# Downward Nominal Wage Rigidity in the OECD*

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**Abstract**

This paper explores the existence of downward nominal wage rigidity (DNWR) in 19 OECD countries, over the period 1973–1999, using data for hourly nominal wages at industry level. Based on a novel nonparametric statistical method, which allows for country and year specific variation in both the median and the dispersion of industry wage changes, we reject the hypothesis of no DNWR. The fraction of wage cuts prevented due to DNWR has fallen over time, from 70 percent in the 1970s to 11 percent in the late 1990s, but the number of industries affected by DNWR has increased. DNWR is more prevalent when inflation is high, unemployment is low, union density is high and employment protection legislation is strict.

JEL: J3, J5, C14, C15, E31
Keywords: Downward nominal wage rigidity, OECD, employment protection legislation, wage setting

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# 1 Introduction

In recent years, a number of countries have adopted explicit inflation targets for monetary policy, reflecting a general agreement that monetary policy must ensure low inflation. The deliberate policy of low inflation has led to renewed interest among academics as well as policy makers for the contention of Tobin (1972) that if policy aims at too low inflation, downward rigidity of nominal wages (DNWR) may lead to higher wage pressure, involving higher equilibrium unemployment (see e.g. Akerlof et al., 1996, 2000, Holden, 1994, and Wyplosz, 2001). Other economists have been less concerned, questioning the existence of DNWR, in particular in low inflation economies (see e.g. Gordon, 1996 and Mankiw, 1996). The issue has also received considerable attention among policy makers, cf. for example (ECB, 2003, OECD, 2002 and IMF, 2002).

To shed light of this issue, a fast growing body of empirical research has explored the existence of DNWR in many OECD countries (see references in section 2 below). Almost all of these studies use various kinds of micro data, mostly of the wage of individual workers, but occasionally also the wage in specific jobs in individual firms. While these studies generally seem to document the existence of DNWR, a number of key questions are still left unresolved. As the different studies vary considerably concerning both type of data and the methods that are used, it is difficult to compare the degree of DNWR across countries and the extent to which DNWR has varied over time. Furthermore, while individual data is necessary to explore whether wages are rigid at employee level, it will often be unable to answer the question of whether firms can circumvent wage rigidity at the individual level. For example, the firm may change the composition of the workforce by turnover, or it may refrain from giving wage increases to some workers to save the loss from being unable to cut wages of other.[1] Correspondingly, even if wage rigidity binds in one firm, jobs might be shifted over to other firms where wages are lower, so that the industry effects

[1]These measures may clearly have other implications, that fall outside the scope of this paper.

are small. Then DNWR may be less important for macroeconomic performance. It therefore seems valuable also to investigate DNWR using industry level data.

This paper explores the existence of DNWR in 19 OECD countries, over the period 1973–1999, using data for hourly nominal earnings at industry level. The study is to be seen as complementary to the large number of micro studies, as it allows for comparisons across different groups of countries, and comparisons over time. More importantly, by using data for the hourly earnings at industry level, our study captures effects of changes in the composition of the workforce, as well as the effect of changes in the wage rates. Furthermore, our study covers a number of countries in Continental Europe, for which there so far is little available evidence of the existence of DNWR, in spite of the considerable policy importance of this issue in relation to the ambitious inflation target of the ECB. Incidentally, in their discussion of the economic evolution in the euro area, both the OECD and the IMF are concerned about DNWR, pointing out the lack of empirical evidence (OECD, 2002 and IMF, 2002). In ECB's recent evaluation of its monetary policy framework, it is concluded that '...the importance in practice of downward nominal rigidities is highly uncertain and the empirical evidence is not conclusive, particularly for the euro area' (ECB, 2003, page 14).

Our paper is also relevant for the recent research on business cycles and monetary policy. While price rigidities have been a major issue for decades, several recent contributions have argued that wage stickiness may play a key role (e.g. Erceg et al., 2000, Smets and Wouters, 2003 and Hall, 2005). In this literature, wage and price stickiness are usually implemented without allowing for possible asymmetry. However, in an era of low inflation, it seems important also to explore whether nominal wages are rigid downwards, as this might exacerbate rigidities in a downturn of the economy.

To investigate the extent of DNWR, we apply a novel statistical method. The advantage of the method is that it uses much weaker assumptions than most previous analysis, implying that the results should be more robust. First, the method is based on a nonparametric analysis, using data for hourly earnings only, so that no assumptions concerning explanatory variables or specific functional forms are involved. Second, we allow for country and year specific variation in the median and the dispersion of wage changes, while most other tests are based on more restrictive assumptions. Our robustness test nevertheless indicates that the method is able to detect more than 90 percent of the DNWR that exists in the data.

In addition to investigate the extent of DNWR, we explore potential determinants of DNWR that are suggested in the theoretical literature. As we have a panel of 19 countries over 27 years, we are able to explore the effect on DNWR of economic and institutional variables like inflation, unemployment, employment protection legislation, union density, which are often difficult to evaluate in studies from a single country. Such information is useful as it sheds light on both possible explanations for DNWR, and on how the extent of DNWR might be affected by economic policy.

The paper is organised as follows. In Section 2, we briefly present the main theoretical explanations for DNWR, and we refer to related empirical literature. The empirical approach is laid out in Section 3. In section 4, we document the empirical results on DNWR and discuss the robustness of our method. In Section 5, we explore the determinants of nominal wage rigidity. Section 6 concludes.

## 2 Theoretical framework and related literature

In the literature, two alternative explanations of the existence of DNWR have been proposed. The most common explanation, advocated by e.g. Blinder and Choi (1990) and Akerlof et al. (1996), is that employers avoid nominal wage cuts because both they and (in particular) the employees think that a wage cut is unfair. The other explanation, proposed by MacLeod and Malcomson (1993) in a individual bargaining framework, and Holden (1994) in a collective agreement framework, is that nominal wages are given in contracts that can only be changed by mutual consent. Both these theories predict that nominal wage cuts will be prevented in some, but not all circumstances. For the purpose of detecting DNWR, there is no need to distinguish between these two explanations of DNWR, and, as argued by Holden (1994), they are likely to be complementary.[2] However, we investigate whether institutional variables can explain the extent of DNWR in section 5, as predicted by the contract explanation.

Empirical work on DNWR have grown rapidly in recent years, with various types of evidence. Blinder and Choi (1990), Akerlof et al. (1996), Bewley (1999) and Agell and Lundborg (2003) report results from interviews and surveys of employees and employers. A few papers document the existence of DNWR on aggregate time-series data, see e.g. Holden (1998), Fortin and Dumont (2000) and Wyplosz (2001). However, the great majority of studies explores large micro-data sets, following either of two types of approaches. The first type, initiated by the skewness-location approach of McLaughlin (1994), focuses on the effect of inflation on the distribution of wage changes; Christofides and Leung (2003), Lebow et al. (2003), Nickell and Quintini (2003) and Elsby (2004) are recent applications. The second type, referred to as the earnings function approach by Knoppik and Beissinger (2003), adds other explanatory variables that are usually included in wage equations, see e.g. Fehr and Gotte (2005) and Altonji and Devereux (2000). Our study is of the first type, thus a brief discussion of this method is warranted. As is well known (see e.g. discussion in Knoppik and Beissinger, 2003 or Nickell and Quintini, 2003), the validity of variants of this type of approach rests on various restrictive assumptions concerning the *notional* distribution of wage changes, i.e. the wage changes that would prevail in the absence of DNWR, following the terminology of Akerlof et al. (1996). The LSW statistic, suggested by Lebow et al. (1995), requires that the notional distribution is symmetric. The Kahn test (Kahn, 1997) allows for asymmetry of the notional wage change distribution, as long as the

[2]Efficiency wage theories and insider-outsider theories are also sometimes mentioned as explanations of DNWR, but these theories explain real wage rigidity and need additional assumptions to generate DNWR.

shape of the notional distribution is invariant to inflation, i.e. the only effect of inflation on the distribution of wage changes comes in the form of DNWR. As illustrated in Figure 2 below, the wage change distribution is asymmetric in our data, and dispersion changes over time (as does inflation), so both these methods are problematic in our case. The Nickell and Quintini (2003) method is based on the assumption (or approximation) that the probability of a nominal wage cut is a quadratic function of the median wage change. As will become apparent below, we construct the notional wage change distribution based on the wage change observations in the high inflation years 1973–92, assuming the same shape of the notional distribution in all country year samples, but allowing for country year variation in the median and the dispersion of wage changes.

In general these studies document that nominal wages are rigid downwards. However, with the exception of Dessy (2002), different methods and data in the above-mentioned studies make it in general difficult to compare the degree of downward nominal wage rigidity across countries.[3]

# 3 Empirical approach

We use an unbalanced panel of industry level data for the annual percentage growth of gross hourly earnings for manual workers from the manufacturing, mining and quarrying, electricity, gas and water supply, and construction sectors of 19 OECD countries in the period 1973–1999. The countries included in the sample are Austria, Belgium, Canada, Germany, Denmark, Spain, Finland, France, Greece, Ireland, Italy, Luxembourg, Netherlands, Norway, New Zealand, Portugal, Sweden, the UK and the US. The main data source for wages are harmonized hourly earnings from Eurostat and wages in manufacturing from ILO.[4] One observation is thus denoted $\Delta w_{jit}$ where $j$ is index for industry, $i$ is index for country and $t$ is index for year. There are all

[3]The International Wage Flexibility Project, organised by William Dickens and Erica Groshen, may change that, as it comprises studies on comparable micro data for many OECD countries.

[4]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.

together 9509 observations distributed across 449 country-year samples, on average 21 industries per country-year. More details on the data are provided in the appendix.

As most other studies of DNWR use micro data, it is useful to discuss the difference between DNWR at individual versus industry level. The average wage growth in an industry can be decomposed into two parts: the average wage growth for job stayers, and the effects of compositional changes, where the wages of new workers differ from the wages of those who leave. As to the former component, considering the average wage growth rather than for a single person will tend to reduce the incidence of nominal wage cuts (given that the economy-wide wage change is positive), as the average wage change has a lower variance than individual wage changes. The latter component – compositional changes – may be positive or negative, so the effect on the incidence of nominal wage cuts is ambiguous. Nevertheless, if DNWR prevents wage cuts for some workers, without affecting the wage for others, there will be an effect on average industry wages that we may detect in our data. Yet the fact that our data are based on the average of many workers, and are affected by compositional changes, will reduce our ability to detect the impact of individual DNWR, as these effects may be seen as 'noise' relative to individual DNWR. Thus, we are likely to detect less DNWR than one usually finds in micro data. However, as these effects are not related to inflation, they will not cause a deficit in the wage change distribution that depends on inflation. In other words, these effects will not lead to us to find DNWR that is not caused by DNWR at the individual level. (In section 4 below, we undertake robustness checks to substantiate this claim.)

Note also that if firms respond to individual DNWR by exploiting other 'avenues of flexibility', for example by giving lower wage growth to other workers, or changing the composition of the workforce, then individual DNWR will have less or no impact on average industry wages. In this case we will not find any DNWR. Yet in this situation one may argue that the individual DNWR gives a misleading picture of excessive rigidity, as firms in this case are able to manage the wage

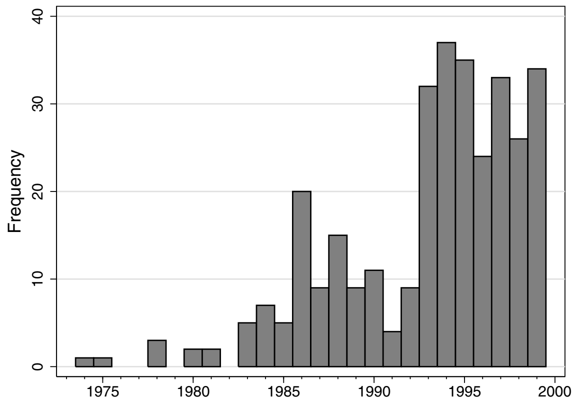


*Figure 1: The number of wage cuts over time.*

costs by other means.

A further aspect is that in most micro studies, a nominal wage cut is understood as a reduction in hourly nominal pay for a job stayer. This may lead to biased estimates due to self selection, if employees quit if their wage is cut, implying that they no longer are job stayers.[5] In contrast, to the extent that such behaviour affects average industry wages, and thus affects our results, it is not a bias, as it would reflect a real impact on firms' wage costs. Micro data studies have, however, an advantage in a much larger number of observations, with the possibility of controlling for other explanatory variables. Overall, it seems worthwhile to explore DNWR with both types of data.

There are no nominal wage cuts in 331 (74%) of the country-year samples. In our data we observe, however, no less than $Y = 324$ events of nominal wage cuts, i.e. 3.4 percent of all observations. There were fewer wage cuts in the 1970s, early 1980s and early 1990s, while most wage cuts occurred after 1992, cf. Figure 1. Table A1 in the data appendix reports the distribution of wage cuts and observations across countries and years.

As an illustration Figure 2 displays box plots of annual wage changes in Portugal, as well as a histogram of the wage changes in 29 industries in Portugal in 1998. We see that the average and the dispersion of wage growth vary over time, with a falling trend. The histogram for 1998 seems consistent with the idea that DNWR has prevented some nominal wage cuts, compressing

[5]Assuming that the higher wage of the job quitter does not reflect higher productivity, which seems reasonable in a situation where the firm wants to cut the wage.

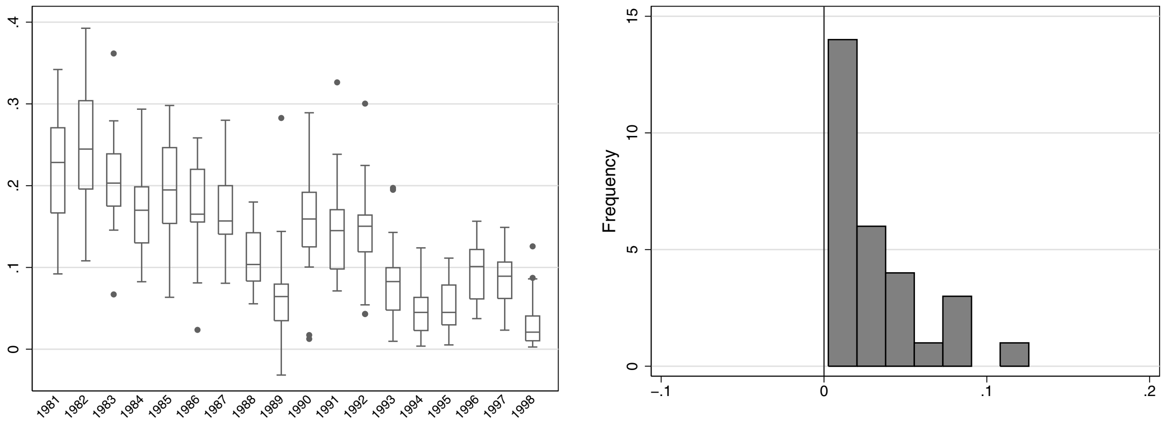


*Figure 2: Box plots of annual wage growth in Portugal (left) and histogram of annual wage growth in 1998 (right). The box plot illustrates the distribution of wage changes within a country-year. The box extends from the 25th to the 75th percentile with the median inside the box. The whiskers emerging from the box indicate the tails of the distributions and the dots represent outliers.*

the empirical wage change distribution relative to the notional by pushing the left tail to positive values. However, to evaluate this hypothesis properly, we need to use a formal statistical method.

To detect whether the empirical distribution is compressed relative to the notional distribution (i.e. without DNWR), we must specify the notional distribution, as well as compare the notional distribution with the empirical outcomes. We construct the shape of the notional distribution on the basis of all observations for the high inflation period 1973–1992, assuming the same shape in all country-years, except that we allow for the median and dispersion to differ across country-year samples. Thus, our assumptions are less restrictive than the Kahn test which would be biased in our sample, due to the fact that both dispersion and inflation fall over time. The constructed shape may also be affected by DNWR, but this effect should be small given that we only use observations from the high-inflation years where DNWR is less likely to be binding. Alternatively, we could have assumed that the notional distribution was normal. However, as illustrated in Figure 3 below, this would not be a good approximation.

To compare the notional distributions with the empirical outcomes, we simulate all country-year samples based on the notional distributions, and count the number of wage cuts in the simulations. If the empirical outcomes were affected by DNWR, the simulations based on the notional distributions will involve a higher number of wage cuts than what actually took place. If this difference is sufficiently large (which will be made more precise below), we conclude that DNWR has been binding in some country-year samples. In the next section, our test is presented more formally.

### 3.1 The formal test

As mentioned above, our test is based on the assumption that the shape of the wage change distribution is the same (in the absence of possible DNWR) in all country-year samples, except that the median and dispersion may vary among country-year samples. To ensure robustness to DNWR and outliers, we follow Nickell and Quintini (2003) and measure dispersion by the range between the 75th and the 35th percentiles, rather than the standard deviation. Using the 35th percentile as the lower range reduces the risk that it is affected by DNWR. For the same reasons, we use the median rather than the mean. Under these assumptions, we construct an underlying distribution of wage changes based on the sample of 7117 empirical wage change observations for the high inflation period 1973–92, where the empirical wage changes are normalised with respect to the country-year specific median ($\mu_{it}$) and inter percentile range ($P75_{it} - P35_{it}$), i.e.

$$\Delta w_s^n \equiv \left( \frac{\Delta w_{jit} - \mu_{it}}{P75_{it} - P35_{it}} \right), \qquad s = 1, \ldots, 7117 \tag{1}$$

For simplicity we use subscript $s$ which runs over all $j$, $i$ and $t = 1973, \ldots, 1992$. The left panel of Figure 3 compares the underlying distribution of wage changes with the standard normal distribution; we notice that the underlying distribution is skewed with the mean at 2.9 percent.

The country-year specific distribution of *notional* wage changes are calculated on the basis of the underlying wage changes, $\Delta w_s^n$, adjusting for the country-year specific median and inter percentile range. The right panel of Figure 3 compares the empirical distribution for Portugal in 1998 with the corresponding notional distribution (i.e. the underlying distribution after ad-

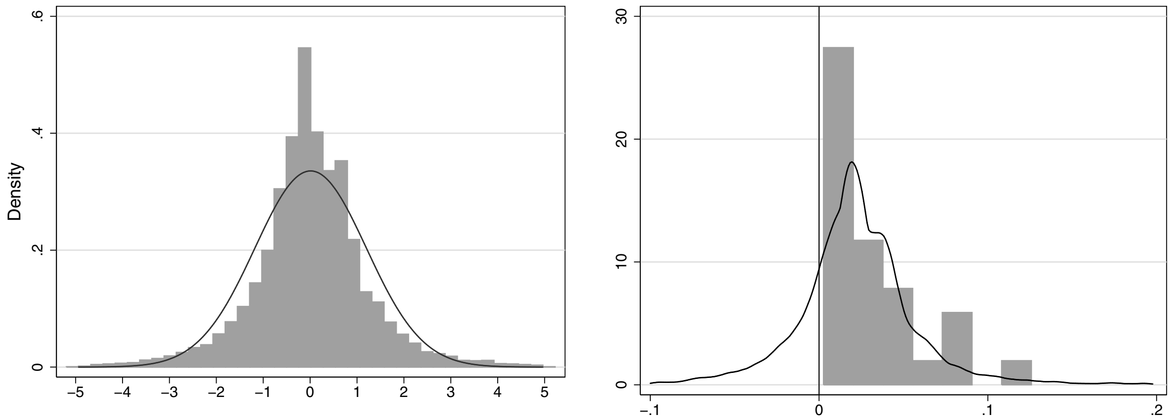


*Figure 3: Left: Histogram of the normalised underlying distribution of wage changes and the normal density (solid line). 79 extreme observations are omitted. Right: Histogram of observed wage changes and the notional wage change distribution in Portugal 1998.*

justment for the empirical median and dispersion in Portugal in 1998). Thus, by construction the notional distribution and the empirical histogram have identical median and inter percentile range, but the shapes differ, as the notional distribution is based on the shape of the normalised underlying distribution illustrated to the left in Figure 3. We observe that the country-specific notional distribution indicates a considerable probability of negative wage changes, in contrast to the empirical outcome.

One complication is that the empirical samples, as well as the moments based on them, are stochastic and thus burdened with unknown uncertainty. To allow for that, we use a bootstrap method. More specifically, for each of the 449 country-year samples, we

- bootstrap the empirical wage changes (for example, in a country-year with 24 observations, we make 24 random draws from the empirical sample of 24 industry wage changes, with replacement),
- count the number of bootstrapped wage cuts in the country-year, $y_{it}^B$,
- calculate the country-specific bootstrapped median, $\mu_{it}^B$ and the 35th and 75th percentiles, $P35_{it}^B$ and $P75_{it}^B$,

- construct the country-year specific distribution of notional wage changes by adjusting the underlying wage change distribution for the country-specific bootstrapped median and the bootstrapped inter percentile range

$$\Delta\widetilde{w}_s^{it} \equiv \Delta w_s^n \left(P75_{it}^B - P35_{it}^B\right) + \mu_{it}^B, \qquad s = 1, \ldots, 7117 \tag{2}$$

- calculate the corresponding country-year specific probability of a notional wage cut in country-year $it$ as the incidence of notional wage cuts out of the total sample of notional wage changes $S = 7117$

$$\tilde{q}_{it} \equiv \frac{\#\Delta\widetilde{w}_s^{it} < 0}{S}, \qquad s = 1, \ldots, 7117 \tag{3}$$

- simulate the number of notional wage cuts in each country-year specific sample, $\widehat{y}_{it}$, by drawing from a binomial distribution using the country-specific notional probabilities $\tilde{q}_{it}$.

We then compare the total number of bootstrapped wage cuts $y^B = \sum_{it} y_{it}^B$ for all 449 country-year samples with the total number of simulated notional wage cuts, $\widehat{y} = \sum_{it} \widehat{y}_{it}$. If the empirical samples are affected by DNWR, there will be a tendency that there are more simulated wage cuts than bootstrapped wage cuts, i.e. $\widehat{y} > y^B$. We therefore repeat this procedure 5000 times, undertaking a new bootstrap for each country-year sample each time, and count the number of times where $\widehat{y} > y^B$ (denoted $\#(\widehat{y} > y^B)$). The null hypothesis is rejected with a level of significance at 5 percent if $1 - \#(\widehat{y} > y^B)/5000 \leq 0.05$.

Given our assumption that the shape of the notional wage change distributions is the same in all country-year samples, while the median wage growth and dispersion may vary, constructing the underlying wage change distribution by use of 7117 observations should ensure a high degree of accuracy in our notional country-specific distributions. Furthermore, 5000 simulations will ensure a close approximation to the distribution of the total number of wage cuts if there were no

DNWR.[6] Thus, the significance level of our test should be reliable. However, if DNWR is at work in some country-year samples that are used in constructing the underlying wage change distribution, the underlying and notional wage change distribution will be compressed, as these are based on the empirical distributions for all country-year samples. Likewise, if DNWR compresses the inter percentile range in certain country year samples, the associated notional country year specific distribution will also be compressed. Thus, in these cases the notional probabilities will be biased downwards, reducing the number of simulated wage cuts. This will reduce the power of our test.However, under $H_0$, there is no DNWR, and thus no downward bias. Hence this aspect will not affect the significance level of our test.

## 4 Results

There are more simulated than bootstrapped wage cuts in all 5000 simulations. Thus we reject the null hypothesis comfortably with a p-value of 0, and we may conclude that DNWR has been at work in our sample. To illustrate the power of the test we plot the histograms of the number of simulated and bootstrapped wage cuts in Figure 4. The distribution of the simulated wage cuts are almost entirely to the right of the distribution of the bootstrapped cuts. On average, we simulate $\widehat{Y} = 417.0$ notional wage cuts and bootstrap 324 wage cuts (due to the large number of simulations, the bootstrapped average of 324 clearly equals the number of observed wage cuts, $Y$). The average fraction of notional wage cuts that is prevented by DNWR, may be expressed by $(1-Y/\widehat{Y})$ which for the whole sample yields $(1-324/417) = 0.22$. Thus, a bit more than one out of five notional wage cuts does not result in an observed wage cut due to DNWR. Another measure which illustrates the economic significance of DNWR, is the average fraction of industry-years affected by DNWR. This fraction is an estimate of the probability than an observation is affected

[6] Given the notional country-year specific distributions it would in principle be straightforward to calculate the probability distribution function for the total number of wage cuts by use of a formulae for draws from multinomial distributions. However, with 9509 observations, drawn from different binomial distributions, this is computationally very demanding. Simulation is computationally simpler, allows for bootstrapping, and still accurate.

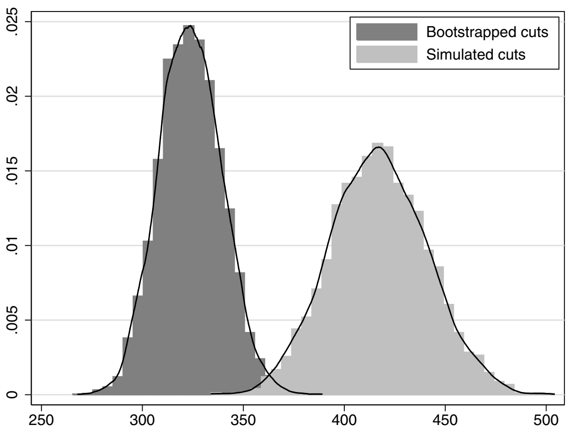


*Figure 4: The frequency distributions of the number of 5000 bootstrapped (empirical) and simulated (notional) wage cuts.*

*Table 1: Results from 5000 simulations on subperiods.*

| *Sample properties:* | 1973–1979 | 1980–1989 | 1990–1994 | 1995–1999 |
|---|---|---|---|---|
| No. of observations ($S$) | 2224 | 3717 | 1906 | 1662 |
| No. of country-years | 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 ($Y$) | 5 | 74 | 93 | 152 |
| Incidence of wage cuts ($Y/S$) | 0.0023 | 0.0199 | 0.0488 | 0.0915 |
| *Simulation results:* | | | | |
| Average simulated wage cuts ($\widehat{Y}$) | 16.7 | 112.8 | 116.0 | 171.5 |
| $\#(\widehat{y} > y^B)$ | 4973 | 4992 | 4794 | 4502 |
| Probability of significance ($p$) | 0.005 | 0.002 | 0.041 | 0.100 |
| Fraction of wage cuts prevented (*FWCP*) | 0.698 | 0.346 | 0.199 | 0.113 |
| Fraction of industry-years affected (*FIYA*) | 0.005 | 0.010 | 0.012 | 0.012 |

Note: $\#(\widehat{y} > y^B)$ is the number of simulations where we simulate more wage cuts than we bootstrap. $FWCP = 1 - Y/\widehat{Y}$. $FIYA = (\widehat{Y} - Y)/S$.

by DNWR and may be calculated by $(\widehat{Y} - Y)/S$ where $S$ is the total number of industry-year observations. For the whole sample the fraction is $(417 - 324)/9509 = 0.010$.

A number of interesting questions arise. Is there evidence for DNWR for different time periods, regions and countries? To what extent is DNWR related to labour market institutions as proposed by theory? 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, see Table 1.

There is evidence of DNWR in all periods although only at the ten percent level in the latter period. In the high-inflation 1970s, the fraction of wage cuts prevented was 70 percent. In the 1980s, it had fallen to 35 percent, and then further to 20 percent in the early 1990s. In the late 1990s, the fraction of wage cuts prevented was 11 percent. However, as nominal wage growth has fallen in line with inflation, the number of industry-years affected by DNWR has increased from 0.5 percent in the 1970s, to 1.0 percent in the 1980s and 1.2 percent in the 1990s.

To investigate whether the change in DNWR over time is significant, we undertake Poisson regressions with the number of observed wage cuts in each country-year sample, $Y_{it}$, as the dependent variable, and normalise on the average number of simulated wage cuts for country-year sample, $\widehat{Y}_{it}$. A Poisson regression seems appropriate as the endogenous variable is based on count data, see Cameron and Trivedi (1998). Adding a time trend, we obtain a trend coefficient of 0.037, which is significant at the one percent level. Thus, the ratio of observed to simulated wage cuts has increased over time, implying that we can conclude that DNWR as measured by the fraction of wage cuts prevented, has fallen over time. Furthermore, we also regress the country-year observations of the fraction of industry-years affected, $(\widehat{Y}_{it} - Y_{it})/S_{it}$ on a time trend (now using OLS, as a Poisson regression is not feasible when some observations are negative). We find a trend coefficient of 0.013 which is significantly positive at the one percent level, indicating that the number of industries affected by DNWR has increased over time.

We then split the sample into four groups or 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), cf. results in columns 2–5 in Table 2.

We find significant DNWR at the one percent level for the Core and Nordic regions, at five percent for the South, and at the ten percent level for the Anglo group. The fraction of wage cuts prevented is high in two regions, 49 percent in the Nordic countries and 41 percent in the South. In the Anglo and Core groups, the fraction of wage cuts prevented is considerably lower, 13 and

*Table 2: Results from 5000 simulations on regions.*

| *Sample properties:* | All regions | Anglo | Core | Nordic | South |
|---|---|---|---|---|---|
| No. of observations ($S$) | 9509 | 2961 | 3110 | 1976 | 1462 |
| No. of country-years | 449 | 129 | 158 | 95 | 67 |
| Observed wage cuts ($Y$) | 324 | 153 | 125 | 18 | 28 |
| Incidence of wage cuts ($Y/S$) | 0.0341 | 0.0517 | 0.0402 | 0.0091 | 0.0192 |
| *Simulation results:* | | | | | |
| Average simulated wage cuts ($\widehat{Y}$) | 417.0 | 176.6 | 158.6 | 34.7 | 47.1 |
| $\#(\widehat{y} > y^B)$ | 5000 | 4621 | 4948 | 4948 | 4921 |
| Probability of significance | 0 | 0.076 | 0.010 | 0.010 | 0.016 |
| Fraction of wage cuts prevented (*FWCP*) | 0.223 | 0.134 | 0.211 | 0.493 | 0.405 |
| Fraction of industry-years affected (*FIYA*) | 0.010 | 0.008 | 0.011 | 0.008 | 0.013 |

21 percent respectively. This difference is roughly in line with what one would expect in view of the differences in labour market institutions. Based on a theoretical framework allowing 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 be increasing in the coverage of collective agreements and in union density. For non-union workers, the strictness of the employment protection legislation (EPL) is key to their possibility of avoiding a nominal wage cut. Thus, one would expect considerable rigidity in the Nordic countries, where both union density and bargaining coverage are high, while EPL is fairly strict (with the exception of Denmark) (in the appendix, we report country-specific indices for labour market institutions). One would also expect considerable rigidity in southern Europe, as EPL is very strict and bargaining coverage fairly high, even if union density is on the low side. In the Core region, even if bargaining coverage is fairly high, and EPL 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, density is lower and EPL weaker than in the other regions, so this is where one would expect the weakest DNWR.

Splitting the sample by combining the regions and the sub-periods implies a smaller number of observations behind each test statistic, and as expected this reduces the significance levels, see

*Table 3: Results from 5000 simulations on regions and sub-periods.*

| Region | | 1973–1979 | 1980–1989 | 1990–1994 | 1995–1999 |
|---|---|---|---|---|---|
| | 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 | 0.0226 | 0.0992 | 0.1310 |
| | Average simulated wage cuts | 3.2 | 42.0 | 67.0 | 64.4 |
| | $\#(\widehat{y} > y^B)$ | 4742 | 4861 | 3866 | 1607 |
| | Probability of significance | 0.052 | 0.028 | 0.227 | 0.679 |
| | Fraction of wage cuts prevented | 1 | 0.385 | 0.120 | 0 |
| | Fraction of industry-years affected | 0.005 | 0.014 | 0.014 | 0 |
| | 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 | 0.0050 | 0.0338 | 0.0307 | 0.1154 |
| | Average simulated wage cuts | 9.4 | 53.7 | 23.6 | 71.8 |
| | $\#(\widehat{y} > y^B)$ | 4506 | 4631 | 4105 | 4162 |
| | Probability of significance | 0.099 | 0.074 | 0.179 | 0.168 |
| | Fraction of wage cuts prevented | 0.571 | 0.256 | 0.240 | 0.122 |
| | Fraction of industry-years affected | 0.007 | 0.012 | 0.010 | 0.016 |
| | 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 | 0.0021 | 0.0034 | 0.0339 | 0.0077 |
| | Average simulated wage cuts | 2.1 | 8.4 | 16.2 | 8.0 |
| | $\#(\widehat{y} > y^B)$ | 3017 | 4633 | 3918 | 4778 |
| | Probability of significance | 0.397 | 0.073 | 0.216 | 0.044 |
| | Fraction of wage cuts prevented | 0.521 | 0.643 | 0.265 | 0.750 |
| | Fraction of industry-years affected | 0.002 | 0.006 | 0.012 | 0.023 |
| | 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 | 0.0101 | 0.0108 | 0.0564 |
| | Average simulated wage cuts | 2.0 | 8.7 | 9.1 | 27.3 |
| | $\#(\widehat{y} > y^B)$ | 4159 | 3947 | 4398 | 4352 |
| | Probability of significance | 0.168 | 0.211 | 0.120 | 0.130 |
| | Fraction of wage cuts prevented | 1 | 0.425 | 0.559 | 0.304 |
| | Fraction of industry-years affected | 0.008 | 0.007 | 0.014 | 0.025 |

Table 3. Thus, these results should be treated more cautiously. It is nevertheless an interesting feature that the fraction of wage cuts prevented 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 the Nordic region and the South, with a more mixed picture in the Anglo and the Core.

In Table 4, we report the results concerning individual countries. As these results are also

*Table 4: Results from 5000 simulations on countries.*

| Country | $S$ | $T$ | $Y$ | $Y/S$ | $\widehat{Y}$ | $\#(\widehat{y} > y^B)$ | $p$ | *FWCP* | *FIYA* |
|---|---|---|---|---|---|---|---|---|---|
| Austria | 408 | 26 | 2 | 0.0049 | 7.3 | 4732 | 0.054 | 0.729 | 0.013 |
| Belgium | 575 | 26 | 31 | 0.0539 | 40.9 | 4672 | 0.066 | 0.243 | 0.017 |
| Canada | 627 | 26 | 57 | 0.0909 | 57.2 | 2410 | 0.518 | 0.004 | 0.000 |
| Denmark | 462 | 24 | 8 | 0.0172 | 13.4 | 4222 | 0.156 | 0.405 | 0.012 |
| Finland | 368 | 23 | 2 | 0.0054 | 5.8 | 4404 | 0.119 | 0.658 | 0.010 |
| France | 556 | 26 | 21 | 0.0378 | 18.0 | 1252 | 0.750 | 0 | 0 |
| Germany | 665 | 26 | 16 | 0.0241 | 16.9 | 2586 | 0.483 | 0.052 | 0.001 |
| Greece | 469 | 26 | 7 | 0.0149 | 7.2 | 2257 | 0.549 | 0.026 | 0.000 |
| Ireland | 463 | 23 | 27 | 0.0583 | 35.2 | 4228 | 0.154 | 0.235 | 0.018 |
| Italy | 312 | 13 | 0 | 0 | 3.1 | 4663 | 0.067 | 1 | 0.010 |
| Luxembourg | 423 | 27 | 32 | 0.0757 | 40.5 | 4282 | 0.154 | 0.235 | 0.018 |
| Netherlands | 483 | 27 | 23 | 0.0476 | 34.9 | 4803 | 0.039 | 0.341 | 0.025 |
| New Zealand | 750 | 27 | 45 | 0.0600 | 54.3 | 4121 | 0.176 | 0.171 | 0.012 |
| Norway | 674 | 27 | 2 | 0.0030 | 4.1 | 3585 | 0.283 | 0.510 | 0.003 |
| Portugal | 411 | 18 | 3 | 0.0073 | 20.4 | 4999 | 0.000 | 0.853 | 0.042 |
| Spain | 270 | 10 | 18 | 0.0667 | 16.4 | 1709 | 0.658 | 0 | 0 |
| Sweden | 472 | 21 | 6 | 0.0127 | 11.4 | 4586 | 0.083 | 0.478 | 0.012 |
| UK | 615 | 26 | 18 | 0.0293 | 21.5 | 3671 | 0.266 | 0.168 | 0.006 |
| US | 506 | 27 | 6 | 0.0119 | 8.4 | 3389 | 0.322 | 0.278 | 0.006 |

Note: $T$ is the number of years. $p$ is the probability of significance. *FWCP* and *FIYA* are set to zero for France and Spain, where we simulate less wage cuts than we observe.

based on fewer observations, and significance levels are lower, the results can only be viewed as indicative. However, DNWR is significant for the Netherlands and Portugal at the five percent level, and Austria, Belgium, Italy, and Sweden at the ten percent level. We observe that for all countries except Canada, France and Spain, the simulations indicate some DNWR, as some notional wage cuts are prevented. It is also noteworthy that the fraction of wage cuts prevented is above 40 percent for all the Nordic countries. A surprising feature is that the South splits in two, with strong DNWR in Portugal and Italy, and no or negligible DNWR in Spain and Greece. The fraction of industry-years affected by DNWR varies from 4.2 percent (Portugal) at the top, to 0 percent (Canada, France and Spain) at the bottom.

To explore the precision of our measures of DNWR, we undertake Poisson regressions with the number of observed wage cuts in each country-year sample, $Y_{it}$, as the dependent variable, normalising on the number of simulated wage cuts, $\widehat{Y}_{it}$, and adding dummies for region, period, combined region and period, as well as for countries. From the confidence intervals for these

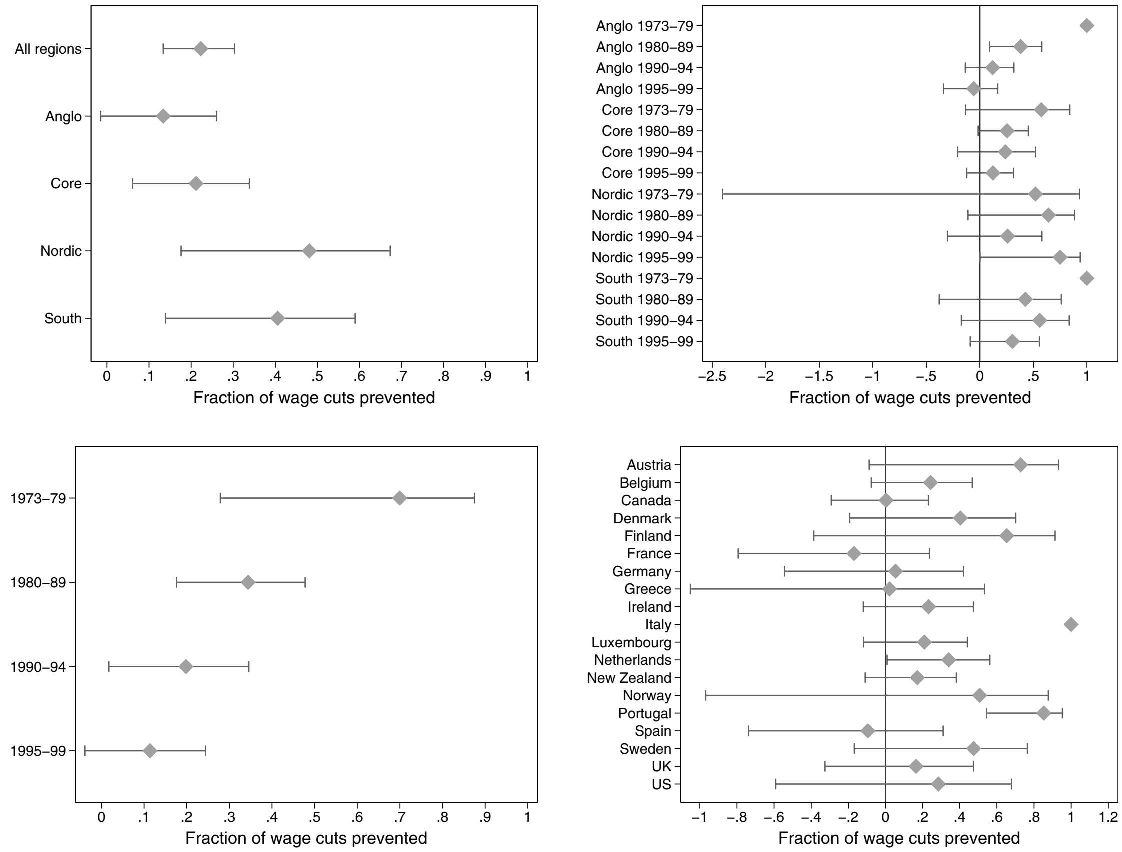


*Figure 5: Estimated fractions of wage cuts prevented with 95% confidence intervals.*

dummies we derive confidence intervals for the fraction of wage cuts prevented for all the respective subsamples, see Figure 5.[7] The confidence intervals are fairly large, and with few exceptions, we are not able to conclude that the fractions are significantly different from one another. The large uncertainty reflects that for many countries, there are few notional wage cuts. This implies that the fraction of wage cuts prevented is very sensitive even to a marginal change in the number of realised wage cuts. Norway is an extreme case, with only 4.1 notional wage cuts; here, two observed wage cuts leads to a fraction of wage cuts prevented of 0.51.

In view of the large uncertainty one should be careful when interpreting the differences between the countries. Nevertheless the estimates may be useful as a benchmark when com-

[7]The Poisson regression yields predicted values for $Y/\widehat{Y}$ from which estimates for $FWCP = 1 - Y/\widehat{Y}$ follow directly. Note also that the point estimates of the fractions in Figure 5 differ slightly from the fractions in the tables, as the former are based on the Poisson regressions, and thus are non-linear, while the latter are linear averages based on the simulations.

paring estimates from micro studies from different countries. Generally, we find less significant evidence for DNWR than previous studies on micro data, but with a rough correspondence when it comes to country differences. For example, Ekberg (2004) documents considerable DNWR in Sweden, while Biscourp et al. (2004) find that wages are flexible downwards in France, both results consistent with our point estimates. Our finding of strong DNWR for Portugal is consistent with the institutional feature that a nominal wage cut for a job stayer is illegal in Portugal. However, for several other countries, specifically the US, Germany and the UK, we detect no significant dnwr, in contrast to recent micro data evidence. For the US, Lebow et al. (2003) document DNWR in the BLS's employment cost index, with the fraction of wage cuts prevented estimated to about one half. Bauer et al. (2003) and Knoppik and Beissinger (2003) find significant DNWR for Germany, the latter estimating the fraction of wage cuts prevented to 70 percent for wage earners, while Elsby (2004) and Nickell and Quintini (2003) document DNWR for the UK.

As discussed in section 3 above, we would expect to find weaker evidence of DNWR than in micro studies, both because our wage data are affected by compositional changes that may be unrelated to DNWR, and because firms may exploit other 'avenues' for flexibility to circumvent rigidity at the individual level. Seen in this light, our evidence of DNWR yields clear additional support to the idea that DNWR does affect firms' wage costs in many European countries, even if the quantitative effect seem moderate.

### 4.1 Robustness

In this section we explore the robustness of our findings. One possible questionable assumption so far is whether the shape of the wage change distribution is the same in all countries and over time. Thus, in the appendix, we also report results based on country-specific and period-specific underlying distributions. More precisely, we construct separate underlying distributions $\Delta w_s^n$

for each country, alternatively for each period, and then proceed with the bootstrap method as before. Because the underlying distributions are based on fewer observations one would expect this method to be more vulnerable to a downward bias by DNWR compressing the underlying and notional distributions. As shown in the appendix, the qualitative results are similar to those reported above; somewhat weaker evidence of DNWR with country-specific distributions, and somewhat stronger evidence with period-specific distributions. However, it is worth noting that with country-specific underlying distributions, the point estimates suggest that there is some DNWR in all countries except the US (but again, there is large uncertainty).

A more fundamental question is to what extent our findings have anything to do with DNWR at all, or whether they just reflect other specific distributional aspects. We address this question in three different ways. First, we 'contaminate' our data by adding additional DNWR for a selected number of countries, and explore how this affects our findings. More precisely, 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 problems, 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 44 percent, as compared to the original results, see Table 5. Taken at face value, these results suggest that our method on average is able to detect 92 percent of the total DNWR in the data (calculated as the computed reduction of 44 percent as compared to the constructed reduction of 48 percent, where $44/48 = 0.92$). The vari-

*Table 5: The effect from adding* DNWR *on the fraction on realised wage cuts.*

| Countries without additional DNWR | | | Countries with additional DNWR | | |
|---|---|---|---|---|---|
| | $\Delta Y$ | ΔFWCR | | $\Delta Y$ | ΔFWCR |
| Austria | 0.000 | 0.004 | Belgium | –0.484 | –0.376 |
| Italy | 0.000 | 0.000 | Canada | –0.491 | –0.466 |
| Luxembourg | 0.000 | 0.011 | Denmark | –0.500 | –0.492 |
| Netherlands | 0.000 | 0.009 | Finland | –0.500 | –0.474 |
| New Zealand | 0.000 | 0.005 | France | –0.476 | –0.424 |
| Norway | 0.000 | 0.033 | Germany | –0.500 | –0.484 |
| Spain | 0.000 | 0.012 | Greece | –0.429 | –0.415 |
| Sweden | 0.000 | 0.029 | Ireland | –0.481 | –0.475 |
| UK | 0.000 | 0.001 | Portugal | –0.333 | –0.327 |
| | | | US | –0.500 | –0.483 |

*Notes:*$\Delta Y$ is the relative change in the number of nominal wage cuts. ΔFWCR is the difference in the fraction of wage cuts realised.

ation among the ten countries is fairly small, varying from a minimum of $37.6/48.4 = 78$ percent for Belgium to a maximum of $47.5/48.1 = 99$ percent for Ireland. For the other countries, the fraction of wage cuts realised is hardly affected (on average, it increases by one percent, with a maximum of three percent for Norway). The fact that we detect less than 100 percent of the additional DNWR is consistent with the downward bias in the estimated DNWR due to DNWR affecting the notional distribution, as discussed in section 3.1 above.

Secondly, we explore whether our findings can be caused by downward *real* wage rigidity (DRWR), that workers for various reasons resist a reduction in their real wages. Bauer et al. (2003) and Barwell and Schweitzer (2004) find evidence for DRWR in Germany and the UK, respectively. Furthermore, Bauer et al. (2003) point out 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.

The quantitative effect of DRWR on our method is not clear. While DRWR clearly will reduce the number of nominal wage cuts when inflation is low, it will also affect the shape of the underlying notional distribution. To explore the quantitative impact, we add DRWR to our data set by randomly eliminating 20 percent of all observations of real wage cuts (i.e. 618 observations) 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 potentially strong impact on any findings of DNWR. However, 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 fraction of wage cuts prevented increases by only six percentage points (from 22 to 28 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 rather limited impact on our results.

Thirdly, we explore whether our results are caused by compositional changes arising from a difference between the wages of new and former workers. Such compositional changes will constitute an additional random component, which may be positive or negative. As a crude illustration of the effect, we add a normally distributed term to our wage data, with zero mean and standard deviation one percent (arbitrarily chosen, but it suffices for illustration). As expected, applying our analysis on these data leads to both more observed and more simulated wage cuts, reducing the overall fraction of wage cuts prevented from 0.22 in the original data to 0.19 with the contaminated data. We conclude that compositional changes cannot explain our findings of DNWR; rather, it is likely to weaken our findings.

## 5 Explaining the number of wage cuts

While the previous analysis documents the existence of DNWR, it does not investigate explicitly whether the incidence of nominal wage cuts depends on economic and institutional variables. As mentioned above, Holden (2004) shows that DNWR is likely to depend on inflation in a non-linear way, as well as on institutional variables like EPL and union density or bargaining coverage. Furthermore, high unemployment may also weaken workers' resistance to nominal wage cuts. Thus, we apply a Poisson regression model of the number of wage cuts in each country-year sample, $Y_{it}$, as the dependent variable (i.e. 449 observations) and with a number of explanatory variables including inflation and inflation squared, an index of EPL, union density, the unemployment rate. We do the analysis in two different ways. First, we normalise on the number of industries in the country-year sample, $S_{it}$, i.e. we explain the incidence of wage cuts. Second, we normalise on the average number of simulated wage cuts, $\widehat{Y}_{it}$, i.e. we explain the fraction of simulated wage cuts that are actually realised. Adding institutional variables as regressors, we can then test directly whether these variables lead to fewer observed than notional wage cuts, i.e. to DNWR.

The conditional density in a Poisson model is

$$f(Y_{it} = y_{it} \mid \mathbf{x}_{it}) = \frac{e^{-\lambda_{it}} \lambda_{it}^{y_{it}}}{y_{it}!} \tag{4}$$

and

$$\ln \lambda_{it} = \mathbf{x}_{it}' \boldsymbol{\beta} \tag{5}$$

where $E(Y_{it} \mid \mathbf{x}_{it}) = \lambda_{it}$, $\mathbf{x}_{it}$ represents the explanatory variables and $\boldsymbol{\beta}$ is the parameter vector. In the Poisson model the variance is equal to the mean. However, data are often characterised by 'overdispersion' and hence at odds with the Poisson assumption. Undertaking the Poisson regression of $Y_{it}/S_{it}$, a goodness-of fit test formally rejects the hypothesis that the data are generated according to the Poisson regression model ($\chi^2(416) = 634.6$). We therefore use a negative binomial regression model, which allows for overdispersion and can be seen as a generalisation of the Poisson model. Specifically, we use two alternative specifications for the Poisson parameter:

$$\ln \lambda_{it} = \mathbf{x}_{it}' \boldsymbol{\beta} + \varepsilon_{it}, \quad \varepsilon_{it} \sim \Gamma(1, \delta) \tag{5'}$$

$$\ln \lambda_{it} = \mathbf{x}_{it}' \boldsymbol{\beta} + \varepsilon_{it}, \quad \varepsilon_{it} \sim \Gamma(1, \phi_i e^{-\alpha_i}) \tag{5''}$$

Including a Gamma distributed error term, $\varepsilon_{it}$, in (5') and (5") allows the variance to mean ratios

*Table 6: Maximum likelihood estimates with standard errors in parenthesis from negative binomial regressions in columns one and two and from Poisson regressions in columns three and four.*

| | Incidence of wage cuts | | Fraction of wage cuts realised | |
|---|---|---|---|---|
| | Pooled | Fixed effects | Pooled | Fixed effects |
| Ln($S_{it}$) | 1 (–) | 1 (–) | – | – |
| Ln(Simulated cuts) | – | – | 1 (–) | 1 (–) |
| EPL | −0.310* (0.104) | −0.785* (0.200) | −0.126* (0.058) | −0.355 (0.288) |
| Union density | −0.803 (0.598) | −1.992* (0.980) | −0.890* (0.371) | −1.790 (1.388) |
| Inflation | −0.484* (0.073) | −0.345* (0.062) | −0.088 (0.047) | −0.044 (0.061) |
| Inflation squared | 0.016* (0.003) | 0.011* (0.003) | 0.003 (0.002) | −0.002 (0.003) |
| Unemployment | 0.116* (0.029) | 0.092* (0.036) | 0.032* (0.015) | 0.007 (0.034) |
| constant | 1.092* (0.463) | 1.855* (0.762) | 0.208 (0.242) | – |
| log-likelihood | –364.6 | –288.5 | –261.4 | –215.0 |
| Number of observations | 422 | 409 | 422 | 409 |

*Notes:* (i) $S_{it}$ is the number of industries in country-year sample $it$. (ii) * indicates significance at 5% level. (iii) Luxembourg is not included 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.

of $Y_{it}$ to be larger than unity. (4) and (5') together yield the pooled negative binomial regression model. In (5"), we also include a country specific fixed effect, $\alpha_i$, to allow for a country specific variance to mean ratio, see Hausman et al. (1984) for details.

The results of the negative binomial model (where we explain the incidence of wage cuts) are presented in the first two columns of Table 6. In accordance with the theoretical predictions, EPL, union density and inflation, all have a significant negative effect on the incidence of nominal wage cuts, although union density is not significant in the pooled specification. High unemployment increases the incidence of wage cuts.

The quantitative impact of the institutional variables is fairly large, even if the effects differ according to the method applied. Using the 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, would increase the incidence of nominal wage cuts by a factor of $exp(-0.785(-1.5)) = 3.2$. This would raise the incidence of wage cuts in Portugal from 0.7 percent to 2.3 percent. Correspondingly, the incidence of wage cuts in Sweden would increase from 1.3 percent to 4.6 percent if EPL were reduced by 1.6 units to the UK level. A reduction in union density from 75 percent (as in Denmark and Finland) to 25 percent (as in Germany and the Netherlands) is associated with an incidence rate which is 2.7 times higher ($exp(-1.992(-0.5))$). For Denmark this implies an increase in the incidence rate from 1.7 to 4.6 percent. A reduction in union density of 20 percentage points, as experienced in the UK from the late 1970s to the late 1990s, implies an increase in the incidence rate by a factor of 1.5.

We then investigate whether institutions affect the extent of DNWR as measured by the average fraction of wage cuts realised ($Y/\widehat{Y}$), by a Poisson regression of $Y_{it}$ normalised on the number of simulated wage cuts $\widehat{Y}_{it}$. The results are presented in columns 3 (pooled) and 4 (fixed effects) of Table 6. Note that in this case the restriction imposed by the Poisson regression relative to the negative binomial regression is accepted easily; indeed the results are the same in the negative binomial model for both specifications.[8] Again, we find a significant negative effect of EPL, union density and unemployment on the number of wage cuts, implying a positive effect on the fraction of wage cuts prevented.

Using the estimates from the pooled model, a reduction in the EPL index by 1.5 units would raise the fraction of wage cuts realised by a factor of 1.2 ($= exp(-0.126(-1.5))$). In the case of Sweden, this would imply an increase in the fraction of wage cuts realised from 52.8 to 63.8 percent, i.e. reducing the fraction of wage cuts prevented from 47.2 to 36.2 percent. Similarly, 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 ($= exp(-0.890(-0.5))$); for Finland, the fraction of wage cuts realised would increase from 33.8 to 52.7 percent.

We have also included other institutional variables: bargaining coverage, temporary employment, and indices of centralisation and coordination. Centralisation turned out to have a negative sign in two of the four regressions in Table 6, and significant at the ten percent level in the pooled negative binomial regression. The other variables had no effect.[9] Adding a time trend in the

[8] The goodness-of-fit test yields $\chi^2(334) = 179.8$.

[9] Regrettably, the data for institutional variables apply to the whole economy, and not to the industry sector. As variation in for example density or coverage in other parts of the economy would affect the density and coverage

regressions in Table 6 gave positive significant coefficients in the models for the incidence of wage cuts, but not in the models for the fraction of wage 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 is an increase 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 the fraction of wage cuts prevented) 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.

## 6 Conclusions

This paper explores the existence of downward nominal wage rigidity (DNWR) in the manufacturing, mining and quarrying, electricity, gas and water supply, and construction sectors of 19 OECD countries, over the period 1973–1999, using data for hourly nominal wages at industry level. Based on a novel nonparametric statistical method, which allows for country and year specific variation in both the median and the dispersion of industry wage changes, we reject the hypothesis of no DNWR for the total sample. Splitting into subsamples, we document the existence of DNWR for the high inflation period 1973–1989, as well as for the low inflation periods 1990–1994 and 1995–1999. Furthermore, we also find evidence for DNWR for groups of countries: the South (Italy, Greece, Portugal, Spain), the Core (Austria, Belgium, France, Germany, Luxembourg, Netherlands), the Nordic region (Denmark, Finland, Norway and Sweden). For the group of native English speaking countries, Anglo (Canada, Ireland, New Zealand, the UK and the US), we find less DNWR, but nevertheless significant at the ten percent level. Dividing further into individual countries, DNWR is statistically significant only for some of the countries: for the

variable, but presumably not affect wage setting in the industry sector, the estimates of these variables might be biased downwards.

Netherlands and Portugal at the five percent level, and Austria, Belgium, Italy and Sweden at the ten percent level. The point estimates indicate some DNWR also for the other countries, with the exception of Canada, France and Spain, but these results are not statistically significant.

Interestingly, our results show that overall, the fraction of notional wage cuts that do not result in observed wage cuts has fallen over time. The simulations indicate that for all countries together, the fraction of wage cuts prevented by DNWR has fallen from 70 percent in the 1970s to 11 percent in the late 1990s. The Nordic countries appear to be an exception; for this group, the fraction of wage cuts prevented is highest in the late 1990s. On the other hand, as inflation has fallen over time, the fraction of industry-years affected by DNWR has increased from less than 0.5 percent in the 1970s, to 1.2 percent in the late 1990s.

To explore the robustness of our findings, we perform our method with various types of 'contaminated' data. First, we add additional DNWR for ten of the countries, by randomly eliminating 50 percent of the observed wage cuts for these countries by setting the wage growth to zero. Performing our method on the contaminated data, we are able to detect 92 percent of the added DNWR, varying from 78 to 99 percent for the individual countries. The results for the other countries for which we have not added DNWR are hardly affected. This indicates that our method does a very good job in detecting the DNWR that exists in the data. Secondly, we add a considerable amount of downward real wage rigidity, DRWR, by eliminating 20 percent of all real wage cuts by setting the nominal wage increase equal to the rate of inflation. This reduces the number of nominal wage cuts by 18 percent, yet it has a rather limited effect on our results, as the fraction of wage cuts prevented only increases by six percentage points, from 22 to 28 percent. In view of the fact that about 30 percent of all our observations are real wage cuts, it seems hard to imagine stronger real wage rigidity than 20 percent. Thus, we conclude that DRWR, if it exists, can only explain a minor part of our findings.

We then proceed to explore whether the extent of DNWR can be explained by economic and institutional variables. As predicted by the theoretical framework of Holden (2004), we find that both strictness of employment protection legislation and union density lead to stronger DNWR: in country-year samples with strict employment protection legislation and high union density, the number of observed wage cuts is significantly reduced both relative to the number of simulated, notional wage cuts and relative to the number of observations. High inflation also leads to a lower incidence of wage cuts. The effect of the institutional variables is fairly strong. For example, weakening the employment protection legislation from a strict to a medium level, would, according to the point estimates, raise the incidence of nominal wage cuts in Portugal from 0.7 to 2.3 percent. A similar change in the employment protection legislation in Sweden, from its current medium level down to the less strict level of the UK, would imply an increase in the fraction of wage cuts realised from 52.8 to 63.8 percent, i.e. reducing the fraction of wage cuts prevented from 47.2 to 36.2 percent. The evolution of the economic and institutional variables can explain the reduction in DNWR over time.

Our study should be seen as complementary to the increasing number of empirical studies on the existence of DNWR based in individual data. In general, we find weaker evidence of DNWR than several recent microstudies. This is consistent with the finding of Wilson (1999) who detect notably less downward wage rigidity for job averages than for individuals. It is also consistent with Card and Hyslop (1997), who find evidence of DNWR on US microdata, but inconclusive evidence for state level data. It is difficult to know whether the weaker evidence only reflects that we detect less of the rigidity that prevails at individual level, or whether it also reflects that DNWR at individual or firm level is circumvented by employment being shifted over from high-wage to low-wage jobs. In either case, our finding of DNWR yields clear additional support to the idea that DNWR does affect firms' wage costs in many OECD countries, especially in Europe. However, the quantitative effect seem moderate.

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# A Data appendix

We have obtained wage data from Eurostat for all countries except Austria, Finland, Norway and Sweden (see below). The precise source is Table HMWHOUR in the *Harmonized earnings* domain of under the *Population and Social Conditions* theme in the NEWCRONOS database. Our wage variable (HMWHOUR) is labelled *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, Finland and Sweden are from Table 5B 'Wages in manufacturing' in LABORSTA, the Labour Statistics Database, ILO. The data are recorded by ISIC, 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 A2, 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 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 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 the 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 | 0/24 | 0/28 | – | – | 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 | 0/25 | 0/28 | – | – | 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 | 0/25 | 0/28 | – | – | 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 | 0/25 | 0/28 | – | – | 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 | 0/25 | 0/28 | – | – | 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 | 0/25 | 0/28 | – | 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 | 0/25 | 0/28 | – | 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 | 0/25 | 1/28 | – | 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 | 0/25 | 0/28 | 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 | 0/25 | 0/28 | 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 | 0/25 | 1/28 | 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 | 0/25 | 1/28 | 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 | 0/25 | 1/28 | 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 | 0/25 | 0/28 | 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 | 0/25 | 1/28 | 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 | 0/25 | 0/28 | 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 | 0/25 | 2/28 | 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 | 1/25 | 5/28 | 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 | 0/25 | 1/28 | 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 | 0/25 | 3/28 | 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 | 0/25 | 9/28 | 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 | 0/25 | 7/28 | 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 | 1/25 | 2/28 | 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 | 0/25 | 1/28 | 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 | 0/25 | 1/28 | 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 | 0/25 | 3/28 | 0/29 | 0/14 | 1/28 | 2/18 | 26/392 |
| 1999 | 0/14 | – | 12/20 | – | 1/16 | 2/30 | – | – | – | – | – | 1/17 | 12/22 | 0/25 | 6/22 | – | 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 | 2/674 | 45/750 | 3/411 | 6/472 | 18/615 | 6/506 | 324/9509 |

*Table A2: 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 A3: 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 A4: 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 A5: 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 |

# B Results with country and period specific underlying distributions

*Table B1: Results with country and period specific underlying distributions. Country specific distributions are based on observations from the high inflation years 1973–92. The period specific underlying distributions are based on observations from the periods 1973–79, 1980–89, 1990–94 and 1995–1999 respectively. Otherwise the method is as in the main text.*

| | Country specific underlying distributions | | | | | | Period specific underlying distributions | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Category | $Y$ | $\widehat{Y}$ | $\#(\widehat{y} > y^B)$ | $p$ | *FWCP* | *FIYA* | $Y$ | $\widehat{Y}$ | $\#(\widehat{y} > y^B)$ | $p$ | *FWCP* | *FIYA* |
| All | 324 | 409 | 4996 | 0.001 | 0.208 | 0.009 | 324 | 425 | 5000 | 0.000 | 0.238 | 0.011 |
| 1970–79 | 5 | 16 | 4918 | 0.016 | 0.683 | 0.005 | 5 | 16 | 4952 | 0.010 | 0.692 | 0.005 |
| 1980–89 | 74 | 109 | 4960 | 0.008 | 0.325 | 0.010 | 74 | 109 | 4977 | 0.005 | 0.326 | 0.010 |
| 1990–94 | 93 | 115 | 4665 | 0.067 | 0.192 | 0.012 | 93 | 121 | 4913 | 0.017 | 0.232 | 0.015 |
| 1995–99 | 152 | 169 | 4125 | 0.175 | 0.097 | 0.010 | 152 | 178 | 4779 | 0.044 | 0.146 | 0.016 |
| British Isles | 153 | 167 | 3917 | 0.217 | 0.084 | 0.005 | 153 | 179 | 4707 | 0.059 | 0.146 | 0.009 |
| Core | 125 | 155 | 4806 | 0.039 | 0.193 | 0.010 | 125 | 160 | 4961 | 0.008 | 0.220 | 0.011 |
| Nordic | 18 | 32 | 4828 | 0.034 | 0.439 | 0.007 | 18 | 36 | 4961 | 0.008 | 0.502 | 0.009 |
| South | 28 | 55 | 4986 | 0.003 | 0.491 | 0.018 | 28 | 50 | 4958 | 0.008 | 0.434 | 0.015 |
| Austria | 2 | 5 | 3861 | 0.228 | 0.571 | 0.006 | 2 | 8 | 4765 | 0.047 | 0.739 | 0.014 |
| Belgium | 31 | 37 | 3871 | 0.226 | 0.162 | 0.010 | 31 | 41 | 4679 | 0.064 | 0.247 | 0.018 |
| Canada | 57 | 57 | 2433 | 0.513 | 0.007 | 0.001 | 57 | 58 | 2543 | 0.491 | 0.013 | 0.001 |
| Germany | 16 | 18 | 2973 | 0.405 | 0.121 | 0.003 | 16 | 18 | 2827 | 0.435 | 0.085 | 0.002 |
| Denmark | 8 | 12 | 3837 | 0.233 | 0.342 | 0.009 | 8 | 14 | 4267 | 0.147 | 0.414 | 0.012 |
| Finland | 2 | 3 | 2573 | 0.485 | 0.278 | 0.002 | 2 | 6 | 4466 | 0.107 | 0.673 | 0.011 |
| France | 21 | 22 | 2539 | 0.492 | 0.032 | 0.001 | 21 | 18 | 1361 | 0.728 | –0.148 | –0.005 |
| Greece | 7 | 12 | 4170 | 0.166 | 0.426 | 0.011 | 7 | 8 | 2537 | 0.493 | 0.094 | 0.002 |
| Ireland | 27 | 33 | 3707 | 0.259 | 0.188 | 0.013 | 27 | 36 | 4310 | 0.138 | 0.251 | 0.020 |
| Italy | 0 | 3 | 4711 | 0.058 | 1.000 | 0.011 | 0 | 3 | 4601 | 0.080 | 1.000 | 0.009 |
| Luxembourg | 32 | 39 | 3763 | 0.247 | 0.174 | 0.016 | 32 | 41 | 4324 | 0.135 | 0.215 | 0.021 |
| Netherlands | 23 | 35 | 4607 | 0.079 | 0.334 | 0.024 | 23 | 35 | 4799 | 0.040 | 0.341 | 0.025 |
| New Zealand | 45 | 51 | 3699 | 0.260 | 0.123 | 0.008 | 45 | 55 | 4172 | 0.166 | 0.176 | 0.013 |
| Norway | 2 | 6 | 4252 | 0.150 | 0.643 | 0.005 | 2 | 5 | 3901 | 0.220 | 0.570 | 0.004 |
| Portugal | 3 | 21 | 4996 | 0.001 | 0.854 | 0.043 | 3 | 21 | 5000 | 0.000 | 0.859 | 0.044 |
| Spain | 18 | 19 | 2548 | 0.490 | 0.042 | 0.003 | 18 | 18 | 2072 | 0.586 | –0.027 | –0.002 |
| Sweden | 6 | 12 | 4453 | 0.109 | 0.481 | 0.012 | 6 | 12 | 4626 | 0.075 | 0.490 | 0.012 |
| UK | 18 | 21 | 3214 | 0.357 | 0.135 | 0.005 | 18 | 22 | 3770 | 0.246 | 0.186 | 0.007 |
| US | 6 | 4 | 1231 | 0.754 | -0.387 | -0.003 | 6 | 9 | 3510 | 0.298 | 0.304 | 0.005 |

*Notes:* see Table 1