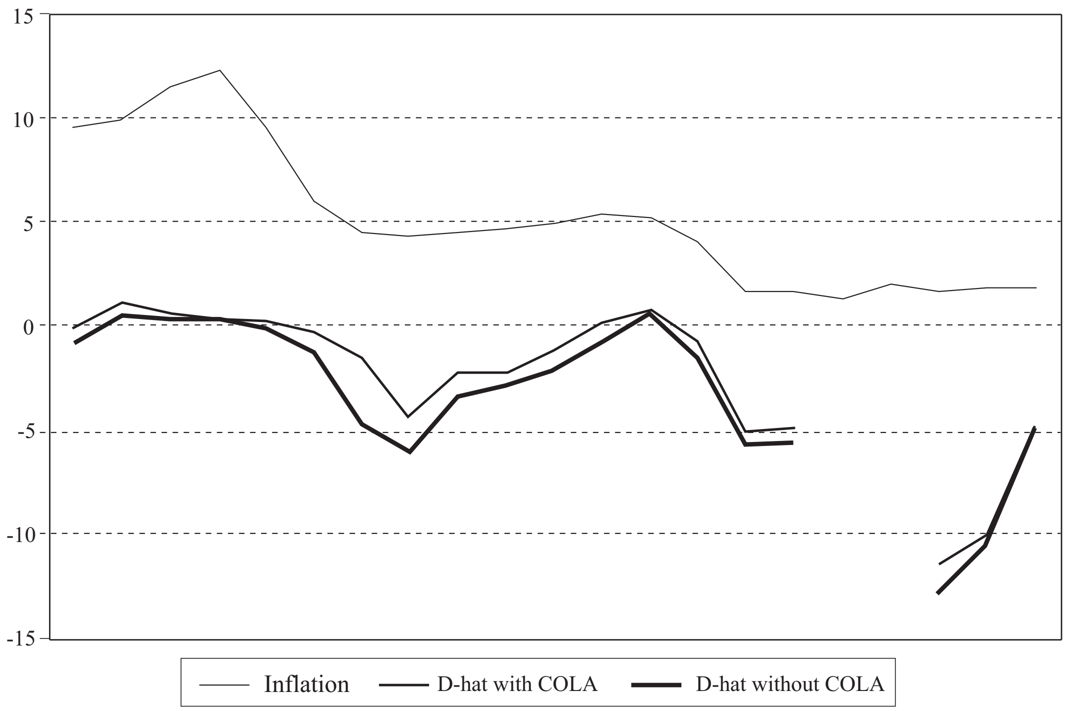
We estimate Probit and Tobit models with the aim of calculating the probability $\Phi(\cdot)$ of a positive adjustment and $1 - \Phi(\cdot)$ of a non-positive (generally a wage freeze) adjustment and the dependence of these probabilities on the rate of inflation. Here, Φ is the cumulative standard normal distribution. We estimate three simple models. In "Probit 1," the index equals unity if nominal wage adjustment is positive and equals zero otherwise. We use the entire sample of 10,945 observations and treat the 102 observations involving wage cuts as freezes. In "Probit 2," we exclude the 102 observations, with the result that the probit index equals zero for wage freezes only. Finally, we estimate a Tobit model using only the 10,843 non-negative observations. All equations include a constant, $\dot{P}$, the provincial unemployment rate, ten industry effects, and five regional effects.

Abbreviated results appear in Table 3. The unemployment rate has a coefficient that is negative and statistically significant, while the inflation rate has a positive and statistically significant coefficient. Statistically significant industry and regional ef-

[22]Note that when the median of the wage-change distribution is itself zero, as in 1993–94, the $\hat{D}_n$ statistic is not defined.

[23]These results are unreported in order to conserve space, but they are available on request to the authors.

Figure 2. Inflation and Test Statistics.



fects are also present. The positive coefficient on the inflation variable suggests that, as inflation moderates, the probability of a non-positive wage adjustment, which is calculated as $1 - \Phi\ (X\beta)$ in the Probit models and $1\text{-}\Phi\ (X\beta/\sigma)$ in the Tobit model, increases. Here, β is the estimated vector of coefficients in Table 3 and σ is the estimated standard deviation of the error term in the Tobit model. We evaluate these probabilities at the mean values of all variables except the inflation rate, which we allow to range from zero to 10%, and plot them in Figure 3.

The three models produce profiles that are extremely close to each other.[24] The probability of a non-positive adjustment is less than 5% at the mean rate of $\dot{P}$ of about 5%. This is remarkably low but exactly what our earlier analysis and Figure 1 would predict. This probability increases steeply as the inflation rate declines, and it is around 25–30% at rates of inflation between 1–2% such as those prevailing during the low-inflation period. That is, freezes become five times more likely in the low-inflation period than in the medium-inflation period. The probability of a freeze or cut is essentially zero at the rates of inflation prevailing in the high-inflation era.

Conclusion

To the extent that downward nominal rigidity is present, it is more likely to be prevalent in the union sector. Accordingly, we use data from collective bargaining agreements reached in Canada between

[24]Standard errors can be calculated using the delta method, and they would be small given the fit (see Greene 2000:824). We do not report them in Figure 3, as doing so would confuse the presentation.

Table 3. Modeling the Incidence of Positive Wage Adjustment.

	Probit 1		*Probit 2*		*Tobit*	
Description	*Coeff.*	*Coeff./ St. Error*	*Coeff.*	*Coeff./ St. Error*	*Coeff.*	*Coeff./ St. Error*
Constant	1.23	8.45	1.23	8.28	2.23	10.31
$\dot{P}$	0.41	29.48	0.40	28.37	0.94	98.37
Regional Unemp. Rate	–0.16	–13.55	–0.16	–13.07	–0.26	–16.04
Industry Effects[a]	yes		yes		yes	
Regional Effects[b]	yes		yes		yes	
σ				2.88	136.91	
No. Observations	10,945		10,843		10,843	
No. Possible Observations	9,701		9,701		9,701	
Log Likelihood	–2,681		–2,543		–25,071	

Note: For Probit equations, the dependent variable is coded as 1 if wage adjustment is positive and as 0 otherwise. For Tobit, zeroes are as in Probit and actual wage adjustment is used when positive.

[a]Eleven industries are distinguished.

[b]Six regions are distinguished.

1976 and 1999 to study the implied wage-change distributions using a variety of techniques and tests. A number of interesting conclusions emerge, although it should be stressed that the results summarized below may not generalize to the economy at large.

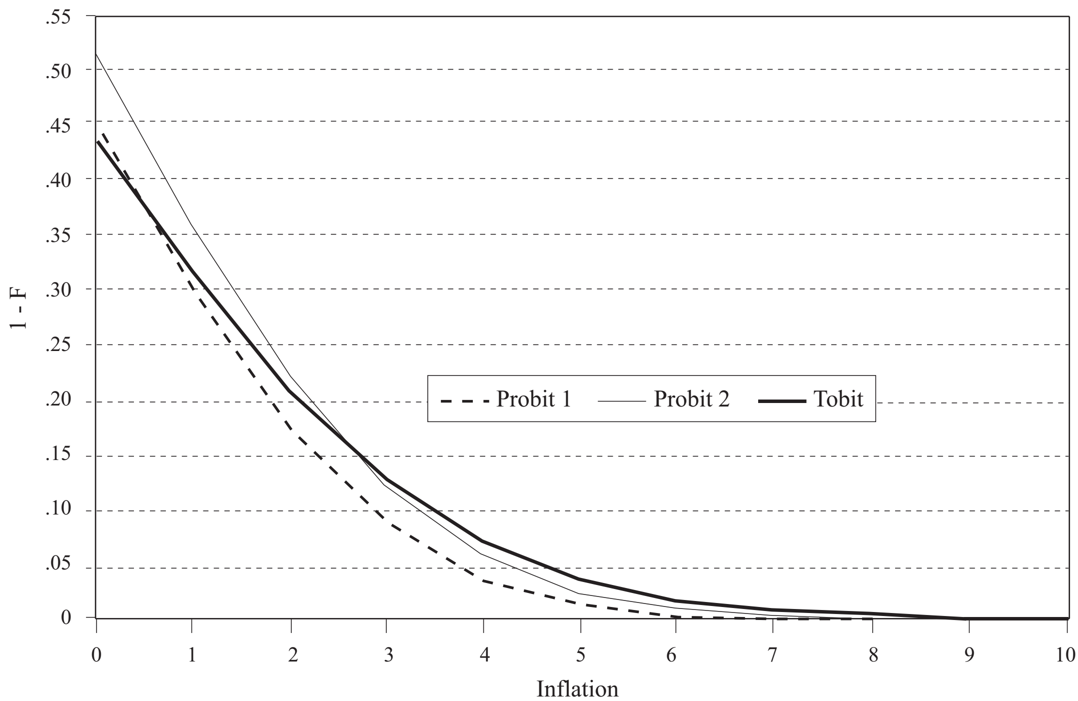
The period under study may be divided into subperiods of high inflation (1976–82), medium inflation (1983–90), and very low inflation (1991–99). In the high-inflation period, the wage-change distributions contain no spikes at zero, have left- and right-hand tails that contain about the same mass, and tend to be symmetric. In the low-inflation period the picture is decidedly different. To begin with, very substantial spikes at zero are in evidence, the most pronounced of which, in 1993, has mass in excess of 50%. In addition, the left-hand tail, including zero, is significantly heavier than the right-hand tail that serves as the no-rigidity counterfactual. During the medium inflation period, when the inflation rate is halved, spikes at zero begin to appear, mass begins to concentrate in the left-hand tail, and the distributions for some years are asymmetric.

One way to get an overview of these results is to employ a non-parametric test-statistic that compares the left and right-hand tails of the wage-change distribution. This statistic is systematically related to the rate of inflation (see Figure 2). As inflation decreases during the 1990s, mass in the left-hand tail, including zero, becomes significantly larger than mass in the right-hand tail. Given that virtually no wage cuts can be identified in the contract data, this suggests that significant absolute downward nominal rigidity emerged during low-inflation periods. It should be stressed that these are periods when the inflation rate in Canada was exceptionally low—in the range of 0.16–2.16%.

Since only 102 observations out of the 10,945 entailed wage reductions, these may be ignored or designated as freezes, and Probit and Tobit models may then be estimated. These estimations suggest that the probability of a freeze was zero in the high-inflation period, around 5% in the medium-inflation era, and around 25–30% in the more recent period of low inflation.

Our results shed light on the inflation as "grease" hypothesis. Absolute (that is, spikes at zero), rather than relative (that is, thinning of the tail below zero), downward *nominal* rigidity is very much in evidence in the contract data. By contrast, we can report little evidence of *real* wage rigidity. The *incidence* of real wage reductions was

Figure 3. Probability of Non-Positive Nominal Wage Change.



high during the high-inflation period, and it remained high even as inflation abated. In the low-inflation period, real wage reductions were frequent because of the substantial concentration of mass at exactly zero, just lower than the average rate of CPI inflation. Nevertheless, the *magnitude* of real wage reductions during the low-inflation period (around 1–2%) was considerably lower than that experienced during the high-inflation period (around 10–12%). Thus, the degree of real wage re-alignment that could be achieved by default (that is, through a nominal wage freeze) during the low-inflation period was modest. This suggests that unless real wage realignment is less needed in low-inflation periods, the labor market for the agents studied may not be functioning as smoothly as in periods of higher inflation. Could such inefficiencies be responsible for some unemployment? This is essentially the position in Fortin (1996), but our paper is not focused on this particular point.

It should be noted that the results above are independent of the indexation provisions in these collective bargaining agreements. Indexation mutes both absolute and relative downward nominal rigidity, but its effects are very modest. The results, in Table 2 and Figure 2, are very similar whether or not cost-of-living adjustments are included in the definition of the wage-change variable.

When the results are broken down by sector (public and private) and length of contract (one year or less and ever one year), tail behavior is not substantially affected. There is evidence that when nominal wage cuts were attained, this was much more likely to happen in the public sector, when governments were sufficiently powerful to brave the morale and other problems associated with such actions. This is a surprising finding that underscores the pervasiveness of downward nominal wage rigidity in the private sector. Also of interest is evidence in the contract data for 1993 and

1994 (the only years with noteworthy wage cuts) that where nominal wages were reduced, subsequent contract adjustments were above those achieved in contracts with freezes and (in 1994 only) indeed wage increases, thereby muting the impact of these reductions.

This rather strong form of downward nominal rigidity in the base wage rates of collective bargaining agreements cannot be dismissed on the grounds that it emanates from data inaccuracies. Changes in benefits could, of course, produce more flexibility, but benefit packages are costly to change and, as Lebow, Stockton, and Wilson (1999) have shown in a different context, this issue is not decisive. Wage drift, overtime premia, and other internal adjustments undoubtedly provide added flexibility in some cases, though these are difficult to take into account. It is likely that the formal and informal agreements that prevail in the non-union sector entail more flexibility, but good-quality, long-term panel data for the non-union sector in Canada are not available. Thus, contract data remain a valuable source of information about the impact on wage behavior of exceptional decreases in the rate of inflation.

Appendix

The test statistic $\hat{D}_n$ is the difference of the empirical distribution function of wage change, *x,* above $2 \times Median$ and at and below 0. Under the null hypothesis, observations in the right-hand tail will occur with the same probability as observations in the left-hand tail, and the test statistic will be centered around zero. Let $F_n(x)$ be the empirical distribution function based on the sample of n observations on x, and use as an estimate of $F_n(x)$,

$$\hat{F}_n(x) = (nn_x a)^{-1} \textstyle\sum_{i=1}^{n} \sum_{j=1}^{n_x} \left[K\left(\frac{X_i - X_j}{a}\right)\right],$$

where n_x denotes observations up to and including the point x. The function $K(\cdot)$ is the kernel function, a known density symmetric about zero, and $a = a_n$ is a sequence of smoothing parameters (bandwidths) such that *an* approaches zero as the sample size n approaches infinity. We use the standard normal density as our kernel. We define the test statistic $\hat{D}_n$ as

$$\hat{D}_n = [1 - F_n(2medx)] - F_n(0)$$

$$\hat{D}_n = \{1 - [n[n_{2medx}) a]^{-1} \sum_{i=1}^{n} \sum_{j=1}^{n2medx} [K(\frac{X_i - X_j}{a})]\} - [n(n_0) a)^{-1} \sum_{i=1}^{n} \sum_{j=1}^{n0} [K(\frac{X_i - X_j}{a})],$$

where $n_2 medx$ and n_0 denote the number of observations up to and including the point of twice the median and of zero, respectively. The distribution of $\hat{D}_n$ based on the comparison of two population proportions is straightforward to construct and follows the standard normal variate. Thus, standard critical values apply.

The smoothing parameter is chosen as $a_n = s_x n^{-1/\alpha}$, where s_x denotes the standard deviation of the sample data. Note that the values of the bandwidth are different for each sample analyzed, as they depend on the particular sample size n and on the standard deviation *sx*. Thus for $s_x = 2$, $\alpha = 8$, and $n = 300$, $a_n = 0.98$, while for $n = 600$, $a_n = 0.89$. In standard density estimation, $\alpha = 5$ is usually chosen. However, evidence from simulations by Ahmad and Li (1997) suggest a larger value of α than 5, since using the latter results in test statistics that tend to reject the null hypothesis of symmetry too often. A larger value of α results in greater smoothing. Hence, we present results, based on the normal kernel, for $\alpha = 8$. Note that $\hat{D}_n$ is two-tailed and that rejections in favor of negative values of $\hat{D}_n$ would signify evidence that the tail at and below zero dominates the tail above the point $2 \times Median$. The significance of such rejections for absolute rigidity is examined in the main text.

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