Downward Nominal Wage Rigidity in the OECD*

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Abstract

Recent microeconomic studies have documented extensive downward nominal wage rigidity (DNWR) for job stayers in many OECD countries, but critics argue that the effect might be undone by e.g. compositional changes and flexibility in wages of new entrants. Using data for hourly nominal wages at industry level, we explore the existence of DNWR on industry wages in 19 OECD countries, over the period 1973–1999. We propose a novel method to detect DNWR. We reject the hypothesis of no DNWR in the overall sample. The fraction of wage cuts prevented due to DNWR has fallen over time, from 61 percent in the 1970s to 16 percent in the late 1990s, but the number of industries affected by DNWR has increased. DNWR is more prevalent when unemployment is low and union density is high. Strict employment protection legislation also leads to fewer wage cuts.

JEL: E3, J3, J5
Keywords: downward nominal wage rigidity, OECD, employment protection legislation, wage setting

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Recent microeconomic studies have found considerable downward nominal wage rigidity (DNWR) for job stayers in OECD countries. The International Wage Flexibility Project (Dickens et al., 2007) finds that, for all 16 countries studied, DNWR prevents wage cuts from taking place, with the fraction of wage cuts prevented being in the range of 9–66 percent (see literature review in section 1). However, when it comes to identifying the effects of DNWR on unemployment and output, the results are disputed. While Fehr and Goette (2005) show that DNWR is associated with higher unemployment in Swiss cantons in the low inflation period of the 1990s, consistent with Tobin's contention (Tobin, 1972), the ECB's evaluation of its monetary policy framework, concludes that '... the empirical evidence is not conclusive, particularly for the euro area' (ECB, 2003, page 14).

One possible explanation for the disputable aggregate effects of DNWR, in spite of strong microeconomic evidence, may be that the DNWR at the individual level is undone by firm behaviour and market mechanisms. With annual job turnover rates above 20 percent, as is the case in many OECD countries (see Haltiwanger, Scarpetta, and Schweiger, 2006), and generally higher worker turnover rates, rigid nominal wages for many individual job stayers need not imply the same rigidity of average wages. Indeed, studying the wage adjustments of different groups of Canadian employees, Fares and Lemieux (2000) find that the bulk of the real-wage adjustment over the business cycle is experienced by new entrants. Fares and Lemieux argue that this may explain why DNWR has little effect on aggregate wage setting, despite it being important for some groups of workers.

The lack of consensus on the macroeconomic effects of DNWR despite the clear evidence of DNWR for individual workers motivates a closer look at the link from individual DNWR to aggregate effects. We explore one part of this link: whether DNWR is apparent in aggregate wages. Specifically, we explore the existence of DNWR in industry-level wage data for 19 OECD countries, for the period 1973–99. If we do find DNWR in industry wages, it would be evidence that not all individual DNWR is undone by compositional effects and market forces. This would strengthen the ex ante case for the contention that DNWR affects output and employment, although it clearly provides no direct evidence. On the other hand, if we are not able to detect any DNWR in industry data, it is not likely that DNWR at the individual level have any effect on output and employment.

In line with previous studies, we construct the notional wage change distribution (i.e., the assumed distribution under flexible wages) on the basis of observations from country-years when the wage growth is high, and thus DNWR is not likely to bind. By comparing empirical and notional country-year specific wage change distributions, we can construct country-year specific estimates of the extent of DNWR, given by the deficit of wage cuts in the empirical samples.

The paper makes several contributions to the literature. First, we propose a novel method of testing the significance of DNWR, which we outline in section 2. The key methodological novelty of our approach is the way we test for the significance of our DNWR estimates. Specifically, we construct country-year specific probabilities of a wage cut from the notional country-year specific distributions. Hence we can compute the probability distribution for the number of notional wage cuts, and compare this distribution with the number of empirical wage cuts. Thus, we can calculate the exact p-value for the number of empirical wage cuts, under the null hypothesis of no DNWR, as given by the notional probabilities. The method is less data intensive than most of the previous methods, allowing us to use it on industry data.

Second, reflecting that we make use of industry data, we have broader coverage across countries and time than previous studies. Thus, we provide consistent evidence of DNWR on industry data across OECD countries over time in section 3. Third, in section 4 we explore how DNWR is affected by economic and institutional variables like inflation, unemployment, employment protection legislation, and union density, which is often difficult to evaluate in studies of a single country. We find that DNWR is more prevalent when union density is high. We do not find any significant effect on DNWR of strict employment protection legislation, although the point estimate is negative with a p-value of 12 percent, and the negative effect on the number of wage cuts is significant. This information is useful, as it sheds light both on possible explanations for DNWR and on how the extent of DNWR might be affected by economic policy. Section 5 concludes the paper.

1 DNWR and industry wages

The body of empirical work on DNWR has grown rapidly in recent years, with various types of evidence being employed. Blinder and Choi (1990); Akerlof, Dickens, and Perry (1996); Bewley (1999); and Agell and Lundborg (2003), among others, report evidence of DNWR based on interviews and surveys of employees and employers. Hanes (2000) consider historical evidence for the US, and Holden (1998) find evidence of DNWR in aggregate wage setting in the Nordic countries. Kimura and Ueda (2001) find mixed evidence on DNWR in industry data for Japan, using an extended Phillips curve specification. However, the great majority of studies explores large micro-data sets, based on personnel files, survey data or administrative data. Inspired by the skewness-location approach of McLaughlin (1994), these studies focus on the effect of inflation on the distribution of wage changes; some of the recent applications are Kahn (1997), Christofides and Leung (2003), Lebow, Saks, and Wilson (2003), Nickell and Quintini (2003), Kuroda and Yamamoto (2003), and Elsby (2006). Some studies extend this analysis by adding other explanatory variables that are usually included in wage equations; see e.g. Altonji and Devereux (2000), Fehr and Goette (2005) and Knoppik and Beissinger (2003). Multi-country studies by Dessy (2002), Dickens et al. (2007), and Knoppik and Beissinger (2005) have strengthened the evidence of extensive DNWR in many OECD countries.

Microeconomic studies typically explore the change in the hourly earnings of job stayers, while the observational unit in our data is the change in the average hourly earnings for all workers in the industry. Numerically, there are two differences between these data types. First, our data are averaged over many workers. Second, our data are affected by compositional changes, since the wages of new workers differ from the wages of those who leave.

One implication of these differences, is that prevalent DNWR among individual workers in an industry, apparent in micro data by a spike in the wage change distribution at zero, would not imply that the industry wage change is zero. Hence, we cannot test for DNWR by looking for spikes. One would expect however that prevalent DNWR at the individual level in a industry in most cases would prevent the average wage change in the industry from being negative. Thus, we are looking for a deficit of negative changes in average wages in the industries. However, there is no one-to-one relationship between DNWR at the individual level and at the industry level. Even if there is considerable DNWR at the individual level, average wage growth at the industry level might be negative if workers with high wages quit, as is the case with older workers who retire. Alternatively, the change in average industry wages might be large positive, if other workers receive large wage increases.

Another issue is that a deficit of negative changes in average industry wages might also be caused by other mechanisms than DNWR, for example systematic cyclical compositional changes, as the share of low-skilled workers may decrease in recessions, pushing up average wages when they are most likely to fall (Solon, Barsky, and Parker, 1994 and Fares and Lemieux, 2000). Such effects may also vary across industries and depend on the cyclical situation, for example if young workers with low pay in industries hit by bad shocks have a stronger tendency to leave their industry in a recession, thus diminishing the negative wage effect of the shock. Distinguishing between DNWR and such asymmetric effects requires a careful specification of the empirical test, which we shall return to below.

An entirely different matter is if binding DNWR for some workers affect the wages of other workers in the industry, so that the effect of average industry wages is undone. One such effect would be if firms respond to downward rigidity for some workers by giving lower wage increases to other workers, or by changing the composition of the workforce, as suggested by Fares and Lemieux (2000). Workers who have their wages cut may also quit voluntarily, and new workers could take their job at the lower wage rate. Binding DNWR in one firm might also affect wages in other firms. For example, in recessions with negative productivity or demand shocks, firms that cannot lower their wages due to DNWR may respond by laying off workers, or at least by reducing the hiring of new workers. If workers have industry-specific skills, the higher number of unemployed workers might contribute to lower wages in other firms in the same industry where wages are flexible. If the lower wages induce other firms to take on more workers, the effects of DNWR in the inflexible firms will be offset. In these cases we will not detect any DNWR in our data.

On the other hand, if wages are rigid in other firms, for example due to unions or employment protection legislation, average wages in the industry will be affected by binding DNWR for some workers. The firms with rigid wages are unlikely to hire more workers, implying that the DNWR also affects industry employment. Thus, we see that whether DNWR at the individual level will affect employment at the industry level to a large extent depends on whether wage rigidity for some workers is reflected in lower wages elsewhere in the industry. A finding of significant DNWR in industry wages would consequently strengthen considerably the case for DNWR affecting output and unemployment.

2 Empirical approach

We use an unbalanced panel of industry-level data on the annual percentage growth of gross hourly earnings for manual workers from the manufacturing industry, mining and quarrying industries, electricity, gas and water supply industries, and construction industry of 19 OECD countries in the period 1973–1999. The countries included in the sample are Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, the UK, and the US. The main data sources for wages are harmonised hourly earn-

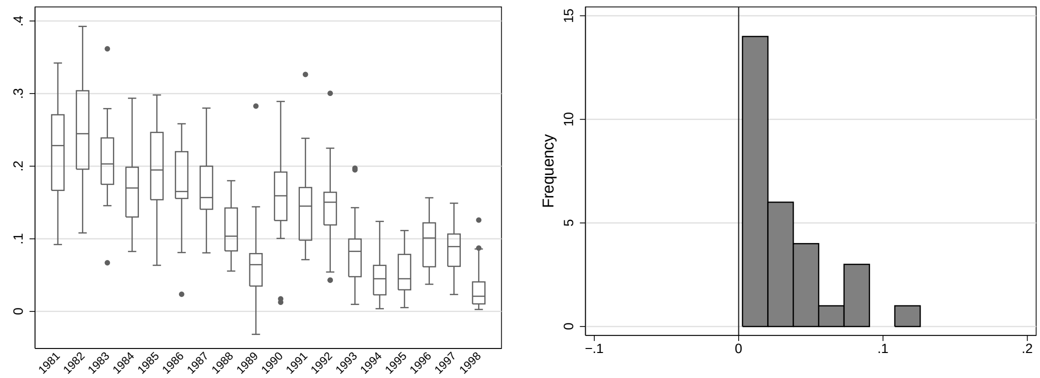

Figure 1: Box plots of annual wage growth in Portugal (left), and histogram of annual wage growth in 1998 (right). The box plots illustrate the distribution of wage changes within a country-year. The box extends from the 25th to the 75th percentile, with the median inside the box. The whiskers emerging from the box indicate the tails of the distributions, and the dots represent outliers.

ings from Eurostat and wages in manufacturing from ILO.[1] One observation is denoted Δw_{jit}, where j is the industry index, i is the country index, and t is the year index. All together, there are 9,509 wage-change observations distributed across 449 country-year samples, with an average of 21 industries per country-year. Figure 1 illustrates the data for Portugal over time (left) and for Portugal in 1998 (right). We see that the median wage growth and dispersion fall over time, and that there were many observations of small wage increases in 1998. There are no nominal wage cuts in 331 (74%) of the country-year samples. We observe, however, $Y = 324$ events of nominal wage cuts, yielding an incidence rate of $q = 3.4$ percent of all observations. The appendix provides more details on the data. Specifically, Table A1 reports the detailed distribution of wage cuts and observations across countries and years.

Following previous literature, we test for the existence of DNWR by comparing the empirical distribution of wage changes to a postulated notional distribution, which is a counterfactual wage-change distribution assumed to prevail in the absence of DNWR. There are two key issues in this approach: how to derive the notional distribution, and how to statistically compare the empirical and notional distributions. As to the former, two main approaches have been used in the literature; it is either assumed that the notional wage

[1] The data for Austria, Canada, Finland, New Zealand, Sweden, and the US are from the ILO, while the data for Norway is from Statistics Norway. The data from the other countries are from Eurostat.

change distribution is symmetric (see Lebow, Stockton, and Wascher, 1995; Card and Hyslop, 1997; and Dickens et al., 2007), or it is assumed that the shape of the notional wage-change distributions is common across time, (e.g. Kahn, 1997 and Nickell and Quintini, 2003). Under the common shape assumption, the shape of the notional distribution is derived from years of high inflation, when presumably DNWR does not affect the wage-change observations. Elsby (2006) shows that if wages are rigid downwards, firms will respond by compressing wage increases. This would cause the symmetry method to downwardly bias the estimated DNWR. Thus, our main method is based on a common-shape assumption, but for robustness we also compare with results using the symmetry assumption.

One popular test of the common-shape type, suggested by Kahn (1997), explores the effect of inflation on the height of histogram bars of the wage-change distribution. However, the Kahn test relies on the assumption that changes in inflation only affect the location of the wage-change distribution, and not the shape or dispersion. This property is violated in our case, as both inflation and wage dispersion fall over time in our data. Here we measure dispersion in the right part of the distribution which is unaffected by DNWR; specifically we use the difference between the 75th and 50th percentiles ($P75 - P50$). We find that for both $P75 - P50$ and inflation, a simple OLS regression with country-fixed effects give a highly significant negative coefficient for the time trend. The Nickell and Quintini (2003) method is based on the approximation that the probability of a nominal wage cut is a function of the median and the dispersion of the wage changes, with a quadratic term in the former. This approximation is exact if the density function of wage changes is linear over the appropriate range, as would be the case for a triangular density function. As we shall see in Figure 2 below, the density function in our data is highly non-linear in the range that is relevant for DNWR. Thus, this method also involves invalid assumptions in our data set.

Our approach is to infer the shape or structural form of the notional distribution directly from the shape of the country-year samples with high nominal and real wage growth. Specifically, we assume that absent any DNWR, the notional nominal wage growth in industry j in country i in year t, Δw_{jit}^N, is stochastic with an unknown distribution G, which is parameterized by $(\mu_{it}^N, \sigma_{it}^N)$, where μ_{it}^N is the median nominal wage growth, and σ_{it}^N is a measure of the dispersion of G. Thus, we allow the location and dispersion of the notional industry wage growth to vary across countries and years, to capture the large variation that exists across countries and across time with respect to monetary policy, wage setting, and industry structure. However, we impose the same structural form (or shape) of G in all country-years.

Imposing a common shape across countries and years as we do is a strong assumption, even if it is shared by the previous multi-country studies in the field referred to in section 1 above. Incidentally, an alternative two-parametric common-shape assumption, the normal distribution, which is common in regression-based studies, would not be suitable due to the higher peak and fatter tails of the empirical distribution (see Figure 2). However, by allowing for country-year specific variation in the median wage growth and the wage growth dispersion, in line with Nickell and Quintini (2003), our approach is less restrictive than other approaches often used in the literature. More importantly, the results from a number of alternative specifications of G, reported in section 3.1 below document that our results are robust.

2.1 Constructing the notional distribution

The structural form of the notional distribution G is constructed on the basis of a subset H of the sample, with $S^H = 1,331$ observations from the 66 country-year samples where both the median nominal and the median real wage growth are located in their respective upper quartiles, so that downward rigidity is assumed not to affect the observations.[2] We derive a normalised specification of G, which we shall refer to as the underlying distribution, by adjusting these observations for their empirical country-year specific location and dispersion. As a further precaution to possible effects of DNWR and outliers, we follow Nickell and Quintini (2003) and measure the location μ_{it}^N by the median (rather than the mean), and the dispersion σ_{it}^N by the range between the 35th and the 75th percentiles (we use the 35th percentile to minimize the possible effect of DNWR; we have also tried other measures of dispersion with similar results). Formally, the underlying distribution is constructed using these 66 samples, subtracting the corresponding country-year specific median (μ_{it}) from the wage growth and dividing by the inter-percentile range ($P75_{it} - P35_{it}$):

$$x_s \equiv \left(\frac{\Delta w_{jit} - \mu_{it}}{P75_{it} - P35_{it}} \right), \qquad \forall\, j, i, t \in H \text{ and } s = 1, \ldots, S^H, \tag{1}$$

where subscript s runs over all j, i and t in the 66 country-year samples. x_s should thus be thought of as an observation from the (normalised) underlying distribution $X \sim G(0, 1)$. There are clearly stochastic disturbances in the

[2]Thus, in these country-year samples, the median nominal wage growth is above the 3rd quartile of 11.8 percent, and the median real wage growth is above 2.8 percent. Four of 1,331 observations in the subset H are below five percent nominal wage growth, of which only one is negative.

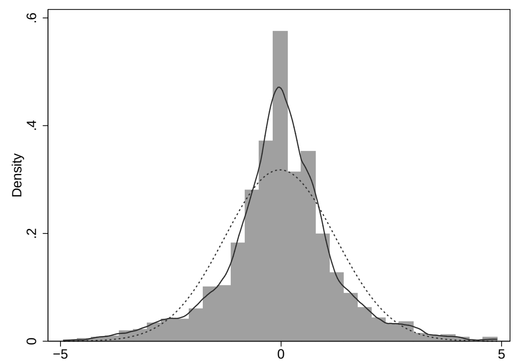

Figure 2: Histogram and kernel density (solid line) of the normalised underlying distribution $G(0, 1)$ compared to the normal density (dotted line). Fourteen extreme observations are omitted from the chart.

normalization due to the empirical country-year median μ_{it} and the empirical inter-percentile range $P75_{it} - P35_{it}$ being stochastic. However, as the underlying distribution consists of 1,331 observations, it should nevertheless give an accurate estimate of the shape of G. Figure 2 compares the underlying distribution (illustrated by the histogram and the kernel density in solid line) with the standard normal distribution (dotted line); we notice that the underlying distribution is slightly skewed to the right, with a coefficient of skewness of 0.26, and with fatter tails than the normal.

Then, for each of the 449 country-years in the full sample, we consider the notional country-year specific distribution of wage changes formed by adjusting the underlying distribution for the country-specific empirical median and inter-percentile range:

$$Z_{it} \equiv X\Big(P75_{it} - P35_{it}\Big) + \mu_{it}, \qquad \forall\, i, t. \tag{2}$$

Thus, we have constructed 449 notional country-year distributions $Z_{it} \sim G(\mu_{it}, P75_{it} - P35_{it})$, each defined by $S^H = 1,331$ notional wage-changes $z_{it}^s = x_s(P75_{it} - P35_{it}) + \mu_{it}$. These notional country-year distributions from which Δw_{jit}^N is drawn, have by construction a G distribution. They have thus the same shape across country-years, but their median and inter-percentile range are the same as their empirical country-year counterparts.

We then turn to the difference between DNWR at the individual and industry level. If there is no individual DNWR in an industry, the empirical industry level wage growth is equal to the notional, i.e. $\Delta w_{jit} = \Delta w_{jit}^N$. If DNWR binds for some workers, without the effect on average wages being undone by resulting changes in the wages of other workers in the industry (as discussed in section 1), the empirical wage growth will be greater than the notional $\Delta w_{jit} > \Delta w^N_{jit}$. However, there is no direct link between the existence of DNWR and whether $\Delta w_{jit} \geq 0$. Compositional effects like retirement of old workers with high wages, or alternatively wage cuts for some workers, may induce $\Delta w_{jit} < 0$ even if DNWR binds for some other workers. Furthermore, DNWR may bind for some workers, pushing Δw_{jit} above Δw^N_{jit}, even if the average notional wage growth is positive, $\Delta w^N_{jit} \geq 0$. In spite of no direct link between the existence of DNWR and whether $\Delta w_{jit} \geq 0$, it is clear that extensive DNWR, will reduce the likelihood of observing industries with negative wage growth, i.e. shifting probability mass of the empirical wage change distribution from below zero to above. Note also that in countries where union wage setting is dominant, DNWR is likely to affect many workers at the same time if it binds. Note also that the evidence from interview studies (e.g. Bewley, 1999) suggests that individuals are more willing to take wage cuts if they see that the firm or industry is really doing poorly. This would also seem to imply that if an industry is hit by a really negative shock, extensive wage cuts might be possible. Thus, in the empirical discussion below, we shall measure industry DNWR by a deficit of probability mass below zero, as compared to the notional wage change distribution. As we do not capture the effect of DNWR in industries where the change in the average wage is not pushed above zero, our estimates are really lower bounds for the prevalence of DNWR.

To what extent the probability mass is moved from below to above zero is clearly increasing in the prevalence of individuals affected by DNWR. However, it is also affected by the position of the notional wage change distribution. To given an extreme example, even if all job stayers are covered by DNWR, the industry wage growth may still be negative if the compositional effect is sufficiently large negative. Thus, we would expect positive, but not perfect correlation between measures of industry and individual DNWR.

2.2 Measuring DNWR

Our null hypothesis is that there is no DNWR, in which case $\Delta w_{jit} = \Delta w^N_{jit}$ is drawn from the distributions of Z_{it}. The alternative hypothesis of DNWR corresponds to $\text{Prob}(\Delta w_{jit} < 0) < \text{Prob}(Z_{it} < 0)$. Note that for all country-year samples it, an estimate for the probability of a notional wage cut $\tilde{q}_{it} \equiv \text{Prob}(Z_{it} < 0)$ is given by the incidence rate of a notional wage cut, i.e. the ratio of the number of notional wage cuts, $\# z^s_{it} < 0$, to the total number of observations in the underlying distribution S^H:

$$\tilde{q}_{it} = \frac{\# z_{it}^s < 0}{S^H}, \qquad s = 1, \dots, S^H. \tag{3}$$

We estimate the extent of DNWR by comparing the incidence rate of notional wage cuts the incidence rate of empirical wage cuts, given by

$$q_{it} = \frac{\# \Delta w_{jit} < 0}{S_{it}}, \qquad \forall\ j \tag{4}$$

where $\#\Delta w_{jit} < 0$ is the number of empirical wage cuts and S_{it} is the number of observations, both in country-year it. For country-years where there is at least one notional wage cut, implying that $\tilde{q}_{it} > 0$, we can calculate the fraction of wage cuts prevented, FWCP, which is an often-used measure of DNWR, as

$$\text{FWCP}_{it} = 1 - q_{it}/\tilde{q}_{it}. \tag{5}$$

If, for example, the incidence of wage cuts in the empirical sample is half of that in the notional distribution, then FWCP = 0.5. Note that if the empirical incidence rate is larger than the notional, the FWCP is negative. Figure 3 illustrates these measures for Portugal in 1998 and France in 1981. By construction, the empirical distribution (histogram) and the notional distribution (for illustrative purposes, we use the kernel density) for the same country-year have the same median and inter-percentile range, but the shapes differ. For Portugal in 1998, the incidence rate of notional wage cuts is 0.22, while the empirical incidence rate is zero, clearly suggesting the existence of DNWR with a FWCP of unity. The empirical and notional incidence rates are zero for France in 1981, so the FWCP is not defined. This reflects that there is no information on which to estimate the extent of DNWR.

As there are only on average 21 industries in each country-year sample, there may be considerable stochastic disturbances in μ_{it}, $P75_{it} - P35_{it}$, and q_{it}, which induce considerable disturbances in $\tilde{q}_{it}$ and FWCP_{it}. Thus, estimates of DNWR in single country-years will be imprecise, which is reflected in the large variation in incidence rates across country-years, see Table A2 in the appendix. However, averages of $\tilde{q}_{it}$ and FWCP_{it} for groups of country-years will be much more precise. Thus, we will focus on incidence rates and the FWCP at various aggregation levels: for countries, regions and periods, as well as for the full sample.[3]

[3]Note we do not weigh the observations by employment shares, because this might induce a bias in our statistical test presented below. The reason is that if there is for example a

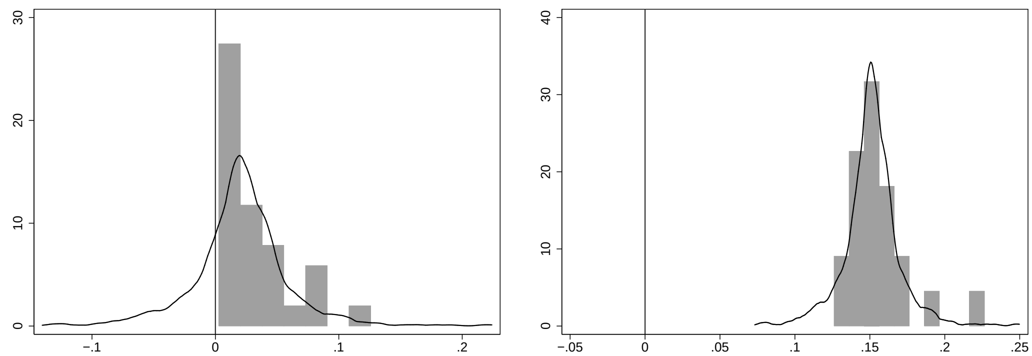

Figure 3: Left: Histogram of observed wage changes and the notional wage-change distribution (solid line) in Portugal 1998. Right: Histogram of observed wage changes and the notional wage-change distribution (solid line) in France 1981.

Another potential problem is that if DNWR binds in some country-years, and compresses the empirical wage change distribution from below to the extent that it affects the $35th$ percentile (and thus reduces the inter-percentile range) or increases the median, the associated notional country-year sample will also be compressed from below. This will involve a downward bias in the notional incidence rate of wage cuts, $\tilde{q}_{it}$, and thus to a downward bias in our estimate of DNWR, i.e. a downward bias in the estimated FWCP. We will return to this issue below.

2.3 Testing the significance of DNWR

As noted above, we test whether there is a deficit of wage cuts in the empirical distributions as compared to what one would expect if the empirical sample was drawn from a notional G distribution, i.e. without DNWR. Given the

tendency that large industries have lower probability of a wage cut than small have, the employment weighted incidence rate would be reduced. However, as we have insufficient evidence on possible differences in the probability of a wage cut across industries, we cannot impose any such differences in the statistical procedure. The upshot of this would be a positive bias in the notional incidence rates as compared to the empirical. This would involve an upward bias in the significance of our DNWR-estimates.

Another issue is that one might want to allow for uncertainty in the country-year specific medians μ. However, such uncertainty would increase the number of notional wage cuts, because the increase in the number of notional wage cuts for small values of μ would exceed the reduction in the number of notional wage cuts for large values of μ. With a higher number of notional wage cuts, the estimated DNWR would be greater, thus ignoring this involves a potential downward bias in our DNWR estimates.

notional probabilities $\tilde{q}_{it}$, this can be done by a straightforward use of the formulae for binomial distributions.

Consider a subsample M consisting of all industry observations in a subset of all country-years, say all years for Portugal. Let D_M denote the number of notional wage cuts in all years for Portugal when we draw from the notional distributions for Portugal, and Y_M the empirical number of wage cuts in Portugal. Then, we reject our null hypothesis of no DNWR in Portugal if the probability that the number of notional wage cuts in Portugal being lower than the number of empirical wage cuts is less than 5 percent, i.e. if $\text{Prob}(D_M \leq Y_M|H_0) \leq 0.05$.[4]

The calculation of $\text{Prob}(D_M \leq Y_M|H_0)$ is, however, extremely demanding computationally, not only for the full sample with 9,509 observations, drawn from 449 different binomial distributions, but also for separate countries.[5] Thus, we compute the p-values on the basis of simulations instead. This is computationally much simpler, yet highly accurate as we use 5000 simulations.

Our simulation method specifically is as follows. For each country-year it in the full sample, we draw S_{it} times (i.e., the number of industries in country-year it) from a binomial distribution with probability $\tilde{q}_{it}$. We then count the number of simulated wage cuts for the aggregation level of interest, $\widehat{Y}_M$, and compare these with the total number of wage cuts in the corresponding empirical distribution, Y_M; e.g. we compare the simulated number of wage cuts for Portugal with the empirical one. We then repeat this procedure 5000 times, and count the number of times where we simulate more notional wage cuts than we observe, denoted $\#(\widehat{Y}_M > Y_M)$. The Null hypothesis is rejected for subsample M with a level of significance at 5 percent if $1 - \#(\widehat{Y}_M > Y_M)/5000 \leq 0.05$. We can also use the simulation results to obtain confidence intervals for our estimate of DNWR.

[4]If the probability of a wage cut differ across industries, the number of wage cuts in the country-year will have a more narrow distribution. This is easiest to see in the extreme case where the probability of a wage cut in a specific industry is either zero or one, in which case the distribution for the number of wage cuts in the country-year collapses to a mass point. By assuming the same probability across industries, we thus allow for maximum dispersion of the number of wage cuts, implying that we err on the cautious side of not detecting significant DNWR.

[5]The number of terms in the formulae for the probability of k wage cuts in a sample of S observations is $S!/(k!(S-k)!)$, which increases rapidly with the number of wage cuts. With $S = 9,509$ and $k = 324$ in the full sample the number of terms is $\exp(1409.5)$.

Table 1: Results from 5000 simulations on subperiods.

Sample properties:	All obs	1973–79	1980–89	1990–94	1995–99
No. of observations (S)	9509	2224	3717	1906	1662
No. of country-years	449	109	175	88	77
Average wage growth (%)		13.78	8.72	5.60	3.99
Average inflation rate (%)		10.30	8.13	4.42	2.19
Average unemployment rate (%)		3.71	6.72	8.49	8.07
Observed wage cuts (Y)	324	5	74	93	152
Incidence of wage cuts (q)	0.034	0.002	0.020	0.049	0.092
Simulation results:					
Fraction of wage cuts prevented (FWCP)	0.259	0.610	0.398	0.231	0.159
Fraction of industry-years affected ($\hat{q} - q$)	0.012	0.004	0.013	0.015	0.017
Probability of significance (p)	0	0.013	0.000	0.002	0.006

Notes: $p = 1 - \#(\widehat{Y} > Y)$ where $\#(\widehat{Y} > Y)$ is the number of simulations where we simulate more wage cuts ($\widehat{Y}$) than we observe (Y). q and $\hat{q}$ are the empirical and average simulated incidence rates, respectively.

3 Results

For the whole sample, there are more simulated than observed wage cuts in all 5000 simulations. Thus we comfortably reject the null hypothesis of no DNWR with a p-value of 0, as reported in the bottom line of the first column of Table 1. For the overall sample, the fraction of wage cuts prevented is FWCP $= 1 - q/\tilde{q} = 1 - 0.034/0.046 = 0.259$.[6] Thus, about one out of four notional wage cuts in the full sample does not result in an observed wage cut due to DNWR. Probably a better measure of the economic relevance of DNWR is the probability that DNWR prevents the industry wage change from being negative. The difference between the notional and empirical incidence rates, $(\tilde{q} - q)$ is An estimate of this probability. For the overall sample, this fraction is 0.012, so about one out of 100 observations (industry-years) were pushed above zero by DNWR. We will refer to this probability as the fraction of industry-years affected. However, DNWR may affect the industry average wage without pushing it above zero as discussed in section 1 and section 2.1.

A number of interesting questions thus arise. Is there evidence for DNWR

[6]Note that this calculation implies that we aggregate the country-year estimates by pooling the empirical observations in the relevant sample (for example all country-years), implying that the country-year notional incidence rates are weighted according to the number of observations within the country-year.

Table 2: Results from 5000 simulations on regions.

Sample properties:	Anglo	Core	Nordic	South
No. of observations (S)	2961	3110	1976	1462
No. of country-years	129	158	95	67
Observed wage cuts (Y)	153	125	18	28
Incidence of wage cuts (q)	0.052	0.040	0.009	0.019
Simulation results:				
Fraction of wage cuts prevented (FWCP)	0.198	0.234	0.497	0.414
Fraction of industry-years affected ($\hat{q} - q$)	0.013	0.012	0.009	0.014
Probability of significance (p)	0.001	0.000	0.000	0.002

Notes: See Table 1

for different time periods, regions and countries? To what extent is DNWR related to labour market institutions? We first investigate whether DNWR has changed over time by splitting the sample into four subperiods: 1973–1979, 1980–1989, 1990–1994, and 1995–1999. From Table 1 we see that the number (and incidence) of wage cuts has increased over time, as the average wage growth (and inflation) decreased, while unemployment increased. DNWR is statistically significant in all periods. In the high-inflation 1970s, the FWCP was 61 percent. In the 1980s, the FWCP had fallen to 40 percent, and then further to 23 percent in the early 1990s, and 16 percent in the late 1990s. However, the number of industry-years affected by DNWR increased from 0.4 percent in the 1970s to 1.7 percent in the late 1990s.

We then split the sample into four regions; Anglo (Canada, Ireland, New Zealand, the UK and the US), Core (Austria, Belgium, France, Germany, Luxembourg and the Netherlands), Nordic (Denmark, Finland, Norway and Sweden) and South (Italy, Greece, Portugal and Spain), and report the results for these areas in columns 2–5 in Table 2. Note that the regions by and large consist of countries with rather similar labour market institutions (see discussion in section 4).

We find significant DNWR at the one percent level for all regions. The FWCP is relatively high in two regions: 50 percent in the Nordic countries and 41 percent in the South. In the Anglo and Core groups, the FWCP is considerably lower, around 20 percent. This difference is roughly in line with what one would expect in view of the cross-country differences in labour market institutions across regions (in Tables A3–A6 in the appendix, we report country-specific indices for labour market institutions). Based on a theoretical framework that allows for bargaining over collective agreements as well as individual bargaining, Holden (2004) argues that workers who have their wage set via unions or collective agreements have stronger protection against a nominal wage cut—thus the extent of DNWR is likely to increase given the coverage of collective agreements and union density. For non-union workers, the strictness of the employment protection legislation (EPL) is important to their likelihood of avoiding a nominal wage cut. Thus, one would expect considerable wage rigidity in the Nordic countries, where both union density and bargaining coverage are high, and the EPL is fairly strict (excepting Denmark). One would also expect considerable wage rigidity in southern Europe, as EPL is very strict and bargaining coverage is fairly high, even if union density is on the low side. In the Core region, even if the bargaining coverage is fairly high, and the EPL is fairly strict, union density is lower than in the Nordic countries, and EPL is less strict than in the South, so one would expect some, but weaker DNWR. Finally, in the Anglo countries, union density is lower and EPL is weaker than in the other regions, so this is where one would expect the weakest DNWR. In section 5 below, we analyse the effect of the institutional variables further.

In Table A7 in the appendix, we report results from splitting the sample by combining regions and sub-periods. This implies a smaller number of observations behind each test statistic and, as expected, this reduces the significance levels. It is nevertheless an interesting feature that the FWCP increased in the late 1990s in the Nordic countries, in contrast to the consistent reduction over time in the other three regions. The fraction of industry-years affected by DNWR has increased in the Nordic region and in the South, while a more mixed picture seen in the Anglo and the Core regions.

In Table 3, we report the results concerning individual countries. We observe that for all countries except Canada, France, Germany, Greece, and Spain, the simulations indicate some DNWR, as some notional wage cuts are prevented (FWCP > 0.2). As these results are also based on fewer observations, the significance levels are lower. DNWR is, however, significant at the 5 percent level for Austria, Belgium, Denmark, Ireland, Italy, Luxembourg, the Netherlands, New Zealand, Portugal, and Sweden, and at the 10 percent level for Finland. It is noteworthy that the FWCP is above 45 percent for all the Nordic countries. A surprising result is that the South splits in two, with strong DNWR in Portugal and Italy, and no DNWR in Spain and Greece. The fraction of industry-years affected by DNWR is highest in Portugal (4.5 percent) and in the Netherlands (3 percent).

To explore the precision of our DNWR measures, we compute 90 percent confidence intervals for the FWCP based on the distributions from the simulations. Figure 4 presents these intervals for all the categories. The confidence

Table 3: Results from 5000 simulations on countries.

Country	S	T	Y	q	FWCP	$(\hat{q} - q)$	p
Austria	408	26	2	0.0049	0.714	0.012	0.025
Belgium	575	26	31	0.0539	0.231	0.016	0.037
Canada	627	26	57	0.0909	0.077	0.008	0.269
Denmark	462	24	8	0.0172	0.463	0.015	0.033
Finland	368	23	2	0.0054	0.663	0.011	0.059
France	556	26	21	0.0378	−0.200	−0.006	0.869
Germany	665	26	16	0.0241	0.060	0.002	0.467
Greece	469	26	7	0.0149	−0.129	−0.002	0.723
Ireland	463	23	27	0.0583	0.325	0.028	0.015
Italy	312	13	0	0	1	0.010	0.037
Luxembourg	423	27	32	0.0757	0.269	0.028	0.023
Netherlands	483	27	23	0.0476	0.387	0.030	0.001
New Zealand	750	27	45	0.0600	0.216	0.017	0.037
Norway	674	27	2	0.0030	0.459	0.003	0.285
Portugal	411	18	3	0.0073	0.860	0.045	0.000
Spain	270	10	18	0.0667	−0.053	−0.003	0.649
Sweden	472	21	6	0.0127	0.469	0.011	0.037
UK	615	26	18	0.0293	0.217	0.008	0.130
US	506	27	6	0.0119	0.308	0.005	0.231

Notes: T is the number of years. See also Table 1

intervals are fairly large, and with few exceptions, we are not able to conclude that the FWCP are significantly different from one another, despite the variation between the estimates.

In view of this large uncertainty, one should be careful when interpreting the differences between the countries. Nevertheless the estimates may be useful as a benchmark when comparing with estimates from microeconomic studies. Figure 5 compares our estimates of the FWCP with those reported by Knoppik and Beissinger (2005) and by Dickens et al. (2007). Remember that because of aggregation and compositional effects, our estimates of FWCP at the industry level is different than the estimates of FWCP at the individual level. Yet it is interesting to note that there is some correspondence across countries, with correlation coefficients of 0.65 and 0.25, respectively. The outliers in both cases are Greece and France. For France, our low estimate is consistent with Biscourp, Dessy and Fourcade (2004), who find considerable wage flexibility in France. For Greece, on the other hand, our negative es-

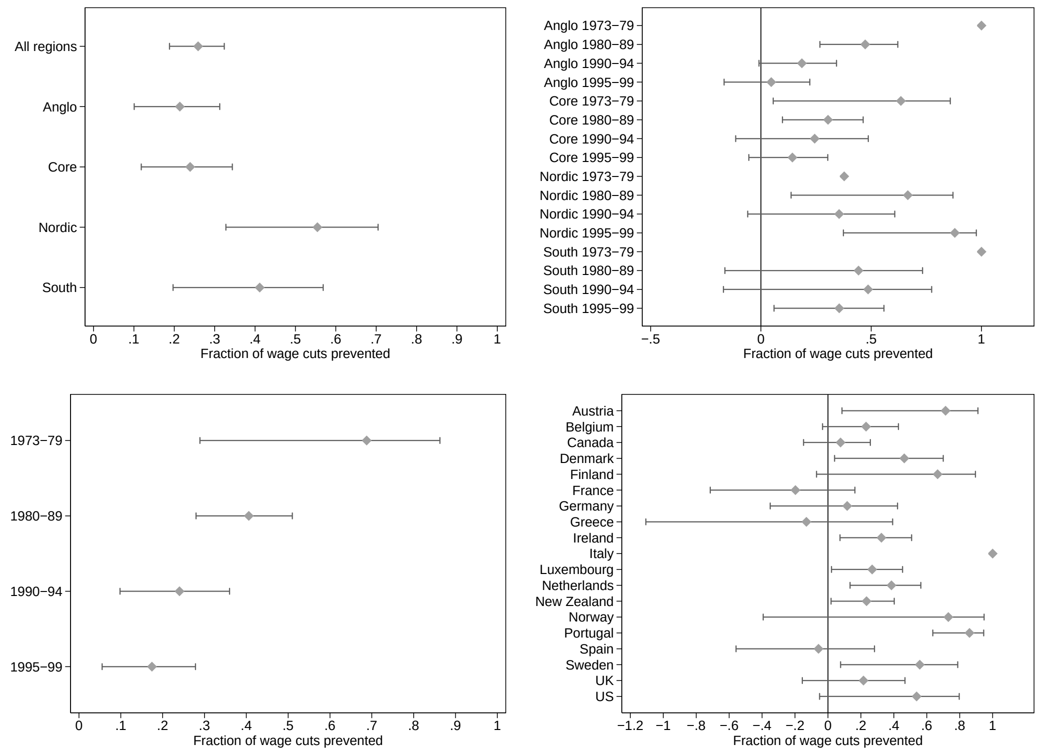

Figure 4: Estimated fractions of wage cuts prevented with 90% confidence intervals.

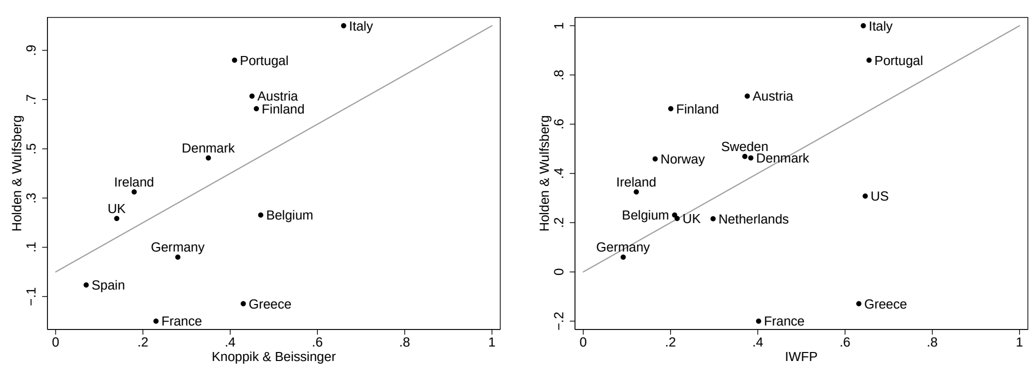

Figure 5: Comparing our estimates of FWCP *with Knoppik and Beissinger (2005) (left) and with Dickens et al. (2007) (right).*

timate seems questionable. Indeed, our estimate based on country-specific underlying distributions (reported below) is much closer to the microeconomic estimates (equal to 0.27), although it is not significant. For the US, Lebow, Saks, and Wilson (2003) estimate the FWCP to be about one-half, which is between our and Dickens et al. (2007), although somewhat closer to the latter. Interestingly, our finding of strong DNWR for Portugal is consistent with that country's institutional feature that a nominal wage cut for a job stayer is illegal.

3.1 Robustness

In this section we'll consider robustness along three dimensions: the construction of the nominal country-year samples, the statistical comparison of the notional and empirical distributions, and the distinction between DNWR and downward *real* wage rigidity (DRWR). As to the first dimension, our results so far are based on the assumption of a common shape across countries and years, where the shape is constructed by use of 66 country-year samples with high median nominal and real wage growth. The restriction to high wage growth samples was made to ensure that the shape of the underlying distribution is not affected by DNWR. Yet it may also involve other pitfalls. For example, country-year samples with high nominal and real wage growth may to a large extent be years with a boom, which may affect the results if there are asymmetric compositional changes in booms, cf. discussion in section 1 above. Thus, we also implement our method with a number of alternative ways of constructing the underlying distribution.

First, we have constructed separate country-specific underlying distributions, G_i, based on all observations for each country, and for each time period in Table 1, G_{period}, and then proceeded with the method as before. Figure 6 shows that the estimates based on country-specific underlying distributions are fairly similar to those based on the common shape assumption (with Finland and Greece as the most notable exceptions), while the estimates based on period-specific underlying distributions are very close to the common-shape estimates. Because these other underlying distributions also include country-year samples with low median wage growth, and thus also samples where DNWR binds, the shape of the underlying and notional distributions will be compressed. As mentioned above, this will induce a downward bias in the estimated DNWR. Indeed, we find somewhat less DNWR, with an overall FWCP equal to 18 percent (country-specific) and 20 percent (period-specific) (see Ta-

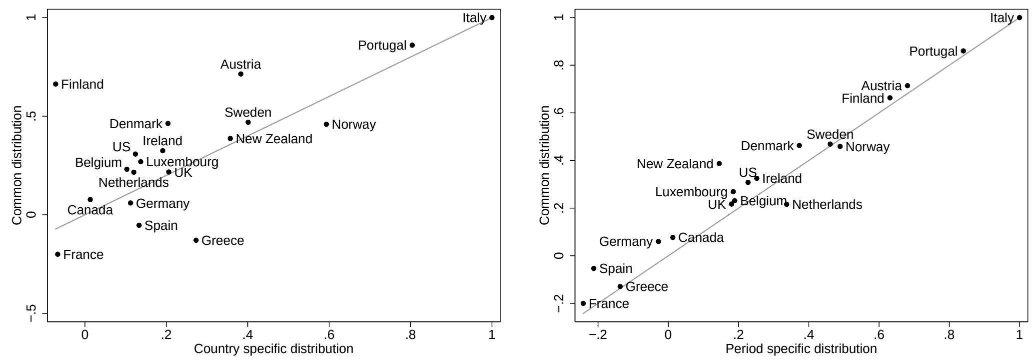

Figure 6: Comparing the estimates of FWCP *using a common underlying distribution with country-specific distributions (left) and periodic-specific distributions (right).*

ble A8 in the appendix).[7] On the other hand, as these sub-samples include all years, and thus both booms and recessions, the associated underlying distributions should not be affected by possible asymmetric compositional changes. This strengthens our interpretation that the deficit of wage cuts in the empirical distributions is indeed caused by DNWR. We have also performed the method with the underlying distribution based on observations from country-years with inflation above 5 percent in one specification, and from country-years before 1993 in another, with results very similar to the period-specific results.

We also undertake the method with a symmetry assumption inspired by Lebow, Stockton, and Wascher (1995) and Card and Hyslop (1997), where the notional distributions are constructed from the empirical ones by replacing observations below the median in all country-years with observations from the upper half of the distributions. Thus, all country-year notional samples are symmetric, but the shape of the distributions differ across country-years. The results turned out to be very similar to the results from the main specification (see Table A8). The estimated FWCP are, however, somewhat lower, which is consistent with a possible downward bias of the symmetry approach, if DNWR makes firms compress wage increases, as is suggested by Elsby (2006).

Observe that the symmetry approach involves no assumptions of equal

[7]One may wonder how we can detect any DNWR at all when we compare each country-year sample with an underlying distribution constructed on the basis of all country-year samples. Then, by construction, if some country-year samples have thinner left tails than the underlying distribution (which we interpret as DNWR), other country-years will have thicker left tails. The point is that these other country-year samples with thicker left tails have higher median wages, so that the thicker left tail does not involve many wage cuts.

shape across country-year samples. In contrast, the main approach makes no assumptions regarding symmetry. This implies that these two approaches are based on orthogonal assumptions. We believe that the very similar results from two orthogonal approaches is strong evidence in favour of the robustness of our results.

The second dimension is the robustness of our new method for a statistical comparison of the notional and empirical samples. As noted above, there is a possible downward bias in our method, if DNWR despite our precautions have compressed the underlying distribution or inter percentile ranges in some country-years. One way of exploring the quantitative importance of this bias is to contaminate the data by adding DNWR, and then see to what extent we are able to detect the DNWR that we have added. If the downward bias is severe, we would presumably detect much less DNWR than we have added. Specifically, we pick ten countries evenly from the four regions (Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Portugal and the US), and by random selection we eliminate half of the nominal wage cuts in each country by setting the associated nominal wage change to zero, thereby reducing the number of wage cuts from 324 to 238. Due to integer restrictions, we in practice eliminate 48 percent of the nominal wage cuts (in Portugal we eliminate one out of three observed wage cuts). Again, we apply our procedure with the contaminated data. With a perfect method, this would reduce the fraction of wage cuts realised (which is equal to one minus the fraction of wage cuts prevented) by on average 48 percent in these countries, without affecting the fraction of wage cuts realised in the other countries.

The results are promising. For the affected countries, the average fraction of wage cuts realised is reduced by 46 percent, as compared to the original results, see Table 4. Taken at face value, these results suggest that our method on average is able to detect 96 percent of the total DNWR in the data (calculated as the computed reduction of 46 percent as compared to the constructed reduction of 48 percent, where $46/48 = 0.96$). The variation among the ten countries is fairly small, varying from a minimum of $42.1/48.4 = 87$ percent for Belgium to a maximum of 100 percent for Finland, Germany, Greece and Ireland. For the other countries, the fraction of wage cuts realised is hardly affected (on average, it decreases by 0.3 percent, with a maximum of 2.2 percent for Norway). These results suggest that the downward bias in the estimated DNWR due to DNWR affecting the notional distribution and/or the inter-percentile ranges is likely to be very small.

The third dimension is whether our results in reality should be attributed to downward *real* wage rigidity (DRWR). Barwell and Schweitzer (2007), Bauer et al. (2007), and Devicienti, Maida, and Sestito (2007) find evidence for DRWR

Table 4: The effect from contaminating the data by adding DNWR.

Countries without additional DNWR	ΔY	ΔFWCR
Austria	0.000	−0.007
Italy	0.000	0.000
Luxembourg	0.000	0.001
Netherlands	0.000	0.002
New Zealand	0.000	−0.003
Norway	0.000	−0.022
Spain	0.000	0.006
Sweden	0.000	0.000
UK	0.000	0.001

Countries with additional DNWR	ΔY	ΔFWCR
Belgium	–0.484	–0.421
Canada	–0.491	–0.472
Denmark	–0.500	–0.495
Finland	–0.500	–0.501
France	–0.476	–0.460
Germany	–0.500	–0.500
Greece	–0.429	–0.430
Ireland	–0.481	–0.481
Portugal	–0.333	–0.329
US	–0.500	–0.497

Notes: ΔY is the contamination of the data in the form of the relative change in the number of nominal wage cuts. ΔFWCR is the resulting percentage change in the fraction of wage cuts realised. An exact test would yield a $\Delta FWCR$ identical to ΔY for each country.

in the UK, Germany, and Italy, while Dickens et al. (2007) find DRWR in all the 16 OECD countries they study. These results also indicate that by not allowing for DRWR, there is a risk that the extent of DNWR is overestimated. In our data, however, almost 30 percent of all observations are negative real wage changes, by itself a clear sign that if DRWR exists, it is certainly not absolute.

It is hard to distinguish between DNWR and DRWR, particularly in industry data, where random components will imply that we are unlikely to find sharp effects exactly at the threshold points of zero nominal and real growth. In a companion paper, Holden and Wulfsberg (2007), we address this issue more thoroughly. Here, we limit ourselves to a simple exercise to shed light on the possible quantitative effects of DRWR on our results. We add DRWR to our data set by randomly eliminating 20 percent of all observations of real wage cuts (618 observations), and by setting the associated nominal wage change equal to the rate of inflation. This reduces the total number of nominal wage cuts by 18 percent, from 324 to 265, with a potentially strong impact on any findings of DNWR. However, even when applying our method with the manipulated data, it turns out that our measure of DNWR is not much affected: Eliminating real wage cuts involves a compression of the notional wage-change distributions, implying that the overall FWCP increases by only four percentage points (from 26 to 30 percent). Thus, we conclude that while DRWR may have affected our results, it seems unlikely that the effect is large, in view of the fact that a fairly strong DRWR of 20 percent had a very limited impact on our results.

4 DNWR and labor market institutions

While the previous analysis documents the existence of DNWR, it does not explicitly investigate whether DNWR depends on economic and institutional variables. As mentioned above, Holden (2004) shows that the extent of DNWR is likely to decrease with inflation (in a non-linear way), as well as on institutional variables like the strictness of the EPL and union density or bargaining coverage. Furthermore, high unemployment may weaken workers' resistance to nominal wage cuts. Thus, we regress the extent of DNWR as measured by the FWCP in each country-year sample on inflation, inflation squared, unemployment, and institutional variables to test whether the these variables are related to DNWR. Regrettably, the data for institutional variables apply to the whole economy, and not to the specific industry sectors. The coefficient estimates for these variables might be biased downwards, as variation in, for example, density or coverage in other parts of the economy would affect the density and coverage variable, but presumably not affect wage setting in the industry sector. On the other hand, if we do find an effect, it seems likely that it will be due to the data capturing the relationship that we are interested in.

Technically, we undertake Poisson regressions where the number of observed wage cuts in each country-year sample, Y_{it}, depends on the average number of simulated wage cuts for each country-year sample, $\widehat{Y}_{it}$, and the explanatory variables mentioned above, $\mathbf{x}_{it}$. A Poisson regression seems appropriate, as Y_{it} is the number of times we observe an event (see Cameron and Trivedi, 1998). The conditional density of the number of observed wage cuts in the Poisson model is

$$f(Y_{it} = y_{it} \mid \widehat{Y}_{it}, \mathbf{x}_{it}) = \frac{e^{-\lambda_{it}} \lambda_{it}^{y_{it}}}{y_{it}!}. \tag{6}$$

Furthermore, we assume that the Poisson parameter, λ_{it}, is given by

$$\lambda_{it} = \widehat{Y}_{it} e^{\mathbf{x}'_{it}\boldsymbol{\beta}}, \qquad \text{if } \widehat{Y}_{it} > 0. \tag{7}$$

where $\boldsymbol{\beta}$ is the unknown parameter vector we want to estimate. Because $\lambda_{it} = \mathrm{E}\left(Y_{it} \mid \widehat{Y}_{it}, \mathbf{x}_{it}\right)$ and using the definition of the FWCP we get

$$1 - \text{FWCP} = \frac{Y_{it}}{\widehat{Y}_{it}} = e^{\mathbf{x}'_{it}\boldsymbol{\beta} + \varepsilon_{it}}, \qquad \text{if } \widehat{Y}_{it} > 0 \tag{8}$$

where ε_{it} is an error term. In columns (1)–(6) of Table 5 we present both pooled and fixed effects estimates of different specifications of (8). For the

Table 5: Maximum likelihood estimates with standard errors from Poisson regressions in columns (1)–(6) and from negative binomial regressions in columns (7) and (8). The standard errors in square brackets are robust as the observations are clustered by country.

	Fraction of wage cuts realised (1–FWCP)						Incidence of wage cuts	
	Pooled	Fixed effects	Pooled	Fixed effects	Pooled	Fixed effects	Pooled	Fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EPL	–	–	–	–	−0.104	−0.430	−0.291*	−0.716**
					(0.059)	(0.293)	(0.101)	(0.196)
					[0.068]		[0.168]	
Union density	–	–	−0.881**	−1.774	−0.966**	−2.125	−0.606	−1.676*
			(0.365)	(1.383)	(0.377)	(1.423)	(0.585)	(0.964)
			[0.296]		[0.371]		[0.778]	
Inflation	−0.114**	−0.100*	−0.099**	−0.057	−0.096**	−0.041	−0.505**	−0.364**
	(0.047)	(0.052)	(0.048)	(0.061)	(0.048)	(0.063)	(0.073)	(0.062)
	[0.030]		[0.037]		[0.043]		[0.113]	
Inflation squared	0.004*	0.004	0.004*	0.003	0.004**	0.003	0.017**	0.012**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
	[0.002]		[0.002]		[0.002]		[0.005]	
Unemployment	0.033**	−0.013	0.030**	−0.000	0.033**	0.008	0.109**	0.093**
	(0.016)	(0.033)	(0.015)	(0.035)	(0.016)	(0.036)	(0.029)	(0.036)
	[0.014]		[0.010]		[0.012]		[0.039]	
constant	−0.369**	–	−0.049	–	0.118	–	−1.987**	1.519**
	(0.191)		(0.226)		(0.243)		(0.454)	(0.762)
	[0.178]		[0.220]		[0.187]		[0.759]	
log-likelihood	–262.1	–210.9	–259.1	–210.6	–257.5	–209.0	–360.9	–287.5
Number of observations	282	278	282	278	282	278	422	409

Notes: (i) * and ** indicates significance at 10% and 5% levels using robust standard errors in pooled regressions. (ii) Luxembourg is excluded because of lack of EPL data. In addition, Italy is excluded from the fixed effects models as there are no observed wage cuts in this country. (iii) There are fewer observations in column (1)–(6) because the fraction of wage cuts realised is not defined in 140 country-years where the notional incidence rate is zero.

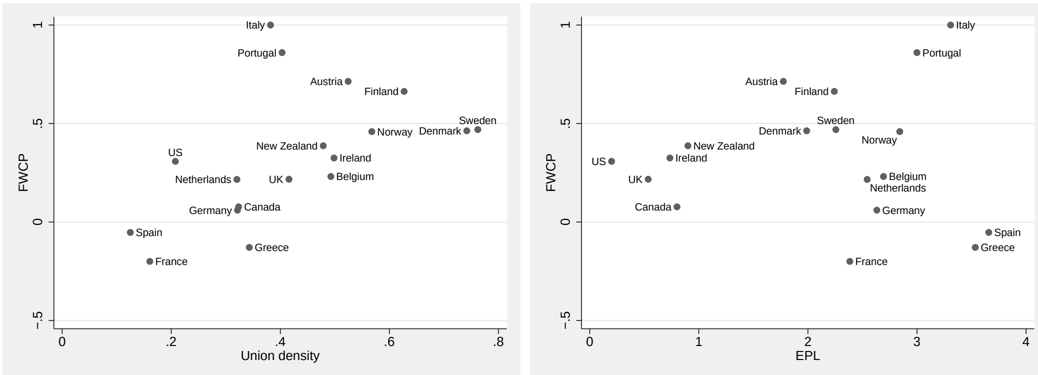

Figure 7: The average country specific FWCP *plotted against union density (left) and* EPL *(right).*

pooled estimates we report 'normal' standard errors and robust standard errors where the observations are clustered by country. In column (1) and (2) we include inflation, inflation squared and unemployment as regressors, leaving out the institutional variables. In column (3) and (4) we add union density as regressor and in column (5) and (6) we also add EPL. Since the notional incidence rate is zero in 140 country-year samples, implying that the FWCP is not defined, these observations are excluded from the Poisson regressions. The restriction that $\mathrm{E}(Y_{it} \mid \widehat{Y}_{it}, \mathbf{x}_{it}) = \mathrm{Var}(Y_{it} \mid \widehat{Y}_{it}, \mathbf{x}_{it}) = \lambda_{it}$, an implicit assumption by the Poisson distribution, is accepted easily in all these model specifications.

Because there is little variation within each country especially in the institutional variables, it is hard to obtain precise estimates controlling for fixed effects, which shows up in the statistically insignificant fixed effects estimates in columns (2), (4), and (6). The pooled estimates in columns (1), (3), and (5), however, are significant indicating that higher unemployment and lower inflation increase the fraction of wage cuts realised and thus reduce the FWCP as expected. The effects of inflation and unemployment are robust to the inclusion of labor market institutions. In column (3) and (5) we find a significant negative effect of union density on the fraction of wage cuts realised, implying a positive effect on the FWCP. EPL has also a positive effect on the FWCP, but the effect is not significant with a p-value of 12 percent. Figure 7 confirms that there is a clear positive relationship between the country average FWCP and union density while the relationship between the FWCP and EPL is less clear. In particular for countries with strict EPL there are wide differences in the estimated FWCP.

In order to illustrate the economic significance of the estimates, we use the pooled model in column (5). According to these estimates a reduction in union density from 75 percent to 25 percent would raise the fraction of wage cuts realised by a factor of 1.6 ($= exp(-0.966(-0.5))$); for Finland, the fraction of wage cuts realised would increase from 33.8 to 54.8 percent.

As a further test of the effect of institutions on DNWR, we exploit the idea that in the absence of DNWR, the institutional variables should not be able to explain the country-year variation in the empirical incidence rate of wage cuts. Thus, we do the same regressions as above, but without including the notional incidence rates in the regressions. While this involves loss of information, it also has the advantage that it does not rely on a specific distribution of the notional wage changes, making it complementary to the former regressions. Here we also include the 140 observations that were lost due to a zero notional incidence rate. Thus, in column (7) and (8) we estimate the pooled and fixed effect of the explanatory variables on the empirical incidence rate of wage cuts, instead of the fraction of wage cuts realised. In other words, in this model we let the expected number of wage cuts, i.e. λ_{it}, depend on the number of observations (i.e. industries) in the country-year sample, S_{it}, instead of $\widehat{Y}_{it}$:

$$\lambda_{it} = \mathrm{E}\left(Y_{it} \mid S_{it}, \mathbf{x}_{it}\right) = S_{it} e^{\mathbf{x}'_{it}\boldsymbol{\beta}}. \tag{7'}$$

With this specification the poisson restriction $\mathrm{E}(Y_{it} \mid S_{it}, \mathbf{x}_{it}) = \mathrm{Var}(Y_{it} \mid S_{it}, \mathbf{x}_{it}) = \lambda_{it}$ is rejected, hence we use the negative binomial regression model, which allows for 'overdispersion'.[8]

In accordance with the theoretical predictions, EPL, union density, and inflation, all have a negative effect on the incidence of nominal wage cuts, although this time it is union density which is not statistically significant. In our view the finding of a significant negative effect of EPL on the incidence of wage cuts even when controlling for fixed effects, strengthens the interpretation that strict EPL induces more DNWR, despite the insignificant effect in the regressions above. The quantitative impact of the institutional variables is fairly large, but the effects differ according to the method applied. Using the

[8]Overdispersion means that the variance in the data is greater than the mean, in contrast to the Poisson assumption that the variance and the mean are equal. Using a goodness-of fit test from a Poisson regression of Y_{it}/S_{it}, we reject no overdispersion with $\chi^2(416) = 634.6$. Specifically, we use two alternative specifications for the error term: (i) $\varepsilon_{it} \sim \Gamma(1, \delta)$, and (ii) $\varepsilon_{it} \sim \Gamma(1, \phi_i e^{-\alpha_i})$. Including a Gamma-distributed error term, ε_{it}, allows the variance-to-mean ratios of Y_{it} to be larger than unity. (6) and (i) together yield the pooled negative binomial regression model. In (ii), we also include a country-specific fixed effect, α_i, to allow for a country-specific variance-to-mean ratio (see Hausman, Hall, and Griliches (1984) for details).

point estimates from the fixed effects model, a reduction in the EPL index by 1.5 units, from the strict level in Portugal to the medium level of Austria or Sweden for example, would increase the incidence of nominal wage cuts by a factor of $exp(-0.716(-1.5)) = 2.9$. The incidence of wage cuts in Portugal would thus increase from 0.7 percent to 2.0 percent.

Note that the positive correlation we find between EPL and DNWR also supports our interpretation that the finding of a deficit of nominal wage cuts is indeed DNWR; if a deficit of wage cuts is not caused by DNWR, but is due to a positive compositional effect in average wages caused by young, low wage workers being kicked out of industries taking a bad shock, it would imply a negative correlation between our DNWR-estimates and EPL, as the latter would reduce firings under negative shocks. We have also included other institutional variables: bargaining coverage, temporary employment, and indices of centralisation and coordination, as suggested by Dessy (2002). However, these variables generally had much lower explanatory power than the variables that are included in Table 5.

Our results are in contrast to recent evidence based on microeconomic data reported in Dickens et al. (2007). Using OLS, with the FWCP as the dependent variable, they find weak and insignificant effect of EPL, and the effect of union density is even negative, although only significant at the 10 percent level.[9] Incidentally, applying OLS to our data, union density is the only significant variable, but with a positive effect on DNWR. More interestingly, however, our discussion in section 2 does suggest that EPL and unions will have stronger effects on DNWR in industry wages than individual wages, consistent with our findings. The reason is that for individual workers the industry effect of DNWR might be undone if there is flexible wage setting for other workers or in other firms. However, in country-years with strict EPL and unions, wage setting is unlikely to be that flexible. For example, a firm for which wages are set in a union contract would, in most cases, be unable to circumvent wage rigidity by replacing high wage workers with workers with lower wages.

To investigate whether the change in DNWR over time as reported in Table 1 is significant, we undertake Poisson regressions of the fraction of wage cuts realised with respect to a time trend. We obtain a trend coefficient of 0.036, which is significant at the 1 percent level, which allows us to conclude that DNWR, as measured by the FWCP, has fallen over time. Adding a time trend in the regressions in Table 5 gave positive significant coefficients in the models for the incidence of wage cuts, but not in the models for the fraction of wage

[9]Dickens et al. (2007) find that union density is positively associated with DRWR, however, significant at the 10 percent level.

cuts realised. The trend coefficient in the fixed effects model is 0.065, implying that the predicted change in the incidence of wage cuts over a period of 27 years increases by a factor of 5.8 (= $exp(0.065(27))$). The overall increase was, however, much greater; as shown in Table 1, the incidence of wage cuts increased from 0.23 percent in the 1970s to 9.15 percent in the late 1990s. Overall, these results indicate that the reduction in DNWR over time (as measured by FWCP) is explained by the evolution of the economic and institutional variables, while there may have been an additional reduction over time in the incidence of wage cuts.

5 Conclusions

Based on a novel method, we document the existence of downward nominal wage rigidity (DNWR) for manual workers in 19 OECD countries over the period 1973–1999, using data for hourly nominal wages at the industry level. Overall, we find that one out of four of the wage cuts that should have taken place under complete flexibility (notional wage cuts) were prevented by DNWR, while slightly more than 1 percent of all industry-year observations have been affected by binding DNWR. To explore the robustness of our results, we have undertaken a number of different specifications of the key assumptions, that qualitatively yield the same results.

Our paper makes four main contributions to the literature on DNWR. A number of recent microeconomic studies, for many different countries, have documented that individual wages for job stayers are rigid downwards. However, the effects on aggregate output and unemployment are disputed. One possible reason for this discrepancy is that compositional changes at the firm or industry level may undo wage rigidities at the individual level. Indeed, this view is consistent with the findings of Fares and Lemieux (2000), who find that most of the real-wage adjustment over the business cycle is related to new entrants. If the aggregate wage effect of binding DNWR for some workers is offset by lower wages of other workers, the rigidity is also unlikely to affect output and unemployment. By documenting the existence of DNWR in industry-level wage data, we show that firm behaviour and market mechanisms may diminish, but do not remove, rigidity at the individual level. In this sense, we view our study as complementary to the increasing number of microeconomic studies of DNWR.

Second, we find that higher union density lead to stronger DNWR, in the sense that more wage cuts are prevented. Stricter employment protection legislation (EPL) also reduces the incidence of nominal wage cuts. The estimated effects of the institutional variables that we find are fairly strong. For example, if the EPL goes from a strict to a medium level, this would, according to the point estimates, raise the incidence of nominal wage cuts in Portugal from 0.7 to 2.3 percent. A reduction in union density from 75 percent to 25 percent would raise the fraction of wage cuts realised by a factor of 1.6 ($= exp(-0.966(-0.5))$); for Finland, the fraction of wage cuts realised would increase from 33.8 to 54.8 percent. Thus, our results suggest that changing labour market institutions would have a considerable impact on wage rigidities.

The effect of institutional variables is consistent with differences in DNWR across countries. Looking into four groups of countries, we find stronger DNWR in two regions: the South (Italy, Greece, Portugal, and Spain) and the Nordic region (Denmark, Finland, Norway, and Sweden), where EPL is stricter and/or unions are stronger, than for the other groups; the Core (Austria, Belgium, France, Germany, Luxembourg, and the Netherlands), and the Anglo region (Canada, Ireland, New Zealand, the UK, and the US).

These findings are also important from a theoretical point of view, as they strengthen the case that DNWR is partly caused by contracts and institutional features, in a individual bargaining framework as argued by MacLeod and Malcomson (1993), and in a collective agreement framework as argued in Holden (1994). Interestingly, the microeconomic study by Dickens et al. (2007) does not find the same positive effect of union density on DNWR that we do. One possible explanation for this difference is that DNWR for individual workers can be undone by wage flexibility for other workers or in other firms, unless this flexibility is prevented by union contracts and possibly also by strict EPL. For example, for a firm facing a union contract, it would be difficult to circumvent wage rigidities by replacing high-wage workers with low-wage workers. However, as we have not be able to test for fairness and morale explanations of DNWR, such hypotheses remain speculative. Furthermore, as argued by Holden (1994), these explanations are likely to be complementary in the sense that fairness and contractual explanations may reinforce each other.

Third, we find that DNWR in the form of the fraction of notional wage cuts that is prevented by DNWR has fallen over time. For all countries together, the FWCP by DNWR has fallen from 60 percent in the 1970s to 16 percent in the late 1990s. The Nordic countries are an exception; for this group, the fraction of wage cuts prevented is highest in the late 1990s. Most of this reduction in DNWR can be explained by the change in economic and institutional variables, by less strict employment protection legislation, by lower union density, and by higher unemployment. However, the reduction in inflation implies that many more industries were affected by DNWR in the 1990s than the 70s, even if the fraction of wage cuts that is prevented by is DNWR was much lower in the latter period. We find that the fraction of industry-years affected by DNWR has increased from 0.4 percent in the 1970s to 1.7 percent in the late 1990s.

Fourth, we make a methodological innovation to this literature by proposing a new method for statistical comparison of the notional and empirical samples. We observe that the incidence rates of nominal wage cuts in the notional country-year samples can be viewed as country-year specific notional probabilities of a wage cut. Thus, conditional on the notional distributions, we can compute the exact p-values for the number of wage cuts in various subsamples by use of formulae for binomial distributions. This method can also be used on micro data. For example, with panel data for wages at firm level, one may explore possible differences in DNWR across industries or regions.

We have not tried to investigate the effects of possible DNWR on output and employment. To do this in a meaningful way would require a more extensive data set in terms of variables, yet it seems as an important extension of our present work.

Overall, our finding of DNWR yields clear additional support to the idea that DNWR has a moderate impact on firms' wage costs in many OECD countries, especially in Europe. Lower union density and higher unemployment, and possibly also weaker employment protection legislation, have led the Fraction of Wage Cuts Prevented FWCP by DNWR to fall over time. Yet the fraction of total industries that have been affected by DNWR has increased over time, due to the lower rates of inflation and lower nominal wage growth in recent years.

A Appendix

We have obtained wage data from Eurostat for all countries except Austria, Canada, Finland, New Zealand Norway, Sweden, and the US (see below). The precise source is Table HMWHOUR in the *Harmonized earnings* domain under the *Population and Social Conditions* theme in the NEWCRONOS database. Our wage variable (HMWHOUR) is labeled *Gross hourly earnings of manual workers in industry*. Gross earnings cover remuneration in cash paid directly and regularly by the employer at the time of each wage payment, before tax deductions and social security contributions payable by wage earners and retained by the employer. Payments for leave, public holidays, and other paid individual absences, are included in principle, in so far as the corresponding days or hours are also taken into account to calculate earnings per unit of time. The weekly hours of work are those in a normal week's work (i.e., not including public holidays) during the reference period (October or last quarter). These hours are calculated on the basis of the number of hours paid, including overtime hours paid. Furthermore, we use data in national currency, and males and females are both included in the data. The data for Germany does not include GDR before 1990 or new *Länder.*

The data are recorded by classification of economic activities (NACE Rev. 1). The sections represented are Mining and quarrying (C), Manufacturing (D), Electricity, gas and water supply (E), and Construction (F). We use data on various levels of aggregation from the section levels (e.g., D Manufacturing) to group levels (e.g., DA 159 Manufacturing of beverages), however, using the most disaggregate level available in order to maximize the number of observations. If, for example, wage data are available for D, DA 158 and DA 159, we use the latter two only to avoid counting the same observations twice.

Wage data for Austria, Canada, Finland, New Zealand, Sweden and the US are from Table 5B 'Wages in manufacturing' in LABORSTA, the Labour Statistics Database, ILO. The data are recorded by ISIC at the three digit level covering the same sectors as the Eurostat data. Wage data for Norway are from Table 210 National Accounts 1970–2003, Statistics Norway, recorded by NACE Rev. 1. The sections represented are the same as for the Eurostat data.

The average number of observations per country-year sample is 20.5, with a standard error of 4.7. The distribution of the number of wage cuts relative to the number of observations on years and countries are reported in Table A1.

We have removed ten extreme observations from the sample.

Data for inflation and unemployment are from the OECD Economic Outlook database.

The primary sources for the employment protection legislation (EPL) index, which is displayed in Table A3, are OECD (2004) for the 1980–1999 period and Lazear (1990) for the years before 1980. We follow the same procedure as Blanchard and Wolfers (2000) to construct time-varying series, which is to use the OECD summary measure in the 'Late 1980s' for 1980–89 and the 'Late 1990s' for 1995–99. For 1990–94, we interpolate the series. For 1973–79, the percentage change in Lazear's index is used to back-cast the OECD measure. However, we are not able to reconstruct the Blanchard and Wolfers data exactly.

Data for union density is from the OECD. Data for Greece for 1978 and 1979 are interpolated, while data before 1977 is extrapolated at the 1977 level.

Data for bargaining coverage is from OECD (2004, Table 3.5) which provide data for 1980, 1990, and 2000. Data for the intervening years are calculated by interpolation while the observations for 1980 are extrapolated backwards. Data for Greece and Ireland is only available for 1994 from ILO (1997, Table

1.2). This observation is extrapolated for the entire period.

The incidence of temporary employment is defined as the fraction of temporary to total employment. Data from 1983 is from the OECD's Corporate Data Environment, Table *Employment by permanency of the (main) job*. Data for Finland 1995 and 1996 and Norway are from Eurostat. Data for Sweden are provided by Statistics Sweden (SCB). Lacking information prior to 1983, we have chosen not to extrapolate the data.

Table A1: The distribution of nominal wage cuts relative to the number of observations by countries and years

Year	*Austria*	*Belgium*	*Canada*	*Germany*	*Denmark*	*Spain*	*Finland*	*France*	*Greece*	*Ireland*	*Italy*	*Luxembourg*	*Netherlands*	*New Zealand*	*Norway*	*Portugal*	*Sweden*	UK	US	*Total*
1973	–	0/20	–	0/23	0/19	–	0/16	0/20	0/12	–	0/24	0/14	0/19	0/28	0/24	–	–	0/21	0/20	0/260
1974	0/16	0/20	0/24	1/23	0/19	–	0/16	0/21	0/13	–	0/24	0/14	0/19	0/28	0/25	–	–	0/21	0/20	1/303
1975	0/16	0/20	0/24	0/24	1/19	–	0/16	0/22	0/13	–	0/24	0/15	0/19	0/28	0/25	–	–	0/21	0/18	1/304
1976	0/16	0/21	0/24	0/24	0/19	–	0/16	0/22	0/13	0/18	0/24	0/15	0/19	0/28	0/25	–	–	0/23	0/18	0/325
1977	0/16	0/21	0/24	0/24	0/19	–	0/16	0/22	0/13	0/18	0/24	0/15	0/19	0/28	0/25	–	–	0/23	0/18	0/325
1978	0/16	0/21	0/24	0/24	0/19	–	0/16	0/22	0/13	0/18	0/24	2/15	0/20	0/28	0/25	–	0/26	0/23	0/18	3/352
1979	0/16	0/21	0/24	0/24	0/20	–	0/16	0/22	0/13	0/20	0/24	0/15	0/19	0/28	0/25	–	0/28	0/22	0/18	0/355
1980	0/16	0/21	0/24	0/24	1/20	–	0/16	0/22	0/13	0/19	0/24	0/15	0/19	1/28	0/25	–	0/28	0/22	0/18	2/354
1981	0/16	0/21	0/23	0/24	0/20	–	0/16	0/22	0/13	0/19	0/24	2/15	0/19	0/28	0/25	0/22	0/28	0/22	0/18	2/375
1982	0/16	0/21	0/20	0/24	0/20	–	0/16	0/21	0/13	0/20	0/24	0/16	0/18	0/28	0/25	0/22	0/28	0/22	0/18	0/372
1983	0/16	0/21	2/20	1/24	0/20	–	0/16	0/21	0/11	0/18	0/24	0/16	0/18	1/28	0/25	0/22	0/27	0/24	0/18	5/369
1984	0/16	0/21	1/28	1/27	0/20	–	0/16	0/22	0/17	0/18	0/24	1/16	0/16	1/28	0/25	0/22	0/27	0/24	0/18	7/385
1985	0/16	0/21	2/28	0/27	0/20	–	0/16	0/23	0/18	1/20	0/24	1/16	0/17	1/28	0/25	0/22	0/28	0/24	0/18	5/391
1986	0/16	6/21	5/28	0/27	2/20	–	0/16	2/23	2/18	1/21	–	0/14	0/18	0/28	0/25	0/22	0/28	0/24	0/18	20/367
1987	0/16	0/21	1/28	0/27	0/20	–	0/16	1/23	0/18	3/20	–	3/14	0/18	1/28	0/25	0/22	0/28	0/24	0/18	9/366
1988	1/16	3/21	0/28	0/27	0/20	–	0/16	5/23	0/18	1/20	–	3/14	0/18	0/28	0/25	0/21	0/28	0/25	0/18	15/367
1989	0/16	0/22	0/28	0/27	0/20	–	0/16	1/23	0/17	2/20	–	0/17	0/17	2/28	0/25	3/24	0/28	0/26	0/20	9/371
1990	0/16	0/24	2/28	0/27	0/20	0/26	0/16	1/23	0/24	1/21	–	1/16	0/17	5/28	1/25	0/23	0/28	0/25	0/20	11/408
1991	0/16	0/24	2/28	0/27	0/20	0/26	0/16	1/23	0/25	0/21	–	0/16	0/17	1/28	0/25	0/23	–	0/25	0/20	4/380
1992	0/16	0/23	3/26	0/24	1/20	0/26	0/16	0/23	1/25	0/21	–	0/17	0/17	3/28	0/25	0/23	1/13	0/25	0/20	9/388
1993	1/16	0/22	5/26	2/24	2/20	1/26	1/16	2/24	0/25	1/21	–	0/17	0/14	9/28	0/25	0/23	5/14	2/25	1/20	32/386
1994	0/16	0/22	4/20	1/26	–	2/26	1/16	8/15	0/25	2/21	–	1/17	0/8	7/28	0/25	0/23	0/14	11/22	0/20	37/344
1995	0/16	19/22	6/20	0/26	–	0/26	0/16	0/10	0/25	6/20	–	0/17	0/10	2/28	1/25	0/23	0/14	1/21	0/20	35/339
1996	0/14	0/27	1/20	7/25	–	4/26	–	0/12	0/25	2/23	–	6/19	0/20	1/28	0/25	0/23	0/14	0/26	2/20	24/347
1997	0/14	2/28	5/20	2/31	0/16	6/29	–	0/27	1/25	4/23	–	7/14	1/23	1/28	0/25	0/23	0/15	3/27	1/18	33/386
1998	0/14	0/28	6/20	1/31	0/16	3/29	–	0/25	3/24	3/23	–	4/17	0/23	3/28	0/25	0/29	0/14	1/28	2/18	26/392
1999	0/14	–	12/20	–	1/16	2/30	–	–	–	–	–	1/17	12/22	6/22	0/25	–	0/14	–	0/18	34/198
Total	2/408	31/575	57/665	16/665	8/462	19/270	2/368	21/556	7/469	27/463	0/312	32/423	23/483	45/750	2/674	3/411	6/472	18/615	6/506	324/9509

Table A2: Country-year specific empirical (top number) and notional (middle number) incidence rates and the associated p-value (bottom number).

Year	Austria	Belgium	Canada	Germany	Denmark	Spain	Finland	France	Greece	Ireland	Italy	Luxembourg	Netherlands	New Zealand	Norway	Portugal	Sweden	UK	US
1973		0.000		0.000	0.000		0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000			0.000	0.000
		0.000		0.000	0.010		0.000	0.000	0.021		0.000	0.049	0.000	0.000	0.003			0.003	0.001
		1.000		1.000	0.830		1.000	1.000	0.775		1.000	0.496	1.000	1.000	0.919			0.939	0.985
1974	0.000	0.000	0.000	0.043	0.000		0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000			0.000	0.000
	0.000	0.000	0.001	0.000	0.000		0.000	0.000	0.005		0.005	0.021	0.005	0.000	0.000			0.000	0.000
	1.000	1.000	0.982	1.000	1.000		1.000	1.000	0.934		0.881	0.743	0.918	1.000	1.000			1.000	1.000
1975	0.000	0.000	0.000	0.000	0.053		0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000			0.000	0.000
	0.005	0.005	0.000	0.013	0.009		0.000	0.000	0.007		0.000	0.021	0.000	0.000	0.005			0.000	0.007
	0.919	0.914	1.000	0.735	0.987		1.000	1.000	0.916		1.000	0.727	1.000	1.000	0.881			1.000	0.885
1976	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000			0.000	0.000
	0.006	0.026	0.000	0.011	0.000		0.000	0.000	0.000	0.008	0.011	0.006	0.000	0.000	0.000			0.000	0.005
	0.908	0.581	1.000	0.776	1.000		1.000	1.000	1.000	0.873	0.762	0.914	1.000	1.000	1.000			1.000	0.922
1977	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000			0.000	0.000
	0.000	0.008	0.000	0.000	0.010		0.041	0.000	0.005	0.026	0.000	0.044	0.000	0.000	0.000			0.005	0.000
	1.000	0.854	1.000	1.000	0.830		0.509	1.000	0.943	0.619	1.000	0.507	1.000	1.000	1.000			0.886	1.000
1978	0.000	0.048	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.133	0.000	0.000	0.000		0.000	0.000	0.000
	0.003	0.007	0.001	0.000	0.012		0.007	0.005	0.000	0.000	0.021	0.079	0.012	0.000	0.000		0.000	0.000	0.002
	0.953	0.991	0.982	1.000	0.795		0.897	0.905	1.000	1.000	0.600	0.891	0.785	1.000	1.000		1.000	1.000	0.960
1979	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000
	0.011	0.007	0.000	0.000	0.003		0.000	0.000	0.000	0.005	0.000	0.063	0.086	0.000	0.005		0.000	0.005	0.000
	0.844	0.867	1.000	1.000	0.942		1.000	1.000	1.000	0.914	1.000	0.376	0.180	1.000	0.863		1.000	0.890	1.000
1980	0.000	0.000	0.000	0.000	0.050		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.036		0.000	0.000	0.000
	0.005	0.001	0.000	0.000	0.094		0.000	0.000	0.000	0.012	0.000	0.055	0.000	0.041	0.035		0.000	0.013	0.000
	0.919	0.984	1.000	1.000	0.428		1.000	1.000	1.000	0.795	1.000	0.429	1.000	0.348	0.740		1.000	0.754	1.000
1981	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.133	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	0.023	0.000	0.000	0.000	0.000		0.005	0.000	0.000	0.000	0.000	0.131	0.001	0.000	0.001	0.035	0.000	0.005	0.000
	0.686	1.000	1.000	1.000	1.000		0.919	1.000	1.000	1.000	1.000	0.685	0.986	1.000	0.979	0.453	1.000	0.890	1.000

Table A2: (continued)

Year	Austria	Belgium	Canada	Germany	Denmark	Spain	Finland	France	Greece	Ireland	Italy	Luxembourg	Netherlands	New Zealand	Norway	Portugal	Sweden	UK	US
1982	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	0.000	0.033	0.000	0.007	0.001		0.001	0.000	0.000	0.018	0.000	0.011	0.000	0.000	0.007	0.026	0.005	0.107	0.000
	1.000	0.494	1.000	0.850	0.985		0.988	1.000	1.000	0.695	1.000	0.834	1.000	1.000	0.827	0.556	0.881	0.084	1.000
1983	0.000	0.000	0.100	0.042	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.056	0.000	0.036	0.000	0.000	0.000	0.000
	0.006	0.013	0.227	0.022	0.026		0.000	0.000	0.008	0.023	0.000	0.055	0.095	0.000	0.106	0.008	0.000	0.007	0.038
	0.908	0.763	0.135	0.904	0.587		1.000	1.000	0.920	0.654	1.000	0.406	0.481	1.000	0.188	0.847	1.000	0.850	0.502
1984	0.000	0.000	0.036	0.037	0.000		0.000	0.000	0.000	0.000	0.000	0.063	0.188	0.000	0.036	0.000	0.000	0.000	0.000
	0.026	0.007	0.029	0.041	0.037		0.000	0.005	0.000	0.050	0.089	0.139	0.280	0.000	0.132	0.018	0.000	0.000	0.047
	0.661	0.867	0.810	0.700	0.472		1.000	0.890	1.000	0.400	0.106	0.327	0.303	1.000	0.099	0.670	1.000	1.000	0.424
1985	0.000	0.000	0.071	0.000	0.000		0.000	0.000	0.000	0.050	0.000	0.063	0.000	0.000	0.036	0.000	0.000	0.000	0.000
	0.012	0.012	0.068	0.051	0.013		0.000	0.000	0.005	0.092	0.000	0.095	0.079	0.000	0.055	0.041	0.000	0.000	0.019
	0.824	0.776	0.707	0.243	0.773		1.000	1.000	0.909	0.441	1.000	0.544	0.247	1.000	0.541	0.395	1.000	1.000	0.711
1986	0.000	0.286	0.179	0.000	0.100		0.000	0.087	0.111	0.048		0.000	0.111	0.000	0.000	0.000	0.000	0.000	0.000
	0.005	0.360	0.057	0.000	0.059		0.005	0.091	0.017	0.077		0.137	0.243	0.000	0.000	0.035	0.000	0.000	0.113
	0.919	0.322	0.996	1.000	0.888		0.919	0.651	0.997	0.509		0.128	0.151	1.000	1.000	0.453	1.000	1.000	0.116
1987	0.000	0.000	0.036	0.000	0.000		0.000	0.043	0.000	0.150		0.214	0.000	0.000	0.036	0.000	0.000	0.000	0.000
	0.056	0.143	0.143	0.006	0.000		0.000	0.001	0.005	0.138		0.107	0.083	0.000	0.000	0.033	0.000	0.007	0.026
	0.400	0.039	0.076	0.850	1.000		1.000	1.000	0.922	0.704		0.946	0.212	1.000	1.000	0.477	1.000	0.850	0.619
1988	0.063	0.143	0.000	0.000	0.000		0.000	0.217	0.000	0.050		0.214	0.111	0.000	0.000	0.000	0.000	0.000	0.000
	0.018	0.209	0.035	0.011	0.000		0.000	0.169	0.000	0.059		0.068	0.063	0.000	0.008	0.061	0.001	0.000	0.000
	0.967	0.333	0.374	0.736	1.000		1.000	0.819	1.000	0.671		0.988	0.899	1.000	0.793	0.251	0.979	1.000	1.000
1989	0.000	0.000	0.000	0.000	0.000		0.000	0.043	0.000	0.100		0.000	0.059	0.000	0.071	0.143	0.000	0.000	0.000
	0.074	0.000	0.026	0.021	0.063		0.035	0.001	0.005	0.122		0.024	0.020	0.048	0.140	0.026	0.000	0.000	0.014
	0.294	1.000	0.474	0.563	0.271		0.563	1.000	0.914	0.554		0.661	0.957	0.292	0.229	0.998	1.000	1.000	0.750
1990	0.000	0.000	0.071	0.000	0.000	0.000	0.000	0.043	0.000	0.048		0.063	0.000	0.040	0.179	0.000	0.000	0.000	0.000
	0.005	0.000	0.059	0.000	0.008	0.044	0.000	0.029	0.005	0.077		0.046	0.001	0.005	0.098	0.026	0.006	0.005	0.020
	0.919	1.000	0.776	1.000	0.847	0.308	1.000	0.861	0.881	0.509		0.835	0.987	0.992	0.948	0.537	0.845	0.893	0.674

Table A2: (continued)

Year	*Austria*	*Belgium*	*Canada*	*Germany*	*Denmark*	*Spain*	*Finland*	*France*	*Greece*	*Ireland*	*Italy*	*Luxembourg*	*Netherlands*	*New Zealand*	*Norway*	*Portugal*	*Sweden*	UK	US
1991	0.000	0.000	0.071	0.000	0.000	0.000	0.000	0.043	0.000	0.000		0.000	0.000	0.000	0.036	0.000		0.000	0.000
	0.001	0.021	0.071	0.000	0.050	0.009	0.011	0.012	0.007	0.086		0.012	0.031	0.005	0.168	0.005		0.014	0.000
	0.988	0.600	0.677	1.000	0.362	0.790	0.834	0.969	0.844	0.150		0.824	0.587	0.893	0.039	0.901		0.698	1.000
1992	0.000	0.000	0.115	0.000	0.050	0.000	0.000	0.000	0.040	0.000		0.000	0.000	0.000	0.107	0.000	0.077	0.000	0.000
	0.000	0.011	0.086	0.000	0.046	0.000	0.075	0.038	0.020	0.107		0.049	0.000	0.026	0.160	0.005	0.000	0.035	0.018
	1.000	0.771	0.817	1.000	0.767	1.000	0.287	0.407	0.915	0.094		0.427	1.000	0.524	0.324	0.886	1.000	0.407	0.695
1993	0.063	0.000	0.192	0.083	0.100	0.038	0.063	0.083	0.000	0.048		0.000	0.000	0.000	0.321	0.000	0.357	0.080	0.050
	0.048	0.014	0.227	0.013	0.136	0.023	0.076	0.050	0.009	0.046		0.131	0.044	0.011	0.340	0.021	0.436	0.060	0.013
	0.822	0.729	0.443	0.997	0.476	0.878	0.655	0.886	0.797	0.750		0.091	0.530	0.768	0.507	0.613	0.378	0.812	0.973
1994	0.000	0.000	0.200	0.038		0.077	0.063	0.533	0.000	0.095		0.059	0.000	0.000	0.250	0.000	0.000	0.500	0.000
	0.014	0.061	0.222	0.020		0.057	0.101	0.523	0.005	0.190		0.131	0.153	0.009	0.290	0.077	0.026	0.531	0.021
	0.804	0.251	0.535	0.909		0.817	0.507	0.631	0.876	0.209		0.325	0.266	0.797	0.409	0.157	0.696	0.467	0.654
1995	0.000	0.864	0.300	0.000		0.000	0.000	0.000	0.000	0.300		0.000	0.000	0.040	0.071	0.000	0.000	0.048	0.000
	0.024	0.691	0.319	0.045		0.039	0.014	0.020	0.029	0.364		0.065	0.000	0.000	0.060	0.140	0.107	0.075	0.067
	0.677	0.983	0.533	0.301		0.355	0.804	0.815	0.485	0.365		0.321	1.000	1.000	0.764	0.031	0.204	0.525	0.251
1996	0.000	0.000	0.050	0.280		0.154		0.000	0.000	0.087		0.316	0.050	0.000	0.036	0.000	0.000	0.000	0.100
	0.003	0.046	0.221	0.185		0.094		0.009	0.002	0.183		0.410	0.125	0.006	0.101	0.058	0.000	0.000	0.001
	0.959	0.282	0.045	0.925		0.909		0.897	0.963	0.182		0.277	0.268	0.860	0.212	0.254	1.000	1.000	1.000
1997	0.000	0.071	0.250	0.065	0.000	0.207		0.000	0.040	0.174		0.500	0.043	0.000	0.036	0.000	0.000	0.111	0.056
	0.068	0.093	0.130	0.140	0.056	0.110		0.000	0.026	0.140		0.523	0.035	0.000	0.017	0.047	0.055	0.114	0.033
	0.375	0.509	0.963	0.172	0.400	0.965		1.000	0.867	0.786		0.536	0.806	1.000	0.922	0.334	0.429	0.628	0.882
1998	0.000	0.000	0.300	0.032	0.000	0.103		0.000	0.125	0.130		0.235	0.000	0.000	0.107	0.000	0.000	0.036	0.111
	0.013	0.055	0.284	0.044	0.047	0.131		0.005	0.106	0.098		0.120	0.026	0.000	0.077	0.219	0.071	0.014	0.000
	0.835	0.206	0.668	0.598	0.466	0.459		0.876	0.754	0.815		0.955	0.551	1.000	0.833	0.001	0.355	0.945	1.000
1999	0.000		0.600		0.063	0.067						0.059	0.545	0.000	0.273		0.000		0.000
	0.026		0.603		0.109	0.101						0.151	0.654	0.000	0.312		0.089		0.020
	0.696		0.575		0.467	0.401						0.249	0.198	1.000	0.446		0.269		0.701

Table A3: Indices for employment protection legislation, EPL.

Year	*AT*	*BE*	*CA*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*NL*	*NO*	*NZ*	*PT*	*SW*	*UK*	*US*
1973	1.32	3.20	0.80	3.20	2.30	4.00	2.30	2.44	3.60	0.76	3.60	2.70	2.90	0.90	3.17	2.57	0.56	0.20
1974	1.39	3.20	0.80	3.20	2.30	4.00	2.30	2.57	3.60	0.83	3.60	2.70	2.90	0.90	3.43	3.03	0.58	0.20
1975	1.47	3.20	0.80	3.20	2.30	4.00	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	3.68	3.50	0.60	0.20
1976	1.61	3.20	0.80	3.20	2.30	3.96	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	3.76	3.50	0.60	0.20
1977	1.76	3.20	0.80	3.20	2.30	3.92	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	3.85	3.50	0.60	0.20
1978	1.91	3.20	0.80	3.20	2.30	3.88	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	3.93	3.50	0.60	0.20
1979	2.05	3.20	0.80	3.20	2.30	3.84	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.02	3.50	0.60	0.20
1980	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1981	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1982	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1983	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1984	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1985	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1986	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1987	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1988	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1989	2.20	3.20	0.80	3.20	2.30	3.80	2.30	2.70	3.60	0.90	3.60	2.70	2.90	0.90	4.10	3.50	0.60	0.20
1990	2.20	3.03	0.80	3.08	2.15	3.65	2.27	2.75	3.58	0.90	3.45	2.60	2.87	0.90	4.03	3.28	0.60	0.20
1991	2.20	2.87	0.80	2.97	2.00	3.50	2.23	2.80	3.57	0.90	3.30	2.50	2.83	0.90	3.97	3.07	0.60	0.20
1992	2.20	2.70	0.80	2.85	1.85	3.35	2.20	2.85	3.55	0.90	3.15	2.40	2.80	0.90	3.90	2.85	0.60	0.20
1993	2.20	2.53	0.80	2.73	1.70	3.20	2.17	2.90	3.53	0.90	3.00	2.30	2.77	0.90	3.83	2.63	0.60	0.20
1994	2.20	2.37	0.80	2.62	1.55	3.05	2.13	2.95	3.52	0.90	2.85	2.20	2.73	0.90	3.77	2.42	0.60	0.20
1995	2.20	2.20	0.80	2.50	1.40	2.90	2.10	3.00	3.50	0.90	2.70	2.10	2.70	0.90	3.70	2.20	0.60	0.20
1996	2.20	2.20	0.80	2.50	1.40	2.90	2.10	3.00	3.50	0.90	2.70	2.10	2.70	0.90	3.70	2.20	0.60	0.20
1997	2.20	2.20	0.80	2.50	1.40	2.90	2.10	3.00	3.50	0.90	2.70	2.10	2.70	0.90	3.70	2.20	0.60	0.20
1998	2.20	2.20	0.80	2.50	1.40	2.90	2.10	3.00	3.50	0.90	2.70	2.10	2.70	0.90	3.70	2.20	0.60	0.20
1999	2.20	2.20	0.80	2.50	1.40	2.90	2.10	3.00	3.50	0.90	2.70	2.10	2.70	0.90	3.70	2.20	0.60	0.20

Table A4: Trade union density, percent.

Year	*AT*	*BE*	*CA*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*LU*	*NL*	*NO*	*NZ*	*PT*	*SW*	*UK*	*US*
1973	60.8	47.6	34.60	32.4	62.2		61.4	22.1	35.8	53.3	43.3	45.0	36.2	53.2	58.18		72.5	45.5	23.50
1974	57.9	49.0	35.00	33.7	65.2		63.2	21.7	35.8	53.9	46.2	45.6	36.0	54.1	59.12		73.5	46.4	23.20
1975	59.0	51.8	36.30	34.6	68.9		65.3	22.2	35.8	55.3	48.0	45.7	37.8	53.8	60.06		74.5	48.3	21.60
1976	59.2	52.6	35.70	35.1	73.0		67.6	21.4	35.8	56.3	50.5	46.7	37.1	52.8	61.00		73.9	49.4	21.60
1977	58.6	53.5	36.50	35.2	74.1		66.4	21.4	35.8	57.0	49.8	47.7	37.2	53.6	63.67		76.0	51.1	23.20
1978	57.6	53.1	36.00	35.5	77.8		66.9	20.7	36.9	57.6	50.4	48.9	37.0	54.0	66.33	60.8	77.0	51.8	22.40
1979	56.7	53.8	35.10	35.3	77.1		68.1	19.2	37.9	57.5	49.7	49.4	36.6	55.5	69.00	60.2	77.3	51.6	23.40
1980	56.7	54.1	34.90	34.9	78.6		69.4	18.3	39.0	57.1	49.6	50.8	35.3	58.3	69.10	59.7	78.0	50.7	22.30
1981	56.4	53.4	35.30	35.1	79.9	7.4	68.3	17.8	38.8	56.6	48.0	52.2	33.5	57.9	65.70	61.8	78.3	50.5	21.00
1982	53.8	52.1	35.80	35.0	80.2	8.4	68.4	17.0	38.4	56.1	46.7	52.5	32.8	58.1	65.10	61.1	78.9	48.7	20.25
1983	53.6	51.9	36.60	35.0	80.8	8.9	68.8	16.0	38.6	57.2	45.5	53.0	31.3	58.1	64.50	57.8	79.6	48.0	19.50
1984	52.1	52.0	34.70	34.9	79.3	8.6	69.0	14.9	38.0	57.0	45.3	53.0	30.0	58.3	59.50	56.3	80.8	47.5	18.20
1985	51.6	52.4	32.60	34.7	78.2	8.9	69.1	13.6	37.5	54.2	42.5	52.3	28.7	57.5	56.00	54.6	81.3	46.2	17.40
1986	50.6	51.5	33.00	33.9	77.4	8.6	70.0	12.5	37.2	51.6	40.4	51.1	27.3	57.1	54.10	51.4	82.5	44.8	17.00
1987	49.6	51.6	32.90	33.3	75.0	9.1	70.7	11.9	36.3	50.2	40.0	49.8	24.9	55.7	52.80	47.7	82.4	44.5	16.50
1988	48.9	51.4	34.30	33.1	73.8	9.6	72.3	11.2	34.9	50.5	39.8	48.1	24.7	56.1	54.20	42.3	81.4	42.6	16.20
1989	48.0	52.4	33.00	32.4	75.6	10.0	73.0	10.7	33.7	51.8	39.4	46.1	25.1	58.0	55.10	37.6	80.7	40.6	15.90
1990	46.9	53.9	32.90	31.2	75.3	11.0	72.3	10.1	32.4	51.1	38.8	44.7	25.5	58.5	51.00	31.7	80.0	39.3	15.50
1991	45.5	54.3	35.30	36.0	75.8	14.7	74.4	10.0	32.4	51.2	38.7	42.6	25.6	58.1	44.40	31.5	80.1	38.5	15.50
1992	44.3	54.3	33.10	33.9	75.8	16.5	76.8	10.2	32.0	51.3	38.9	41.5	25.2	58.1	37.10	29.0	82.9	37.2	15.10
1993	43.2	55.0	32.80	31.8	77.3	18.0	78.8	10.1	31.1	50.0	39.2	40.7	25.9	58.0	34.50	28.6	83.9	36.1	15.10
1994	41.4	54.7	34.20	30.4	77.5	17.6	78.0	10.0	30.3	48.6	38.7	39.6	25.6	57.8	30.20	27.3	83.7	34.2	14.90
1995	41.1	55.7	33.80	29.2	77.0	16.3	79.2	9.8	29.6	47.1	38.1	38.6	25.7	57.3	27.60	25.4	83.1	34.1	14.30
1996	40.1	55.9	34.00	27.8	77.4	16.1	78.8	9.8	28.9	45.4	37.4	38.4	25.1	56.3	24.90	24.8	82.7	33.2	14.00
1997	38.9	56.0	28.80	27.0	75.6	15.7	79.4	9.8	28.6	44.4	36.2	38.0	25.1	55.5	23.60	24.3	82.2	32.1	13.60
1998	38.4	55.4	28.50	25.9	76.8	14.9	77.7	9.8	26.7	42.4	35.7	37.4	24.5	55.5	22.30	23.3	81.3	31.5	13.40
1999	37.4	55.1	27.90	25.6	76.3	14.5	77.4	9.8	26.1	40.6	36.1	35.7	24.6	54.8	21.90	23.5	80.6	31.4	13.40

Table A5: Indices for bargaining coverage.

Year	*AT*	*BE*	*CA*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*NL*	*NO*	*NZ*	*PT*	*SW*	*UK*	*US*
1973	95.0	90.0	37.0	80.0	70.0	60.0	90.0	80.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	70.0	26.0
1974	95.0	90.0	37.0	80.0	70.0	60.0	90.0	80.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	70.0	26.0
1975	95.0	90.0	37.0	80.0	70.0	60.0	90.0	80.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	70.0	26.0
1976	95.0	90.0	37.0	80.0	70.0	60.0	90.0	80.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	70.0	26.0
1977	95.0	90.0	37.0	80.0	70.0	60.0	90.0	80.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	70.0	26.0
1978	95.0	90.0	37.0	80.0	70.0	60.0	90.0	80.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	70.0	26.0
1979	95.0	90.0	37.0	80.0	70.0	60.0	90.0	80.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	70.0	26.0
1980	95.0	90.0	37.0	80.0	70.0	60.0	90.0	80.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	70.0	26.0
1981	95.0	90.0	37.1	80.0	70.0	61.0	90.0	81.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	67.0	25.2
1982	95.0	90.0	37.2	80.0	70.0	62.0	90.0	82.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	64.0	24.4
1983	95.0	90.0	37.3	80.0	70.0	63.0	90.0	83.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	61.0	23.6
1984	95.0	90.0	37.4	80.0	70.0	64.0	90.0	84.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	58.0	22.8
1985	95.0	90.0	37.5	80.0	70.0	65.0	90.0	85.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	55.0	22.0
1986	95.0	90.0	37.6	80.0	70.0	66.0	90.0	86.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	52.0	21.2
1987	95.0	90.0	37.7	80.0	70.0	67.0	90.0	87.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	49.0	20.4
1988	95.0	90.0	37.8	80.0	70.0	68.0	90.0	88.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	46.0	19.6
1989	95.0	90.0	37.9	80.0	70.0	69.0	90.0	89.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	43.0	18.8
1990	95.0	90.0	38.0	80.0	70.0	70.0	90.0	90.0	90.0	90.0	80.0	70.0	70.0	60.0	70.0	80.0	40.0	18.0
1991	95.0	90.0	37.4	78.8	71.0	71.0	90.0	90.0	90.0	90.0	80.0	71.0	70.0	56.5	71.0	81.0	39.0	17.6
1992	95.0	90.0	36.8	77.6	72.0	72.0	90.0	90.0	90.0	90.0	80.0	72.0	70.0	53.0	72.0	82.0	38.0	17.2
1993	95.0	90.0	36.2	76.4	73.0	73.0	90.0	90.0	90.0	90.0	80.0	73.0	70.0	49.5	73.0	83.0	37.0	16.8
1994	95.0	90.0	35.6	75.2	74.0	74.0	90.0	90.0	90.0	90.0	80.0	74.0	70.0	46.0	74.0	84.0	36.0	16.4
1995	95.0	90.0	35.0	74.0	75.0	75.0	90.0	90.0	90.0	90.0	80.0	75.0	70.0	42.5	75.0	85.0	35.0	16.0
1996	95.0	90.0	34.4	72.8	76.0	76.0	90.0	90.0	90.0	90.0	80.0	76.0	70.0	39.0	76.0	86.0	34.0	15.6
1997	95.0	90.0	33.8	71.6	77.0	77.0	90.0	90.0	90.0	90.0	80.0	77.0	70.0	35.5	77.0	87.0	33.0	15.2
1998	95.0	90.0	33.2	70.4	78.0	78.0	90.0	90.0	90.0	90.0	80.0	78.0	70.0	32.0	78.0	88.0	32.0	14.8
1999	95.0	90.0	32.6	69.2	79.0	79.0	90.0	90.0	90.0	90.0	80.0	79.0	70.0	28.5	79.0	89.0	31.0	14.4

Table A6: Indices of centralisation.

Year	*AT*	*BE*	*CA*	*DE*	*DK*	*ES*	*FI*	*FR*	*IE*	*IT*	*NL*	*NO*	*NZ*	*PT*	*SW*	*UK*	*US*
1973	3.0	4.0	1.0	3.0	5.0	5.0	5.0	2.0	4.0	2.0	3.0	4.5	3.0	5.0	5.0	2.0	1.0
1974	3.0	4.0	1.0	3.0	5.0	5.0	5.0	2.0	4.0	2.0	3.0	4.5	3.0	5.0	5.0	2.0	1.0
1975	3.0	3.5	1.0	3.0	5.0	4.0	5.0	2.0	4.0	2.0	3.0	4.5	3.0	4.0	5.0	2.0	1.0
1976	3.0	3.5	1.0	3.0	5.0	4.0	5.0	2.0	4.0	2.0	3.0	4.5	3.0	4.0	5.0	2.0	1.0
1977	3.0	3.5	1.0	3.0	5.0	4.0	5.0	2.0	4.0	2.0	3.0	4.5	3.0	4.0	5.0	2.0	1.0
1978	3.0	3.5	1.0	3.0	5.0	4.0	5.0	2.0	4.0	2.0	3.0	4.5	3.0	4.0	5.0	2.0	1.0
1979	3.0	3.5	1.0	3.0	5.0	4.0	5.0	2.0	4.0	2.0	3.0	4.5	3.0	4.0	5.0	2.0	1.0
1980	3.0	3.0	1.0	3.0	3.0	4.0	4.0	2.0	1.0	3.5	3.0	3.5	3.0	3.0	4.5	1.0	1.0
1981	3.0	3.0	1.0	3.0	3.0	4.0	4.0	2.0	1.0	3.5	3.0	3.5	3.0	3.0	4.5	1.0	1.0
1982	3.0	3.0	1.0	3.0	3.0	4.0	4.0	2.0	1.0	3.5	3.0	3.5	3.0	3.0	4.5	1.0	1.0
1983	3.0	3.0	1.0	3.0	3.0	4.0	4.0	2.0	1.0	3.5	3.0	3.5	3.0	3.0	4.5	1.0	1.0
1984	3.0	3.0	1.0	3.0	3.0	4.0	4.0	2.0	1.0	3.5	3.0	3.5	3.0	3.0	4.5	1.0	1.0
1985	3.0	3.0	1.0	3.0	3.0	3.5	5.0	2.0	2.5	2.0	3.0	4.5	3.0	3.0	3.0	1.0	1.0
1986	3.0	3.0	1.0	3.0	3.0	3.5	5.0	2.0	2.5	2.0	3.0	4.5	3.0	3.0	3.0	1.0	1.0
1987	3.0	3.0	1.0	3.0	3.0	3.5	5.0	2.0	2.5	2.0	3.0	4.5	3.0	3.0	3.0	1.0	1.0
1988	3.0	3.0	1.0	3.0	3.0	3.5	5.0	2.0	2.5	2.0	3.0	4.5	3.0	3.0	3.0	1.0	1.0
1989	3.0	3.0	1.0	3.0	3.0	3.5	5.0	2.0	2.5	2.0	3.0	4.5	3.0	3.0	3.0	1.0	1.0
1990	3.0	3.0	1.0	3.0	3.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0
1991	3.0	3.0	1.0	3.0	3.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0
1992	3.0	3.0	1.0	3.0	3.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0
1993	3.0	3.0	1.0	3.0	3.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0
1994	3.0	3.0	1.0	3.0	3.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0
1995	3.0	3.0	1.0	3.0	2.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0
1996	3.0	3.0	1.0	3.0	2.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0
1997	3.0	3.0	1.0	3.0	2.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0
1998	3.0	3.0	1.0	3.0	2.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0
1999	3.0	3.0	1.0	3.0	2.0	3.0	5.0	2.0	4.0	2.0	3.0	4.5	1.0	4.0	3.0	1.0	1.0

Table A7: Results from 5000 simulations on regions and sub-periods.

Region		1973–79	1980–89	1990–94	1995–99
	No. of observations	698	1149	595	519
	No. of country-years	31	50	25	23
Anglo	Observed wage cuts	0	26	59	68
	Incidence of wage cuts	0	0.023	0.099	0.131
	Fraction of wage cuts prevented	1	0.452	0.185	0.018
	Fraction of industry-years affected	0.002	0.019	0.023	0.002
	Probability of significance	0.191	0.000	0.039	0.459
	No. of observations	794	1183	587	546
	No. of country-years	41	60	30	27
	Observed wage cuts	4	40	18	63
Core	Incidence of wage cuts	0.005	0.034	0.031	0.115
	Fraction of wage cuts prevented	0.515	0.304	0.243	0.144
	Fraction of industry-years affected	0.005	0.015	0.010	0.019
	Probability of significance	0.085	0.005	0.113	0.062
	No. of observations	474	888	354	260
	No. of country-years	23	40	18	14
	Observed wage cuts	1	3	12	2
Nordic	Incidence of wage cuts	0.002	0.003	0.034	0.008
	Fraction of wage cuts prevented	0.374	0.665	0.292	0.760
	Fraction of industry-years affected	0.001	0.007	0.014	0.024
	Probability of significance	0.520	0.016	0.107	0.009
	No. of observations	258	497	370	337
	No. of country-years	14	25	15	13
	Observed wage cuts	0	5	4	19
South	Incidence of wage cuts	0	0.010	0.011	0.056
	Fraction of wage cuts prevented	1	0.447	0.485	0.358
	Fraction of industry-years affected	0.005	0.008	0.010	0.031
	Probability of significance	0.252	0.105	0.098	0.019

Notes: See Table 1

Table A8: Results with country-specific and period-specific underlying distributions and country-year specific symmetric distribution. The period-specific underlying distributions are based on observations from the periods 1973–79, 1980–89, 1990–94, and 1995–1999 respectively.

		Country-specific shape			Period-specific shape			Symmetric		
Category	Y	FWCP	$\hat{q}-q$	p	FWCP	$\hat{q}-q$	p	FWCP	$\hat{q}-q$	p
All	324	0.181	0.008	0.000	0.196	0.008	0.000	0.200	0.009	0.000
1970–79	5	0.602	0.003	0.013	0.603	0.003	0.015	0.501	0.002	0.061
1980–89	74	0.304	0.009	0.000	0.292	0.008	0.000	0.230	0.006	0.009
1990–94	93	0.157	0.009	0.029	0.190	0.011	0.009	0.213	0.013	0.005
1995–99	152	0.087	0.009	0.098	0.112	0.011	0.044	0.159	0.017	0.005
Anglo	153	0.110	0.006	0.054	0.132	0.008	0.026	0.124	0.007	0.029
Core	125	0.158	0.008	0.013	0.172	0.008	0.006	0.220	0.011	0.000
Nordic	18	0.330	0.004	0.033	0.458	0.008	0.001	0.359	0.005	0.018
South	28	0.424	0.014	0.001	0.344	0.010	0.007	0.333	0.010	0.007
Austria	2	0.380	0.003	0.373	0.681	0.010	0.045	0.019	0.000	0.661
Belgium	31	0.102	0.006	0.252	0.189	0.013	0.079	0.205	0.014	0.038
Canada	57	0.014	0.001	0.481	0.013	0.001	0.492	0.047	0.004	0.366
Denmark	8	0.197	0.004	0.335	0.373	0.010	0.106	0.336	0.009	0.138
Finland	2	−0.070	−0.000	0.715	0.631	0.009	0.090	0.601	0.008	0.108
France	21	−0.067	−0.002	0.680	−0.242	−0.007	0.905	−0.050	−0.002	0.663
Germany	16	0.108	0.003	0.365	−0.028	−0.001	0.613	0.060	0.002	0.460
Greece	7	0.271	0.006	0.250	−0.137	−0.002	0.722	−0.000	−0.000	0.605
Ireland	27	0.193	0.014	0.134	0.252	0.020	0.059	0.035	0.002	0.471
Italy	0	1.000	0.009	0.050	1.000	0.009	0.047	—	0.000	1.000
Luxembourg	32	0.138	0.012	0.191	0.185	0.017	0.102	0.275	0.029	0.018
Netherlands	23	0.357	0.026	0.004	0.337	0.024	0.007	0.395	0.031	0.001
New Zealand	45	0.125	0.009	0.176	0.145	0.010	0.141	0.238	0.019	0.022
Norway	2	0.593	0.004	0.131	0.489	0.003	0.252	—	0.000	1.000
Portugal	3	0.804	0.030	0.000	0.840	0.038	0.000	0.823	0.034	0.000
Spain	18	0.129	0.010	0.319	−0.212	−0.012	0.839	0.001	0.000	0.567
Sweden	6	0.401	0.009	0.087	0.461	0.011	0.043	0.454	0.011	0.051
UK	18	0.204	0.007	0.151	0.180	0.006	0.193	0.217	0.008	0.129
US	6	0.119	0.002	0.472	0.227	0.003	0.339	−0.206	−0.002	0.770

Notes: Under the symmetry approach, we do not simulate any notional wage cuts for Italy and Norway, hence the FWCP is not defined for these countries. See also notes to Table 1.

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