Nominal Wage Rigidity in Europe*

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Abstract

This paper explores the existence of downward nominal wage rigidity (DNWR) in the manufacturing sectors of 14 European countries, over the period 1973–1999, using an unbalanced data set of hourly nominal wages at industry level. Based on a novel univariate statistical method, which allows for country and year specific variation in both the mean and standard deviation of industry wage changes, we reject the hypothesis of no DNWR. Splitting into subsamples, we document the existence of DNWR for the periods 1990–94 and 1995–99, but not for 1973–89. Furthermore, we document DNWR for countries in the South and Core of Europe, but not the Nordic and Anglo-Saxon countries.

JEL: J5, C14, C15, E31

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1 Introduction

In recent years, a number of countries have adopted explicit inflation targets for monetary policy, reflecting a general agreement that monetary policy must ensure low inflation. The deliberate policy of low inflation has led to renewed interest for the contention of Tobin (1972) that if policy aims at too low inflation, downward rigidity of nominal wages may lead to higher wage pressure, involving higher equilibrium unemployment (see e.g. Akerlof, Dickens and Perry, 1996, 2000, Holden, 1994, and Wyplosz, 2001). Other economists have been less concerned, questioning both the existence of downward nominal wage rigidity (DNWR), and the possible macroeconomic effects (see e.g. Gordon, 1996).

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 by changing the composition of the workforce allowed by turnover. Then DNWR may be less important for macroeconomic performance. It is therefore important also to investigate DNWR using aggregate data.

This paper explores the existence of DNWR in the industry sectors of 14 European countries, over the period 1973–1999, using an unbalanced data set of 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, based on a harmonized data set (from Eurostat). More importantly, by using data for the hourly earnings at industry level, our study captures any effect of changes in the composition of the workforce, as well as the effect of changes in the wage rates.

To explore 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 univariate 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 mean and standard deviation of wage changes, while for instance the Kahn test (Kahn, 1997) only allows for variation in the mean.

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 results are documented in Section 4. Section 5 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 downward nominal wage rigidity DNWR have been proposed. The most common explanation, advocated by e.g. Blinder and Choi (1990) and Akerlof et al. (1996), is that employers avoid nominal wage cuts because both they and (in particular) the employees think that a wage cut is unfair. The other explanation, proposed by MacLeod and Malcomson (1993) in a individual bargaining framework, and Holden (1994) in a collective agreement framework, is that nominal wages are given in contracts that can only be changed by mutual consent. 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.

Concerning empirical work on DNWR, there is now a fairly large, and rapidly growing, number of recent studies, for many different countries, including Fehr and Götte (2000) for Switzerland, Beissinger and Knoppik (2000) and Knoppik and Beissinger (2001) for Germany, Christofides and Leung (1999), and Fortin and Dumont (2000) for Canada, Holden (1998) for the manufacturing sectors in the Nordic countries, Agell and Bennmarker (2002), Agell and Lundborg (1999) and Ekberg (2002) for Sweden, Kimura and Ueda (1997) for Japan, Smith (2000) and Nickell and Quintini (2001) for the UK, and Bewley (1999), Altonji and Devereux (1999) and Lebow et al. (2000) for the US (the latter four papers also discuss previous empirical findings for the UK and the US). In general these studies document that nominal wages are rigid downwards. More specifically, the studies generally also find (i) a spike in the distribution of nominal wage changes at zero and (ii) that the rate of inflation affects the distribution of nominal wage changes, both features indicating DNWR. Regrettably, different methods and data in the above-mentioned studies make it in general difficult to compare the degree of downward nominal wage rigidity across countries.

3 Empirical approach

We use an unbalanced panel data of industry level annual wage growth from the manufacturing, mining and quarrying, electricity, gas and water supply, and construction sectors of 14 European countries in the period 1973–1999. The data source for wages are harmonized hourly earnings from Eurostat. The countries included in the sample are Belgium, Germany, Denmark, Spain, Finland, France, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Sweden and the UK. 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 $S = 5689$ observations distributed across $N = 277$ country-year samples. More details on data are provided in the appendix.

There are no nominal wage cuts in 72% of the country-year samples. In our data we observe, however, no less than 192 events of nominal wage reductions, i.e., 3.37% of all observations, and 21 events of zero wage change. The existence of nominal wage cuts implies that DNWR is certainly not absolute, but it does not necessarily imply that wages are fully flexible; DNWR may prevent nominal wage cuts in some but not all circumstances. In Figure 1 we illustrate some country-year samples by displaying box plots of annual wage growth in Denmark, France, Germany and the UK. We see from the Figure that the mean and standard deviation of wage growth varies over time between

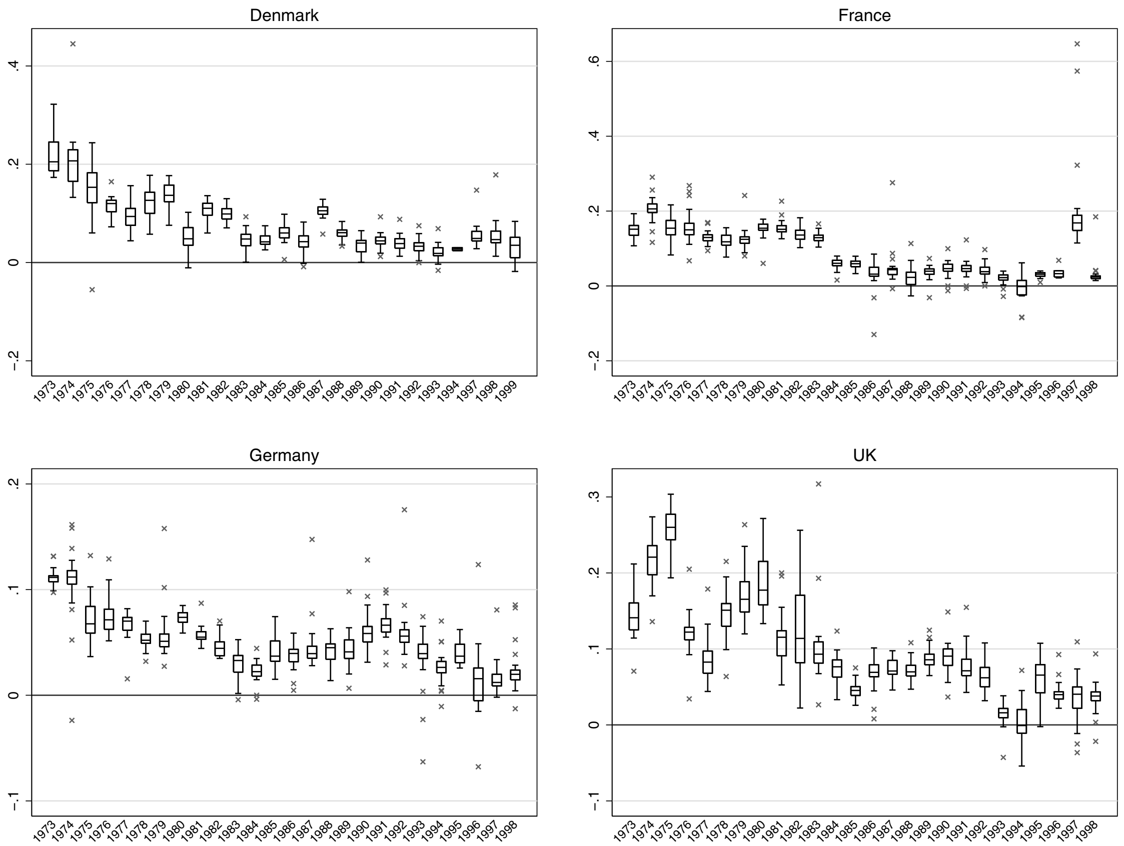

Figure 1: Box plots of annual wage growth in Denmark, France, Germany and the UK. The box plot illustrates the distribution of wage changes within a country-year. The box extends from the 25th to the 75th percentile with the median inside the box. The whiskers emerging from the box indicate the tails of the distributions and the crosses represent outliers.

and within countries. The graphs reflect that wage cuts are rare, but it is not possible to visually detect DNWR from the graphs alone. To detect DNWR, we need to use a formal statistical method.

To this end, let us first define *the notional wage change* as the wage change that would prevail under no DNWR, following Altonji and Devereux (1999) and Fehr and Götte (2000). The key idea of our test is now as follows. If DNWR is at work in some countries and time periods, the notional and observed (or empirical) wage changes may differ, as some observations of empirical wage changes will be non-negative even if the corresponding notional wage change is negative. In such country-year samples, the distribution of empirical wage changes will be compressed from below, reducing the number of negative changes. In contrast, in country-year samples where there is no effect from DNWR, the notional and empirical wage changes will be identical, and there will be no compression of the distribution of empirical wage changes. If the form of the distribution of notional wage changes is the same for all country-year samples, and there are sufficiently many country-year samples with no DNWR, we can get a fair estimate of the form of the distribution of notional wage changes by use of the empirical wage changes from all country-year samples. Clearly, to the extent that DNWR compresses the distribution of empirical wage changes in some country-year samples, our estimate of the form of the distribution of notional wage changes (which is based on the distribution of empirical wage changes) will also be compressed. As we shall argue below, this feature will reduce the power of our test, implying that it may not detect DNWR even if it prevails, but it will not affect the significance level of the test.

More specifically, our test procedure goes as follows: We let the null hypothesis be that there is no DNWR. Under this hypothesis, the notional and empirical wage changes are the same, and we can derive an unbiased estimate of the distribution of the notional wage changes on the basis of the empirical wage changes. In particular, we can derive an estimate of the country-year specific probabilities that an observation of a notional wage change is negative. On the basis of these country-year specific probabilities of notional wage cuts, we can calculate the distribution function of the number of notional wage cuts in the entire sample. Under the null of no DNWR, wage cuts should be as likely in the empirical and notional samples. If DNWR binds in some country-year samples, the distribution of empirical wage changes will be compressed in these samples. As noted above, our estimate of the distribution of notional wage changes will also be compressed, but less so, as it is based on all country-year samples. Thus, there will be fewer wage cuts in the empirical sample than in the notional. We will reject the null of no DNWR if it is sufficiently unlikely that the number of empirical wage cuts could be have generated on the basis of the distribution of the notional wage changes.

3.1 The formal test

To derive a distribution of notional wage changes, we exploit information from all country-year samples. As argued above, we have to make some assumptions on the form of the distribution of the nominal wage changes to be able to proceed. The box-plots in Figure 1 make clear that both the mean and the variance of the wage changes vary over time and between countries. Thus we assume

Assumption 1 *The distribution of nominal wage changes (Δw_{jit}) is the same for all country-year samples, adjusted linearly for country-year specific mean (M_{it}) and standard deviation ($\hat{\sigma}_{it}$), that is* $\Delta w_{jit} \sim d(M_{it}, \hat{\sigma}_{it})$.

We make no further assumptions regarding the form of the distribution of d. Assumption 1 is, however, not innocuous. But other tests often make stronger assumptions; for instance the Kahn test (Kahn, 1997) allows for variation over time in the mean nominal wage change, but not for variation in the standard error. In regression based tests, significance levels are often based an assumption of normality, while no such assumption is necessary here.

As mentioned above, our null hypothesis is

H_0: There is no downward nominal wage rigidity.

Under H_0, empirical and notional wage changes are identical, so that the distribution of the notional wage changes can be derived from the empirical wage changes. Employing Assumption 1 allows us to combine the observations from all country-year samples to a common distribution. Specifically, we obtain a sample of 5689 observations of normalised wage changes by adjusting the empirical wage changes for the country-year specific mean and standard deviation, i.e.

$$\Delta w_s^n \equiv \left(\frac{\Delta w_{jit} - M_{it}}{\hat{\sigma}_{it}} \right), \qquad s = 1, \ldots, 5689 \tag{1}$$

For simplicity we use subscript s which runs over all j, i and t. For our test, we need information about the distribution of normalised wage changes for each country-year sample. To this end we compute country-year specific samples of *notional* wage changes by scaling the sample of normalised wage changes, Δw_s^n, with the country-year mean and standard deviation:

$$\Delta \widetilde{w}_s^{it} \equiv \left(\Delta w_s^n \hat{\sigma}_{it} + M_{it} \right), \qquad s = 1, \ldots, 5689 \tag{2}$$

Thus, for each of the 277 country-year samples, we have a sample of $S = 5689$ notional wage changes, based on an adjustment to the appropriate mean and standard deviation. As noted above, if DNWR is at work in some country-year samples, these samples of notional wage changes will be compressed relative to the "true", underlying distribution of notional wage changes, as these samples are based on the empirical wage changes from all country-year samples. Under H_0, however, there is no DNWR, and thus no compression, so that this feature will not affect the significance level of our test.

From these country-year samples of notional wage changes we obtain an estimate of the probability of a nominal notional wage cut in country-year it as the proportion of wage cuts out of the total sample of observations S

$$\tilde{q}_{it} \equiv \frac{\#\Delta\tilde{w}_s^{it} < 0}{S}, \qquad s = 1, \ldots, 5689 \tag{3}$$

Even under H_0, these probabilities will be subject to sampling error, due to the uncertainty in our estimate of the country-year specific means and standard deviation. While such sampling error may be considerable for each country-year specific probability, it seems, nevertheless, likely that such uncertainty will cancel out in our test procedure below, where we exploit all the 277 country-year specific probabilities $\tilde{q}_{it}$. Thus, in the sequel, we will treat $\tilde{q}_{it}$ as given.[1]

Samples of the empirical and notional wage changes for four country-years are compared in Figure 2. For illustrative purposes we display kernel densities[2] to represent the histograms. The panels to the left represents country-years where empirical wage cuts are observed, while the panels to the right represent country-years where the distributions of wage growth is entirely above zero. One immediate difference is that the empirical samples are less smooth than the notional, which reflects that there are only 20–30 observations behind the empirical samples while there are 5689 observations be-

[1] As the uncertainty in the probabilities can be estimated, we will test for this assumption in future work.
[2] We use the Epanechnikov function.

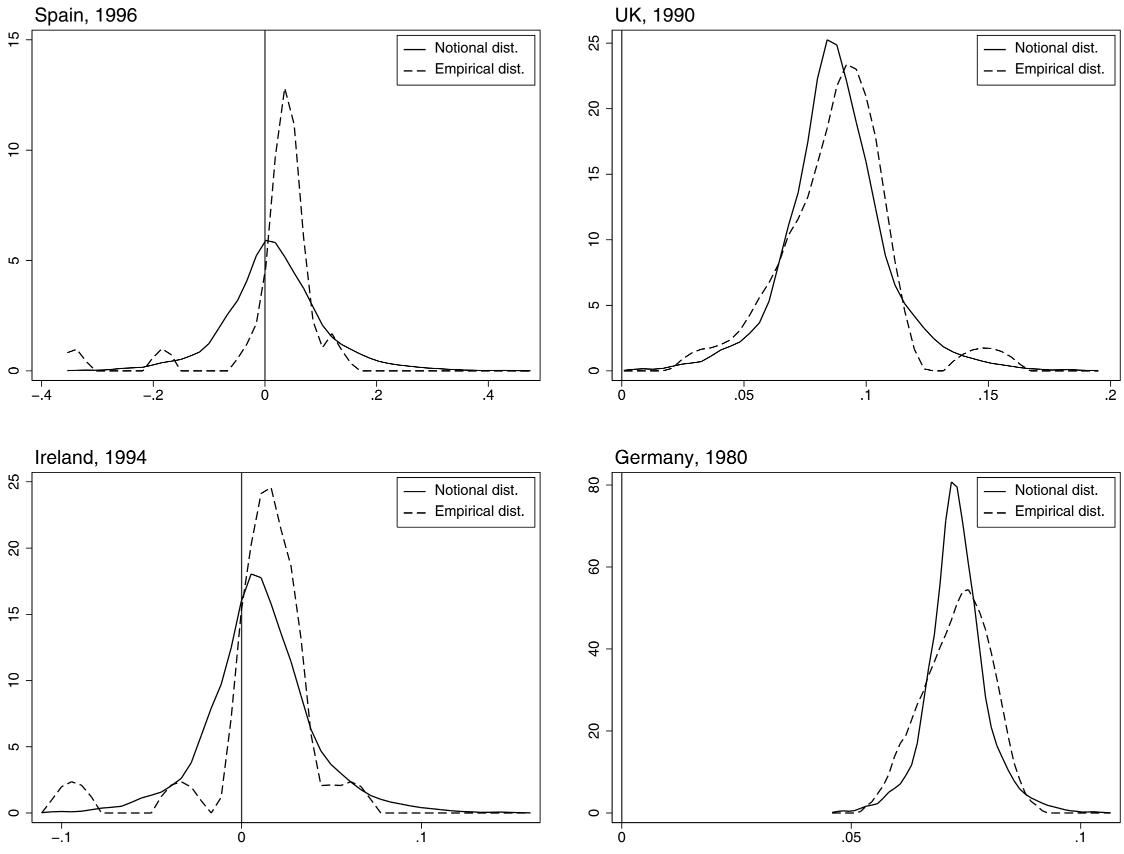

Figure 2: Empirical (dashed line) and notional (solid line) distributions for Spain (1996), UK (1990), Ireland (1994) and Germany (1980)

hind the notional ones. Thus, the small peaks in the empirical samples below zero are a result of observed nominal wage cuts in a small sample. More importantly, we see in the left panels that the notional samples seem to have a higher mass below zero than the empirical samples, consistent with the notion that downward nominal wage rigidity prevents wage cuts and thus compresses the distribution of the empirical samples. The distribution of notional wage changes is less compressed, as it is based on all country-year samples, including the majority where DNWR is not an issue as the mean wage change is too large (as illustrated in the panels to the right).

The main question is whether the difference in the proportion of wage cuts between the empirical and notional samples is significant. According to our null hypothesis that there is no nominal rigidity, the apparent lack of wage cuts in the empirical samples is just the outcome of a number of random events. The alternative is

H_1: Downward nominal wage rigidity reduces the number of nominal wage cuts in the empirical samples.

To test H_0 more formally, we exploit that, under H_0, the distribution of empirical and notional wage changes is the same, so that the country-year specific probabilities $\tilde{q}_{it}$ will also apply to the empirical wage changes. Thus, using these probabilities, we can calculate the distribution function for the number of wage cuts in the total sample. Let z_s be a dummy variable equal to unity if there is a wage cut in observation s ($s = 1, 2, \ldots, 5689$) and equal to zero otherwise. Then, under H_0, the probability that $z_s = 1$ is equal to $\tilde{q}_s$ (i.e. $\tilde{q}_{it}$ assigned to observation s). Let $Z = \Sigma_s z_s$ denote the total number of wage cuts in the entire sample, and define $\Omega = \{1, 2, \ldots, S\}$. The probability that n out of S draws are wage cuts is then

$$\Pr(Z = n) = \sum_{\substack{J \subset \Omega \\ \#J = n}} \left[\prod_{s \subset J} \tilde{q}_s \prod_{s \subset \Omega - J} (1 - \tilde{q}_s) \right] \tag{4}$$

where J denotes the set of observations in which a wage cut occurs, containing n elements. In equation (4), there are $S!/(n!(S-n)!)$ terms, reflecting the vast number of different combinations of observations of wage cuts given that there are n in total, so while (4) in principle can be computed, it is much easier, and sufficiently accurate to calculate (4) by use of simulation, cf. below. Let

$$G(n) = \sum_{i=0}^{n} \Pr(Z = i)$$

be the probability under H_0 that the number of wage cuts is less than or equal to n. $G(n)$ being small implies that if H_0 is true, then it was unlikely that there has been only n wage cuts. More formally, we reject H_0 if $G(X) \leq \tau$, where X is the number of wage cuts in the empirical sample (which is 192) and τ is the level of significance.

The last step of our procedure is therefore to simulate the wage setting in the 277 country-year samples by drawing z_s with a probability $\tilde{q}_s$ for all of the $s = 1, 2, \ldots, 5689$ observations, and count the number of simulated wage cuts in total, Z. We repeat this simulation 5000 times and count the number of times $Z > 192$ (i.e. the number of observed wage cuts). The null hypothesis is rejected with a level of significance at 5% if

$$1 - \frac{\#(Z > 192)}{5000} \leq 0.05$$

4 Results

In 5000 of 5000 simulations we simulate more wage cuts than observed in the data and thus reject the null hypothesis comfortably. Hence, we conclude that DNWR has been at work in our sample.

To illustrate the result it is useful to compare the probability of a notional nominal wage cut, $\tilde{q}_{it}$, to the empirical probabilities of a nominal wage cut which we will de-

Table 1: Empirical moments for $\hat{q}_{it}$ and $\tilde{q}_{it}$

	Mean	*Standard deviation*
$\hat{q}_{it}$	0.0345	0.0912
$\tilde{q}_{it}$	0.0455	0.1007

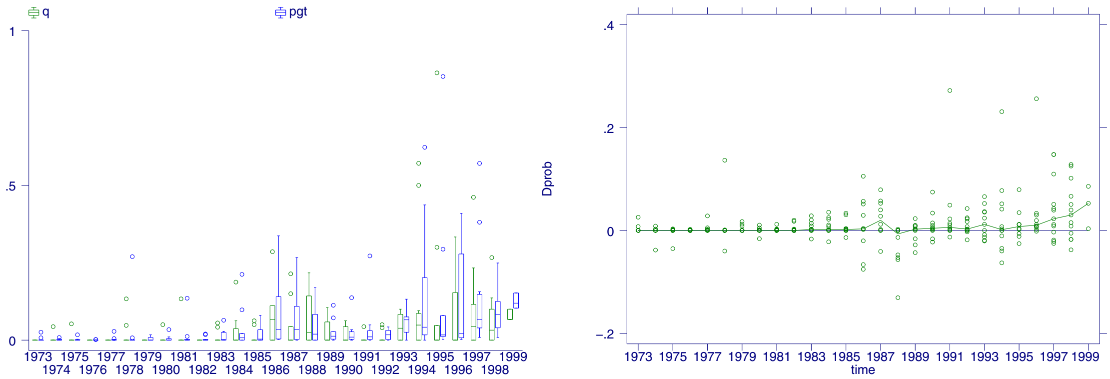

Figure 3: The distribution of $\hat{q}_{it}$ and $\tilde{q}_{it}$ (left) and their difference (right) over time.

note $\hat{q}_{it}$. While $\tilde{q}_{it}$ is defined by (3), $\hat{q}_{it}$ is simply computed by counting the number of observed wage cuts in for each country-year sample, X_{it}, divided by the number of observations, S_{it}. Table 1 shows the mean and standard deviations of $\hat{q}_{it}$ and $\tilde{q}_{it}$. We see that the mean of $\tilde{q}_{it}$ is 0.0109 (30%) larger than the mean of $\hat{q}_{it}$, while the standard deviation is 10% larger. It is also interesting to note that while there are no wage cuts in 31% of the country-year samples of notional wage changes, the corresponding number for the country-year samples of empirical wage changes is 72%. The simulation result shows that these differences are significant.

A number of interesting questions arise. The simulations only report evidence of nominal wage rigidity, but of course nothing of its causations. To what extent has DNWR varied over time? Does the degree of DNWR vary among countries? In Figure 3 we plot $\hat{q}_{it}$ and $\tilde{q}_{it}$ over time. From the left panel we see that there are more wage cuts in

Table 2: Results from 5000 simulations on subperiods.

	1973–1989	1990–1994	1995–1999
No. of observations	3464	1195	1030
No. of country-years	172	56	49
Observed wage cuts (X)	59	42	91
Proportion of wage cuts (%)	1.70	3.51	8.83
$\#(Z > X)$	4141	5000	4999
Probability of significance	0.1718	0	0.0002

the latter half of the 1980s and latter half of the 1990s. It is, however, the difference between $\hat{q}_{it}$ and $\tilde{q}_{it}$ which is of interest. From the right panel of the figure we plot $\tilde{q}_{it} - \hat{q}_{it}$ over time (solid line is the annual mean). We see that despite the simulation result, the probabilities of notional wage cuts are generally not much different from the empirical probabilities. For some country-year samples, the empirical probability is even larger than the notional, which reflects small sample properties, but probably also that the notional probabilities are biased downwards, as discussed above.

We first investigate whether nominal wage rigidity has changed over time by splitting the sample into three subperiods 1973–1989, 1990–1994 and 1995–1999. The first subperiod is characterised with higher inflation rates than the latter two. We have split the nineties in two, as there may be extra interest in the evolution of DNWR during this recent period of low inflation. Performing the simulations as explained above yields the results in Table 2. There are more observations in the first subperiod than in the latter two due to the longer period. The proportion of nominal wage cuts is much higher in the 1990s than before, but the simulations show that there should have been even more if wages were flexible downwards. There is strong evidence of downward nominal wage rigidity in the 1990s, but no such evidence for the 1970s and 80s. This difference between subperiods probably reflects that the 1990s was a period of low inflation, with low nominal wage growth, implying that DNWR had a non-negligible effect on the empirical wage changes, which we are able to detect in our method. In contrast, in the 1970s and 80s, wage growth was generally higher, and any DNWR that might exist would be of less relevance, and thus more difficult to detect.

Nominal rigidities may also be related to labour market institutions. Based on a theoretical framework allowing for bargaining over collective agreements as well as individual bargaining, Holden (2002) argues that the extent of DNWR is likely to depend on three key factors: the coverage of collective agreements, the legal framework at renegotiation of collective agreements, and the strictness of the employment protection legislation for non-union workers. 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 individual countries. The test, however, uses comparatively weak assumptions, which has the downside that it requires a fair amount of observations. We therefore split the sample into regions which have comparable labour market institutions instead. We operate with four regions; Anglo-Saxon (Ireland and the UK), Core (Belgium, Germany, Luxembourg, Netherlands), Nordic (Denmark, Finland and Sweden, but regrettably few observations from the latter two countries) and South (France, Italy, Greece, Portugal, Spain). The results from simulations using these regions are presented in Table 3

In the empirical samples, there are relatively more wage cuts in the Core and Anglo-

Table 3: Results from 5000 simulations on regions.

	South	Core	Anglo-Saxon	Nordic
No. of observations	1447	2643	1074	525
No. of country-years	49	129	32	67
Observed wage cuts (X)	28	109	45	10
Proportion of wage cuts (%)	1.94	4.12	4.19	1.90
$\#(Z > X)$	5000	4996	3897	1661
Probability of significance	0	0.0008	0.2206	0.6678

Saxon regions than in the South and Nordic regions. Not surprisingly, we reject the hypothesis of no DNWR in the South, a region where bargaining coverage is fairly high (see e.g. Calmfors et al (2001), table 4.4), employment protection legislation is very strict (OECD, 1999), and employers often have had a weak position when trying to enforce a nominal wage cut (Holden, 2002). We also document DNWR for the Core, in spite of more observed wage cuts here; this is a region with generally high bargaining coverage and fairly strong employment protection legislation. DNWR is not rejected for the Anglo-Saxon region, where incidentally bargaining coverage is lower, and employment protection legislation is less strict, than in most of the rest of Europe. Perhaps somewhat surprisingly, we cannot reject no DNWR for the Nordic countries. One should bear in mind, however, that the number of observations is much lower for this region, reducing the power of the test. Furthermore, most (85 percent) of the observations are for Denmark, a country with lower bargaining coverage, and much weaker employment protection legislation than the other Nordic countries.

5 Conclusions

This paper explores the existence of downward nominal wage rigidity (DNWR) in the manufacturing sectors of 14 European countries, over the period 1973–1999, using an unbalanced data set of hourly nominal wages at industry level. Based on a novel univariate statistical method, which allows for country and year specific variation in both the mean and the standard deviation 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 low inflation periods 1990–94 and 1995–99, but not for 1973–89. Furthermore, we also find evidence for DNWR for countries in the South (France, Italy, Greece, Portugal, Spain) and Core (Belgium, Germany, Luxembourg, Netherlands) of Europe, but not for the Nordic countries (mainly Denmark, but also some observations for Finland and Sweden) or for the UK and Ireland. These differences are consistent with the argument of Holden (2002), that the existence and importance of DNWR is likely to be related to the coverage of collective agreements and the strictness of employment protection: bargaining coverage is generally higher in the South and Core of Europe, than in the UK, Ireland and Denmark.

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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. To avoid counting the same observations twice, if, for example, wage data are available for D, DA 158 and DA 159, we use the latter two only.

The average number of observations per country-year sample is 20.5, with a standard error of 4.7. The distribution of observations on years and countries are reported in Table 4.

Table 4: The distribution of observations over countries and years

Year	*be*	*dew*	*dk*	*es*	*fi*	*fr*	*gr*	*ie*	*it*	*lu*	*nl*	*pt*	*se*	*uk*	*Total*
1973	20	23	19	–	–	20	13	–	24	14	19	–	–	21	173
1974	20	23	19	–	–	21	13	–	24	14	19	–	–	21	174
1975	20	24	19	–	–	22	13	–	24	15	19	–	–	21	177
1976	21	24	19	–	–	22	13	18	24	15	19	–	–	23	198
1977	21	24	19	–	–	22	13	18	24	15	19	–	–	23	198
1978	21	24	19	–	–	22	13	18	24	15	20	–	–	23	199
1979	21	24	20	–	–	22	13	20	24	15	19	–	–	22	200
1980	21	24	20	–	–	22	13	20	24	15	19	–	–	22	200
1981	21	24	20	–	–	22	13	20	24	15	19	21	–	22	221
1982	21	24	20	–	–	21	13	20	24	16	18	21	–	22	220
1983	21	24	20	–	–	21	11	18	24	16	18	21	–	24	218
1984	21	27	20	–	–	22	17	18	24	16	16	21	–	24	226
1985	21	27	20	–	–	23	18	20	24	16	17	21	–	24	231
1986	21	27	20	–	–	23	18	21	–	14	18	21	–	24	207
1987	21	27	20	–	–	23	18	20	–	14	18	21	–	24	206
1988	21	27	20	–	–	23	18	20	–	14	18	21	–	25	207
1989	22	27	20	–	–	23	18	20	–	17	17	19	–	26	209
1990	24	27	20	26	–	23	25	21	–	16	17	22	–	25	246
1991	24	27	20	26	–	23	25	21	–	17	17	22	–	25	247
1992	23	24	20	26	–	23	25	21	–	17	17	22	–	25	243
1993	22	24	20	26	–	23	25	21	–	17	14	22	–	25	239
1994	22	26	2	26	–	14	25	21	–	17	8	22	15	22	220
1995	22	26	–	26	–	9	25	20	–	17	–	22	15	21	203
1996	27	25	–	26	–	10	25	22	–	18	–	22	15	25	215
1997	28	31	10	30	2	28	25	22	–	13	23	22	15	26	275
1998	28	31	10	30	2	27	24	22	–	15	23	28	15	27	282
1999	–	–	10	30	–	–	–	–	–	15	–	–	–	–	55
Total	575	665	446	272	4	554	472	462	312	418	431	391	75	612	5689