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Downward Nominal Wage Rigidity in the OECD

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Abstract

Recent micro studies have documented extensive downward nominal wage rigidity (DNWR) for job stayers in many OECD countries, but the effect on aggregate variables remains disputed. This paper explores the existence of DNWR on wages at industry level in 19 OECD countries, over the period 1973–1999, using data for hourly nominal wages. Based on a novel nonparametric statistical method, we reject the hypothesis of no DNWR. 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, 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

1 Introduction

Recent micro studies have found considerable downward nominal wage rigidity (DNWR) for job stayers in OECD countries. The International Wage Flexibility Project (Dickens et al., 2005) find that for all the 16 countries that are studied, DNWR prevents wage cuts from taking

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place, with the fraction of wage cuts prevented being in the range 9–66 percent. These results complement and extend previous studies by Lebow et al. (2003), Dessy (2002), and Knoppik and Beissinger (2005). The extensive DNWR found in micro studies, combined with monetary policy aiming for low rates of inflation, make Tobin's contention that this combination leads to greater wage pressure and higher unemployment (Tobin, 1972) again of great policy relevance.

However, when it comes to identifying the aggregate effects of DNWR, the results are more disputed. Fehr and Gotte (2005) show that DNWR is associated with higher unemployment among Swiss cantons. Moreover, several papers have found empirical support for Tobin's contention that low inflation may lead to higher unemployment (see e.g. Akerlof et al., 1996, Karanassou et al., 2003 and Dickens et al., 2005), yet other economists are skeptical towards the reliability of these findings (e.g. Gordon, 1996, Camba-Mendez et al., 2003, Mankiw, 1996 and Svensson, 2001). 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).

One possible explanation for the disputable aggregate evidence 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. Fares and Lemieux (2000) point out that DNWR for 'stable' workers may not prevent employers from hiring new workers at lower nominal wages than they would have done in other circumstances. Indeed, studying the wage adjustments of different groups of Canadian employees, Fares and Lemieux conclude that the bulk of the real-wage adjustment over the business cycle is experienced by new entrants, for whom DNWR is least likely to bind. Furthermore, they argue that this may explain why DNWR has little effect on aggregate wage setting, despite it being important for some groups of workers. Fares and Lemieux' results are consistent with the findings of Card and Hyslop (1997), who on US data find evidence of DNWR for individual workers, but no corresponding evidence on state data.

To explore the effect of DNWR on wages at more aggregate levels, we study industry level wage data for 19 OECD countries, for the period 1973–99. Based on the idea of previous studies, we construct the notional or counterfactual 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, i.e. when DNWR is not likely to bind. However, in contrast to previous studies, we compare the notional and empirical wage change distributions by use of a simulation method. In section 4, we document the empirical results on DNWR and discuss the robustness of our method. Our robustness tests indicate that the method is able to detect more than 90 percent of the DNWR that exists in the data.

In addition to investigating the extent of DNWR in aggregate data, we explore in Section 5 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, and union density, which are often difficult to evaluate in studies from a single country. We find that DNWR is more prevalent under high union density and strict employment protection legislation. 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. Section 6 concludes the paper.

2 DNWR and industry wages

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), among others, report evidence of DNWR based on interviews and surveys of employees and employers. However, the great majority of studies explores large micro-data sets, based on personnel files, survey data or administrative data, and 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; Kahn (1997), Christofides and Leung (2003), Lebow et al. (2003), Nickell and Quintini (2003) and Elsby (2004) are some of the 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). More recently, multi-country studies by Dessy (2002), Knoppik and Beissinger (2005) and Dickens et al. (2005) have strengthened the evidence of extensive DNWR in most or all OECD countries.

The clear evidence of DNWR for individual workers is, as mentioned above, not reflected in a similar consensus on the macro economic effects of DNWR. While a few papers find evidence of DNWR on aggregate time-series data, see e.g. Holden (1998), Fortin and Dumont (2000) and Wyplosz (2001), these effects are disputed. This motivates a closer look at the link from individual DNWR to aggregate effects. We focus on one part of this link, whether DNWR is apparent in wages at industry level.

Micro studies typically explore the change in hourly earnings of job stayers, while the observational unit in our data is the change in average hourly earnings for all manual workers in the industry. Numerically, the difference between these data types can be grouped in two. First, our data entails averaging over all job stayers, and, second, they are affected by compositional changes, i.e. that the wages of new workers differ from the wages of those who leave. Concerning the first component, averaging over job stayers may mask wage cuts for single workers if other workers receive wage increases. This 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.

As for compositional changes, one may expect to find both systematic and random effects. First, there will be a systematic negative effect on wage growth, as older workers who leave the labour force on average have higher wages than younger, newcomers to the labour market. Second, one may expect systematic cyclical effects, as the share of low-skilled workers may increase in expansions, cf. Solon et al. (1994) and Fares and Lemieux (2000). This latter compositional effect is likely to dampen fluctuations in wage growth, reducing the number of wage cuts. This is because in recessions, when wage growth for job stayers is likely to be low, the increased share of high-skilled workers will imply a positive compositional effect. As these two effects will have opposite impact on the number of wage cuts, the overall effect is ambiguous.

The random element arising from unsystematic turnover may be considered as 'noise' relative to individual wage rigidity. The noise effect will imply that we find more wage cuts, and thus less rigidity, as also indicated by our robustness checks below.

The effects discussed above need not be caused by DNWR, even if they may affect our estimate of DNWR. However, it is important to take into account the possibility that DNWR for some workers have implications for the wages of other workers in the industry. One such effect would be if firms respond to downward rigidity at the individual level by e.g. 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 wage cut may also quit voluntarily, and new workers could take their job at the lower pay. Another possibility would be that binding wage rigidity in some firms leads to stronger wage reductions in other firms, which may offset the effects on industry employment.

To illustrate this latter point, consider the following stylised model, of an industry consisting of a continuum of symmetric firms, with measure one. Initially, the wage is the same in all firms. There are two relations. First, there is a wage setting relation, where the real wage in firm i, ω_i, is a function of unemployment among workers in the industry, u, average real wage in the industry, ω, and industry productivity α;

$$\omega_i = W(u, \omega, \alpha). \tag{1}$$

The partial derivatives satisfy $W_1 < 0$, $W_2, W_3 > 0$. Equation (1) may be derived in an efficiency wage model or in a bargaining framework, where the real wage in general depends on outside opportunities, including the relevant unemployment rate and the relevant outside wage, as well as on the productivity of the firm, see e.g. Layard et al. (1991) and Blanchflower and

Oswald (1995). The positive effect of an increase in outside wages may reflect that a higher wage is needed to recruit workers, that workers' reservation wage increases, or possibly also pure 'envy' effects. The second relationship is the labour market equilibrium condition, where unemployment is given as the difference between the exogenous labour supply $\bar{l}$, and labour demand, which is a function of the real wage and productivity, $l(\omega, \alpha)$,

$$u = \bar{l} - l(\omega, \alpha). \tag{2}$$

The partial derivatives satisfy $l_1 < 0,\ l_2 > 0$. Exogenous labour supply to the industry is adopted to simplify the exposition, but the qualitative results would hold also under weaker assumptions. What we want to capture is that the existence of industry-specific skills and various frictions will imply that a reduction in industry employment will lead to higher unemployment among workers previously employed in the industry.

Assume now that a negative productivity shock takes place, pushing nominal wages down, but DNWR prevents this from happening in some firms. More specifically, assume that in a fraction $(1-\gamma)$ of all firms, DNWR prevents the nominal wage from falling, which implies that the real wage will be higher than it otherwise would have been. We consider the effect on unemployment, wages in the flexible part of the industry, ω_F, and the industry wage, ω, of higher real wages due to binding wage rigidity in some firms, ω_R. Total differentiation of (1) and (2) gives us

$$d\omega_F = W_1 du + W_2\left(\gamma d\omega_F + (1-\gamma) d\omega_R\right) \tag{3}$$

$$du = -l_1 \gamma d\omega_F - l_1(1-\gamma) d\omega_R \tag{4}$$

which yields

$$\frac{d\omega_F}{d\omega_R} = \frac{(-W_1 l_1 + W_2)(1-\gamma)}{1 + W_1 l_1 \gamma - W_2 \gamma} \gtrless 0 \tag{5}$$

$$\frac{du}{d\omega_R} = -l_1\left(\gamma \frac{(-W_1 l_1 + W_2)(1-\gamma)}{1 + W_1 l_1 \gamma - W_2 \gamma} + (1-\gamma)\right) > 0 \tag{6}$$

$$\frac{d\omega}{d\omega_R} = \gamma \frac{d\omega_F}{d\omega_R} + (1-\gamma) = \gamma \frac{(-W_1 l_1 + W_2)(1-\gamma)}{1 + W_1 l_1 \gamma - W_2 \gamma} + (1-\gamma) > 0 \tag{7}$$

Wage rigidity in some firms unambiguously leads to higher industry wages and higher unemployment. However, the effect on the wage in firms with flexible wage setting is ambiguous. Higher outside wages has a positive direct effect on wages in the flexible firms, but the indirect effect via higher unemployment is negative, so the overall effect is uncertain. If the remainder of the industry labour market is competitive (corresponding to the limit case where the partial derivative of the wage with respect to unemployment converges to minus infinity), the effect of wage rigidity in some firms is fully absorbed by wage flexibility in other firms as $\frac{d\omega_F}{d\omega_R}$ converges to $\frac{d\omega_F}{d\omega_R} = -\frac{1-\gamma}{\gamma} < 0$, implying that $d\omega$ and du converge to zero. In contrast, if unemployment has little impact on the wage setting, i.e. W_1 small numerically, then wage rigidity in some firms will lead to higher wages also in the firms with flexible wages, amplifying both the positive effect on industry wages and the effect on the rate of unemployment.

The lesson from this exercise is that the effect on unemployment of DNWR in a part of the industry depends crucially on the extent to which wages in other parts of the industry respond to the increase in unemployment. If DNWR pushes up average wages in the industry, unemployment will also increase. In contrast, if wages fall in the remainder of the industry, the impact on unemployment will be dampened, and possibly offset completely. It therefore seems valuable to complement previous studies on micro data by investigating effects of DNWR on industry level data.[1]

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 harmonised hourly earnings from Eurostat and wages in manufacturing from ILO.[2] One observation is thus denoted Δw_{jit} where j is index for industry, i is index for country and t is index for year. There are all 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.

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

[1] Matched employer-employee data would clearly be of great additional value, shedding new light on some of the problems associated with individual data in this setting, but it would not capture the overall effects on industry wages of jobs being shifted among firms.

[2] 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.

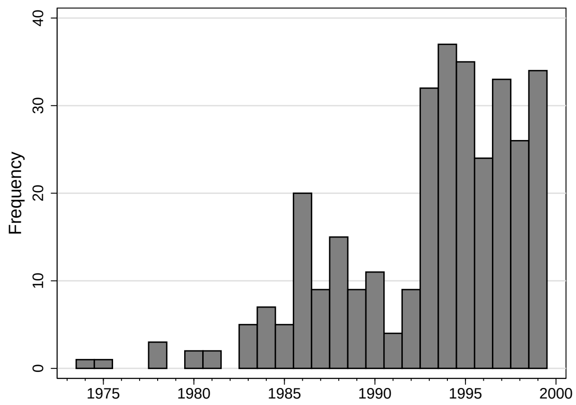

Figure 1: The number of wage cuts over time.

distribution of wage cuts and observations across countries and years.

We use a novel variant of the skewness-location approach of McLaughlin (1994), where DNWR in the empirical distribution of wage changes is detected by a comparison with a postulated notional (i.e. flexible) distribution of wage changes. The critical issue is the validity of the assumptions that are made when constructing the notional distribution, see e.g. discussion in Knoppik and Beissinger (2003) or Nickell and Quintini (2003). The notional distribution is usually based on the empirical wage distribution in high inflation years, when DNWR is less likely to be binding. 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 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. The Nickell and Quintini (2003) method is based on the assumption (or 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 illustrated for Portugal in Figure 2, the wage change distribution is asymmetric in our data, and the dispersion changes over time, as does inflation. As we shall see below (Figure 3), the density function is also non-linear. Thus, all these methods involve problematic assumptions in our case.

We construct country-year specific notional wage change distributions by allowing the locus (median) and dispersion to vary between country-year samples, but assuming the same *shape* for the notional distributions in all country-years. The shape of the notional distributions is then constructed on the basis of observations from country-year samples with high median nominal and real wage growth, to ensure that the shape is not affected by downward nominal or real wage rigidity.

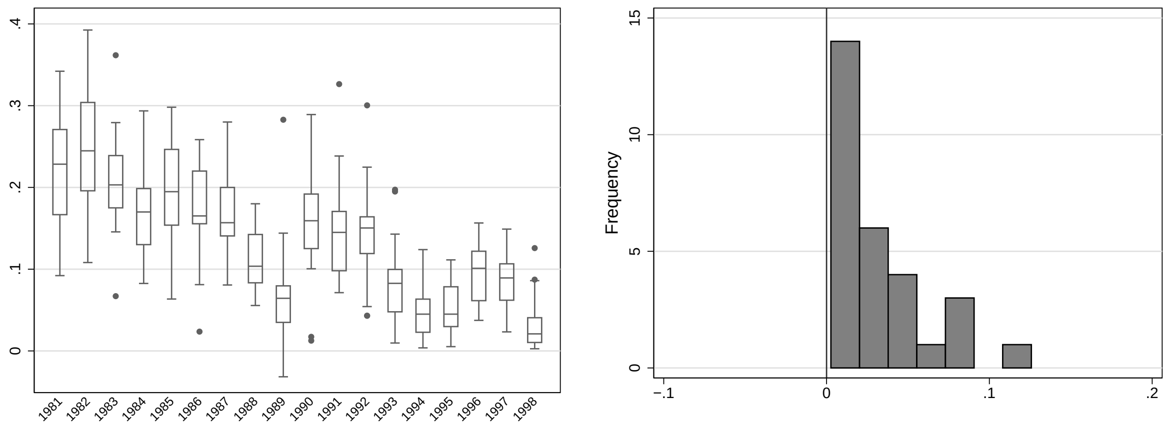

Figure 2: Box plots of annual wage growth in Portugal (left) and histogram of annual wage growth in 1998 (right). The box plot illustrates the distribution of wage changes within a country-year. The box extends from the 25th to the 75th percentile with the median inside the box. The whiskers emerging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

Assuming the same shape may seem overly restrictive, in view of the large differences in wage setting and industry structure among countries, and the large changes over time. However, some common assumptions across time or countries is a feature that is hard to avoid, and it is implicitly or explicitly shared by the alternative methods discussed above. For example, assuming a specific parametric distribution would also involve an assumption of a constant shape across time and countries. Furthermore, assuming that the notional distribution was normal, as is common in regression based studies, would not be a good approximation to the empirical distribution, see Figure 3. By allowing for country-year specific variation in the median and the dispersion, in line with Nickell and Quintini (2003), our approach is in fact less restrictive by other approaches often used in the literature. However, in section 4.1 below, we report results from a number of alternative assumptions, as a check of the robustness of our results.

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 would 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.

3.1 The formal test

To minimise the effect of downward wage rigidity on the notional distributions, we construct the underlying notional distribution based on the 1331 observations from the country-year samples where both the median nominal and the median real wage growth are among their respective upper quartiles.[3] As a further precaution to ensure robustness to DNWR and outliers, we follow Nickell and Quintini (2003) and measure the location by the median, and the dispersion by the range between the 75th and the 35th percentiles, rather than the mean and the standard deviation (we have also tried other measures with similar results). Using the 35th percentile as the lower range reduces the risk that it is affected by DNWR. Under these assumptions, we construct an underlying distribution of wage changes where the empirical wage changes are normalised with respect to the country-year specific median (μ_{it}) and inter percentile range ($P75_{it} - P35_{it}$), i.e.

$$\Delta w_s^n \equiv \left(\frac{\Delta w_{jit} - \mu_{it}}{P75_{it} - P35_{it}} \right), \qquad s = 1, \ldots, 1331 \tag{8}$$

where subscript s runs over all j, i and t. Figure 3 compares the underlying distribution of wage changes (histogram, with kernel density in solid line) with the standard normal distribution (dotted line); we notice that the underlying distribution is slightly skewed right.[4]

Then, for each of the 449 country-year samples, we

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

$$\Delta \widetilde{w}_s^{it} \equiv \Delta w_s^n \Big(P75_{it} - P35_{it} \Big) + \mu_{it}, \qquad s = 1, \ldots, 1331 \tag{9}$$

- 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 $S = 1331$

$$\tilde{q}_{it} \equiv \frac{\# \Delta \widetilde{w}_s^{it} < 0}{S}, \qquad s = 1, \ldots, 1331 \tag{10}$$

- 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 $\tilde{q}_{it}$,

If the empirical samples are affected by DNWR, there will be a tendency that there are more simulated wage cuts than observed wage cuts, i.e. $\widehat{y} = \sum_{it} \widehat{y}_{it} > y = \sum_{it} y_{it}$. We repeat the procedure 5000 times, and count the number of times where $\widehat{y} > y$ (denoted $\#(\widehat{y} > y)$). The

[3] 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.

[4] The coefficient of skewness is 0.26.

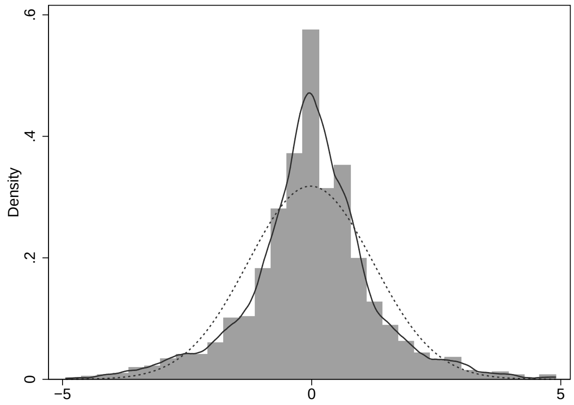

Figure 3: Histogram and kernel density (solid line) of the normalised underlying distribution of wage changes compared to the normal density (dotted line). 14 extreme observations are omitted.

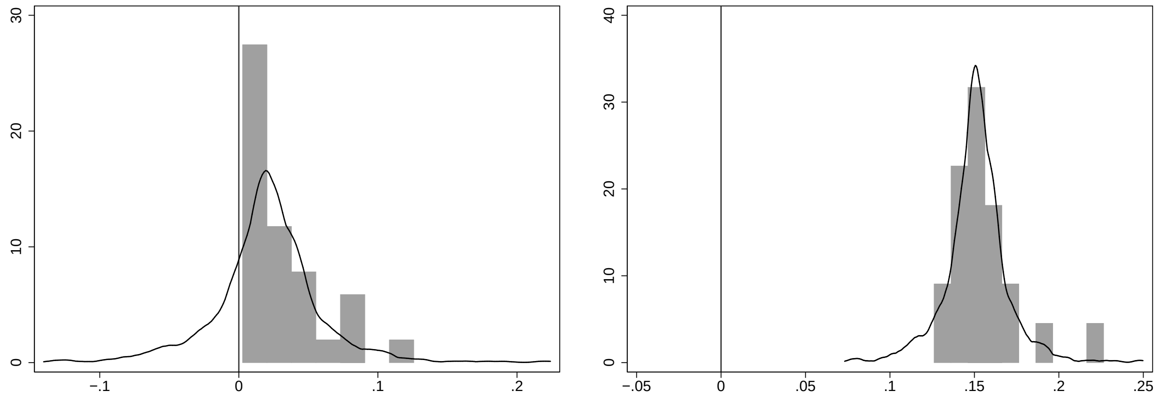

Figure 4: Left: Histogram of observed wage changes and the notional wage change distribution (solid line) in Portugal 1998. Right: Histogram of observed wage changes and the notional wage change distribution (solid line) in France 1981.

null hypothesis is rejected with a level of significance at 5 percent if $1 - \#(\widehat{y} > y)/5000 \leq 0.05$.[5]

Figure 4 compares the empirical distribution (histogram) with the notional distribution for two country-years. By construction, the empirical and the notional distributions have the same median and inter percentile range, but the shapes differ. In spite of no observed wage cuts, the probability mass to the left of zero indicates that the probability of a notional wage cut was considerable in Portugal 1998. For France 1981, no wage cuts took place, and the probability of a notional wage cut was also zero.

Note that 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 distributions will be compressed. Likewise, if DNWR compresses the inter percentile range in certain country

[5]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 and still accurate.

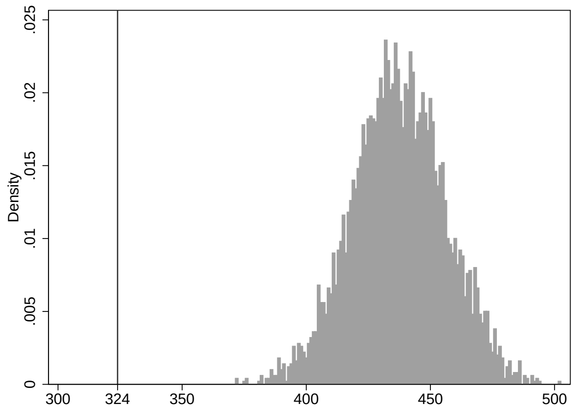

Figure 5: The frequency distributions of the number of 5000 simulated (notional) wage cuts.

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 observed 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 wage cuts in Figure 5. On average, we simulate $\widehat{Y} = 437.5$ notional wage cuts while we observe $Y = 324$ wage cuts in the data. The average fraction of notional wage cuts that is prevented by DNWR is FWCP $= (1 - Y/\widehat{Y}) = 0.259$ for the whole sample. Thus, about one out of four notional wage cuts does not result in an observed wage cut due to DNWR. A probably better measure of the importance of DNWR, is the probability than an observation is affected by DNWR. An estimate of this probability is the average fraction of industry-years affected (FIYA) calculated by $(\widehat{Y} - Y)/S$, where S is the total number of industry-year observations. For the whole sample the fraction is $(437.5 - 324)/9509 = 0.012$. Only about one out of a hundred observations (industry-years) are affected by DNWR.

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.

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 ($\widehat{Y}$)	12.8	122.9	120.9	180.8
$\#(\widehat{y} > y^B)$	4935	5000	4989	4970
Probability of significance (p)	0.013	0.000	0.002	0.006
Fraction of wage cuts prevented ($FWCP$)	0.610	0.398	0.231	0.159
Fraction of industry-years affected ($FIYA$)	0.004	0.013	0.015	0.017

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

There is evidence of DNWR in all periods. In the high-inflation 1970s, the fraction of wage cuts prevented was 61 percent. In the 1980s, it 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 around 1.7 percent in the late 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 0.036, 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.

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 in columns 2–5 in Table 2. Note that the regions include countries with rather similar labour market institutions, cf. discussion below.

We find significant DNWR at the one percent level for all regions. The fraction of wage cuts prevented is 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 differ-

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 ($\widehat{Y}$)	437.5	190.8	163.1	35.8	47.7
$\#(\widehat{y} > y^B)$	5000	4997	4998	4999	4990
Probability of significance	0	0.001	0.000	0.000	0.002
Fraction of wage cuts prevented ($FWCP$)	0.259	0.198	0.234	0.497	0.414
Fraction of industry-years affected ($FIYA$)	0.012	0.013	0.012	0.009	0.014

ence is roughly in line with what one would expect in view of the differences in labour market institutions (in the appendix, we report country-specific indices for 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). 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.

In Table 3, 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 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. We observe that for all

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	1.6	47.5	72.4	69.3
	$\#(\widehat{y} > y^B)$	4045	4999	4803	2704
	Probability of significance	0.191	0.000	0.039	0.459
	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
	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	8.2	57.5	23.8	73.6
	$\#(\widehat{y} > y^B)$	4574	4973	4437	4691
	Probability of significance	0.085	0.005	0.113	0.062
	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
	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	1.6	8.9	16.9	8.3
	$\#(\widehat{y} > y^B)$	2398	4918	4464	4954
	Probability of significance	0.520	0.016	0.107	0.009
	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
	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	1.4	9.0	7.8	29.6
	$\#(\widehat{y} > y^B)$	3740	4475	4508	4906
	Probability of significance	0.252	0.105	0.098	0.019
	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

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 five percent level for Austria, Belgium, Denmark, Ireland, Italy, Luxembourg, the Netherlands, New Zealand, Portugal and Sweden, and Finland at the ten percent level. It is noteworthy that the fraction of wage cuts prevented 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 the Netherlands (3 percent). To explore the precision of our measures of DNWR, we compute 90% confidence intervals for the fraction of wage cuts prevented based on the distributions from the simulations. Figure 6 presents these intervals for all the categories. The confidence 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 the 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 micro studies. Generally, recent micro studies find significant evidence of DNWR, while this is not the case for all our countries in our study. This difference is in spite of the fact that our data also covers the 1970s and 1980s, for which the estimated fraction of wage cuts prevented is higher, while many micro studies are based on data for the 1990s. However, when it comes to point estimates, there is a rough correspondence across countries. Figure 7 compares our estimates of the fraction of wage cuts prevented with those reported by Knoppik and Beissinger (2005) and by Dickens et al. (2005); correlation coefficients are 0.65 and 0.25, respectively. The outliers in both cases are Greece and France. For France, our low estimate is consistent with Biscourp et al. (2004), who find that wages are flexible downwards in France. For Greece, on the other hand, our negative estimate seems questionable. Indeed, our estimate based on country-specific underlying distributions reported below, is much closer to the micro estimates, equal to 0.27, although it is not significant. For the US, Lebow et al. (2003) estimate the fraction of wage cuts prevented to about one half is between the two estimates, although somewhat closer to Dickens et al. (2005). Interestingly, 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.

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 across countries and time. To test this assumption we apply a Kolmogorov-Smirnov test of equality between the common underlying distribution against alternatives where the underlying distribution is constructed separately for each country, and where it is constructed separately for each of the four time periods. The assumption of a common underlying distribution passes easily in all $19 + 27 = 46$ tests with the lowest p-value of 0.211.

Nevertheless, we also perform our method with a number of alternative ways of constructing

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	7.0	4873	0.025	0.714	0.012
Belgium	575	26	31	0.0539	40.3	4817	0.037	0.231	0.016
Canada	627	26	57	0.0909	61.8	3653	0.269	0.077	0.008
Denmark	462	24	8	0.0172	14.9	4837	0.033	0.463	0.015
Finland	368	23	2	0.0054	5.9	4703	0.059	0.663	0.011
France	556	26	21	0.0378	17.5	655	0.869	–0.200	–0.006
Germany	665	26	16	0.0241	17.0	2665	0.467	0.060	0.002
Greece	469	26	7	0.0149	6.2	1385	0.723	–0.129	–0.002
Ireland	463	23	27	0.0583	40.0	4923	0.015	0.325	0.028
Italy	312	13	0	0	3.0	4815	0.037	1	0.010
Luxembourg	423	27	32	0.0757	43.8	4886	0.023	0.269	0.028
Netherlands	483	27	23	0.0476	37.5	4994	0.001	0.387	0.030
New Zealand	750	27	45	0.0600	57.4	4814	0.037	0.216	0.017
Norway	674	27	2	0.0030	3.7	3576	0.285	0.459	0.003
Portugal	411	18	3	0.0073	21.4	5000	0.000	0.860	0.045
Spain	270	10	18	0.0667	17.1	1754	0.649	–0.053	–0.003
Sweden	472	21	6	0.0127	11.3	4813	0.037	0.469	0.011
UK	615	26	18	0.0293	23.0	4352	0.130	0.217	0.008
US	506	27	6	0.0119	8.7	3843	0.231	0.308	0.005

Note: T is the number of years. p is the probability of significance.

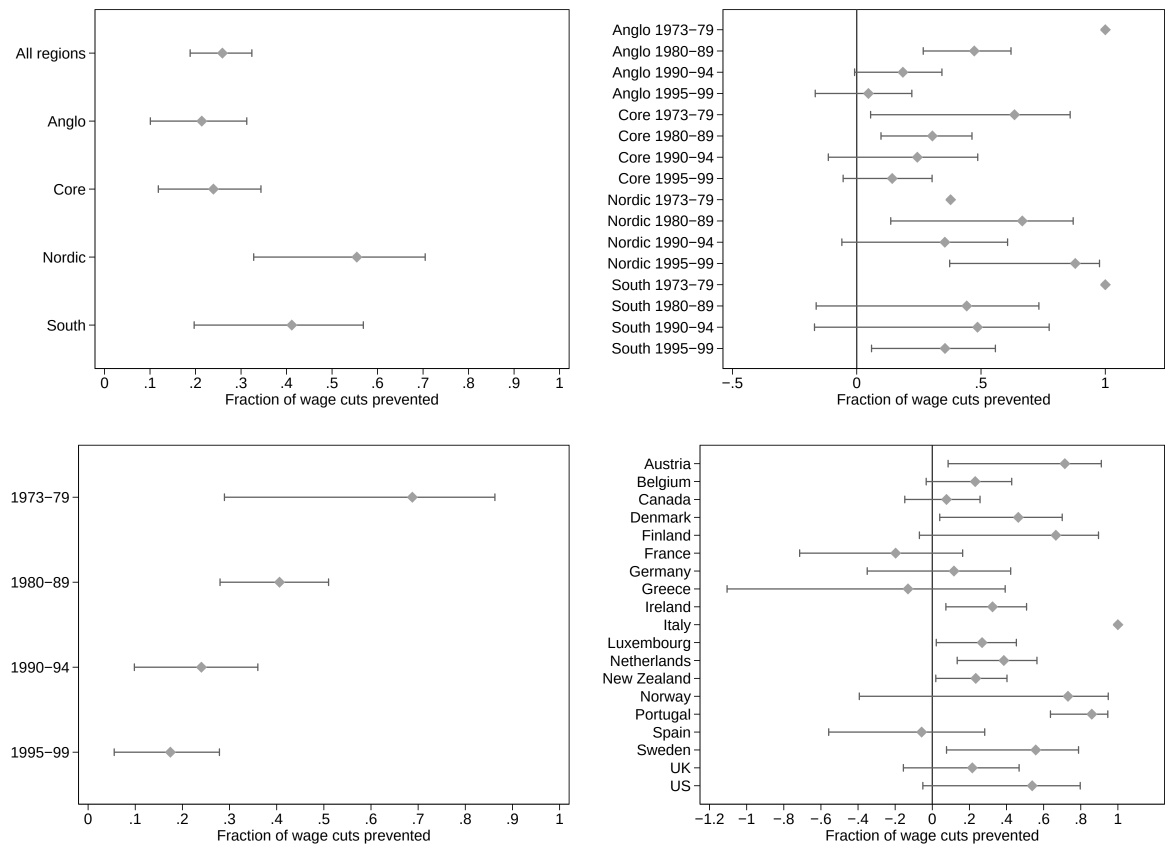

Figure 6: Estimated fractions of wage cuts prevented with 90% confidence intervals.

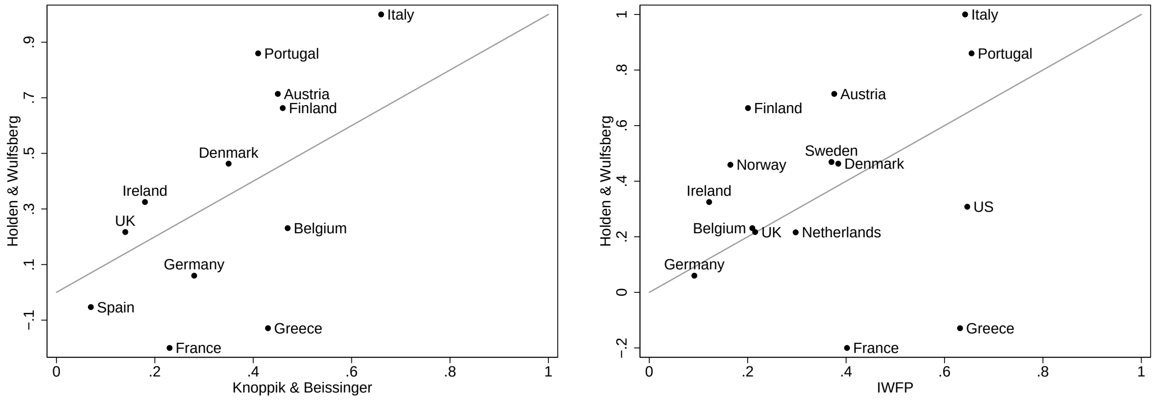

Figure 7: Comparing our estimates of FWCP *with Knoppik and Beissinger (2005) (left) and with Dickens et al. (2005) (right).*

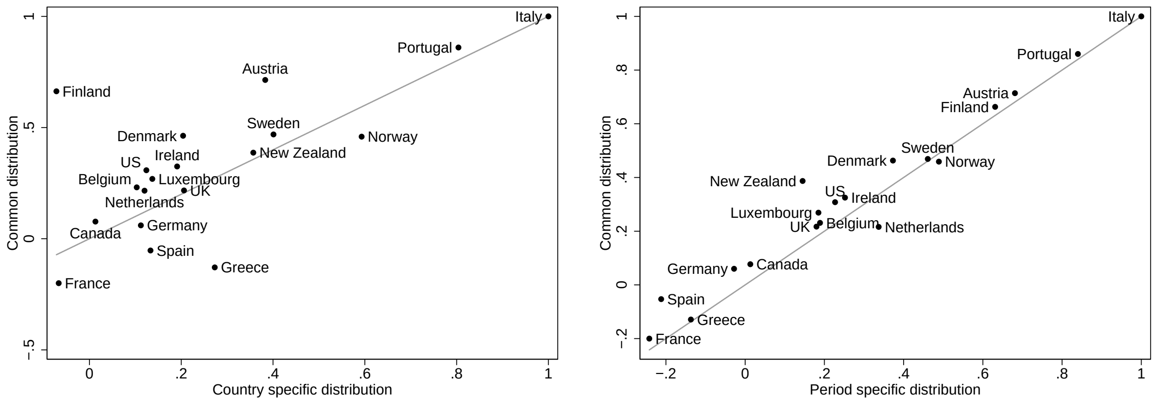

Figure 8: Comparing the estimates of FWCP *using a common underlying distribution with country specific distributions (left) and periodic specific distributions (right) .*

the underlying distribution. Figure 8 shows that the estimates from the main specification are rather similar to estimates based on country-specific underlying distributions, and highly similar to those based on period-specific underlying distributions. More precisely, we have constructed separate underlying distributions Δw_s^n based on all observations for each country, alternatively for each period, and then proceeded with the method as before. Because the underlying distributions are based on fewer observations, without explicit selection of high wage growth samples, one would expect this method to be more vulnerable to a downward bias by DNWR compressing the underlying and notional distributions. Indeed, we find somewhat less DNWR, with overall FWCP of 18 percent (country-specific) and 20 percent (period-specific), see Table B1 in Appendix B).

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. Finally, we have performed the method with a symmetry assumption inspired by Card and Hyslop (1997), where observations below the median in all country-years are replaced by observations from the upper half of the distributions. The symmetric underlying distribution is then constructed on the basis of observations from all country-years. The results turned out to be very similar to the results from the main specification. (These latter results are not reported.)

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 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 5. 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). The fact that we on average 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), i.e. 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, while Dickens et al., 2005 find DRWR for all the 16 OECD countries they study. 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

Table 5: The effect from contaminating the data by adding DNWR.

Countries without additional DNWR			Countries with additional DNWR		
	ΔY	ΔFWCR		ΔY	ΔFWCR
Austria	0.000	–0.007	Belgium	–0.484	–0.421
Italy	0.000	0.000	Canada	–0.491	–0.472
Luxembourg	0.000	0.001	Denmark	–0.500	–0.495
Netherlands	0.000	0.002	Finland	–0.500	–0.501
New Zealand	0.000	–0.003	France	–0.476	–0.460
Norway	0.000	–0.022	Germany	–0.500	–0.500
Spain	0.000	0.006	Greece	–0.429	–0.430
Sweden	0.000	0.000	Ireland	–0.481	–0.481
UK	0.000	0.001	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.

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 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.

Thirdly, we explore whether our results could caused by random changes, due to measurement errors, or possibly compositional changes arising from a difference between the wages of new and former workers. 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 26 percent in the original data to 19 with the contaminated data. We conclude that measurement errors or 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

$$f(Y_{it} = y_{it} \mid \mathbf{x}_{it}) = \frac{e^{-\lambda_{it}} \lambda_{it}^{y_{it}}}{y_{it}!} \tag{11}$$

and

$$\ln\lambda_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} \tag{12}$$

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 a variance to mean ratio which is larger than unity ('overdispersion') and hence at odds with the Poisson assumption. We can investigate this possibility by undertaking a goodness-of fit test from a Poisson regression of Y_{it}/S_{it}. The hypothesis of no overdispersion is clearly rejected ($\chi^2(416) = 634.6$). 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:

$$\ln\lambda_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + \varepsilon_{it}, \quad \varepsilon_{it} \sim \Gamma(1, \delta) \tag{12'}$$

$$\ln\lambda_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + \varepsilon_{it}, \quad \varepsilon_{it} \sim \Gamma(1, \phi_i e^{-\alpha_i}) \tag{12''}$$

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)	−0.104 (0.059)	−0.430 (0.293)
Union density	−0.803 (0.598)	−1.992* (0.980)	−0.966* (0.377)	−2.125 (1.423)
Inflation	−0.484* (0.073)	−0.345* (0.062)	−0.096* (0.048)	−0.041 (0.063)
Inflation squared	0.016* (0.003)	0.011* (0.003)	0.004 (0.002)	0.003 (0.003)
Unemployment	0.116* (0.029)	0.092* (0.036)	0.033* (0.016)	0.008 (0.036)
constant	1.092* (0.463)	1.855* (0.762)	0.118 (0.243)	—
log-likelihood	–364.6	–288.5	–257.5	–209.0
Number of observations	422	409	282	278

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.

Including a Gamma distributed error term, ε_{it}, in (12') and (12") allows the variance to mean ratios of Y_{it} to be larger than unity. (11) and (12') together yield the pooled negative binomial regression model. In (12"), we also include a country specific fixed effect, α_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 the EPL index 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.[6] In the pooled regression, we find a significant negative effect of EPL (although only at the ten percent level) and union density on the fraction of wage cuts realized, implying a positive effect on the fraction of wage cuts prevented. Unemployment has a negative 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 ($= exp(-0.104(-1.5))$). In the case of Sweden, this would imply an increase in the fraction of wage cuts realised from 52.8 to 61.7 percent, i.e. reducing the fraction of wage cuts prevented from 47.2 to 38.3 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 ($= exp(-0.966(-0.5))$); for Finland, the fraction of wage cuts realised would increase from 33.8 to 54.8 percent.

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 6.[7] Adding a time trend in the 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 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.

Interestingly, these results are in contrast to recent evidence based on microeconomic data reported in Dickens et al. (2005), where one finds weak and insignificant effect of EPL on DNWR, while union density in fact has a negative effect on DNWR, although only significant at

[6] The goodness-of-fit test yields $\chi^2(410) = 280.9$.

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

the 10 percent level.[8] One possible explanation of this striking difference would be if DNWR for individual workers that is caused by institutional variables like EPL and union contracts prevails also in industry wages, as firms might be unable to circumvent such rigidities by replacing high wage workers by low wage workers. In contrast, DNWR for job stayers that is caused by concern for fairness and morale, and the possible adverse effects on productivity if wages are cut, might be undone by firm behaviour and market mechanisms in the absence of EPL and unions.

6 Conclusions

Based on a novel nonparametric statistical 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 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), have been prevented by DNWR, while slightly more than one 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, with qualitatively the same results. Our method has also been successful in tests with various forms of 'contaminated data'.

Our paper makes three main contributions to the literature on DNWR. A number of recent micro studies, for many different countries, have documented that individual wages for job stayers are rigid downwards. However, the aggregate effects are disputed. One possible reason for this 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) and Card and Hyslop (1997); the latter find evidence of DNWR on US microdata, but inconclusive evidence for state level data. By documenting the existence of DNWR at industry level data, we show that firm behaviour and market mechanisms may diminish, but do not remove, rigidity at individual level. In this sense we view our study as complementary to the increasing number of micro studies of DNWR.

Second, we explore whether the extent of DNWR can be explained by economic and institutional variables. We find that stricter employment protection legislation (EPL) and higher union density lead to stronger DNWR: in country-year samples with strict EPL and high union density, the incidence of nominal wage cuts is reduced significantly. The estimated effects of the institutional variables that we find is fairly strong. For example, weakening the EPL from a strict to a medium level, would, according to the point estimates, raise the incidence of nominal

[8] Dickens et al. (2005) find that union density is positively associated with DRWR, significant at the 10 percent level.

wage cuts in Portugal from 0.7 to 2.3 percent. A similar change in the EPL 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 61.7 percent, i.e. reducing the fraction of wage cuts prevented from 47.2 to 38.3 percent. Thus, our results suggest that changing labour market institutions would have considerable impact on wage rigidities.

The effect of institutional variables is consistent with differences in DNWR across countries. Splitting into groups of countries, we find stronger DNWR in two groups, the South (Italy, Greece, Portugal, Spain) and the Nordic region (Denmark, Finland, Norway and Sweden), where EPL is stricter and/or unions are stronger than in the other groups; the Core (Austria, Belgium, France, Germany, Luxembourg, 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 for DNWR in part being caused by contracts and institutional features, as argued by MacLeod and Malcomson (1993) in a individual bargaining framework, and Holden (1994) in a collective agreement framework. Interestingly, the micro study of Dickens et al., 2005 do not find the same positive effect of EPL and union density on DNWR that we do. One possible explanation for this difference is that DNWR for individual workers that is caused by institutional variables like EPL and unions prevails also in industry wages, as firms might be unable to circumvent such rigidities by replacing high wage workers by low wage workers. In contrast, DNWR for job stayers that is caused by concern for fairness and morale, might be undone by firm behaviour and market mechanisms in the absence of EPL and unions. 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 contract explanations may reenforce each other.

Third, we explore the change in DNWR over time. 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 fraction of wage cuts prevented by DNWR has fallen from 60 percent in the 1970s to 16 percent in the late 1990s. The Nordic countries is 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, lower union density, and higher unemployment. However, in spite of the reduction in DNWR, the fall in inflation has implied that more industries are affected by DNWR. 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.

In an important paper, Barro (1977) argued that short run wage rigidity may reflect intertemporal risk sharing between the employer and an risk averse employee, with no allocative effects, i.e. no effects on employment decisions. On this argument, a finding of significant DNWR need not imply effects on employment and output. While we cannot rule out this argument, there are several reasons for why we still believe that DNWR in industry wages is likely to affect employment and output under low inflation. One reason is that in most OECD countries, the majority of workers have their wages set in collective bargaining, often at more centralised levels. Presumably, such wage rigidity, being largely exogenous to the firm, would affect firms' employment decisions, in particular as to hirings. This is reflected in the dominating explanations of OECD unemployment, where wage setting and labour market institutions play a key role (Layard et al., 1991 and Nickell et al., 2005). On this basis, one would expect that increased wage pressure due to binding DNWR would lead to higher unemployment. A second reason is that several recent contributions have argued that wage rigidity is a necessary and important piece in explanations of business cycle fluctuations, see e.g. Erceg et al. (2000), Smets and Wouters (2003), Shimer (2004) and Hall (2005), the latter two explicitly discussing Barro's argument. In an era of low inflation, the existence of DNWR would exacerbate wage stickiness in a downturn of the economy, presumably amplifying the effect on vacancies and employment. A third reason is that binding DNWR would presumably also push up prices, leading to higher inflation than we would otherwise observe. In most OECD countries, higher inflation will be met by higher real interest rates, which would have a short run dampening effect on output and employment.

Overall, our finding of DNWR yields clear additional support to the idea that DNWR has some, but moderate impact on firms' wage costs in many OECD countries, especially in Europe. Weaker employment protection legislation, lower union density, and higher unemployment have implied that the fraction of wage cuts prevented by DNWR has fallen 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.

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A Data 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 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, 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, 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. 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*	$\widehat{Y}$	$\#(\widehat{y} > y^B)$	p	*FWCP*	*FIYA*
All	324	395.6	5000	0.000	0.181	0.008	403.1	5000	0.000	0.196	0.008
1970–79	5	12.6	4935	0.013	0.602	0.003	12.6	4924	0.015	0.603	0.003
1980–89	74	106.3	4999	0.000	0.304	0.009	104.6	4999	0.000	0.292	0.008
1990–94	93	110.4	4853	0.029	0.157	0.009	114.8	4957	0.009	0.190	0.011
1995–99	152	166.4	4509	0.098	0.087	0.009	171.1	4782	0.044	0.112	0.011
Anglo	153	172.8	4729	0.054	0.110	0.006	176.2	4869	0.026	0.132	0.008
Core	125	148.4	4934	0.013	0.158	0.008	150.9	4968	0.006	0.172	0.008
Nordic	18	26.9	4837	0.033	0.330	0.004	33.2	4995	0.001	0.458	0.008
South	28	48.6	4997	0.001	0.424	0.014	42.7	4964	0.007	0.344	0.010
Austria	2	3.2	3135	0.373	0.380	0.003	6.3	4774	0.045	0.681	0.010
Belgium	31	34.5	3739	0.252	0.102	0.006	38.2	4603	0.079	0.189	0.013
Canada	57	57.8	2594	0.481	0.014	0.001	57.7	2538	0.492	0.013	0.001
Denmark	8	10.0	3327	0.335	0.197	0.004	12.8	4468	0.106	0.373	0.010
Finland	2	1.9	1427	0.715	–0.070	–0.000	5.4	4548	0.090	0.631	0.009
France	21	19.7	1601	0.680	–0.067	–0.002	16.9	476	0.905	–0.242	–0.007
Germany	16	17.9	3175	0.365	0.108	0.003	15.6	1937	0.613	–0.028	–0.001
Greece	7	9.6	3748	0.250	0.271	0.006	6.2	1390	0.722	–0.137	–0.002
Ireland	27	33.4	4331	0.134	0.193	0.014	36.1	4707	0.059	0.252	0.020
Italy	0	2.9	4752	0.050	1.000	0.009	2.9	4767	0.047	1.000	0.009
Luxembourg	32	37.1	4046	0.191	0.138	0.012	39.3	4489	0.102	0.185	0.017
Netherlands	23	35.8	4982	0.004	0.357	0.026	34.7	4966	0.007	0.337	0.024
New Zealand	45	51.4	4120	0.176	0.125	0.009	52.6	4294	0.141	0.145	0.010
Norway	2	4.9	4343	0.131	0.593	0.004	3.9	3740	0.252	0.489	0.003
Portugal	3	15.3	5000	0.000	0.804	0.030	18.8	5000	0.000	0.840	0.038
Spain	18	20.7	3406	0.319	0.129	0.010	14.9	806	0.839	–0.212	–0.012
Sweden	6	10.0	4567	0.087	0.401	0.009	11.1	4787	0.043	0.461	0.011
UK	18	18.6	4247	0.151	0.204	0.007	22.0	4036	0.193	0.180	0.006
US	6	6.8	2639	0.472	0.119	0.002	7.8	3304	0.339	0.227	0.003

Notes: see Table 1