Downward Nominal Wage Rigidity in Europe*

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First draft: 14th February 2003
This version: October 2004

Abstract

This paper explores the existence of downward nominal wage rigidity (DNWR) in the industry sectors of 16 European countries, over the period 1973–1999, using data for hourly nominal wages at industry level. Based on a novel nonparametric statistical method, which allows for country and year specific variation in both the median and the dispersion of industry wage changes, we reject the hypothesis of no DNWR. The fraction of wage cuts prevented due to DNWR has fallen over time, from 70 percent in the 1970s to 20 percent in the 1990s, but the number of industries affected by DNWR has increased. Wage cuts are less likely in countries and years with high inflation, low unemployment, high union density and strict employment protection legislation.

JEL: J5, C14, C15, E31
Keywords: Downward nominal wage rigidity, European countries, employment protection legislation

Preliminary version – UNDER REVISION

*We wish to thank Lars Holden and Tore Schweder for invaluable help in the formulation of the statistical methods that we use. We are also grateful to Bill Dickens, Mike Elsby, Christoph Knoppik, Alan Manning, Halvor Mehlum and seminar participants at ESEM2003, Norges Bank and University of Oslo for useful comments to earlier drafts. Views and conclusions expressed in this paper are those of the authors alone and cannot be attributed to Norges Bank.

1 Introduction

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

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

This paper explores the existence of DNWR in the industry sectors of 16 European countries, over the period 1973–1999, using data for hourly nominal earnings at industry level. The study is to be seen as complementary to the large number of micro studies, as it allows for comparisons across different groups of countries, and comparisons over time. More importantly, by using data for the hourly earnings at industry level, our study captures effects of changes in the composition of the workforce, as well as the effect of changes in the wage rates. Furthermore, our study covers a number of countries in Continental Europe, for which there so far is little available evidence of the existence of DNWR, in spite of the considerable policy importance of this issue in relation to the ambitious inflation target of the ECB.

To investigate the extent of DNWR, we construct a statistical method not previously used on this issue (at least to the best of our knowledge). The advantage of the method is that it uses much weaker assumptions than most previous analyses, implying that the results should be more robust. First, the method is based on a nonparametric analysis, using data for hourly earnings only, so that no assumptions concerning explanatory variables or specific functional forms are involved. Second, we allow for country and year specific variation in the median and the dispersion of wage changes, while most other tests are based on more restrictive assumptions.

To further explore the determinants of nominal wage ridigidy, we regress the incidence of nominal wage cuts in each country-year sample on economic and institutional variables, like inflation, unemployment, employment protection legislation, union density, etc.

The paper is organised as follows. In Section 2, we briefly present the main theoretical explanations for DNWR, and we refer to related empirical literature. The empirical approach is laid out in Section 3, while the empirical results on DNWR are documented in Section 4. In Section 5, we explore the determinants of nominal wage rigidity. Section 6 concludes. The data we use are described in the Appendix.

2 Theoretical framework and related literature

In the literature, two alternative explanations of the existence of DNWR have been proposed.[1] The most common explanation, advocated by e.g. Blinder & Choi (1990) and Akerlof et al. (1996), is that employers avoid nominal wage cuts because both they and (in particular) the employees think that a wage cut is unfair. The other explanation, proposed by MacLeod & Malcomson (1993) in a individual bargaining framework, and Holden (1994) in a collective agreement framework, is that nominal wages are given in contracts that can only be changed by mutual consent. Both these theories predict that nominal wage cuts will be prevented in some, but not all circumstances. For our purposes, there is no need to distinguish between these two explanations of DNWR, and, as argued by Holden (1994), they are likely to be complementary.

Empirical work on DNWR have grown rapidly in recent years, with various types of evidence. Blinder & Choi (1990), Akerlof et al. (1996), Bewley (1999) and Agell & Lundborg (2003) report results from interviews and surveys of employees and employers. A few papers document the existence of DNWR on aggregate data, see Holden (1998) and Fortin & Dumont (2000). However, the great majority of studies explore large micro-data sets, following either two types of approaches. The first type, initiated by the skewness-location approach of McLaughlin (1994), focuses on the effect of inflation on the distribution of wage changes; Christofides & Leung (2003), Lebow et al. (2003) and Nickell & Quintini (2003) are recent applications. The second type, referred to as the "earnings function approach" by Knoppik & Beissinger (2003), add other explanatory variables that are usually included in wage equations, see e.g. Fehr & Gotte (2003) and Altonji & Devereux (2000). Our study is of the first type, thus a brief discussion of this method is warranted. As is well known (see e.g. discussion in Knoppik & Beissinger (2003) or Nickell & Quintini (2003)), the validity of variants within this type of approach rests of various restrictive additional assumptions concerning the underlying or *notional* distribution of wage changes (fol-

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

lowing the terminology of Akerlof et al. (1996)), i.e. the wage changes that would prevail in the absence of DNWR. The LSW statistic, suggested by Lebow et al. (1995), requires that the notional nominal wage change distribution is symmetric. The Kahn test (Kahn, 1997) allows for asymmetry of the notional wage change distribution, as long as the shape of the notional distribution is invariant to inflation, i.e. the only effect of inflation on the distribution of wage changes comes in the form of DNWR. As illustrated in figure 2 below, the wage change distribution is asymmetric in our data, and dispersion changes over time (as does inflation), so both these methods are problematic in our case. The Nickell & Quintini (2003) method is based on the assumption (or approximation) that the probability of a nominal wage cut is a quadratic function of the median wage change.

In general these studies document that nominal wages are rigid downwards. However, with the exception of Dessy (2002), different methods and data in the above-mentioned studies make it in general difficult to compare the degree of downward nominal wage rigidity across countries.[2]

3 Empirical approach

We use an unbalanced panel of industry level data for the annual growth rate of gross hourly earnings for manual workers from the manufacturing, mining and quarrying, electricity, gas and water supply, and construction sectors of 16 European countries in the period 1973–1999. The countries included in the sample are Austria, Belgium, Germany, Denmark, Spain, Finland, France, Greece, Ireland, Italy, Luxembourg, Netherlands, Norway, Portugal, Sweden and the UK. The main data source for wages are harmonized hourly earnings from Eurostat.[3] The observational unit 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 7650 observations distributed across $N = 379$ country-

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

[3] Data for Austria, Finland and Sweden are from the ILO, while data for Norway is from Statistics Norway.

year samples, on average 20 industries per country-year. More details on data are provided in the appendix.

Before proceeding, let us first note that an observation of a nominal wage cut in our data differs in several respect from an observation of a nominal wage cut in most studies based on micro data. In micro studies, a nominal wage cut is usually understood as a reduction in hourly nominal pay for a job stayer. In our data, covering average hourly earnings for manual workers in an industry, a wage cut might be caused by a reduction in average hourly pay for job stayers, but it might also be caused by changes in the composition of the workers, within firms or between firms. Thus, our data involves considerable 'noise' relative to observations at the individual level, so we are unlikely to uncover all the rigidity that may exist at the individual level. Yet precisely because our data also captures other 'avenues' for flexibility, it may yield a better measure of rigidity at industry level. Furthermore, estimates based on individual data for job stayers may also be biased due to self selection, if employees whose wage is cut may quit, and thus no longer be job stayers. On the other hand, micro data studies have an advantage in a much larger number of observations, with the possibility of controlling for other explanatory variables. Overall, it seems worthwhile to explore DNWR with both types of data.

There are no nominal wage cuts in 295 (78%) of the country-year samples. In our data we observe, however, no less than 217 events of nominal wage reductions, i.e., 2.84% of all observations. There were fewer wage cuts in the 1970s, early 1980s and early 1990s, while most wage cuts occured after 1992, cf. figure 1. Table A1 in the Data Appendix reports the distribution of wage cuts and observations across countries and years.

As an illustration figure 2 displays box plots of annual wage growth in Portugal, as well as a histogram of the wage changes in 28 industries in Portugal in 1998. We see that the average and the dispersion of wage growth vary over time. The histogram for 1998 seems consistent with the idea that DNWR has prevented some nominal wage cuts, compressing the empirical

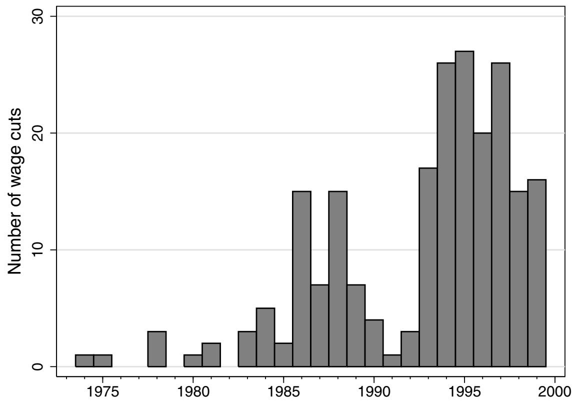

Figure 1: The number of wage cuts over time.

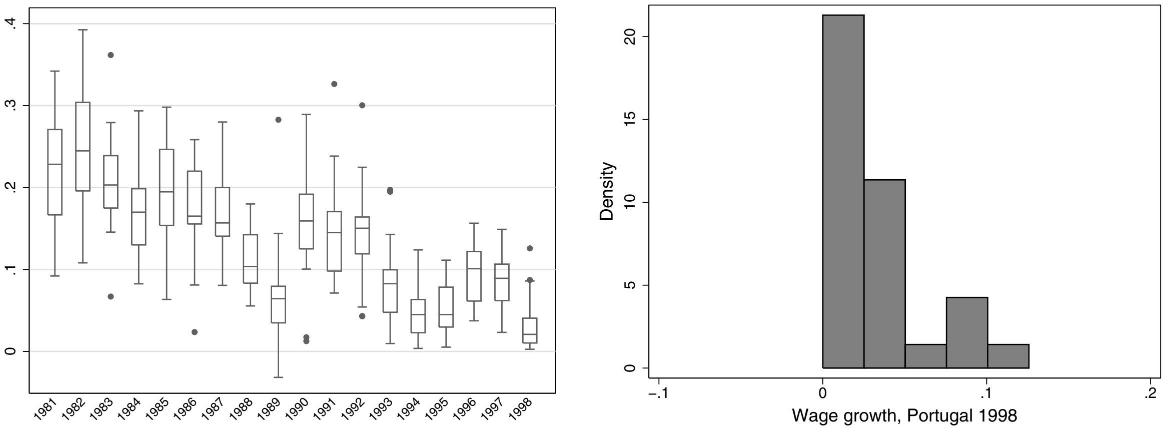

Figure 2: Box plots of annual wage growth in Portugal (left) and histogram of annual wage growth in 1998 (right). The box plot illustrates the distribution of wage changes within a country-year. The box extends from the 25th to the 75th percentile with the median inside the box. The whiskers emerging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

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

To detect whether the empirical distribution is compressed relative to the notional, we must obtain an estimate for the notional distribution, as well as compare the notional distributions with the empirical outcomes. We estimate the shape of the notional distribution on the basis of all observations for the period 1973–1992, assuming the same shape in all country-years, except that we allow for the median and dispersion to differ across country-year samples. The estimated shape may also be affected by DNWR, but this effect should be small given that we only use observations from the high-inflation years where DNWR is less likely to be binding. Alternatively, we could have assumed that the notional distribution was normal, however, as illustrated in Figure 3 below, this would not be a good approximation.

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

3.1 The formal test

As mentioned above, our test is based on the assumption that the median and dispersion of the wage change distribution may vary among country-year samples, but otherwise the shape of the distribution is the same (in the absence of possible DNWR). To ensure robustness to outliers, we measure dispersion by the inter quartile range (i.e. the difference between the $75th$ percentile and the $25th$ percentile) rather than the standard deviation (for the same reason, we use the median rather than the mean). Under these assumptions, we obtain an underlying distribution of wage changes based on the sample of 5726 empirical wage change observations for the period 1973–92,[4] where the empirical wage changes are adjusted for the country-year specific median (μ_{it}) and inter quartile range IQR_{it}, i.e.

$$\Delta w_s^n \equiv \left(\frac{\Delta w_{jit} - \mu_{it}}{IQR_{it}} \right), \qquad s = 1, \ldots, 5726 \tag{1}$$

[4]We also tried country-specific normalised distributions, but this had little impact on the qualititative results.

For simplicity we use subscript s which runs over all j, i and $t = 1973, \ldots, 1992$.

The country-year specific samples of *notional* wage changes are constructed on the basis of the underlying wage change distribution 1, by adjusting the underlying normalised wage changes, Δw_s^n, with the country-year specific median and inter quartile range. However, as the country-year specific $25th$ percentiles may be affected by DNWR, leading to a downward bias in the inter quartile ranges, we estimate the inter quartile range by two times the difference between the $75th$ percentile and the median. In effect, we assume that the notional wage change distribution is symmetric with respect to the $25th$ and $75th$ percentiles.[5] However, in the appendix, we also report results without this assumption, where we use the observed inter quartile range to calculate the country-specific notional distribution, cf. the third bullet point below. In the appendix, we also report results based on country-specific underlying distribution, i.e. where we estimate separate Δw_s^n for each country. Under both these alternatives, the qualitative results are similar to those reported in the main text, although the evidence of DNWR is somewhat weaker, as would be expected as these alternatives are more sensitive to a downward bias due to DNWR.

Figure 3 compares the underlying distribution of wage changes with the standard normal distribution; we notice that the underlying distribution is skewed with the mean at 2.2 percent. The right panel of figure 3 compares the country-specific notional distribution for Portugal 1998, i.e the underlying distribution after adjustment for the mean and inter quartile range in Portugal 1998, with the empirical distribution. We observe that country-specific notional distribution indicates a considerable probability of negative wage changes, in contrast to the empirical outcome.

One complication is that the empirical samples, as well as the moments based on them, are stochastic and thus burdened with unknown uncertainty. To allow for that, we use a bootstrap

[5]This is a much weaker assumption than assuming complete symmetry of the notional distribution, as used by LSW 1995 and Card & Hyslop (1997). While Card & Hyslop (1997) point out that most conventional models of wage determination imply symmetry, Elsby (2004) shows that DNWR is likely to affect also the upper tail of the distribution, as wage setters may set lower wage increases in years where DNWR does not bind, to reduce the risk that DNWR will bind in the future.

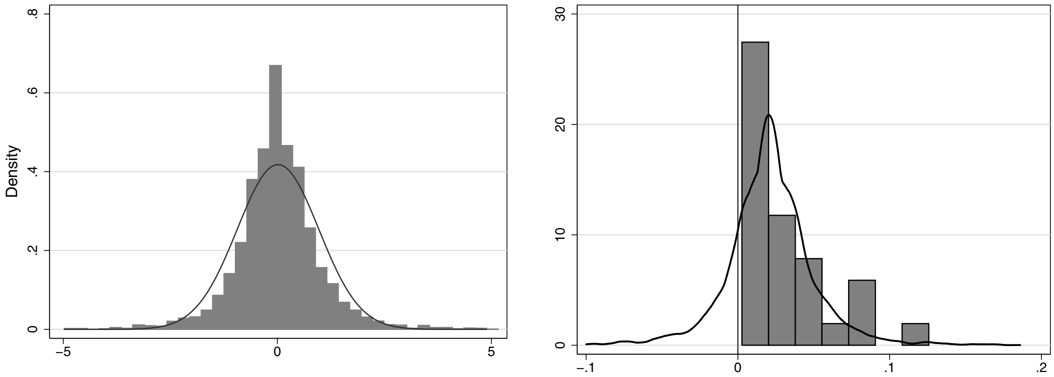

Figure 3: Left: Histogram and normal density (solid line) of the normalised underlying distribution of wage growth. 20 extreme observations are omitted. Right: Histogram of observed wage growth and notional wage distribution in Portugal 1998.

method. More specifically, for each of the 379 country-year samples, we bootstrap the empirical country-year sample (for example, in a country-year with 24 observations, we make 24 random draws from the empirical sample of 24 industry wage changes, with replacement). Then we

- count the number of bootstrapped wage cuts in the country-year, y_{it}^{B},
- calculate country-specific bootstrapped median, μ_{it}^{B}, and $75th$ percentile, $P75_{it}^{B}$,
- construct the country-year specific distribution of notional wage changes by adjusting the underlying wage change distribution for the country-specific bootstrapped median and $75th$ percentile

$$\Delta\widetilde{w}_{s}^{it} \equiv \Delta w_{s}^{n}\left(2(P75_{it}^{B} - \mu_{it}^{B})\right) + \mu_{it}^{B}, \qquad s = 1, \ldots, 5726 \tag{2}$$

- calculate the corresponding country-year specific probabilities of a notional wage cut in country-year it as the proportion of notional wage cuts out of the total sample of observations $S = 5726$

$$\tilde{q}_{it} \equiv \frac{\#\Delta\widetilde{w}_{s}^{it} < 0}{S}, \qquad s = 1, \ldots, 5726 \tag{3}$$

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

- compare the total number of bootstrapped wage cuts $Y^B = \sum_{it} y^B_{it}$ for all 379 country-year samples with the total number of simulated notional wage cuts, $\widehat{Y} = \sum_{it} \widehat{y}_{it}$.

If the empirical samples are affected by DNWR, there will be a tendency that there are more simulated wage cuts than bootstrapped wage cuts, i.e. $\widehat{Y} > Y^B$. We therefor repeat this procedure 5000 times, undertaking a new bootstrap for each country-year sample each time, and count the number of times where $\widehat{Y} > Y^B$. The null hypothesis is rejected with a level of significance at 5% if $1 - \#(\widehat{Y} > Y^B)/5000 \leq 0.05$.

Note that if the notional distributions are correctly estimated, 5000 simulations will ensure a close approximation to the distribution of the total number of wage cuts if there were no DNWR. [6] Thus, the significance level of our test should be reliable. However, if DNWR is at work in some country-year samples, the empirical wage distribution will be compressed, and so will our estimates of the underlying and notional wage changes, as these are based on the empirical distributions for all country-year samples. Thus, the notional probabilities will also be biased downwards, reducing the number of simulated wage cuts, which will reduce the power of our test.

4 Results

There are more simulated than bootstrapped wage cuts in all 5000 simulations. Thus we reject the null hypothesis comfortably with a p-value of 0, and we may conclude that DNWR has been at work in our sample. To illustrate the power of the test we plot the histogram of the number of simulated and bootstrapped wage cuts in Figure 4. On average, we simulate 304 notional

[6] Given the notional country-years specific distributions it would in principle be straightforward to calculate the probability distribution function for the total number of wage cuts by use of a formulae for draws from multinomial distributions. However, with 7650 observations, this is computationally very demanding. Simulation is computationally simpler, allows for bootstrapping, and still accurate.

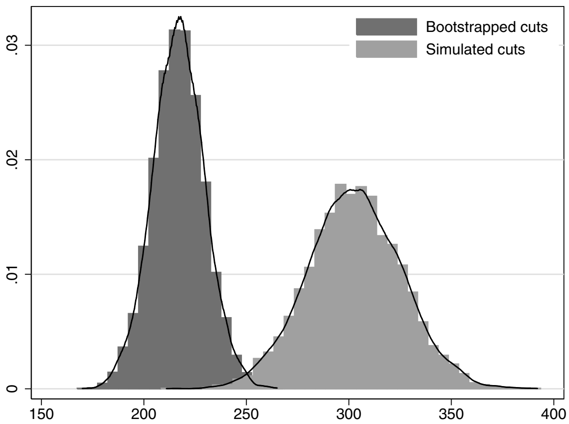

Figure 4: The frequency distributions of the number of 5000 bootstrapped (empirical) and simulated (notional) wage cuts.

wage cuts and bootstrap 217 wage cuts (due to the large number of simulations, the bootstrapped average of 217 clearly equals the number of observed wage cuts). The average fraction of notional wage cuts that do not result in an observed wage cut due to DNWR, may be expressed by $(1-Y/\widehat{Y})$ where Y is the number of observed wage cuts and $\widehat{Y}$ is the average number of simulated cuts. For the whole sample this fraction is $(1-217/304)=0.29$. Thus, a bit more than one out of five notional wage cuts does not result in an observed wage cut due to DNWR. Another measure which illustrates the economic significance of DNWR is the average fraction of industry-years affected by DNWR. This fraction may be calculated by $(\widehat{Y}-Y)/S$ where S is the number of industry-years. For the whole sample this fraction is $(304-217)/7650=0.011$.

A number of interesting questions arise. Is there evidence for DNWR for different time periods, regions and countries? To what extent is DNWR related to labour market institutions as proposed by theory? We first investigate whether DNWR has changed over time by splitting the sample into four subperiods 1973–1979, 1980–1989, 1990–1994 and 1995–1999, see Table 1.

There is evidence of DNWR in all periods. In the high-inflation 1970s, on average almost 70 percent of the notional wage cuts did not result in observed wage cuts. In the 1980s and early 1990s, about 30 percent of the notional wage cuts did not result in observed wage cuts, while in the late 1990s, the probability that DNWR prevented a notional wage cut leading to an observed

Table 1: Results from 5000 simulations on subperiods using bias adjusted probabilities.

Sample properties:	1973–1979	1980–1989	1990–1994	1995–1999
No. of observations	1754	3016	1546	1334
No. of country-years	89	153	75	62
Average wage growth	14.27%	8.92%	6.22%	4.31%
Average inflation rate	10.33%	8.21%	4.75%	2.27%
Average unemployment rate	3.49%	7.30%	8.38%	8.37%
Observed wage cuts (Y)	5	57	51	104
Proportion of wage cuts (%)	0.29	1.89	3.30	7.80
Simulation results:				
Average simulated wage cuts	16	89	72	127
$\#(\widehat{Y} > Y^B)$	4911	4950	4856	4672
Probability of significance	0.018	0.010	0.029	0.066
Fraction of wage cuts prevented	0.687	0.360	0.295	0.181
Fraction of industry-years affected	0.006	0.011	0.014	0.017

wage cut was 18 percent. While the results indicate that the fraction of wage cuts prevented by DNWR decreased over time, the average fraction of industry-years affected by DNWR increased from 0.6 percent in the 1970s to 1.1 percent in the 1980s, 1.4 percent in the early 1990s and finally 1.7 percent in the late 1990s.

Nominal rigidities may be related to 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 depend on the coverage of collective agreements and union density. For non-union workers, the strictness of the employment protection legislation (EPL) is key to their possibility of avoiding a nominal wage cut. As documented by among others OECD (1999), such institutions differ considerably among European countries, and it would therefore be interesting to investigate existence of DNWR for regions as well as individual countries.

We first split the sample into regions which have comparable labour market institutions. We operate with four regions; the British Isles, Core (Austria, Belgium, France, Germany, Luxem-

Table 2: Results from 5000 simulations on regions using bias adjusted probabilities.

Sample properties:	All regions	British Isles	Core	Nordic	South
No. of observations	7650	1078	3110	1984	1478
No. of country-years	379	49	158	97	75
Observed wage cuts (Y)	217	45	125	18	29
Proportion of wage cuts (%)	2.84	4.17	4.02	0.91	1.96
Simulation results:					
Average simulated wage cuts	304	58	161	35	50
$\#(\widehat{Y} > Y^B)$	5000	4374	4917	4913	4894
Probability of significance	0	0.125	0.017	0.017	0.021
Fraction of wage cuts prevented	0.287	0.224	0.226	0.483	0.421
Fraction of industry-years affected	0.011	0.012	0.012	0.008	0.014

bourg and the Netherlands), the Nordic Region (Denmark, Finland, Norway and Sweden) and South (Italy, Greece, Portugal and Spain). The results from simulations using these regions are presented in columns 2–5 in Table 2.

We reject the hypothesis of no DNWR for all regions, but the British Isles. In the South, a region where bargaining coverage is fairly high (see e.g. Calmfors et al., 2001, table 4.4) and EPL is very strict OECD (1999), 42 percent of the notional wage cuts did not result in an observed cut, while 1.4 percent of the industries were affected by DNWR. In the Core, where there is generally high bargaining coverage and fairly strong EPL, 23 of the notional wage cuts did not result in observed cuts, which is considerably lower than in the South. In the British Isles, 22 percent of the notitional wage cuts were prevented by DNWR. In this region, EPL is less strict than in most of the rest of Europe; however, union density and bargaining coverage are fairly high in Ireland, but not in the UK. 1.2 percent of the industries were affected by DNWR in the Core, and the British Isles. In the Nordic region 48 percent of the notitional wage cuts were prevented by DNWR.

Splitting the sample by combining the regions and the sub-periods reduces the significance levels, see Table 3. These results should be treated more cautiously, as they are based on a smaller number of observations. At the ten percent level, we find significant DNWR in the Core (1970s

Table 3: Results from 5000 simulations on regions and sub-periods using bias adjusted probabilities.

Region		1973–1979	1980–1989	1990–1994	1995–1999
	No. of observations	228	432	227	191
	No. of country-years	11	20	10	8
British	Observed wage cuts (Y)	0	8	17	20
Isles	Proportion of wage cuts (%)	0	1.85	7.49	10.47
	Average simulated wage cuts	2	14	22	20
	$\#(\widehat{Y} > Y^B)$	3709	4303	3891	2200
	Probability of significance	0.258	0.139	0.222	0.560
	Fraction of wage cuts prevented	1	0.440	0.237	0
	Fraction of industry-years affected	0.007	0.015	0.023	0
	No. of observations	794	1183	587	546
	No. of country-years	41	60	30	27
	Observed wage cuts (Y)	4	40	18	63
Core	Proportion of wage cuts (%)	0.50	3.38	3.07	11.54
	Average simulated wage cuts	10	57	24	70
	$\#(\widehat{Y} > Y^B)$	4513	4616	4102	3754
	Probability of significance	0.097	0.077	0.180	0.249
	Fraction of wage cuts prevented	0.604	0.293	0.267	0.104
	Fraction of industry-years affected	0.008	0.014	0.011	0.013
	No. of observations	474	888	362	260
	No. of country-years	23	40	20	14
	Observed wage cuts (Y)	1	3	12	2
Nordic	Proportion of wage cuts (%)	0.21	0.34	3.31	0.77
	Average simulated wage cuts	2	8	16	8
	$\#(\widehat{Y} > Y^B)$	2950	4503	3716	4704
	Probability of significance	0.410	0.099	0.257	0.059
	Fraction of wage cuts prevented	0.531	0.647	0.252	0.757
	Fraction of industry-years affected	0.002	0.006	0.011	0.024
	No. of observations	258	513	370	337
	No. of country-years	14	33	15	13
	Observed wage cuts (Y)	0	6	4	19
South	Proportion of wage cuts (%)	0	1.17	1.08	5.64
	Average simulated wage cuts	2	10	9	29
	$\#(\widehat{Y} > Y^B)$	4074	3788	4344	4329
	Probability of significance	0.185	0.242	0.131	0.134
	Fraction of wage cuts prevented	1	0.384	0.580	0.340
	Fraction of industry-years affected	0.008	0.007	0.015	0.029

Table 4: Results from 5000 simulations on countries using bias adjusted probabilities.

Country	No. of observations	No. of years	Observed wage cuts (Y)	Prop. of wage cuts (%)	Average simulated wage cuts	$\#(\widehat{Y} > Y^B)$	Probability of significance	Fraction of wage cuts prevented	Fraction of industry-years affected
Austria	408	26	2	0.49	7	4621	0.076	0.721	0.013
Belgium	575	26	31	5.39	42	4683	0.063	0.268	0.020
Denmark	464	25	8	1.65	14	4191	0.162	0.426	0.013
Finland	368	23	2	0.54	6	4216	0.157	0.656	0.010
France	556	26	21	3.78	19	1740	0.652	0	0
Germany	665	26	16	2.41	18	2878	0.424	0.114	0.003
Greece	469	26	7	1.49	7	2194	0.561	0.016	0.000
Ireland	463	23	27	5.83	36	4210	0.158	0.260	0.020
Italy	312	13	0	0	3	4430	0.114	1	0.010
Luxembourg	423	27	32	7.57	39	3877	0.225	0.182	0.017
Netherlands	483	27	23	4.76	35	4717	0.057	0.351	0.026
Norway	674	27	2	0.30	4	3243	0.351	0.469	0.003
Portugal	411	18	3	0.73	22	4999	0.000	0.863	0.046
Spain	286	18	19	6.64	18	1958	0.608	0	0
Sweden	478	22	6	1.26	11	4471	0.106	0.472	0.011
UK	615	26	18	2.93	22	3427	0.315	0.162	0.006

and 1980s) and the Nordic region (1980s and late 1990s). For all regions with the exeption of the Nordic region, the fraction of notional wage cuts that did not lead to observed cuts has fallen over time, consistent with the aggregate picture as seen in Table 1. The fraction of industry-years affected by DNWR has increased the Nordic region, the South and the British Isles.

In Table 4, we report the results concerning individual countries. Bearing in mind that that the results are based on fewer observations, we observe that for all countries except France and Spain, the simulations indicate that some of the notional wage cuts do not result in observed wage cuts due to DNWR. For four countries (Austria, Belgium, the Netherlands, and Portugal), DNWR is significant at the ten percent level. For the other countries, DNWR is not statistically significant, even if the average fraction of notional wage cuts that do not result in observed cuts for some countries is more than 40 percent in the Nordic countries and as high as 11 percent for

Germany and 16 percent for the UK. This illustrates the considerable uncertainty involved in this measure. The fraction of industry-years affected by DNWR varies from 4.6 (Portugal) percent at the top, to 0 (France and Spain) percent at the bottom.

To further explore the reliability of our measures of DNWR, we undertake Poisson regressions with the number of observed wage cuts in each country-year sample it, Y_{it}, as the dependent variable, and normalise on the number of simulated wage cuts, $\widehat{Y}_{it}$. A Poisson regression seems appropriate as the endogenous variable is based on count data, see Cameron & Trivedi (1998). Adding dummys for region, period, combined region and period, as well as for countries, we are then able to derive confidence intervals for the fraction of wage cuts prevented for all the respective subsamples, see Figure 5. Note that the point estimates of the fractions in Figure 5 differ slightly from the fractions in the tables, as the former are based on the Poisson regressions, and thus are non-linear, while the latter are linear averages based on the simulations. The confidence intervals are fairly large, and with few exceptions, we are not able to conclude that the fractions are significantly different from one another.

However, we also undertake a Poisson regression of Y_{it}, as the dependent variable, normalising on $\widehat{Y}_{it}$, and adding a time trend. The estimated trend coefficient is 0.035 and is significantly positive at the one percent level, implying that we can conclude that DNWR as measured by the fraction of wage cuts prevented, has fallen over time. Furthermore, we also regress the country-year observations of the fraction of industry-years affected, $(\widehat{Y}_{it} - Y_{it})/S_{it}$ on a time trend (now using OLS, as a Poisson regression is not feasible when some observations are negative). We find a trend coefficient of 0.010 with a p-value of 5.2 percent, indicating that the number of industries affected by DNWR has increased over time.

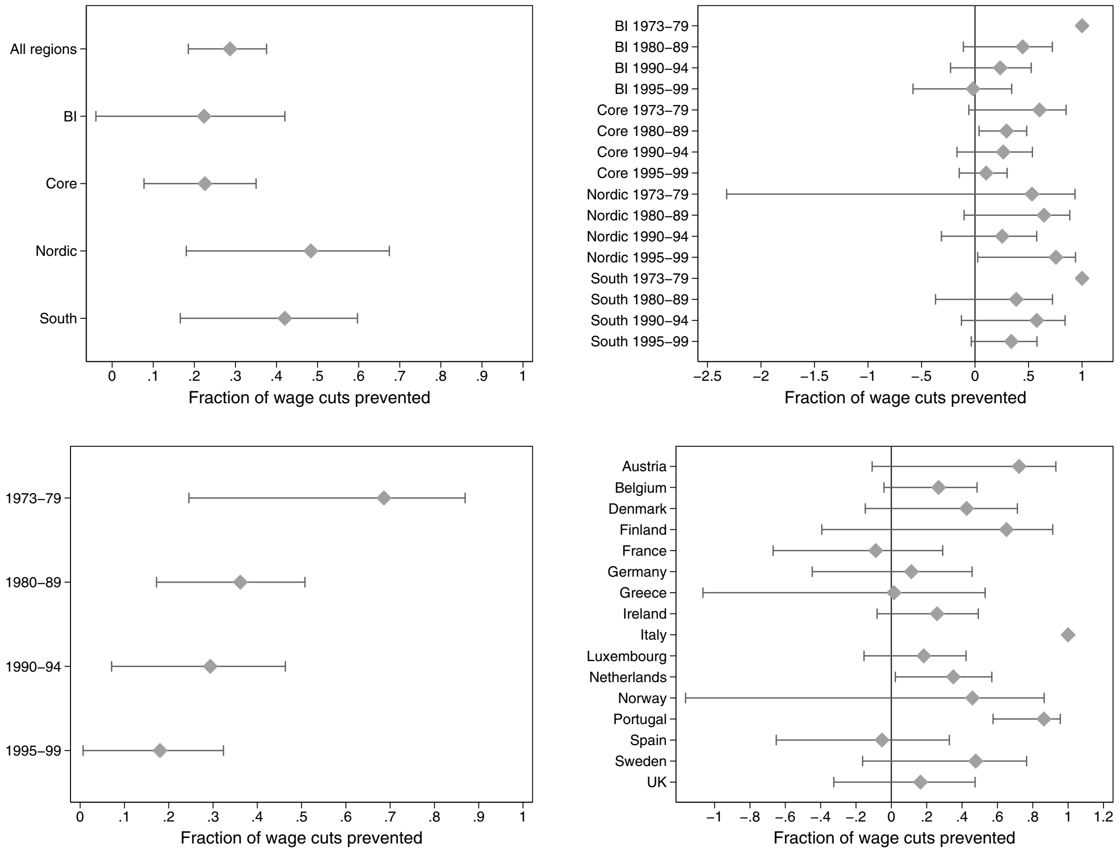

Figure 5: Estimated fractions of wage cuts prevented with 95% confidence intervals.

5 Explaining the number of wage cuts

While the previous analysis documents the existence of DNWR, it does not shed light on to what extent the incidence of nominal wage cuts depends on economic and institutional variables. Treating the number of wage cuts in each country-year sample as one observation, we have 379 observations. As mentioned above, Holden (2004) shows that the incidence of wage cuts is likely to depend on inflation in a non-linear way, as well as on institutional variables like EPL and union density/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, with a number of explanatory variables including inflation and inflation squared, an index of EPL, union density, the unemployment rate, as well as an interaction between EPL and inflation.[7] We do the analysis in two different ways. First, we normalise on the number of industries in the country-year sample, S_{it}, i.e. we explain the incidence of wage cuts. Second, we normalise on the number of simulated wage cuts, $\widehat{Y}_{it}$, i.e. we explain the fraction of wage cuts prevented. Adding institutional variables as regressors, we can then test directly whether these variables lead to fewer observed that notional wage cuts, i.e. to DNWR.

The conditional density in a Poisson model is

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

where $E(Y_{it} \mid \mathbf{x}_{it}) = \lambda_{it}$, and

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

where $\mathbf{x}_{it}$ represents the explanatory variables and $\boldsymbol{\beta}$ is the parameter vector. In the Poisson model the variance is equal to the mean. However, data are often characterised by 'overdispersion' and hence at odds with the Poisson assumption. Undertaking the Poisson regression of Y_{it}/S_{it}, a goodness-of fit test formally rejects the hypothesis that the data are generated according to the Poisson regression model ($\chi^2(254) = 389.6$). We therefore use a negative binomial regression model, which allows for overdispersion and can be seen as a generalisation of the Poisson model. Specifically, we use two alternative specifications for the Poisson parameter:

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

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

Including a Gamma distributed error term, ε_{it}, in (5') and (5") allows the variance to mean ratios of Y_{it} to be larger than unity. (4) and (5') yield the pooled negative binomial regression model.

[7]Regrettably, the data for union density and bargaining coverage apply to the whole economy, and not to the industry sector. As variation in density or coverage in other parts of the economy would affect the density/coverage variable, but presumably not affect wage setting in the industry sector, the estimates of these variables might be biased downwards. See further details in the Data Appendix.

Table 5: Maximum likelihood estimates with standard errors in parenthesis.

	Negative binomial		Poisson	
	Pooled	Fixed effects	Pooled	Fixed effects
Ln(S_{it})	1 (–)	1 (–)	–	–
Ln(Simulated cuts)	–	–	1 (–)	1 (–)
EPL	−0.836* (0.300)	−1.004* (0.365)	−0.337* (0.136)	−0.697* (0.327)
Union density	−1.306 (0.709)	−2.217 (1.169)	−0.944* (0.431)	−2.725 (2.989)
Inflation	−0.848* (0.189)	−0.565* (0.145)	−0.248* (0.099)	−0.287* (0.129)
Inflation squared	0.009 (0.005)	0.005 (0.005)	0.001 (0.004)	−0.002 (0.004)
Unemployment	0.095* (0.031)	0.104* (0.041)	0.035* (0.017)	0.034 (0.043)
EPL × inflation	0.149* (0.066)	0.106 (0.056)	0.057 (0.034)	0.104* (0.046)
constant	2.676 (0.935)	2.080 (1.306)	0.737 (0.457)	–
log-likelihood	−253.6	−203.9	−183.3	−143.6
Number of observations	352	339	352	339

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 these countries.

In (5”), 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 are presented in the first two columns of Table 5. 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 in the fixed effects model, union density is only significant at the 10 percent level. High unemployment increases the incidence of wage cuts. The interaction between EPL and inflation is significantly positive in the fixed effects model, reflecting that the negative effect of EPL on the incidence of wage cuts do not apply in high-inflation periods, when wage cuts are rare irrespective of EPL. In an earlier version we also tried to include bargaining coverage, an index of temporary employment and the interaction of inflation and union density in the fixed effects model. They all entered with the expected sign, but they were jointly insignificant with a $\chi^2(3) = 2.37$.

We then investigate whether institutions affect the extent of DNWR as measured by the average fraction of wage cuts prevented $(1 - Y/\widehat{Y})$, 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 5. Note that in this case the restriction imposed by the Poisson regression relative to the negative binomial regression is accepted easily; indeed the results are the same in the negative binomial model for both specifications.[8] Again, we find a significant negative effect on the number of wage cuts, implying a positive effect of EPL and union density on the fraction of wage cuts prevented. Also, there is a positive effect of inflation, which implies that there are fewer wage cuts prevented when inflation is lower. In the fixed effects model, union density is insignificant. In an earlier version we also tried to include bargaining coverage, an index of temporary employment and the interaction of inflation and union density in the fixed effects model. They all entered with the expected sign, but they were jointly insignificant with a $\chi^2(3) = 1.37$.

6 Conclusions

This paper explores the existence of downward nominal wage rigidity (DNWR) in the manufacturing, mining and quarrying, electricity, gas and water supply, and construction sectors of 16 European countries, over the period 1973–1999, using data for hourly nominal wages at industry level. Based on a novel nonparametric statistical method, which allows for country and year specific variation in both the median and the dispersion of industry wage changes, we reject the hypothesis of no DNWR for the total sample. Splitting into subsamples, we document the existence of DNWR for the high inflation period 1973–1989, as well as for the low inflation periods 1990–1994 and 1995–1999. Furthermore, we also find evidence for DNWR for groups of countries: the South (Italy, Greece, Portugal, Spain), the Core (Austria, Belgium, France, Germany, Luxembourg, Netherlands), and the Nordic region (Denmark, Finland, Norway and Sweden), but not the British Isles. Dividing further into individual countries, the results indicate that, for all countries except France and Spain, some of the notional wage cuts do not lead to observered wage cuts due to DNWR. However, DNWR is statistically significant only for some of the coun-

[8] The goodness-of-fit test yields $\chi^2(254) = 133.0$.

tries: for Austria, Belgium, the Netherlands, and Portugal at ten percent level.

Interestingly, our results show that the fraction of notional wage cuts that do not result in observed wage cuts has fallen over time, for all the groups of countries we consider. The simulations indicate that for all countries together, the fraction of wage cuts prevented has fallen from 70 percent in the 1970s to 20 percent in the late 1990s. On the other hand, as the inflation has fallen over time, the fraction of industry-years affected by DNWR has increased from less than 0.6 percent in the 1970s, to 1.7 percent in the late 1990s.

We then proceed to explore whether the incidence of nominal wage cuts can be explained by economic and institutional variables. Treating the incidence of nominal wage cuts in each country-year sample as one observation, we find significant negative effect of inflation, the strictness of employment protection legislation and of union density. We also find that inflation, the strictness of employment protection legislation and union density have significant positive impact on our measure of DNWR: in country-year samples with high inflation, strict employment protection legislation and high union density, the number of observed wage cuts is significantly reduced relative to the number of simulated, notional wage cuts.

Our study should be seen as complementary to the increasing number of empirical studies on the existence of DNWR based in individual data. Compared to these studies, our approach has the advantage that it focusses on industry level effects, and thus is not subject to the critique that significant DNWR at individual or firm level might be circumvented by employment being shifted over from high-wage to low-wage jobs. In comparison, Card & Hyslop (1997) find evidence of DNWR on US microdata, but inconclusive evidence for state level data. On the other hand, as we (obviously) have much fewer observations than most micro-studies, and use weak assumptions – no functional form assumption, and allowing for time and country variation in the median and dispersion of wage changes – our test presumably has lower power. Indeed, Knoppik & Beissinger (2003) find significant DNWR for Germany, while we do not.

We are reluctant to draw strong policy conclusions from our study. Overall in our sample, DNWR is significant but of moderate size. Labour markets appear to adapt to lower inflation, as 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, suggesting that the overall effect on DNWR of a more determined effort towards low inflation, as the monetary policy of the ECB arguably implies, are uncertain.

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A Data appendix

We have obtained our wage data from Eurostat. 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. 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.

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.

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

The primary sources for the employment protection legislation index, which is displayed in Table A2, are Lazear (1990) for the period 1973–79 and OECD (1999) for the remaining years. We follow the same procedure as Blanchard & 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, and use the percentage change in Lazear's index to back-cast the OECD measure. However, we are not able to reconstruct the Blanchard and Wolfers data exactly.

Data for union density until 1995 is from Nickell et al. (2002, Table 5). For the remaining years we interpolate using observations for 2001 from EIRO (2003, Table 9). Data for Greece (1985 and 1995), Ireland (1985 and 1993) and Luxembourg (1987 and 1995) are from ILO (1997, Table 1.2). Data for intervening years are produced by interpolation, while we extrapolate before 1985(87) and after 1993(95).

Data for bargaining coverage until 1994 are from Nickell et al. (2002, Table 4), which provide data with five year intervals. Yearly data are calculated by interpolation. EIRO (2003, Table 1) presents data for 2000 (1999 for Portugal and 2001 for the Netherlands) which allows us to interpolate for the late 1990s. Data for Greece and Ireland are only available for 1994 from ILO (1997, Table 1.2).

Data for the incidence of temporary employment is from Young (2003, Table 4.1), which provides observations with five year intervals from 1985–2000. We interpolate to obtain yearly data and extrapolate before 1985.

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

Year	*Austria*	*Belgium*	*Germany*	*Denmark*	*Spain*	*Finland*	*France*	*Greece*	*Ireland*	*Italy*	*Luxembourg*	*Netherlands*	*Norway*	*Portugal*	*Sweden*	*UK*	*Total*
1973		0/20	0/23	0/19	–	0/16	0/20	0/12	–	0/24	0/14	0/19	0/24	–	–	0/21	0/212
1974	0/16	0/20	1/23	0/19	–	0/16	0/21	0/13	–	0/24	0/14	0/19	0/25	–	–	0/21	1/231
1975	0/16	0/20	0/24	1/19	–	0/16	0/22	0/13	–	0/24	0/15	0/19	0/25	–	–	0/21	1/234
1976	0/16	0/21	0/24	0/19	–	0/16	0/22	0/13	0/18	0/24	0/15	0/19	0/25	–	–	0/23	0/255
1977	0/16	0/21	0/24	0/19	–	0/16	0/22	0/13	0/18	0/24	0/15	0/19	0/25	–	–	0/23	0/255
1978	0/16	0/21	0/24	0/19	–	0/16	0/22	0/13	0/18	0/24	2/15	0/20	0/25	–	0/26	0/23	3/282
1979	0/16	0/21	0/24	0/20	–	0/16	0/22	0/13	0/20	0/24	0/15	0/19	0/25	–	0/28	0/22	0/285
1980	0/16	0/21	0/24	1/20	–	0/16	0/22	0/13	0/19	0/24	0/15	0/19	0/25	–	0/28	0/22	1/284
1981	0/16	0/21	0/24	0/20	–	0/16	0/22	0/13	0/19	0/24	2/15	0/19	0/25	0/22	0/28	0/22	2/306
1982	0/16	0/21	0/24	0/20	0/2	0/16	0/21	0/13	0/20	0/24	0/16	0/18	0/25	0/22	0/28	0/22	0/308
1983	0/16	0/21	1/24	0/20	1/2	0/16	0/21	0/11	0/18	0/24	0/16	0/18	0/25	0/22	0/27	0/24	3/305
1984	0/16	0/21	1/27	0/20	0/2	0/16	0/22	0/17	0/18	0/24	1/16	0/16	0/25	0/22	0/27	0/24	5/313
1985	0/16	0/21	0/27	0/20	0/2	0/16	0/23	0/18	1/20	0/24	1/16	0/17	0/25	0/22	0/28	0/24	2/319
1986	0/16	6/21	0/27	2/20	0/2	0/16	2/23	2/18	1/21	–	0/14	0/18	0/25	0/22	0/28	0/24	15/295
1987	0/16	0/21	0/27	0/20	0/2	0/16	1/23	0/18	3/20	–	3/14	0/18	0/25	0/22	0/28	0/24	7/294
1988	1/16	3/21	0/27	0/20	0/2	0/16	5/23	0/18	1/20	–	3/14	0/18	0/25	0/21	0/28	0/25	15/295
1989	0/16	0/22	0/27	0/20	0/2	0/16	1/23	0/17	2/20	–	0/17	0/17	0/25	3/24	0/28	0/26	7/297
1990	0/16	0/24	0/27	0/20	0/26	0/16	1/23	0/24	1/21	–	1/16	0/17	1/25	0/23	0/28	0/25	4/332
1991	0/16	0/24	0/27	0/20	0/26	0/16	1/23	0/25	0/21	–	0/16	0/17	0/25	0/23	0/6	0/25	1/310
1992	0/16	0/23	0/24	1/20	0/26	0/16	0/23	1/25	0/21	–	0/17	0/17	0/25	0/23	1/13	0/25	3/314
1993	1/16	0/22	2/24	2/20	1/26	1/16	2/24	0/25	1/21	–	0/17	0/14	0/25	0/23	5/14	2/25	17/312
1994	0/16	0/22	1/26	0/2	2/26	1/16	8/15	0/25	2/21	–	1/17	0/8	0/25	0/23	0/14	11/22	26/278
1995	0/16	19/22	0/26	–	0/26	0/16	0/10	0/25	6/20	–	0/17	0/10	1/25	0/23	0/14	1/21	27/271
1996	0/14	0/27	7/25	–	4/26	–	0/12	0/25	2/23	–	6/19	0/20	0/25	0/23	0/14	0/26	20/279
1997	0/14	2/28	2/31	0/16	6/29	–	0/27	1/25	4/23	–	7/14	1/23	0/25	0/23	0/15	3/27	26/320
1998	0/14	0/28	1/31	0/16	3/29	–	0/25	3/24	3/23	–	4/17	0/23	0/25	0/29	0/14	1/28	15/326
1999	0/14	–	–	1/16	2/30	–	–	–	–	–	1/17	12/22	0/25	–	0/14	–	16/138
Total	2/408	31/575	16/665	8/464	20/286	2/368	21/556	7/469	27/463	0/312	32/423	23/483	2/674	3/411	6/478	18/615	217/7650

Table A2: Indices for employment protection legislation

Year	*BE*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*NL*	*PT*	*SW*	*UK*
1973	3.10	3.20	2.10	3.89	2.30	2.44	3.60	0.76	4.10	2.70	3.16	2.57	0.46
1974	3.10	3.20	2.10	3.89	2.30	2.57	3.60	0.83	4.10	2.70	3.42	3.03	0.48
1975	3.10	3.20	2.10	3.89	2.30	2.70	3.60	0.90	4.10	2.70	3.67	3.50	0.50
1976	3.10	3.20	2.10	3.86	2.30	2.70	3.60	0.90	4.10	2.70	3.75	3.50	0.50
1977	3.10	3.20	2.10	3.82	2.30	2.70	3.60	0.90	4.10	2.70	3.83	3.50	0.50
1978	3.10	3.20	2.10	3.78	2.30	2.70	3.60	0.90	4.10	2.70	3.92	3.50	0.50
1979	3.10	3.20	2.10	3.74	2.30	2.70	3.60	0.90	4.10	2.70	4.00	3.50	0.50
1980	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1981	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1982	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1983	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1984	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1985	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1986	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1987	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1988	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1989	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1990	2.93	3.08	1.95	3.60	2.25	2.75	3.60	0.90	3.97	2.60	4.03	3.28	0.50
1991	2.77	2.97	1.80	3.50	2.20	2.80	3.60	0.90	3.83	2.50	3.97	3.07	0.50
1992	2.60	2.85	1.65	3.40	2.15	2.85	3.60	0.90	3.70	2.40	3.90	2.85	0.50
1993	2.43	2.73	1.50	3.30	2.10	2.90	3.60	0.90	3.57	2.30	3.83	2.63	0.50
1994	2.27	2.62	1.35	3.20	2.05	2.95	3.60	0.90	3.43	2.20	3.77	2.42	0.50
1995	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50
1996	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50
1997	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50
1998	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50
1999	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50

Table A3: Indices for union denisty

Year	*BE*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*LU*	*NL*	*PT*	*SW*	*UK*
1973	0.48	0.32	0.62	0.09	0.61	0.22	0.37	0.53	0.43	0.53	0.36	0.61	0.72	0.50
1974	0.49	0.34	0.65	0.09	0.63	0.22	0.37	0.54	0.46	0.53	0.36	0.61	0.73	0.52
1975	0.52	0.35	0.69	0.09	0.65	0.22	0.37	0.56	0.48	0.53	0.38	0.61	0.74	0.54
1976	0.53	0.35	0.73	0.09	0.68	0.21	0.37	0.57	0.50	0.53	0.37	0.61	0.75	0.55
1977	0.54	0.35	0.74	0.09	0.66	0.21	0.37	0.57	0.50	0.53	0.37	0.61	0.78	0.57
1978	0.53	0.35	0.78	0.09	0.67	0.21	0.37	0.58	0.50	0.53	0.37	0.61	0.79	0.57
1979	0.54	0.35	0.77	0.09	0.68	0.19	0.37	0.58	0.50	0.53	0.37	0.61	0.80	0.57
1980	0.53	0.35	0.79	0.09	0.69	0.19	0.37	0.57	0.50	0.53	0.35	0.61	0.80	0.56
1981	0.53	0.35	0.80	0.09	0.68	0.18	0.37	0.57	0.48	0.53	0.33	0.61	0.81	0.55
1982	0.52	0.35	0.80	0.10	0.68	0.17	0.37	0.56	0.47	0.53	0.32	0.61	0.82	0.54
1983	0.52	0.35	0.81	0.10	0.69	0.16	0.37	0.57	0.46	0.53	0.31	0.61	0.83	0.53
1984	0.52	0.34	0.79	0.12	0.69	0.15	0.37	0.57	0.45	0.53	0.29	0.61	0.84	0.53
1985	0.51	0.34	0.78	0.12	0.69	0.14	0.37	0.56	0.43	0.53	0.28	0.56	0.84	0.51
1986	0.49	0.34	0.77	0.12	0.70	0.13	0.35	0.53	0.41	0.53	0.27	0.51	0.84	0.50
1987	0.49	0.33	0.75	0.12	0.71	0.12	0.34	0.51	0.41	0.53	0.24	0.46	0.84	0.49
1988	0.48	0.33	0.74	0.13	0.72	0.12	0.33	0.52	0.41	0.52	0.24	0.41	0.82	0.47
1989	0.49	0.33	0.76	0.13	0.73	0.11	0.32	0.53	0.40	0.51	0.24	0.37	0.82	0.45
1990	0.50	0.32	0.75	0.14	0.73	0.10	0.31	0.52	0.40	0.49	0.24	0.32	0.80	0.44
1991	0.52	0.33	0.76	0.16	0.75	0.10	0.29	0.53	0.40	0.48	0.25	0.32	0.80	0.43
1992	0.53	0.32	0.76	0.18	0.77	0.10	0.28	0.51	0.40	0.47	0.25	0.32	0.83	0.41
1993	0.54	0.30	0.77	0.20	0.79	0.10	0.27	0.50	0.40	0.46	0.24	0.32	0.86	0.40
1994	0.54	0.29	0.77	0.20	0.79	0.10	0.26	0.48	0.39	0.45	0.25	0.32	0.91	0.38
1995	0.54	0.27	0.77	0.18	0.80	0.10	0.24	0.46	0.39	0.43	0.24	0.32	0.90	0.37
1996	0.56	0.28	0.79	0.18	0.80	0.10	0.26	0.46	0.38	0.44	0.25	0.32	0.88	0.35
1997	0.59	0.28	0.81	0.17	0.79	0.10	0.27	0.46	0.38	0.46	0.25	0.31	0.86	0.34
1998	0.61	0.29	0.82	0.17	0.79	0.09	0.28	0.45	0.37	0.47	0.26	0.31	0.84	0.33
1999	0.64	0.29	0.84	0.16	0.79	0.09	0.30	0.45	0.37	0.48	0.26	0.31	0.83	0.32

Table A4: Indices for bargaining coverage

Year	*BE*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*NL*	*PT*	*SW*	*UK*
1973	0.83	0.90	0.69	0.65	0.95	0.80	0.90	0.90	0.86	0.70	0.64	0.73	0.70
1974	0.84	0.90	0.70	0.66	0.95	0.81	0.90	0.90	0.86	0.71	0.65	0.74	0.71
1975	0.85	0.90	0.70	0.66	0.95	0.82	0.90	0.90	0.85	0.72	0.65	0.75	0.72
1976	0.86	0.90	0.70	0.66	0.95	0.82	0.90	0.90	0.85	0.73	0.66	0.76	0.72
1977	0.87	0.90	0.71	0.67	0.95	0.83	0.90	0.90	0.85	0.74	0.67	0.76	0.71
1978	0.88	0.91	0.71	0.67	0.95	0.84	0.90	0.90	0.85	0.74	0.68	0.77	0.71
1979	0.89	0.91	0.72	0.68	0.95	0.84	0.90	0.90	0.85	0.75	0.69	0.78	0.70
1980	0.90	0.91	0.72	0.68	0.95	0.85	0.90	0.90	0.85	0.76	0.70	0.79	0.70
1981	0.90	0.91	0.72	0.68	0.95	0.86	0.90	0.90	0.85	0.77	0.71	0.79	0.69
1982	0.90	0.91	0.73	0.69	0.95	0.86	0.90	0.90	0.85	0.78	0.72	0.80	0.68
1983	0.90	0.90	0.73	0.69	0.95	0.87	0.90	0.90	0.85	0.78	0.73	0.81	0.66
1984	0.90	0.90	0.74	0.70	0.95	0.88	0.90	0.90	0.85	0.79	0.74	0.82	0.65
1985	0.90	0.90	0.74	0.70	0.95	0.89	0.90	0.90	0.85	0.80	0.75	0.82	0.64
1986	0.90	0.90	0.73	0.71	0.95	0.89	0.90	0.90	0.85	0.81	0.75	0.83	0.62
1987	0.90	0.90	0.72	0.72	0.95	0.90	0.90	0.90	0.84	0.81	0.76	0.84	0.60
1988	0.90	0.90	0.71	0.74	0.95	0.91	0.90	0.90	0.84	0.82	0.77	0.85	0.58
1989	0.90	0.90	0.70	0.75	0.95	0.91	0.90	0.90	0.83	0.82	0.78	0.85	0.56
1990	0.90	0.90	0.69	0.76	0.95	0.92	0.90	0.90	0.83	0.83	0.79	0.86	0.54
1991	0.90	0.90	0.69	0.76	0.95	0.93	0.90	0.90	0.83	0.83	0.77	0.87	0.51
1992	0.90	0.91	0.69	0.77	0.95	0.94	0.90	0.90	0.82	0.84	0.75	0.88	0.47
1993	0.90	0.92	0.69	0.77	0.95	0.94	0.90	0.90	0.82	0.84	0.73	0.88	0.44
1994	0.90	0.92	0.69	0.78	0.95	0.95	0.90	0.90	0.82	0.85	0.71	0.89	0.40
1995	0.90	0.93	0.69	0.78	0.95	0.96	0.90	0.90	0.82	0.86	0.69	0.90	0.37
1996	0.90	0.93	0.69	0.79	0.95	0.96	0.90	0.90	0.81	0.86	0.67	0.90	0.33
1997	0.90	0.94	0.69	0.79	0.95	0.97	0.90	0.90	0.81	0.87	0.65	0.91	0.29
1998	0.90	0.94	0.69	0.80	0.95	0.98	0.90	0.90	0.81	0.87	0.63	0.92	0.26
1999	0.90	0.95	0.69	0.80	0.95	0.99	0.90	0.90	0.81	0.88	0.61	0.93	0.22

Table A5: Indices for incidence of temporary employment

Year	*BE*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*LU*	*NL*	*PT*	*SW*	*UK*
1973	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1974	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1975	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1976	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1977	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1978	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1979	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1980	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1981	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1982	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1983	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1984	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1985	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1986	6.58	10.10	12.00	15.60	16.50	5.88	20.18	7.54	4.80	4.44	7.52	14.40	13.00	6.54
1987	6.26	10.20	11.70	15.60	16.50	7.06	19.26	7.78	4.90	4.18	7.54	15.40	13.00	6.18
1988	5.94	10.30	11.40	20.37	16.50	8.24	18.34	8.02	5.00	3.92	7.56	16.40	13.00	5.82
1989	5.62	10.40	11.10	25.13	16.50	9.42	17.42	8.26	5.10	3.66	7.58	17.40	13.00	5.46
1990	5.30	10.50	10.80	29.90	16.50	10.60	16.50	8.50	5.20	3.40	7.60	18.40	13.00	5.10
1991	5.30	10.48	11.06	30.92	16.50	10.92	15.24	8.84	5.60	3.28	8.24	16.74	13.00	5.46
1992	5.30	10.46	11.32	31.94	16.50	11.24	13.98	9.18	6.00	3.15	8.88	15.08	13.00	5.82
1993	5.30	10.44	11.58	32.96	16.50	11.56	12.72	9.52	6.40	3.03	9.52	13.42	13.00	6.18
1994	5.30	10.42	11.84	33.98	16.50	11.88	11.46	9.86	6.80	2.90	10.16	11.76	13.00	6.54
1995	5.30	10.40	12.10	35.00	16.50	12.20	10.20	10.20	7.20	2.98	10.80	10.10	13.00	6.90
1996	6.04	10.88	11.72	34.44	16.74	12.84	10.78	9.34	7.78	3.07	11.40	12.16	13.26	6.84
1997	6.78	11.36	11.34	33.88	16.98	13.48	11.36	8.48	8.36	3.15	12.00	14.22	13.52	6.78
1998	7.52	11.84	10.96	33.32	17.22	14.12	11.94	7.62	8.94	3.23	12.60	16.28	13.78	6.72
1999	8.26	12.32	10.58	32.76	17.46	14.76	12.52	6.76	9.52	3.32	13.20	18.34	14.04	6.66

B Results without bias adjustment

category	Observed wage cuts (Y)	Average simulated wage cuts	$\#(\widehat{Y} > Y^B)$	Probability of significance	Fraction of wage cuts prevented	Fraction of industry-years affected
All	217	275	4995	0.001	0.210	0.008
1970–79	5	12	4751	0.050	0.587	0.004
1980–89	57	75	4787	0.043	0.241	0.006
1990–94	51	65	4664	0.067	0.219	0.009
1995–99	104	122	4658	0.068	0.148	0.014
British Isles	45	54	4107	0.179	0.160	0.008
Core	125	148	4760	0.048	0.154	0.007
Nordic	18	28	4731	0.054	0.365	0.005
South	29	45	4861	0.028	0.354	0.011
Austria	2	6	4373	0.125	0.641	0.009
Belgium	31	38	4394	0.121	0.190	0.013
Germany	16	15	2007	0.599	0	0
Denmark	8	11	3598	0.280	0.281	0.007
Spain	19	18	1950	0.610	0	0
Finland	2	5	3944	0.211	0.568	0.007
France	21	18	1108	0.778	0	0
Greece	7	6	1555	0.689	0	0
Ireland	27	34	4069	0.186	0.198	0.014
Italy	0	3	4517	0.097	1.000	0.008
Luxembourg	32	38	3833	0.233	0.151	0.013
Netherlands	23	33	4740	0.052	0.307	0.021
Norway	2	3	2644	0.471	0.274	0.001
Portugal	3	18	5000	0.000	0.836	0.037
Sweden	6	10	4225	0.155	0.391	0.008
UK	18	20	3015	0.397	0.097	0.003

C Results with country specific underlying distributions

category	Observed wage cuts (Y)	Average simulated wage cuts	$\#(\widehat{Y} > Y^B)$	Probability of significance	Fraction of wage cuts prevented	Fraction of industry-years affected
All	217	266	4977	0.005	0.185	0.006
1970–79	5	12	4801	0.040	0.601	0.004
1980–89	57	72	4585	0.083	0.205	0.005
1990–94	51	64	4542	0.092	0.201	0.008
1995–99	104	118	4405	0.119	0.121	0.011
British Isles	45	47	2859	0.428	0.043	0.002
Core	125	147	4764	0.047	0.152	0.007
Nordic	18	26	4500	0.100	0.318	0.004
South	29	45	4874	0.025	0.363	0.011
Austria	2	4	3478	0.304	0.489	0.005
Belgium	31	36	3769	0.246	0.127	0.008
Germany	16	16	2280	0.544	0.004	0.000
Denmark	8	10	3325	0.335	0.228	0.005
Spain	19	15	1054	0.789	0	0
Finland	2	2	2282	0.544	0.194	0.001
France	21	21	2130	0.574	0	0
Greece	7	12	4206	0.159	0.408	0.010
Ireland	27	28	2689	0.462	0.045	0.003
Italy	0	3	4678	0.064	1.000	0.010
Luxembourg	32	39	4172	0.166	0.183	0.017
Netherlands	23	32	4598	0.080	0.281	0.019
Norway	2	3	3090	0.382	0.402	0.002
Portugal	3	15	4992	0.002	0.801	0.029
Sweden	6	10	4334	0.133	0.412	0.009
UK	18	19	2573	0.485	0.039	0.001