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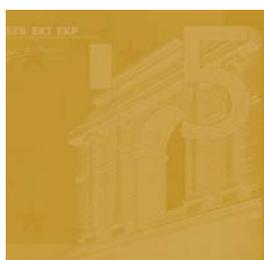
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DOWNWARD NOMINAL WAGE
RIGIDITY IN THE OECD [1]

by Steinar Holden [2]
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ECB/CEPR Labour Market Workshop on "Wage and Labour Cost Dynamics"

This paper was presented at the ECB/CEPR Labour Market Workshop on "Wage and Labour Cost Dynamics", held on 14-15 December 2006 in Frankfurt am Main, Germany. The workshop was organized by Torben M Andersen (Universitet Aarhus and CEPR), Samuel Bentolila (CEMFI and CEPR), Ana Lamo (ECB) and Jarkko Turunen (ECB). The conference programme, including papers, can be found on the ECB's web site http://www.ecb.int/events/conferences/html/wage_and_labour.en.html

The views expressed in the paper are the author's own and do not necessarily reflect those of the Eurosystem.

CONTENTS

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. Using data for hourly nominal wages, we explore the existence of DNWR on wages at the industry level in 19 OECD countries, over the period 1973–1999. Based on a novel 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: E3, J3, J5
Keywords: Downward nominal wage rigidity, OECD, employment protection legislation, wage setting

1 Non-technical summary

The optimal inflation target depends of the costs and benefits of inflation. One of the possible benefits of inflation that has attracted most attention is that it may grease the workings of the labour market. As argued by Tobin (1972), if wages are rigid downwards in nominal terms, the existence of some inflation will reduce the risk that downward wage rigidity is binding. Hence some inflation will alleviate wage pressure, thus involving a permanent reduction in unemployment.

However, the practical relevance of this effect is heavily disputed. The discussion is less on the existence of downward nominal wage rigidity (DNWR), as there is now a number of empirical studies on micro data that have found considerable DNWR for job stayers in most OECD countries, including the International Wage Flexibility Project (Dickens et al., 2006). The key issue of contention is whether this DNWR at individual level matters for monetary policy.

On the one hand, several studies find that DNWR and/or low inflation is associated with higher unemployment (see e.g. Fehr and Gotte (2005), Akerlof et al., 1996, Karanassou et al., 2003 and Dickens et al., 2006). On the other hand, many 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).

A possible reason for the contradictory 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. Indeed, studying the wage adjustments of different groups of Canadian employees, Fares and Lemieux (2000) 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. These results are consistent with the findings of Card and Hyslop (1997), who based on US data find evidence of DNWR for individual workers, but no corresponding evidence on state-level data.

To explore the potential existence of DNWR in wages at more aggregate levels, we study industry level wage data for 19 OECD countries, for the period 1973–99. Following previous studies, we test for DNWR by comparing the empirical wage change distributions with a postulated notional or counterfactual wage change distribution (i.e. the assumed distribution under flexible wages). The notional wage change distributions are constructed on the basis of observations from country-years when the wage growth is high, i.e. when DNWR is not likely to bind. A novelty of our approach is that we construct country-year specific notional probabilities of

wage cuts from the notional country-year specific distributions. Given these notional probabilities, we can compute the probability distribution for the notional number of wage cuts, and compare the empirical number of wage cuts with this distribution. Thus, we can calculate the exact p-value for the empirical number of wage cuts, under the Null hypothesis of no DNWR.

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. These results are robust to a number of different modifications of the key assumptions.

Our paper makes three main contributions to the literature on DNWR. First, 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 studies that document the existence of DNWR in micro data.

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 are 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 wage cuts in Portugal from 0.7 to 2.3 percent. Thus, our results suggest that changing labour market institutions would have a considerable impact on wage rigidities.

The effect of institutional variables is consistent with differences in DNWR across countries. Splitting countries into groups, 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 support that idea that DNWR is 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. (2006) does 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 can be undone by wage flexibility for other workers or in other firms, unless prevented by strict EPL and/or union contracts. For example, for a firm facing a union contract, it would be difficult to circumvent wage rigidities by replacing high-wage workers by low-wage workers. However, as we have not be able to test for fairness and morale explanations of DNWR, such hypotheses remain speculative.

The third contribution is that 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, from 60 percent in the 1970s to 16 percent in the late 1990s. The fall in inflation has nevertheless 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.

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.

2 Introduction

Recent micro economic studies have found considerable downward nominal wage rigidity (DNWR) for job stayers in OECD countries. The International Wage Flexibility Project (Dickens et al., 2006) finds that for all the 16 countries that are studied, DNWR prevents wage cuts from taking place, with the fraction of wage cuts prevented being in the range of 9–66 percent, see literature review in section 3 below. 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 (see e.g. Akerlof et al., 1996, Karanassou et al., 2003 and Dickens et al., 2006), 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 wage change distribution (i.e. the assumed distribution under flexible wages) on the basis of observations from country-years when the wage growth is high and thus DNWR is not likely to bind. However, our method for comparing the notional and empirical wage change distributions is simple and novel. Based on the notional wage change distributions,

we construct country-year specific notional probabilities of a wage cut. Given these notional probabilities, we can compute the probability distribution for the number of notional wage cuts, and compare with the number of empirical wage cuts. Thus, we can calculate the exact p-value for the number of empirical wage cuts, under the Null hypothesis of no DNWR, as given by the notional probabilities. In section 5, we document the empirical results 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 6 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 7 concludes the paper.

3 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 (2006) 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. (2006) have strengthened the evidence of extensive DNWR in most or all OECD countries.

The clear evidence of DNWR for individual workers is 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 the 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, second, our data 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 decrease in recessions, cf. Solon et al. (1994) and Fares and Lemieux (2000). The lower share of low-skilled workers in recessions will ceteris paribus lead to higher average wages, thus it will reduce the number of wage cuts in recessions, when they are most likely. A third point is that the compositional effects will depend on to what extent job movements in general are part of a career upwards, involving higher wages, or whether job movements instead might be undertaken by less productive workers leaving for jobs with lower job requirements. As the former two effects have opposite impact on the number of wage cuts, and the third is ambiguous, the overall effect is clearly ambiguous. Turnover also involves a random element which 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.

Another issue that needs consideration, is to what extent binding DNWR for some workers may affect the wages of other workers in the industry. One such effect would be if firms respond to downward rigidity for some workers 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.

Binding DNWR in one firm might also affect wages in other firms. For example, in recessions with negative productivity or demand shocks, firms that cannot lower their wages due to DNWR may respond by laying off workers, or at least reduce the hiring of new workers. If workers have industry-specific skills, the higher number of unemployed workers might contribute to lower wages in other firms in the industry, where wages are flexible, thus offsetting the effect of DNWR in the inflexible firms. However, the extent to which this will happen depends on how wages are set in the remaining firms. If wage setting in other firms are influenced by e.g. unions
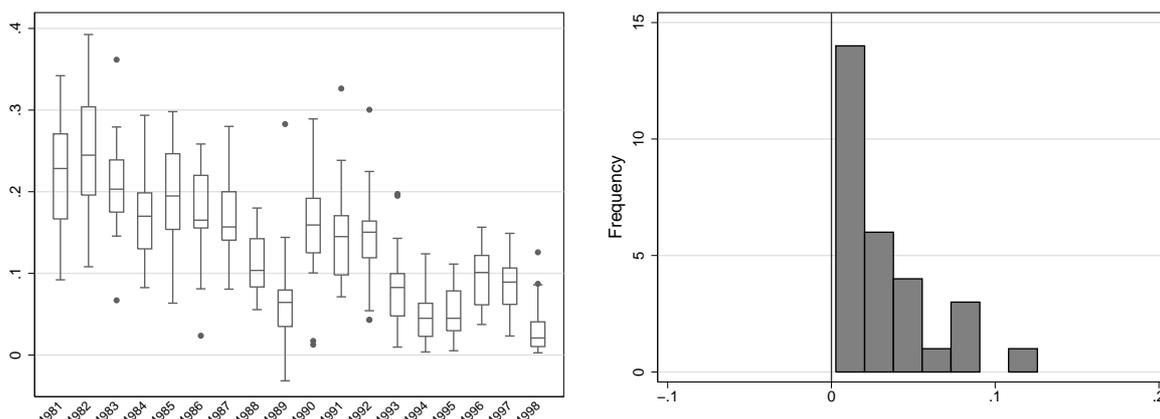
Figure 1: Box plots of annual wage growth in Portugal (left) and histogram of annual wage growth in 1998 (right). The box plot illustrates the distribution of wage changes within a country-year. The box extends from the 25th to the 75th percentile with the median inside the box. The whiskers emerging from the box indicate the tails of the distributions and the dots represent outliers.

or employment protection legislation, the workers in these firms may resist the dampening effect on wages of higher unemployment. In this case the industry effect of DNWR in some firms is not undone.

4 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.[1] 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 wage change observations distributed across 449 country-year samples, on average 21 industries per country-year. Figure 1 illustrates the data for Portugal over time (left) and for Portugal in 1998 (right). We see that the median wage growth and dispersion fall over time and that there were many small observations of wage increases in 1998. There are no nominal wage cuts in 331 (74%) of the country-year samples. We observe, however, no less than $Y = 324$ events of nominal wage cuts, i.e. an incidence rate of $q = 3.4$ percent of all observations. Table A1 in the data appendix for the detailed distribution of wage cuts and observations across countries and years. The data appendix provides more details on the data.

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

To detect DNWR we use a novel variant of the skewness-location approach of McLaughlin (1994), where the empirical distribution of wage changes is compared to a postulated notional distribution, i.e. a counterfactual wage change distribution assumed to prevail in the absence of DNWR. The key issue in this approach is clearly how to derive the notional distribution. In the literature, two main approaches have been used, either to assume that the notional wage change distribution is symmetric (e.g. Lebow et al. (1995), Card and Hyslop (1997) and Dickens et al. (2006)), or to assume that the shape of the notional wage change distributions is common across time, (e.g. Kahn, 1997 and Nickell and Quintini, 2003). In the latter case the shape of the notional distribution is derived from years with high inflation, where presumably DNWR does not affect the wage change observations. Elsby (2006) shows that if wages are rigid downwards, firms will respond by compressing wage increases. This would cause the symmetry method to involve a downward bias in the estimated DNWR. In addition, the wage change distribution may be asymmetric for other reasons than DNWR, for example if industries are subject to different shocks. Thus, our main method is based on a common shape assumption. However, for robustness we will also compare with the results using a symmetry assumption.

One popular test of the common shape type, suggested by (Kahn, 1997), explores the effect of inflation on the height of histogram bars of the wage change distribution. However, the Kahn test relies on the assumption that the notional wage change distribution is invariant to inflation. This property is violated in our case, as both the median wage growth and the dispersion fall over time in our data, and both have a strong positive correlation with inflation (correlation coefficients are 0.87 and 0.42 respectively). This is illustrated for Portugal in Figure 1. 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 we shall see below (Figure 2), the density function in our data is highly non-linear in the range that is relevant for DNWR. Thus, this method also involves problematic assumptions in our case.

A further issue is that in most micro studies of DNWR, there has been a focus on the existence of a spike at zero wage growth, caused by DNWR. Due to compositional effects and averaging over many workers, there is no spike at zero in our data. Thus, as will be clear below, we instead focus on the extent of missing wage cuts in the empirical distributions.

We construct the notional wage change distributions on the basis of country-year samples with high median nominal and real wage growth, to minimize the effect of possible downward nominal and real wage rigidity. However, to allow for the large variation that exists across countries and time with respect to monetary policy, wage setting and industry structure, we

include country-year specific variation in the location (median) and dispersion of the notional wage change distributions. Thus, we only impose the same shape in all country-years, but not the same location and dispersion. This corresponds to assuming the same two-parameter distribution for all country-years, except that we have not chosen a specific parametric distribution, but use the shape of the empirical wage change distribution from the high inflation years. Note that an alternative assumption, common in regression based studies, namely that the notional distribution is normal, would not be suitable due to the higher peak and fatter tails of the empirical distribution, see Figure 2. By allowing for country-year specific variation in the median and the dispersion, in line with Nickell and Quintini (2003), our approach is less restrictive than other approaches often used in the literature. Furthermore, in section 5.1 below, we report results from a number of alternative assumptions including symmetry, as a check of the robustness of our results.

4.1 Constructing the notional distribution

More specifically, the notional distributions are constructed as follows. First, we construct the underlying distribution based on a subset H of the sample, with $S^H = 1331$ observations from the 66 country-year samples where both the median nominal and the median real wage growth are among their respective upper quartiles.[2] To avoid as far as possible that the measures of location and dispersion are affected by 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). The underlying distribution of wage changes is then constructed by use of the 66 samples with high median nominal and real wage growth, by subtracting the corresponding country-year specific median (μ_{it}) and dividing by the inter percentile range ($P75_{it} - P35_{it}$), i.e.

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

where subscript s runs over all j, i and t in the 66 country-year samples. Figure 2 compares the underlying distribution of wage changes (illustrated by the histogram and the kernel density in solid line) with the standard normal distribution (dotted line); we notice that the underlying distribution is asymmetric as it slightly skewed right.[3]

Then, for each of the 449 country-years in the overall sample, we construct a notional

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[2]Thus, in these country-year samples, the median nominal wage growth is above the 3rd quartile of 11.8 percent, and the median real wage growth is above 2.8 percent. Four of 1331 observations are below five percent nominal wage growth, of which only one is negative.

[3]The coefficient of skewness is 0.26.
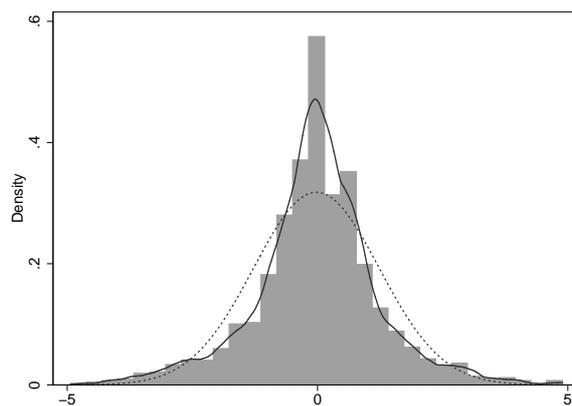
Figure 2: Histogram and kernel density (solid line) of the normalised underlying distribution of wage changes compared to the normal density (dotted line). 14 extreme observations are omitted.

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

$$\Delta \widetilde{w}_s^{it} \equiv \Delta w_s^u \Big(P75_{it} - P35_{it} \Big) + \mu_{it}, \qquad \forall \; i,t, \text{ and } s = 1, \dots, S^H \tag{2}$$

Thus, we have then constructed 449 notional country-year samples, each consisting of $S^H = 1331$ wage change observations. These notional country-year samples have by construction the same median and interpercentile range as their empirical country-year counterparts, whereas the shape is common for all notional country-year samples, given by the shape of the underlying distribution.

Given the notional country-year specific distributions, we can explore the extent of DNWR by comparing the notional and the empirical distributions. The point estimate of the extent of DNWR follows directly from comparing the incidence of wage cuts in the empirical and the notional distributions. Thus, for all country-year samples it we calculate the empirical incidence rate as

$$q_{it} = \frac{\#\Delta w_{jit} < 0}{S_{it}}, \qquad \forall \; j \tag{3}$$

where $\#\Delta w_{jit} < 0$ is the number of wage cuts and S_{it} is the number of observations in country-year it. The notional incidence rate is calculated as

$$\tilde{q}_{it} = \frac{\#\Delta \widetilde{w}_s^{it} < 0}{S^H}, \qquad s = 1, \dots, 1331 \tag{4}$$

For country-years where there is at least one notional wage cut, implying that $\tilde{q}_{it} > 0$, we can calculate an often used measure of DNWR, which is the fraction of wage cuts prevented, FWCP
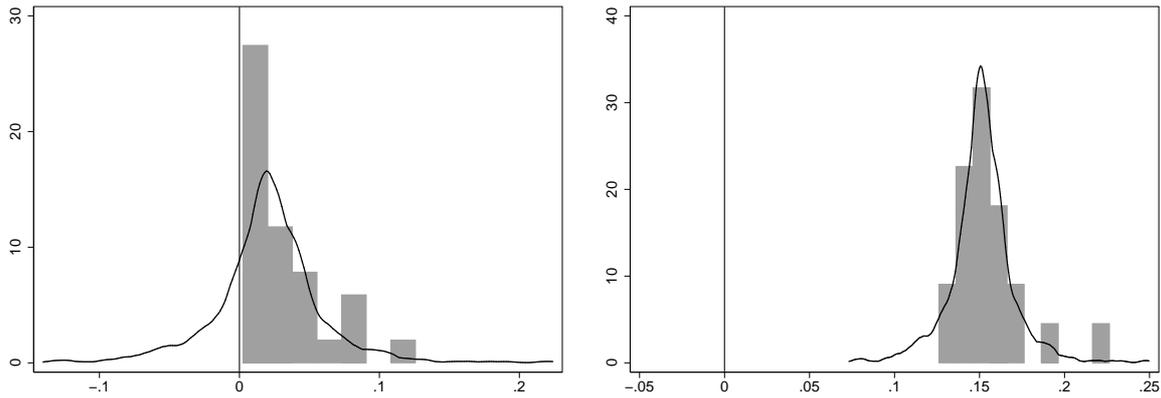
Figure 3: Left: Histogram of observed wage changes and the notional wage change distribution (solid line) in Portugal 1998. Right: Histogram of observed wage changes and the notional wage change distribution (solid line) in France 1981.

defined as

$$FWCP_{it} = 1 - q_{it}/\tilde{q}_{it} \tag{5}$$

If, for example, the incidence of wage cuts in the empirical sample is half of that in the notional distribution, then $FWCP = 0.5$. Note that if the empirical incidence rate is larger than the notional, the FWCP is negative. Figure 3 illustrate these measures for Portugal 1998 and France 1981. By construction, the empirical distribution (histogram) and the notional distribution (for illustrative purposes, we use the kernel density) for the same country-year have the same median and inter percentile range, but the shapes differ. In Portugal 1998 the incidence rate of notional wage cuts is 0.22, while the empirical incidence rate is zero, clearly suggesting the existence of DNWR with a FWCP of unity. The empirical and notional incidence rates are zero for France 1981, so the FWCP is not defined. Obviously there is a lot of variation in the incidence rates (and thus the FWCP) across the country-year samples, which are reported in Table A2 in the appendix.[4] In order to look for interesting patterns in these estimates across countries, we will calculate aggregate incidence rates and the FWCP for countries, periods as well as the overall sample (i.e. all country-years).

We aggregate the country-year estimates by pooling the empirical observations in the relevant sample (eg all country-years) implying that the country-year notional incidence rates are weighted according to the number of observations within the country-year. For the overall sample, the fraction of wage cuts prevented is $FWCP = 1 - q/\tilde{q} = 1 - 0.034/0.046 = 0.259$. Thus, about one out of four notional wage cuts in the overall sample does not result in an observed wage cut due to DNWR. A probably better measure of the economic relevance of DNWR is the probability than an observation is affected by DNWR. An estimate of this probability

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[4]The empirical and notional incidence rates are zero for 140 country-year samples, while a FWCP of unity is observed in 196 country-years.

is the fraction of industry-years affected calculated by the difference between the notional and empirical incidence rates, i.e. $(\tilde{q} - q)$. For the overall sample this fraction is 0.012 i.e. about one out of 100 observations (industry-years) were affected by DNWR.

Below we will present estimates of the fraction of wage cuts prevented by DNWR and fraction of industry-years affected for different countries, regions and periods. But before that we provide a statistical test of the significance of our estimates.

4.2   Testing the significance of DNWR

To test whether our estimates of the FWCP are statistically significant, we exploit that the incidence rate in the notional wage change distribution can be viewed as the probability of a wage cut if there was no DNWR. In other words, under the Null hypothesis of no DNWR, the number of wage cuts in country-year it with, say, 20 industries, is given by 20 independent draws from the binomial distribution with probability $\tilde{q}_{it}$. Using the formulae for binomial distributions, we can then calculate the exact probability for any number of wage cuts for each country-year under the Null hypothesis of no rigidity, see Table A2. The estimate of FWCP is significantly larger than 0 at the 10% level for ten country-years only, which is not very surprising given the few number of observations per country-year samples.[5]

For samples covering more than one country-year, the number of wage cuts under the Null hypothesis of no DNWR, is given by the combination of several binomial distributions, with respective incidence rates $\tilde{q}_{it}$ as probabilities. Thus, in principle, we can also compute the exact probability of significance (p-value) for the number of wage cuts in the empirical distribution for these samples. This calculation is, however, computationally extremely demanding not only for the overall sample with 9509 observations, drawn from 449 different binomial distributions, but also for separate countries.[6] Thus, we compute the p-values on the basis of simulations instead, which is computationally much simpler, and still highly accurate.

Specifically, our simulation method goes as follows. For each country-year it, we draw S_{it} times (i.e. the number of industries in country-year it) from a binomial distribution with probability $\tilde{q}_{it}$. We then add up all the simulated wage cuts for the aggregation level of interest, e.g. all country years $(\widehat{Y})$, and compare with the total number of wage cuts in the corresponding empirical distribution, $Y = 324$. We then repeat this procedure 5000 times, and count the number of times where we simulate more notional wage cuts than we observe, for the overall sample denoted $\#(\widehat{Y} > Y)$. The Null hypothesis is rejected with a level of significance at

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[5]These are Belgium 1987 ($FWCP = 1$, $p = 0.039$), Canada 1987 (0.75, 0.076), Canada 1996 (0,77 0.045), Ireland 1992 (1, 0.094), Luxembourg 1993 (1, 0.091), New Zealand 1984 (0.73, 0.099), New Zealand 1991 (0.79, 0.039), Portugal 1995 (1, 0.031), Portugal 1998 (1, 0.008) and UK 1982 (1, 0.084).

[6]The number of terms in the formulae for the probability of k wage cuts in a sample of S observations is $S!/(k!(S-k)!)$, which increases rapidly with the number of wage cuts. With $S = 9509$ and $k = 324$ in the overall sample the number of terms is $exp(1409.5)$

5 percent if $1 - \#(\widehat{Y} > Y)/5000 \le 0.05$. We can also use the simulation results to obtain confidence intervals for our estimate of DNWR.

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 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; an issue we shall return to below. However, under the Null hypothesis of no DNWR, there is no downward bias. Hence this aspect will not affect the significance level of our test.

5   Results

For the whole sample, there are more simulated than observed wage cuts in all 5000 simulations. Thus we reject the Null hypothesis of no DNWR comfortably with a p-value of 0, as reported in the bottom line of the first column of Table 1. 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?

We first investigate whether DNWR has changed over time by splitting the sample into four subperiods 1973–1979, 1980–1989, 1990–1994 and 1995–1999. From Table 1 we see that the number (and incidence) of wage cuts has increased over time as the average wage growth (and inflation) decreased while unemployment increased. DNWR is statistically significant in all periods. In the high-inflation 1970s, the 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 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),

Table 1: Results from 5000 simulations on subperiods.

Sample properties:	All observations	1973–79	1980–89	1990–94	1995–99
No. of observations (S)	9509	2224	3717	1906	1662
No. of country-years	449	109	175	88	77
Average wage growth		13.78%	8.72%	5.60%	3.99%
Average inflation rate		10.30%	8.13%	4.42%	2.19%
Average unemployment rate		3.71%	6.72%	8.49%	8.07%
Observed wage cuts (Y)	324	5	74	93	152
Incidence of wage cuts (q)	0.0341	0.0023	0.0199	0.0488	0.0915
Simulation results:					
Fraction of wage cuts prevented ($FWCP$)	0.259	0.610	0.398	0.231	0.159
Fraction of industry-years affected ($\hat{q} - q$)	0.012	0.004	0.013	0.015	0.017
Probability of significance (p)	0	0.013	0.000	0.002	0.006

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

Table 2: Results from 5000 simulations on regions.

Sample properties:	Anglo	Core	Nordic	South
No. of observations (S)	2961	3110	1976	1462
No. of country-years	129	158	95	67
Observed wage cuts (Y)	153	125	18	28
Incidence of wage cuts (q)	0.0517	0.0402	0.0091	0.0192
Simulation results:				
Fraction of wage cuts prevented ($FWCP$)	0.198	0.234	0.497	0.414
Fraction of industry-years affected ($\hat{q} - q$)	0.013	0.012	0.009	0.014
Probability of significance (p) 0	0.001	0.000	0.000	0.002

Note: See Table 1

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 difference 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

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

Region		1973–1979	1980–1989	1990–1994	1995–1999
Anglo	No. of observations	698	1149	595	519
	No. of country-years	31	50	25	23
	Observed wage cuts	0	26	59	68
	Incidence of wage cuts	0	0.0226	0.0992	0.1310
	Fraction of wage cuts prevented	1	0.452	0.185	0.018
	Fraction of industry-years affected	0.002	0.019	0.023	0.002
	Probability of significance	0.191	0.000	0.039	0.459
Core	No. of observations	794	1183	587	546
	No. of country-years	41	60	30	27
	Observed wage cuts	4	40	18	63
	Incidence of wage cuts	0.0050	0.0338	0.0307	0.1154
	Fraction of wage cuts prevented	0.515	0.304	0.243	0.144
	Fraction of industry-years affected	0.005	0.015	0.010	0.019
	Probability of significance	0.085	0.005	0.113	0.062
Nordic	No. of observations	474	888	354	260
	No. of country-years	23	40	18	14
	Observed wage cuts	1	3	12	2
	Incidence of wage cuts	0.0021	0.0034	0.0339	0.0077
	Fraction of wage cuts prevented	0.374	0.665	0.292	0.760
	Fraction of industry-years affected	0.001	0.007	0.014	0.024
	Probability of significance	0.520	0.016	0.107	0.009
South	No. of observations	258	497	370	337
	No. of country-years	14	25	15	13
	Observed wage cuts	0	5	4	19
	Incidence of wage cuts	0	0.0101	0.0108	0.0564
	Fraction of wage cuts prevented	1	0.447	0.485	0.358
	Fraction of industry-years affected	0.005	0.008	0.010	0.031
	Probability of significance	0.252	0.105	0.098	0.019

Note: See Table 1

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 section 5 below, we analyse the effect of the institutional variables further.

In Table 3, we report results from splitting the sample by combining regions and sub-periods.

Table 4: Results from 5000 simulations on countries.

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

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

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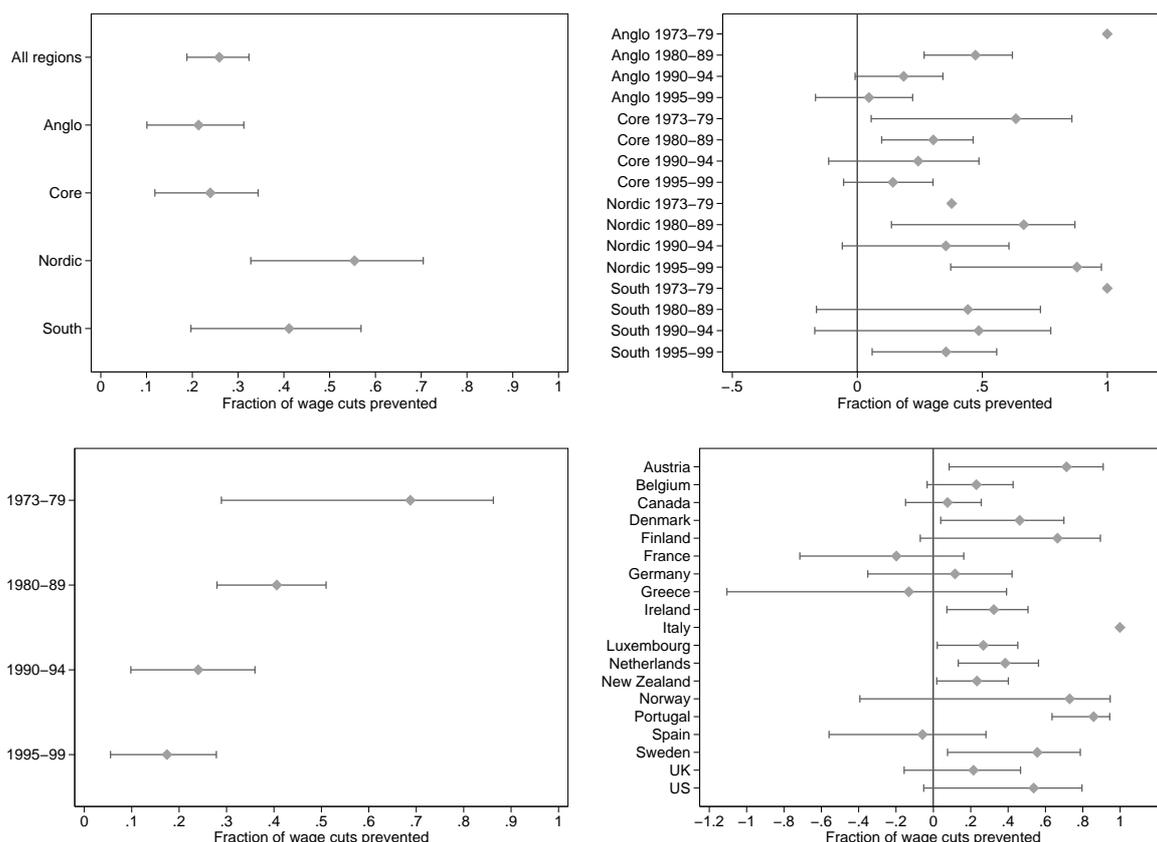
Figure 4: Estimated fractions of wage cuts prevented with 90% confidence intervals.

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 4 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. Figure 5 compares our estimates of the fraction of wage cuts prevented with those reported by Knoppik and Beissinger (2005) and by Dickens et al. (2006). As discussed in section 2 above, the extent of DNWR may differ between micro and industry data, and we shall return to this issue in section 5 below. Yet it is of interest to note that there is some correspondence across countries, with correlation coefficients of 0.65 and 0.25, respectively. The outliers in both cases are Greece and France. For France, our low estimate is consistent with Biscourp et al. (2004), who find considerable wage flexibility 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
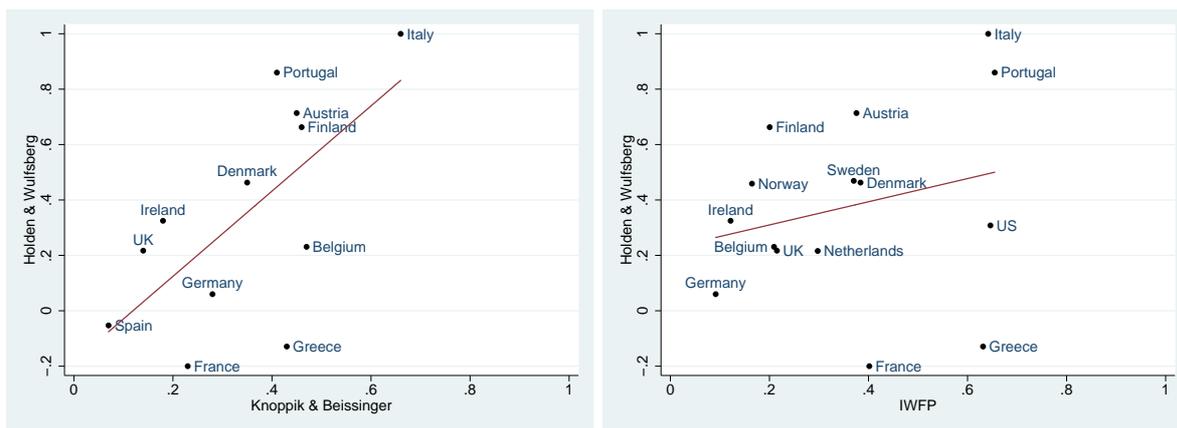
Figure 5: Comparing our estimates of FWCP *with Knoppik and Beissinger (2005) (left) and with Dickens et al. (2006) (right).*

the fraction of wage cuts prevented to about one half is between the two estimates, although somewhat closer to Dickens et al. (2006). 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.

5.1 Robustness

We explore the robustness of our findings in a number of different directions. To test our maintained assumption of a common shape for the wage change distributions across countries and years, we apply a Kolmogorov-Smirnov test. Concretely, we test for the 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 the underlying distribution. Figure 6 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^u 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).
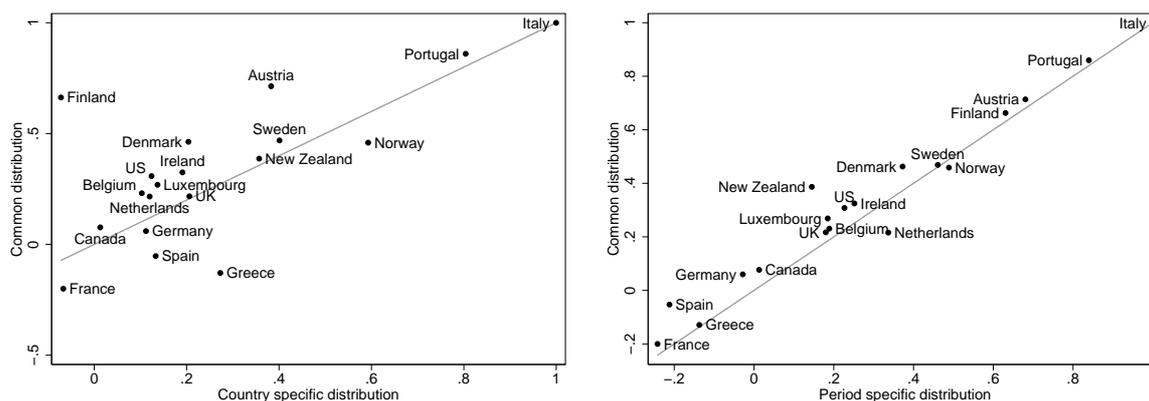
Figure 6: Comparing the estimates of FWCP *using a common underlying distribution with country specific distributions (left) and periodic specific distributions (right).*

The selection of samples for the underlying distribution, where we used 66 samples with high median nominal and real wage growth, is clearly somewhat arbitrary. Thus, 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 Lebow et al. (1995) and Card and Hyslop (1997), where observations below the median in all country-years are replaced by observations from the upper half of the distributions. Thus, all country-year samples are symmetric, but the distributions differ across country-years according to the part above the median of the country-year specific empirical distributions. The results turned out to be very similar to the results from the main specification, cf. Table B1. The estimated FWCP are however somewhat lower, consistent with a possible downward bias of the symmetry approach, if DNWR makes firms compress wage increases, as suggested by Elsby (2006).

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

As noted above, there is a possible downward bias in our method, if DNWR despite our precautions have compressed the underlying distribution or inter percentile ranges in some country-years. One way of exploring the quantitative importance of this bias is to contaminate the data by adding DNWR, and then see to what extent we are able to detect the DNWR that we have added. If the downward bias is severe, we would presumably detect much less DNWR than we have added. Specifically, we pick ten countries evenly from the four regions (Belgium,

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

Countries without additional DNWR	ΔY	ΔFWCR	Countries with additional DNWR	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. An exact test would yield a $\Delta FWCR$ identical to ΔY for each country.

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). These results suggest that there is some downward bias in the estimated DNWR due to DNWR affecting the notional distribution and/or the interpercentile ranges, but that the effect is not large.

A further point is to what extent our findings can be caused by downward *real* wage rigidity (DRWR). Bauer et al. (2003) and Barwell and Schweitzer (2004) find evidence for DRWR in Germany and the UK, respectively, while Dickens et al. (2006) 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 observations are negative real wage changes, by itself a clear sign that if DRWR exists, it is certainly not absolute.

It is hard to distinguish between DNWR and DRWR, in particular on industry data, where random components will imply that we are unlikely to find sharp effects exactly at the threshold points of zero nominal and real growth. In a companion paper, Holden and Wulfsberg (2007), we address the issue more thoroughly. Here, we limit ourselves to a simple exercise to shed light on the possible quantitative effects of DRWR on our results. Concretely, 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.

6 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 the strictness of the employment protection legislation (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. Regrettably, the data for institutional variables apply to the whole economy, and not to specific industry sectors. 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. On the other hand, if we do find an effect, it seems likely that this is due to the data capturing what we are after.

We do the analysis in two different ways. First, we allow the number of observed wage cuts within a country-year to depend on the number of observations (industries) in the country-year sample, S_{it}, thus estimating the effect of the explanatory variables on the empirical incidence rate of wage cuts. In the absence of DNWR, the institutional variables should not be able to explain the variation in the incidence rate. Second, we allow the number of observed wage cuts within a country-year to depend on the number of notional wage cuts, $\widehat{Y}_{it}$, thus estimating the effect of the explanatory variables on the fraction of notional 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

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

and

$$\ln\lambda_{it} = \mathbf{x}_{it}'\boldsymbol{\beta}, \tag{7}$$

where $E(Y_{it} \mid \mathbf{x}_{it}) = \lambda_{it}$, $\mathbf{x}_{it}$ represents the explanatory variables and $\boldsymbol{\beta}$ is the parameter vector. However, as the maintained hypothesis in the Poisson model of no overdispersion is rejected, we use a negative binomial regression model, which allows for overdispersion and can be seen as a generalisation of the Poisson model.[7] Specifically, we use two alternative specifications for the Poisson parameter:

$$\ln\lambda_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + \varepsilon_{it}, \quad \varepsilon_{it} \sim \Gamma(1,\delta) \tag{7'}$$

$$\ln\lambda_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + \varepsilon_{it}, \quad \varepsilon_{it} \sim \Gamma(1,\phi_i e^{-\alpha_i}) \tag{7''}$$

Including a Gamma distributed error term, ε_{it}, in (7') and (7'') allows the variance to mean ratios of Y_{it} to be larger than unity. (6) and (7') together yield the pooled negative binomial regression model. In (7''), 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

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[7]'Overdispersion' means that the variance in the data is greater than the mean, in contrast to the Poisson assumption that the variance and the mean are equal. Using a goodness-of fit test from a Poisson regression of Y_{it}/S_{it}, we reject no overdispersion with $\chi^2(416) = 634.6$.

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
$\text{Ln}(S_{it})$	1 (−)	1 (−)	−	−
Ln(Simulated cuts)	−	−	1 (−)	1 (−)
EPL	−0.291* (0.101)	−0.716* (0.196)	−0.104 (0.059)	−0.430 (0.293)
Union density	−0.606 (0.585)	−1.676 (0.964)	−0.966* (0.377)	−2.125 (1.423)
Inflation	−0.505* (0.073)	−0.364* (0.062)	−0.096* (0.048)	−0.041 (0.063)
Inflation squared	0.017* (0.003)	0.012* (0.003)	0.004 (0.002)	0.003 (0.003)
Unemployment	0.109* (0.029)	0.093* (0.036)	0.033* (0.016)	0.008 (0.036)
constant	−1.987* (0.454)	1.519* (0.762)	0.118 (0.243)	—
log-likelihood	−360.9	−287.5	−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. (iv) There are fewer observations in column 3 and 4 because the fraction of wage cuts realised are not defined in 140 country-years where the notional incidence rate is zero.

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 $exp(-0.716(-1.5)) = 2.9$. This would raise the incidence of wage cuts in Portugal from 0.7 percent to 2.0 percent. A reduction in union density in Denmark from 75 percent down to 25 percent (as in Germany and the Netherlands) is associated with an increase in the incidence rate from 1.7 to 3.9 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 1.4.

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] 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.

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

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. 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. (2006), 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 the 10 percent level.[9] While our analysis does not shed light on why Dickens et al. (2006) do not find any effect of EPL and unions on DNWR for individual workers, our discussion in section 2 above does suggest that EPL and unions will have stronger effect on DNWR in industry wages, exactly like we find in the Poisson regressions above. The reason for this is that the industry effect of DNWR for individual workers might be undone if there is flexible wage setting for other workers or in other firms. However, in country-years with strict EPL and unions, wage setting is unlikely to be that flexible. For example, a firm for which wages are set in a union contract would in most cases be unable to circumvent wage rigidity by replacing high wage workers with workers with lower wages.

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[9]Dickens et al. (2006) find that union density is positively associated with DRWR, significant at the 10 percent level.

7   Conclusions

Based on a novel method, we document the existence of downward nominal wage rigidity (DNWR) for manual workers in 19 OECD countries, over the period 1973–1999, using data for hourly nominal wages at 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 micro data, 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 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. (2006) does not find the same positive effect of EPL and union density on DNWR that we do. One possible explanation for this difference is if DNWR for individual workers can be undone by wage flexibility for other workers or in other firms, unless prevented by strict EPL and/or union contracts. For example, for a firm facing a union contract, it would be difficult to circumvent wage rigidities by replacing high wage workers by low wage workers. However, as we have not be able to test for fairness and morale explanations of DNWR, such hypotheses remain speculative. Furthermore, as argued by Holden (1994), these explanations are likely to be complementary in the sense that fairness and 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 are an exception; for this group, the fraction of wage cuts prevented is highest in the late 1990s. Most of this reduction in DNWR can be explained by the change in economic and institutional variables, by less strict employment protection legislation, 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.

In spite of these arguments, the effect of DNWR on employment and output is clearly an empirical issue of great interest for future research. In such work, we hope that our country-year specific estimates of DNWR may be useful.

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

Data for union density is from 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	0/19	–	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	1/25	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	1/17	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	–	–	3/24	–	–	1/17	12/22	0/25	6/22	0/23	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. Country-year specific empirical (top number) and notional (middle number) incidence rates and the associated p-value (bottom number).

Year	Austria	Belgium	Canada	Germany	Denmark	Spain	Finland	France	Greece	Ireland	Italy	Luxembourg	Netherlands	New Zealand	Norway	Portugal	Sweden	UK	US
1973		0.000		0.000	0.000		0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000			0.000	0.000
		0.000		0.000	0.010		0.000	0.000	0.021		0.000	0.049	0.000	0.000	0.003			0.003	0.001
		1.000		1.000	0.830		1.000	1.000	0.775		1.000	0.496	1.000	1.000	0.919			0.939	0.985
1974	0.000	0.000	0.000	0.043	0.000		0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000			0.000	0.000
	0.000	0.000	0.001	0.000	0.000		0.000	0.000	0.005		0.005	0.021	0.005	0.000	0.000			0.000	0.000
	1.000	1.000	0.982	1.000	1.000		1.000	1.000	0.934		0.881	0.743	0.918	1.000	1.000			1.000	1.000
1975	0.000	0.000	0.000	0.000	0.053		0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000			0.000	0.000
	0.005	0.005	0.000	0.013	0.009		0.000	0.000	0.007		0.000	0.021	0.000	0.000	0.005			0.000	0.007
	0.919	0.914	1.000	0.735	0.987		1.000	1.000	0.916		1.000	0.727	1.000	1.000	0.881			1.000	0.885
1976	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000			0.000	0.000
	0.006	0.026	0.000	0.011	0.000		0.000	0.000	0.000	0.008	0.011	0.006	0.000	0.000	0.000			0.000	0.005
	0.908	0.581	1.000	0.776	1.000		1.000	1.000	1.000	0.873	0.762	0.914	1.000	1.000	1.000			1.000	0.922
1977	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000			0.000	0.000
	0.000	0.008	0.000	0.000	0.010		0.041	0.000	0.005	0.026	0.000	0.044	0.000	0.000	0.000			0.005	0.000
	1.000	0.854	1.000	1.000	0.830		0.509	1.000	0.943	0.619	1.000	0.507	1.000	1.000	1.000			0.886	1.000
1978	0.000	0.048	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.133	0.000	0.000	0.000		0.000	0.000	0.000
	0.003	0.007	0.001	0.000	0.012		0.007	0.005	0.000	0.000	0.021	0.079	0.012	0.000	0.000		0.000	0.000	0.002
	0.953	0.991	0.982	1.000	0.795		0.897	0.905	1.000	1.000	0.600	0.891	0.785	1.000	1.000		1.000	1.000	0.960
1979	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000
	0.011	0.007	0.000	0.000	0.003		0.000	0.000	0.000	0.005	0.000	0.063	0.086	0.000	0.005		0.000	0.005	0.000
	0.844	0.867	1.000	1.000	0.942		1.000	1.000	1.000	0.914	1.000	0.376	0.180	1.000	0.863		1.000	0.890	1.000
1980	0.000	0.000	0.000	0.000	0.050		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.036		0.000	0.000	0.000
	0.005	0.001	0.000	0.000	0.094		0.000	0.000	0.000	0.012	0.000	0.055	0.000	0.041	0.035		0.000	0.013	0.000
	0.919	0.984	1.000	1.000	0.428		1.000	1.000	1.000	0.795	1.000	0.429	1.000	0.348	0.740		1.000	0.754	1.000
1981	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.133	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	0.023	0.000	0.000	0.000	0.000		0.005	0.000	0.000	0.000	0.000	0.131	0.001	0.000	0.001	0.035	0.000	0.005	0.000
	0.686	1.000	1.000	1.000	1.000		0.919	1.000	1.000	1.000	1.000	0.685	0.986	1.000	0.979	0.453	1.000	0.890	1.000
1982	0.000	0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	0.000	0.033	0.000	0.007	0.001		0.001	0.000	0.000	0.018	0.000	0.011	0.000	0.000	0.007	0.026	0.005	0.107	0.000
	1.000	0.494	1.000	0.850	0.985		0.988	1.000	1.000	0.695	1.000	0.834	1.000	1.000	0.827	0.556	0.881	0.084	1.000

Year	Austria	Belgium	Canada	Germany	Denmark	Spain	Finland	France	Greece	Ireland	Italy	Luxembourg	Netherlands	New Zealand	Norway	Portugal	Sweden	UK	US
1983	0.000	0.000	0.100	0.042	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.056	0.000	0.036	0.000	0.000	0.000	0.000
	0.006	0.013	0.227	0.022	0.026		0.000	0.000	0.008	0.023	0.000	0.055	0.095	0.000	0.106	0.008	0.000	0.007	0.038
	0.908	0.763	0.135	0.904	0.587		1.000	1.000	0.920	0.654	1.000	0.406	0.481	1.000	0.188	0.847	1.000	0.850	0.502
1984	0.000	0.000	0.036	0.037	0.000		0.000	0.000	0.000	0.000	0.000	0.063	0.188	0.000	0.036	0.000	0.000	0.000	0.000
	0.026	0.007	0.029	0.041	0.037		0.000	0.005	0.000	0.050	0.089	0.139	0.280	0.000	0.132	0.018	0.000	0.000	0.047
	0.661	0.867	0.810	0.700	0.472		1.000	0.890	1.000	0.400	0.106	0.327	0.303	1.000	0.099	0.670	1.000	1.000	0.424
1985	0.000	0.000	0.071	0.000	0.000		0.000	0.000	0.000	0.050	0.000	0.063	0.000	0.000	0.036	0.000	0.000	0.000	0.000
	0.012	0.012	0.068	0.051	0.013		0.000	0.000	0.005	0.092	0.000	0.095	0.079	0.000	0.055	0.041	0.000	0.000	0.019
	0.824	0.776	0.707	0.243	0.773		1.000	1.000	0.909	0.441	1.000	0.544	0.247	1.000	0.541	0.395	1.000	1.000	0.711
1986	0.000	0.286	0.179	0.000	0.100		0.000	0.087	0.111	0.048		0.000	0.111	0.000	0.000	0.000	0.000	0.000	0.000
	0.005	0.360	0.057	0.000	0.059		0.005	0.091	0.017	0.077		0.137	0.243	0.000	0.000	0.035	0.000	0.000	0.113
	0.919	0.322	0.996	1.000	0.888		0.919	0.651	0.997	0.509		0.128	0.151	1.000	1.000	0.453	1.000	1.000	0.116
1987	0.000	0.000	0.036	0.000	0.000		0.000	0.043	0.000	0.150		0.214	0.000	0.000	0.036	0.000	0.000	0.000	0.000
	0.056	0.143	0.143	0.006	0.000		0.000	0.001	0.005	0.138		0.107	0.083	0.000	0.000	0.033	0.000	0.007	0.026
	0.400	0.039	0.076	0.850	1.000		1.000	1.000	0.922	0.704		0.946	0.212	1.000	1.000	0.477	1.000	0.850	0.619
1988	0.063	0.143	0.000	0.000	0.000		0.000	0.217	0.000	0.050		0.214	0.111	0.000	0.000	0.000	0.000	0.000	0.000
	0.018	0.209	0.035	0.011	0.000		0.000	0.169	0.000	0.059		0.068	0.063	0.000	0.008	0.061	0.001	0.000	0.000
	0.967	0.333	0.374	0.736	1.000		1.000	0.819	1.000	0.671		0.988	0.899	1.000	0.793	0.251	0.979	1.000	1.000
1989	0.000	0.000	0.000	0.000	0.000		0.000	0.043	0.000	0.100		0.000	0.059	0.000	0.071	0.143	0.000	0.000	0.000
	0.074	0.000	0.026	0.021	0.063		0.035	0.001	0.005	0.122		0.024	0.020	0.048	0.140	0.026	0.000	0.000	0.014
	0.294	1.000	0.474	0.563	0.271		0.563	1.000	0.914	0.554		0.661	0.957	0.292	0.229	0.998	1.000	1.000	0.750
1990	0.000	0.000	0.071	0.000	0.000	0.000	0.000	0.043	0.000	0.048		0.063	0.000	0.040	0.179	0.000	0.000	0.000	0.000
	0.005	0.000	0.059	0.000	0.008	0.044	0.000	0.029	0.005	0.077		0.046	0.001	0.005	0.098	0.026	0.006	0.005	0.020
	0.919	1.000	0.776	1.000	0.847	0.308	1.000	0.861	0.881	0.509		0.835	0.987	0.992	0.948	0.537	0.845	0.893	0.674
1991	0.000	0.000	0.071	0.000	0.000	0.000	0.000	0.043	0.000	0.000		0.000	0.000	0.000	0.036	0.000		0.000	0.000
	0.001	0.021	0.071	0.000	0.050	0.009	0.011	0.012	0.007	0.086		0.012	0.031	0.005	0.168	0.005		0.014	0.000
	0.988	0.600	0.677	1.000	0.362	0.790	0.834	0.969	0.844	0.150		0.824	0.587	0.893	0.039	0.901		0.698	1.000
1992	0.000	0.000	0.115	0.000	0.050	0.000	0.000	0.000	0.040	0.000		0.000	0.000	0.000	0.107	0.000	0.077	0.000	0.000
	0.000	0.011	0.086	0.000	0.046	0.000	0.075	0.038	0.020	0.107		0.049	0.000	0.026	0.160	0.005	0.000	0.035	0.018
	1.000	0.771	0.817	1.000	0.767	1.000	0.287	0.407	0.915	0.094		0.427	1.000	0.524	0.324	0.886	1.000	0.407	0.695

Table A2: (continued)

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

Table A3: Indices for employment protection legislation, EPL

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

Table A4: Trade union density, percent

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

Table A5: Indices for bargaining coverage

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

Table A6: Indices of centralisation

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

B Results with country specific and period specific underlying distributions and country-year specific symmetric distribution

Table B1: Results with country specific and period specific underlying distributions and country-year specific symmetric distribution. The period specific underlying distributions are based on observations from the periods 1973–79, 1980–89, 1990–94 and 1995–1999 respectively.

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

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

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