ARE REAL WAGES RIGID DOWNWARDS?

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Abstract

This paper explores the existence of downward real wage rigidity (DRWR) in 19 OECD countries, over the period 1973–1999, using data for hourly nominal earnings at industry level. Based on a nonparametric statistical method, which allows for country and year specific variation in both the median and the dispersion of industry wage changes, we find evidence of some downward rigidity of real wages in OECD countries overall, as well as for regions and time periods. There is some evidence that real wage cuts are less prevalent under strict employment protection legislation and high union density. Generally, we find stronger evidence for downward nominal than for downward real wage rigidity.

JEL Code: J3, J5, C14, C15, E31.

Keywords: downward real wage rigidity, OECD, employment protection legislation, wage setting.

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1 Introduction

Whether real wages are rigid downwards is important for the effects of adverse shocks to the economy. In early explanations of the persistent European unemployment problem, a leading idea was that when unemployment had risen due to various types of shocks, real wages remained high, preventing unemployment from coming down again (see e.g. Grubb et al., 1983 and Bruno and Sachs, 1985). More recently, real wage rigidity has become a key part in several contributions to business cycle and monetary policy literature. Danthine and Kurmann (2005), Erceg et al. (2000) and Smets and Wouters (2003) find that wage rigidity plays an important role when calibrating DSGE models to data. Blanchard and Gali (2005) argue that real wage rigidity is a crucial element in understanding inflation persistence, while Hall (2005) and Shimer (2005) argue that real wage rigidity is necessary to explain the large cyclical variation in vacancies. However, other contributions have disputed some of these conclusions, see Krause and Lubik (2006) and Mortensen and Nagyál (2006).

The rising interest in wage rigidity increases the need for more empirical evidence on the extent of wage rigidity in different countries. We focus on one specific aspect of wage sluggishness, namely whether real wages are rigid downwards. Downward real wage rigidity (DRWR) will be of particular relevance for how the economy functions in a downturn as it affects how adverse shocks may lead to unemployment rather than lower wages. Our analysis also sheds some light on whether wage rigidity is asymmetric.

Recently, several studies including the International Wage Flexibility Project have found empirical evidence for the existence of considerable DRWR in a number of OECD countries, mostly based on micro data (see Dickens et al., 2005, Barwell and Schweitzer, 2004, Bauer et al., 2004, Christofides and Li, 2005 and Cornelissen and Hübler, 2005). In contrast to these studies, we explore the existence of DRWR at industry level, based on data from 19 OECD countries for the period 1973–99, covering in total 449 country-year samples. More specifically, we investigate whether there are 'too few' real wage cuts in the country-year specific distributions of industry wage changes, compared to what one would expect without downward rigidity.

The studies on micro data provide valuable evidence of wage rigidity for individual job stayers. However, it is not clear that rigid real wages for job stayers will imply the same rigidity at more aggregate levels. Firms may respond to individual wage rigidity by other means, as giving lower wage growth to other workers, or by changing the composition of the work force. And even if wage rigidity binds in some firms, wages may fall in other firms so that jobs are shifted over to them. Consistent with this, Farès and Lemieux (2001) find in Canadian data that most of the real wage adjustments over the business cycle is experienced by new entrants.

If the effects of DRWR for individual job stayers is weakened by such mechanisms, it is not clear what the aggregate effects are. One possibility is that the individual rigidity is offset entirely, in which case one would not expect it to have important macroeconomic or allocative effects. Another possibility is that these mechanisms are unimportant, making wage rigidity more difficult to detect, but not removing the implications of it. It is difficult to distinguish between these possibilities using data for individual job stayers only. In contrast, if we detect DRWR in industry data, we know that the rigidity prevails in spite of compositional effects.

An alternative to our study of industry wages would be to look directly for evidence of real wage rigidity on aggregate times series data, followed by a study of the macroeconomic implications. In an influential study, Layard et al. (1991) find among other things evidence of asymmetric real wage rigidity in a number of OECD countries, indicating resistance by workers to allowing adverse terms-of-trade shocks to push down wages. More recently, Nickell et al. (2003) and Nunziata (2005) find evidence of real wage resistance, consistent with the notion that wage setters oppose a reduction in wages relative to consumer prices. Compared to this literature, we limit the focus to a test aimed directly at DRWR, which is rarely done in times series work. Furthermore, we benefit from a panel data set across countries, years and industries, providing more information in the data than most studies on aggregate data. In particular, the broader scope across countries and time than other studies of wage rigidity increases our ability to explore whether wage rigidity is affected by economic and institutional variables. Overall, our study should detect other aspects than previous studies on other types of data, and thus be complementary to these studies.

The method we use builds on our previous work on downward nominal wage rigidity (Holden and Wulfsberg, 2007). It is a non-parametric variant of the skewness-location approach of McLaughlin (1994), using data for real hourly earnings only. The idea of our test is as follows. We construct notional (i.e. if no rigidity exists) country-year specific distributions of wage changes, deriving the shape of the distributions on the basis of country-year samples with high real and nominal wage growth, where downward rigidities are less likely to bind. We condition on the empirical location and dispersion of the country-year samples, to allow for the variation in productivity growth, markups, and the extent of sectoral shocks that will exist in an extensive data set as the one we use. Based on the country-year specific notional distributions, we can calculate the probability of a real wage cut for each country year. We then simulate over all country-years, using the country-year specific notional probabilities, and compare the number of simulated, notional real wage cuts with the number of empirical real wage cuts. If the number of notional wage cuts is significantly larger than the empirical counterpart, we conclude that wages are rigid downwards. Robustness checks in Holden and Wulfsberg (2007) indicate that the method has very good properties in detecting the downward wage rigidity that exists in the data.

Most previous work on downward rigidity of wages has focused on nominal rigidity (see surveys in Camba-Mendez et al., 2003, and Holden, 2004). Downward rigidity might apply to nominal values if people care about nominal wages (as some studies indicate they do), if contracts are in nominal terms, or if inflation serves as a vehicle for coordinated reduction in real wages (as implied by Keynes' argument for the existence of downward rigidity of nominal wages). Yet it is real, not nominal wages, that rational agents should care about. There are also several reasons for why we would expect real wages to be rigid downwards, cf. below. Thus it seems reasonable also to explore the existence of DRWR. Distinguishing between downward real and nominal wage rigidity is crucial among other things for the interaction between inflation and wage rigidity.

The remainder of the paper is organised as follows. In section 3 we lay out the theoretical framework, and compare with related literature. Section 4 presents our data, and discusses the empirical approach. Results are given in section 5 and 6, while section 7 concludes.

2 Why DRWR?

The idea that wage setting is influenced by wage aspirations of the wage setters has been suggested and discussed by many economists. As mentioned in the introduction, this idea was highly influential as an early explanation of the persistent European unemployment (see discussion and references in Alogoskoufis and Manning, 1988). It was pointed out that workers had learned to expect a steady growth in real wages induced by the fast growth in the 1950s and 60s, and that this would lead to greater wage pressure if productivity growth became less favourable. The theoretical justification for such an effect was however disputed, see e.g. the sharp critique by Phelps (1992).

Various types of efficiency wage or bargaining models provide a number of explanations as to why, in a situation of high unemployment, real wages do not fall so as to clear the labour market. However, these theories do not by themselves give a role for path dependence where wages are rigid compared to past or aspired levels. Downward rigidity in the sense that the past wage level have an independent effect on the current wage level, in addition to the effect of other factors, requires a role of effects that are usually not included in standard economic models.

More recent work has put forward two main explanations for downward rigidity of real wages. Within the rationality assumptions usually adopted in economics, Ellingsen and Holden (1998) and Postlewaite et al. (2004) show that real wage resistance may follow if consumption patterns are costly to change. For example, if it is costly to sell a house and buy a cheaper one, a risk neutral agent may become risk loving when faced with an unexpected reduction in the real wage. Furthermore, a utilitarian union might prefer to fight to uphold the real wage if an adverse shock takes place, so as to preserve the consumption patterns for the majority of the members, even if this implies that a minority of the workers are laid off and have to sell their house.

A second, behavioural justification of DRWR is to argue up front that agents specifically dislike negative changes in their income. There is now a considerable experimental evidence documenting that many individuals do display such behaviour. A number of studies have documented the existence of *loss aversion*, i.e. that people are more averse to losses relative to their reference level than they are attracted to the same-sized gains (Kahneman and Tversky, 1979). Loss aversion gives rise to *the endowment effect*, which describes the feature that once a person possesses a good, he values it more. Falk and Fehr (2005) show in experiments that employers abstain from accepting bids from workers that involve undercutting of prevailing wages, in the fear that lower wages may have an adverse effect on efforts. The same finding is documented in recent questionnaire and interview studies of managers and owners of firms, see e.g. Bewley (1999) and Agell and Lundborg (2003). Thus, there are good reasons to take seriously the possibility that real wages can be rigid downwards.

3 DRWR and the distribution of wage changes

As a framework for the empirical exercise, we formulate a simple model of firm-level wage bargaining, where loss aversion with respect to past real wages is the source of DRWR. The formulation draws upon Bhaskar (1990), Driscoll and Holden (2004), and in particular McDonald and Sibly (2001). We have chosen a union-firm framework, in part because in most OECD countries, the majority of the workers are in fact covered by collective agreements. However, the key features could also be derived in other settings, e.g. in an efficiency wage framework, as long as one maintains the crucial assumption that workers experience a utility loss if their wage falls.

Let the profits of the firm be a decreasing function of the real wage w,[1]

$$\pi = w^{1-\eta}, \qquad \text{where } \eta > 2. \tag{1}$$

η is the elasticity of product demand. A worker in a job with a given number of hours is assumed to have an indirect utility function which depends on the current and past real wages w and w_{-1}

$$V = w^{1+D\mu} w_{-1}^{-D\mu}, \qquad \text{where } \mu \geq 0 \tag{2}$$

[1] This profit function follows from a model of monopolistic competition where firms set the output price facing a downward sloping demand curve, η is the elasticity of demand, labour is the only production factor, and there is constant returns to scale. Irrelevant constants are omitted.

and where D is a dummy variable which is equal to unity if real wages fall, i.e. if $w < w_{-1}$, and zero otherwise. As long as real wages do not fall, utility is simply linear in real wages. However, we allow for the possibility that workers compare their current wage with their past wage (if $\mu > 0$), incurring an additional utility loss if the wage falls. In this case, utility is still continuous in current and past real wages, and strictly increasing in current real wages. Yet there is a kink in the utility function at the point where the wage is equal to its past value, implying that utility is non-differentiable from the left (i.e. for $w < w_{-1}$) at the point $w = w_{-1}$. All workers are organised in a union, and the union is assumed to represent the interests of the median worker, who, under a layoff by seniority rule is certain to keep his job. Thus (2) can also be thought of as the payoff function of the union.

We model the wage setting by use of the (symmetric) Nash Bargaining Solutionwhere the bargaining outcome is the wage that maximises the product of the firm's and the union's gain from reaching an agreement, i.e. the payoffs as compared to the disagreement points, π_0 for the firm, (for simplicity set to zero), and V_0 for the union.[2]

$$w = \text{argmax}\left[w^{1-\eta}\left(w^{1+D\mu}w_{-1}^{-D\mu} - V_0\right)\right] \quad \text{s.t. } \pi \geq 0 \text{ and } V \geq V_0 \tag{3}$$

The disagreement point of the union, $V_0 > 0$, will depend on variables that influence workers' payoff if the bargainers fail to reach an agreement, e.g.the rate of unemployment, unemployment benefits and outside wages. As shown in appendix A, the solution to (3) is given as follows.

$$w = \begin{cases} \left(\frac{\eta-1}{\eta-\mu-2}w_{-1}^{\mu}V_0\right)^{\frac{1}{1+\mu}} & \text{if } V_0 < V_0^L, \\ w_{-1} & \text{if } V_0 \in [V_0^L, V_0^H], \\ \frac{\eta-1}{\eta-2}V_0 & \text{if } V_0 > V_0^H. \end{cases} \tag{4}$$

[2]We neglect that if the bargaining outcome is affected by past wages, rational agents should take the effect on future bargaining outcomes into consideration during the negotiations. The risk that DRWR may bind in the future, pushing wages up, will lead wage setters to choose a lower wage today, see Holden (1997) and Elsby (2004). However, this will not prevent the effect of DRWR that binds, which is what we look for in the empirical analysis.

where the two critical values for V_0 are defined as

$$\begin{aligned} V_0^L &= \tfrac{\eta-\mu-2}{\eta-1} w_{-1} \\ V_0^H &= \tfrac{\eta-2}{\eta-1} w_{-1} > V_0^L \end{aligned} \qquad (5)$$

As in a standard model without a kink in utility function (e.g. Layard et al., 1991), the wage is a markup over the workers' disagreement point, where the markup depends on the elasticity of product demand η. However, due to the non-differentiability, the outcome also depends on the past wage. If workers are in a weak position due to a low disagreement point, $V_0 < V_0^L$, their real wage will be cut. Yet their resistance towards accepting a cut in their real wage will imply that they get a higher real wage than they would have got if their past real wage had been lower. In Figure 1, this is illustrated by the solid line – the bargaining outcome – coinciding with the upper dashed curve. If workers are in a strong position, $V_0 > V_0^H$, they will get a real wage increase. Yet as they do not have to resist a wage cut, they fight less for higher wages. Thus, the outcome indicated by the solid line in Figure 1 coincides with the lower dashed line. For medium levels of the disagreement point, the real wage remains constant, as the workers are not able to push wages up, nor is the firm able to push wages down.

The histogram in Figure 2 provides a graphical illustration of the wage change distribution from the bargaining model (4). There are many symmetric firms, and the workers' disagreement point is treated as a random variable with normal distribution. We also add an error term to the wage change, to capture among other things the effect of inflation surprises, in view of the fact that wages usually are set on annual basis, and in nominal terms, with none or partial price indexation. The solid line in Figure 2 represents the wage change distribution in the absence of rigidities ($\mu = 0$), in the literature denoted the *notional* wage change distribution (Akerlof et al., 1996). We observe that there is a deficit of negative real wage changes in the histogram compared to the the notional. The parameters of the model are chosen so that 40 percent of the notional real wage changes are negative (see the figure caption for parameter values). Of these potential

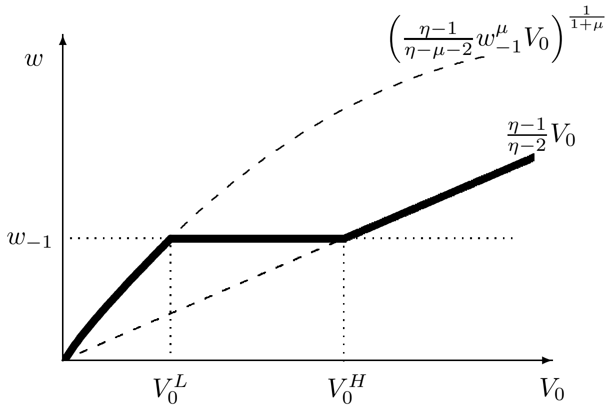

Figure 1: The effect of downward real wage rigidity on real wages.

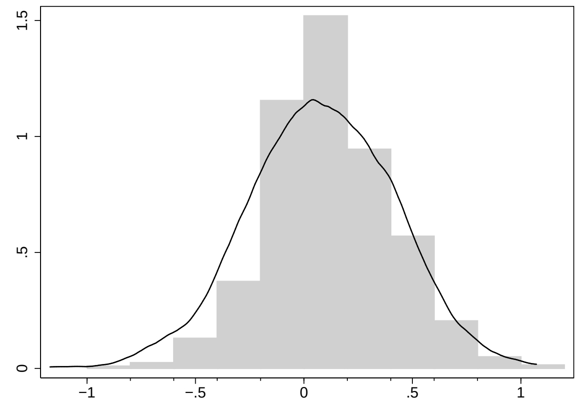

Figure 2: A notional distribution of real wage changes (solid line) and a histogram of a distribution of real wage changes. ($\eta = 3, \mu = 0.1,\ V_0 \sim N(-0.62, 0.35), V_0^L \approx V^{P30}, V_0^H \approx V_0^{P40}$ and $\varepsilon \sim N(0, 0.01)$.)

wage cuts, 15 percent are prevented by DRWR (because $\mu = 0.1$), implying that the rigid wage change distribution is skewed to the right.

Note that the model implies that the deficit of wage cuts depends on the size of the cut. Of notional wage changes below –2 percent, 31 percent are prevented by DRWR, and this percentage increases to 33 for notional wage changes below –5 percent. The intuition behind this feature, that the percentage reduction in the number of small wage cuts is lower, is that while small wage cuts are prevented by DRWR, the larger wage cuts are reduced to smaller cuts by the DRWR. In our case the latter effect dominates the former so that the net effect is in fact a surplus of small wage cuts between –2 and 0 percent compared to the notional.

The theoretical model allows us to make a point on how DRWR relates to the literature which is concerned with the weak response of real wages to unemployment. As pointed out by Alogoskoufis and Manning (1988), one can decompose this weakness into two conceptually different mechanisms: (i) a small direct effect of unemployment on real wages, and (ii) a sluggish adjustment of real wages. In our model, the latter effect is represented by a positive partial effect of past wages, i.e. $\mu > 0$ which is what we look for in the empirical exercises below. The first effect corresponds to a small partial derivative $\partial V_0/\partial U$, (where U is unemployment) which would lead to a reduced dispersion of the distribution of wage changes. This reduced dispersion would, however, not depend on the location of the distribution, i.e. on whether the real wage change is positive or negative.

3.1 Effects of aggregation, compositional changes, spillover and expectations

In contrast to recent micro studies on DRWR, which typically explore the change in hourly earnings of individual job stayers, the observational unit in our data is the change in the average hourly earnings for all manual workers in the industry. There are two key differences. First, our data entails averaging over all job stayers. Second, they are affected by compositional changes, i.e. that the wages of new workers differ from the wages of those who leave (see formal framework in appendix B).

Concerning the wage increase for job stayers, averaging over many workers may mask wage cuts for single workers if other workers receive wage increases. This will tend to reduce the incidence of real wage cuts (given that the economy-wide wage change is positive), as the average wage change has a lower variance than individual wage changes.

As for compositional changes, one may expect to find both systematic and random effects. There will be a systematic negative effect as older workers who leave the labour force on average have higher wages than younger, newcomers to the labour market. This will increase the number of wage cuts. Second, one may expect cyclical effects, as the share of low-skilled workers may increase in expansions, cf. Solon et al. (1994). This latter compositional effect is likely to dampen fluctuations in wage growth, thus reducing the number of wage cuts, as in recessions, when wage growth for job stayers is likely to be low, the increased share of high-skilled workers will imply a positive compositional effect. Overall, the effect of systematic compositional changes on the number of wage cuts is ambiguous. In contrast, the random element arising from unsystematic turnover may be considered as 'noise' relative to individual wage rigidity. The noise effect will imply that we find less DRWR.

In addition to the aggregation and composition effects discussed above, we must take into account that DRWR for some workers have implications for the wages of other workers in the industry. One such effect would be if firms respond to downward rigidity at the individual level by e.g. giving lower wage increases to other workers, or by changing the composition of the workforce. Workers whose wage is cut may quit, and the replacements may accept the lower wage. Furthermore, binding wage rigidity in some firms may raise industry unemployment, pushing down wages in other firms. If these other firms respond by increasing their hiring, it may offset the effects on industry employment. Thus, it seems important also to explore the extent of DRWR at the industry level.

In practice, wage setting is based on wage setters' expected rate of inflation. This implies that wage setters whose expected rate of inflation is below the actual rate of inflation, may end up with a negative real wage change even if binding DRWR pushes the expected real wage change up to zero. However, in our main approach, we try to detect the amount of downward wage rigidity that is present in *actual* real wages, irrespective of whether the flexibility is caused by flexible wage setting, compositional effects or expectational errors. Thus we deflate by the actual rate of inflation. Yet as a sensitivity test, we also try our approach using estimates of expected inflation.

Most of the previous literature on DRWR focusses on the existence of DRWR at zero, i.e. constant real wages. However, the upshot of the theoretical model and discussion above is that in our case it is not obvious that we should focus exclusively on zero. First, the stylised theoretical model shows that DRWR may push up the real wage change, even in the case where the real wage change is negative, implying that the deficit of wage cuts compared to the notional distribution is greater for rates below zero. Second, compositional and other effects mentioned above may lead to downward rigidities at different levels than zero, even if the rigidity is at zero for individual employees. Third, if DRWR binds for wage setters with expected rate of inflation that is, say, one percent below the actual rate of inflation, the real wage change is pushed up to minus one percent. Thus, in addition to real wage rigidity at zero (preventing real wage cuts), we consider rigidity at –2 and –5 percent (i.e. $\Delta w < -2$ and $\Delta w < -5$). For comparison, we also consider *nominal* wage rigidity, i.e. if $\Delta w + \pi < 0$ where π is the inflation rate.

Overall, our study is complementary to previous studies on micro data. On the one hand, aggregation and compositional effects will weaken our ability to detect DRWR, most likely implying that we will detect less DRWR than micro studies. On the other hand, if we do find DRWR in our data, this would imply that DRWR at the individual level is not offset by wage flexibility for other workers in the industry, making it more likely that the DRWR also affects aggregate variables.

4 Empirical approach

We use an unbalanced panel of industry level data for the annual percentage growth of gross hourly earnings for manual workers from the manufacturing, mining and quarrying, electricity, gas and water supply, and construction sectors of 19 OECD countries in the period 1973–1999. The countries included in the sample are Austria, Belgium, Canada, Germany, Denmark, Spain, Finland, France, Greece, Ireland, Italy, Luxembourg, Netherlands, Norway, New Zealand, Portugal, Sweden, the UK and the US. The main data source for wages are harmonised hourly earnings from Eurostat and wages in manufacturing from ILO, measured in a reference period which is typically October or the last quarter of the year.[3] To measure real wages we deflate the nominal wage with the average consumer price index over the year. Thus, we look for rigidity of consumer real wages, not producer real wages, as our theoretical motivation for DRWR is from workers' preferences, which relate to consumer real wages. One observation of real wage growth is denoted Δw_{jit} where j is index for industry, i is index for country and t is index for year. There are all together 9509 observations distributed across 449 country-year samples, on average 21 industries per country-year.

In our data we observe no less than $Y = 3092$ events of real wage cuts, i.e. 32.5 percent of all observations. And only 72 (16 percent) of the 449 country-year samples are without any real wage cuts. Table C1 in the data appendix reports the distribution of real wage cuts and observations across countries and years. More details on the data are provided in appendix C.

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

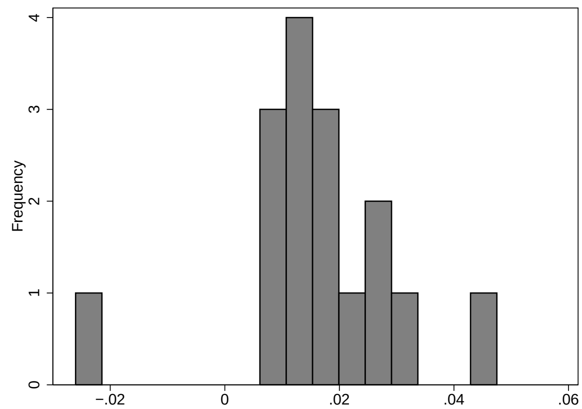

Figure 3: Histogram of real wage growth in Austria, 1988.

To explore the existence of DRWR, we extend the method that we use to detect downward *nominal* wage rigidity in Holden and Wulfsberg (2007). To understand the basic idea of the method, consider the distribution of the real wage changes for 16 industries in Austria 1988 in Figure 3. In this histogram, there are evidently fewer negative real wage changes than in a hypothetical notional distribution as in Figure 2. However, there are two problems with this kind of 'eyeball econometrics'. First, we don't know how the the notional distribution looks like as it is not observed. Second, even if we knew the notional distribution, we need a statistical method to infer whether a deficit of real wage cuts constitutes a significant discrepancy between the empirical and the notional distribution.

We approach these problems by constructing the notional wage change distributions on the basis of country-year samples with high median nominal and real wage growth (details are outlined below), on the presumption that these samples are much less affected by any downward rigidities. Comparing country-year empirical histograms of wage growth, reveal not surprisingly, that the location of the distribution varies considerably between countries and over time, presumably depending on variables like inflation and aggregate productivity growth. The dispersion of the distribution also varies considerably across countries and time, depending on the size and dispersion of industry specific shocks in that country-year. Thus, it seems imperative to allow for cross country variation in location and dispersion also of the notional distributions. To do this, we construct country-year specific notional distributions by adjusting the underlying distribution with the empirical country-year specific median and inter percentile range.

Earlier studies have typically used methods like the LSW statistic (Lebow et al., 2003) or the Kahn test (Kahn, 1997) which involve more restrictive assumptions than our, see discussion in Nickell and Quintini (2003) and Holden and Wulfsberg (2007). The Nickell-Quintini method (Nickell and Quintini, 2003) allows for variation in dispersion across years, but it involves the approximation that the density function is linear over the range relevant for wage rigidity; an assumption that does not hold in our case. Recently, Christofides and Nearchou (2006) have suggested an extension of the Kahn method with much less restrictive assumptions. Overall, by allowing for country-year specific variation in location and dispersion, we allow for more variation than most other methods that are used in the literature.

4.1 Constructing the notional distribution

Specifically, we construct an underlying distribution based on a subset H of the sample, with $S^H = 1331$ observations from the 66 country-year samples where both the median nominal and the median real wage growth are among their respective upper quartiles.[4] To mitigate any effect of DNWR and outliers, we follow Nickell and Quintini (2003) and measure the location by the median, and the dispersion by the range between the 75th and the 35th percentiles, rather than the mean and the standard deviation. The underlying distribution of wage changes is then constructed by use of the 66 samples with high median nominal and real wage growth, by subtracting the corresponding country-year specific median (μ_{it}) and dividing by the inter percentile range ($P75_{it} - P35_{it}$), i.e.

$$\Delta w_s^u \equiv \left(\frac{\Delta w_{jit} - \mu_{it}}{P75_{it} - P35_{it}} \right), \qquad \forall\ j, i, t \in H \text{ and } s = 1, \ldots, S^H \tag{6}$$

[4] Thus, in these country-year samples, the median nominal wage growth is above the 3rd quartile of 11.8 percent, and the median real wage growth is above 2.8 percent.

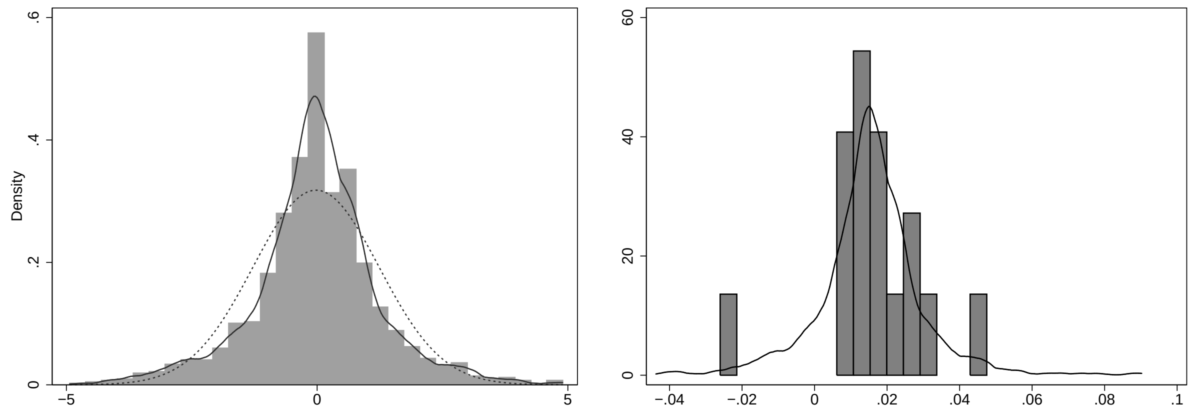

Figure 4: Left: Histogram and kernel density (solid line) of the normalised underlying distribution of wage changes compared to the normal density (dotted line). 14 extreme observations are omitted. Right: Histogram of observed real wage changes and the notional real wage change distribution (solid line) in Austria, 1988.

where subscript s runs over all j, i and t in the 66 country-year samples. The left panel of Figure 4 compares the underlying notional distribution of wage changes (illustrated by the histogram and the kernel density in solid line) with the standard normal distribution (dotted line); we notice that the underlying distribution is asymmetric as it slightly skewed right.[5]

Then, for each of the 449 country-years in the overall sample, we construct notional country-year specific distributions of wage changes by adjusting the underlying wage change distribution for the country-specific observed median and inter percentile range

$$\Delta \widetilde{w}_s^{it} \equiv \Delta w_s^u \Big(P75_{it} - P35_{it} \Big) + \mu_{it}, \qquad \forall\ i, t, \text{ and } s = 1, \ldots, S^H \tag{7}$$

Thus, we have then constructed 449 notional country-year distributions, each consisting of $S^H = 1331$ wage change 'observations'. In the right panel of Figure 4 we have plotted notional distribution for Austria in 1988 together with the empirical histogram. The notional country-year distributions have by construction the same median and interpercentile range as their empirical country-year counterparts, whereas the shape is common for all notional country-year samples,

[5]The coefficient of skewness is 0.26.

given by the shape of the underlying notional distribution.

To explore the validity of the assumption of a common shape of all the notional distributions, we have undertaken Kolmogorov-Smirnov tests of equality between the common underlying distribution against one alternative where the underlying distribution is constructed separately for each country, and one where it is constructed separately for each of the four time periods. The assumption of a common underlying distribution passes easily in all $19 + 27 = 46$ tests with the lowest p-value of 0.211. However, we also try a large number of alternatives to explore the sensitivity of the assumptions, cf. discussion below.

Given the notional country-year specific distributions, we can explore the extent of DRWR at different thresholds by comparing the lower tails of the notional and the empirical distributions. The point estimate of the extent of DRWR follows directly from comparing the incidence of wage cuts in the empirical and the notional distributions at zero, –2 and –5 percent. In order to investigate DRWR at zero percent, we calculate the empirical incidence for each country-year sample it rate as

$$q_{it} = \frac{\#\Delta w_{jit} < 0}{S_{it}}, \qquad \forall\ j \tag{8}$$

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

$$\tilde{q}_{it} = \frac{\#\Delta \widetilde{w}_s^{it} < 0}{S^H}, \qquad s = 1, \ldots, 1331 \tag{9}$$

For country-years where there is at least one notional real wage cut, implying that $\tilde{q}_{it} > 0$, we can calculate an often used measure of DRWR, which is the fraction of real wage cuts prevented, FWCP defined as

$$FWCP_{it} = 1 - q_{it}/\tilde{q}_{it} \tag{10}$$

If, for example, the incidence of wage cuts in the empirical sample is half of that in the notional distribution, then $FWCP = 0.5$, while FWCP is negative if the empirical incidence rate is larger than the notional.

For example, in Austria 1988 the incidence rate of notional real wage cuts, $\tilde{q}_{it}$, is 11 percent while the empirical incidence rate, q_{it} is 6 percent (one real wage cut out of 16 observations). The difference in incidence rates implies that the FWCP was 0.5, providing suggestive evidence for DRWR in in Austria 1988.

There is a lot of variation in the incidence rates (and thus the FWCP's) across the country-year samples. In order to look for interesting patterns in these estimates across countries, we will calculate aggregate incidence rates and the FWCP for countries, periods as well as the overall sample (i.e. all country-years). We aggregate the country-year estimates by pooling the empirical observations in the relevant sample (eg all country-years) implying that the country-year notional incidence rates are weighted according to the number of observations within the country-year. For the overall sample, the fraction of wage cuts prevented at zero percent is $FWCP = 1 - q/\tilde{q} = 1 - 0.325/0.337 = 0.037$. Thus, only about four out of one hundred notional real wage cuts in the overall sample do not result in an observed wage cut due to DNWR. To investigate DRWR at –2 and –5 percent we compute the incidence rates and FWCP accordingly. For the whole sample the fraction of notional real wage changes below the –2 percent level that are prevented by DRWR is 0.113 and at – 5 percent the FWCP is 0.184, i.e. both are considerably higher than the FWCP at zero percent.

The finding of higher FWCP for negative rates of change is consistent with the feature of the theoretical model in section 3 above that DRWR is pushing up real wages even when the real wage change is negative. Interestingly, a calibrated version of the theoretical model provides a remarkably close approximation to the overall empirical results. Choosing two parameter values, $\eta = 3$, $\mu = 0.033$ and drawing V_0 from the normalised underlying distribution as given by (6) (instead of a normal distribution), we obtain FWCP of 0.037, 0.126 and 0.162 at 0, –2 and –5 percent respectively. This close fit strengthens the interpretation from the theoretical model that the higher FWCP for negative rates of change, –2 and –5, is caused by DRWR pushing up real wages even when the real wage change is negative. However, one cannot rule out other explanations, cf. further discussion below.

To test whether our estimates of the FWCP are statistically significant, we exploit that the incidence rate in the notional wage change distribution can be viewed as the probability of a wage cut if there was no DRWR. In other words, under the Null hypothesis of no DRWR, the number of wage cuts in country-year it with, say, 20 industries, is given by 20 independent draws from the binomial distribution with probability $\tilde{q}_{it}$. For samples covering more than one country-year, the number of wage cuts under the Null hypothesis of no DNWR, is given by the combination of several binomial distributions, with respective incidence rates $\tilde{q}_{it}$ as probabilities. Calculating this by use of the appropriate formulaes is, however, computationally extremely demanding, thus we compute the p-values for the number of wage cuts in the empirical samples on the basis of simulations. This is computationally much simpler, and still highly accurate.

Specifically, our simulation method goes as follows. For each country-year it, we draw S_{it} times (i.e. the number of industries in country-year it) from a binomial distribution with probability $\tilde{q}_{it}$. We then add up all the simulated real wage cuts for the relevant country-years, e.g. for all country-years ($\widehat{Y}$), and compare with the total number of wage cuts in the corresponding empirical distribution, e.g. $Y = 3211$. We then repeat this procedure 5000 times, and count the number of times where we simulate more notional wage cuts than we observe, for the overall sample denoted $\#(\widehat{Y} > Y)$. The Null hypothesis is rejected with a level of significance at 5 percent if $1 - \#(\widehat{Y} > Y)/5000 \leq 0.05$. We can also use the simulation results to obtain confidence intervals for our estimate of DNWR.

Note that if DRWR binds in some country-year samples that are used in constructing the underlying wage change distribution, the underlying wage change distribution will be compressed. Likewise, if DRWR affects our measure of the dispersion in certain country-year samples, the associated notional country-year specific distribution will also be compressed. Thus, in these cases the notional probabilities will be biased downwards, reducing the number of simulated wage cuts. This will reduce the power of our test. However, if there is no DRWR, there will be no downward bias, so it will not affect the significance level of our test.

5 Results

Table 1 displays the main results. From the first result columns, we note that in the overall sample, there is highly significant DRWR at zero (i.e. constant real wages), but as noted above the FWCP is only 3.7 percent. Distinguishing between time periods, the downward rigidity appears stronger in the 1970s and late 1990s, with FWCP of 6–7 percent, than in the 1980s and early 1990s.

Table 1 also reports the FWCP across geographical regions; Anglo (Canada, Ireland, New Zealand, the UK and the US), Core (Austria, Belgium, France, Germany, Luxembourg and the Netherlands), Nordic (Denmark, Finland, Norway and Sweden) and South (Italy, Greece, Portugal and Spain). The classification is largely based on geography and language, but typically, countries in the same region are fairly similar when it comes to labour market institutions. Generally, there is a tendency of high unionisation and fairly strict employment protection legislation (EPL) in the Nordic countries, moderate unionisation and stricter EPL in the South, moderate unionisation and moderate EPL in the Core, and lower unionisation and weaker EPL in the Anglo countries. While the point estimates indicate some DRWR for all regions, this is only significant for the Anglo and Core regions.

The subsequent columns show that wages are more rigid at lower growth rates than zero, with FWCP for the overall sample of 11.3 percent at –2, and 18.4 percent at –5. At –2, DRWR is significant for all time periods. The FWCP is highest in the 1970s (16.2 percent). DRWR is highest in the Core region (18.8 percent) and around 11 percent in the Anglo and Nordic countries. The FWCP is significant in all regions except the South. At –5, the estimated FWCP is above 30 percent both in the Core and in the Nordic regions, while in the South, the FWCP is only 9 percent, with a p-value of 6 percent.

As noted above, the larger estimated FWCP at negative growth rates than at zero is consistent with the theoretical model in section 3, where DRWR pushes up negative wage changes, reducing the number of large wage cuts, but also increasing the number of small wage cuts. However, more prevalent downward rigidity at –2 and –5 might also be caused by rigidity at zero for individuals and possibly also firms, combined with some downward flexibility due to compositional changes.

Table 1: The fraction of real wage cuts prevented (FWCP) estimated at 0, –2 and –5 percent and the fraction of nominal wage cuts prevented. p-values in parenthesis.

		Evaluation criteria							
		$\#\Delta w < 0$		$\#\Delta w < -0.02$		$\#\Delta w < -0.05$		$\#(\Delta w + \pi) < 0$	
Category	S	Y	$FWCP$	Y	$FWCP$	Y	$FWCP$	Y	$FWCP$
All observations	9505	3092	0.037 (0.000)	1372	0.113 (0.000)	449	0.184 (0.000)	324	0.260 (0.000)
Periods									
1970–79	2224	453	0.067 (0.016)	214	0.162 (0.000)	59	0.309 (0.000)	5	0.612 (0.011)
1980–89	3717	1545	0.028 (0.024)	755	0.096 (0.000)	270	0.157 (0.000)	74	0.399 (0.000)
1990–94	1906	645	0.020 (0.241)	229	0.109 (0.017)	63	0.195 (0.032)	93	0.231 (0.002)
1995–99	1662	449	0.058 (0.041)	174	0.129 (0.016)	57	0.146 (0.105)	152	0.159 (0.005)
Regions									
Anglo	2961	1274	0.027 (0.054)	568	0.113 (0.000)	188	0.172 (0.001)	153	0.199 (0.001)
Core	3110	788	0.063 (0.004)	248	0.188 (0.000)	48	0.347 (0.000)	125	0.234 (0.000)
Nordic	1976	515	0.032 (0.125)	235	0.117 (0.002)	45	0.311 (0.000)	18	0.498 (0.000)
South	1462	515	0.024 (0.214)	321	0.043 (0.147)	168	0.090 (0.058)	28	0.411 (0.001)

Note: S is the number of observations, Y is the number of observed wage cuts below the relevant limit

Furthermore, it may reflect that some wage setters have inflation expectations below the actual rate of inflation.

For comparison, the last columns report the result on downward nominal wage rigidity, DNWR. We observe that the FWCPs are almost always higher for nominal than for real rigidity, the only exception being the Core region, where there is high real rigidity at the –5 level. The most notable difference is for the South, where the FWCP applying to nominal rigidity is more than 40 percent, and thus four times as high as the corresponding measure for real rigidity at –5 percent. When we combine time periods and regions we find that DRWR at –2 and –5 percent is prevalent in the Anglo, Core and Nordic regions in the 1970s and 80s, see Table D1 in the appendix. In contrast, in the South, there is never significant DRWR, even if the point estimates for the FWCP at –2 and –5 are small and positive in most time periods.

Table 2: The fraction of real wage cuts prevented (FWCP) estimated at 0, –2 and –5 percent and the fraction of nominal wage cuts prevented.

		Evaluation criteria							
		$\#\Delta w < 0$		$\#\Delta w < -0.02$		$\#\Delta w < -0.05$		$\#(\Delta w + \pi) < 0$	
Country	S	Y	*FWCP*	Y	*FWCP*	Y	*FWCP*	Y	*FWCP*
Austria	408	60	0.109 (0.153)	8	0.555 (0.005)	0	1.000 (0.035)	2	0.715 (0.027)
Belgium	575	169	0.035 (0.258)	69	0.216 (0.002)	15	0.387 (0.012)	31	0.232 (0.034)
Canada	627	289	0.033 (0.198)	101	0.099 (0.120)	24	0.269 (0.055)	57	0.078 (0.260)
Denmark	462	161	−0.022 (0.708)	76	0.055 (0.280)	21	0.296 (0.015)	8	0.460 (0.039)
Finland	368	69	0.097 (0.144)	15	0.488 (0.001)	0	1.000 (0.000)	2	0.664 (0.063)
France	556	116	0.013 (0.456)	39	−0.049 (0.674)	8	−0.008 (0.609)	21	−0.196 (0.870)
Germany	665	160	0.080 (0.055)	24	0.171 (0.199)	4	−0.610 (0.893)	16	0.062 (0.453)
Greece	469	195	0.013 (0.401)	133	0.002 (0.511)	71	0.044 (0.339)	7	−0.126 (0.720)
Ireland	463	171	0.020 (0.366)	85	0.148 (0.035)	35	0.190 (0.093)	27	0.326 (0.012)
Italy	312	76	0.004 (0.514)	45	0.033 (0.435)	22	−0.014 (0.587)	0	1.000 (0.040)
Luxembourg	423	125	0.130 (0.015)	58	0.209 (0.022)	18	0.376 (0.016)	32	0.268 (0.022)
Netherlands	483	158	0.033 (0.251)	50	0.167 (0.041)	3	0.533 (0.103)	23	0.386 (0.002)
New Zealand	750	328	0.025 (0.227)	189	0.106 (0.010)	84	0.060 (0.257)	45	0.218 (0.034)
Norway	674	133	0.010 (0.456)	47	0.057 (0.312)	2	0.708 (0.023)	2	0.472 (0.267)
Portugal	411	163	0.044 (0.197)	106	0.143 (0.010)	64	0.196 (0.009)	3	0.859 (0.000)
Spain	270	81	0.028 (0.403)	37	−0.166 (0.858)	11	−0.214 (0.799)	18	−0.060 (0.661)
Sweden	472	152	0.071 (0.055)	97	0.089 (0.031)	22	−0.099 (0.755)	6	0.469 (0.038)
UK	615	199	0.033 (0.235)	98	0.110 (0.047)	35	0.274 (0.003)	18	0.217 (0.127)
US	506	287	0.023 (0.226)	95	0.110 (0.039)	10	0.265 (0.158)	6	0.304 (0.241)

Note: See Table 1

Table 2 shows the results for individual countries. At the –2 level, DRWR is significant with a FWCP around 0.5 in Austria and Finland, and also significant with lower FWCP in 9 additional countries (Belgium, Ireland, Luxembourg, Netherlands, New Zealand, Portugal, Sweden, the UK and the US), while there is no indication of DRWR in 8 countries (Canada, Denmark, France, Germany, Greece, Italy, Norway, Spain). Figure 5 plots the country estimates of DNWR versus

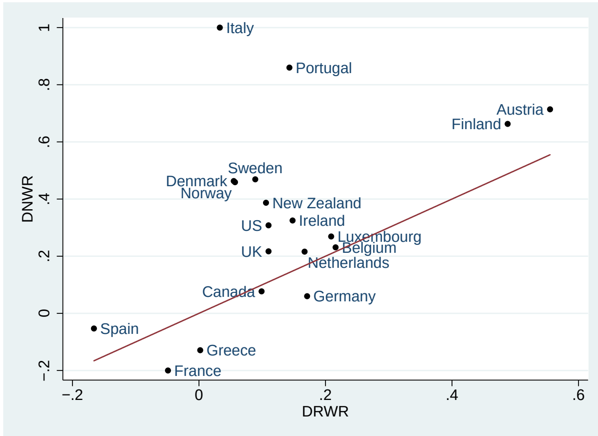

Figure 5: DNWR *and* DRWR *by country.*

DRWR at –2 percent. There is a clear positive correlation, the outliers being Italy and Portugal where DNWR is much stronger.

As an exploration of the robustness of our results, we have varied the key assumptions concerning the shape, the location and the dispersion of the notional distributions. As to the shape of the underlying distribution, we have tried country-specific and period-specific distributions in addition to the common shape assumption. While there is considerable variation in the results from different methods, the broad picture remains the same; the details and results from the robustness tests are reported in the appendix E.

Based on data for individual job stayers, Dickens et al. (2005) find DRWR at the zero level with FWCP ranging from around 5 percent in Greece and the US to around 50 percent in France, Finland and Sweden, with most countries in the range 15–35 percent. Compared to these results, our estimated FWCP are much smaller, in particular at the zero level, but also at –2 and –5 percent. Our lower estimates is as should be expected due to the existence of compositional effects and the scope for firms circumventing wage rigidity at the individual level. Note also that the measure of DRWR in Dickens et al. (2005) is based on individual real wages, thus it will be strongly affected by the wage change distribution within firms and industries, in contrast to our measure based on industry averages.

Table 3: The fraction of industry-years affected (FIYA) estimated at 0, –2 and –5 percent and the fraction of nominal wage cuts prevented. p-values in parenthesis.

		Evaluation criteria							
		$\#\Delta w < 0$		$\#\Delta w < -0.02$		$\#\Delta w < -0.05$		$\#(\Delta w - \pi) < 0$	
Category	S	Y	*FIYA*	Y	*FIYA*	Y	*FIYA*	Y	*FIYA*
All observations	9505	3092	0.012 (0.000)	1372	0.018 (0.000)	449	0.011 (0.000)	324	0.012 (0.000)
Periods									
1970–79	2224	453	0.015 (0.016)	214	0.019 (0.000)	59	0.012 (0.000)	5	0.004 (0.011)
1980–89	3717	1545	0.012 (0.024)	755	0.021 (0.000)	270	0.014 (0.000)	74	0.013 (0.000)
1990–94	1906	645	0.007 (0.241)	229	0.015 (0.017)	63	0.008 (0.032)	93	0.015 (0.002)
1995–99	1662	449	0.017 (0.041)	174	0.016 (0.016)	57	0.006 (0.105)	152	0.017 (0.005)
Regions									
Anglo	2961	1274	0.012 (0.054)	568	0.024 (0.000)	188	0.013 (0.001)	153	0.013 (0.001)
Core	3110	788	0.017 (0.004)	248	0.018 (0.000)	48	0.008 (0.000)	125	0.012 (0.000)
Nordic	1976	515	0.009 (0.125)	235	0.016 (0.002)	45	0.010 (0.000)	18	0.009 (0.000)
South	1462	515	0.009 (0.214)	321	0.010 (0.147)	168	0.011 (0.058)	28	0.013 (0.001)

Note: S is the number of observations, Y is the number of observed wage cuts below the relevant limit

Table 3 displays the fraction of industry-years that are affected by downward rigidity, calculated as the incidence rate of notional wage changes that are below zero, –2 and –5 percent respectively, multiplied by the FWCP evaluated at each threshold. We observe that 1.8 percent of all industry year wage changes are pushed up above the –2 percent threshold, and this is higher than for any of the other thresholds. This estimate is fairly stable across time periods, and the geographical variation is also limited, ranging from 1.0 percent in the South to 2.4 percent in the Anglo countries.

5.1 Expectational errors

To explore other possible explanations for our results, we pursue a number of alternative routes. One possibility is that downward rigidity in reality is applying to *expected* real wages, and that expectational errors lead to additional flexibility. To analyze this possibility, we have simulated under the main procedure but using expected real wage changes, where the measure of expected inflation is derived as country specific AR1 processes of actual inflation. The results are qualitatively similar, even though the estimated FWCP are somewhat smaller: 0.024, 0.066 and 0.165 at levels zero, –2 and –5 respectively (results available at request). The tendency of weaker downward rigidity for expected rather than actual real wages is the opposite of what one would expect if expectational errors as regards inflation is a key cause of real wage flexibility. Even if this finding may also reflect that our estimate for the expected rate of inflation is noisy, it nevertheless suggests that expectational errors is not important for real wage flexibility.

5.2 Symmetric or asymmetric rigidity

We also do the analysis with an entirely different identifying assumption, namely assuming symmetry within each country-year notional sample, following Card and Hyslop. Thus, rather than using a common underlying distribution , we construct for each country-year sample the notional distribution by replacing the observations below the median by the mirror image of the observations above the median. Note that this approach makes no assumptions across country years, in contrast to the main approach, which makes no assumptions on symmetry. As these methods are based on orthogonal assumptions, they constitute a strong test of the robustness of our results. For example, our finding of DRWR using our main approach might in principle be caused by inflation affecting the shape of the wage change distribution apart from what is captured by location and dispersion. However, unless such an effect is asymmetric, it would not lead to a finding of DRWR using the symmetry method. As shown in Table F1 in the appendix, the estimated FWCP are somewhat lower, but the results are qualitatively similar. This finding strengthens our belief that our results are indeed caused by DRWR.

Note that findings from the main approach should be interpreted as evidence of *downward* rigidity, with no bearing on whether the rigidity is symmetric or asymmetric. In contrast, by the symmetry method, we also find evidence for the existence of *asymmetric* rigidity. The finding of

Table 4: The FWCP *at –2 percent by inflation intervals in percent.*

	$\langle -\infty, 2\rangle$	$[2, 4\rangle$	$[4, 6\rangle$	$[6, 8\rangle$	$[8, 10\rangle$	$[10, \infty\rangle$
FWCP	0.155	0.179	0.234	0.072	0.168	0.052
No. of country-years	69	107	67	50	47	109

asymmetry is of independent interest, as it suggests that even if a shock is reversed, real wages need not revert to their original level.

5.3 Real or nominal wage rigidity?

One possible alternative interpretation of our finding of DRWR at -2 and -5 is that the missing real wage cuts in fact are caused by downward *nominal* wage rigidity. We test for this possibility by exploring whether there is any relationship between the FWCP and the rate of inflation. If our findings of DRWR are caused solely by DNWR, the FWCP will be zero for high rates of inflation, and positive for low rates. However, while we see from Table 4 that the FWCP at the -2 percent level is considerably higher when inflation is below two percent i.e. consistent with the downward rigidity being caused by DNWR, than if inflation is above ten percent, the FWCP is even higher for country-years where inflation rates are between four and six percent. Thus, if this deficit of real wage cuts is caused by downward nominal rigidity, there must be some downward nominal rigidity at four percent nominal wage growth, and not only at constant nominal wages.

The idea that DNWR may in fact lead to increased nominal wages is consistent with findings by Holden (1989) and Cramton and Tracy (1992). These papers point out that in a wage bargaining where no strike takes place (often referred to as holdout), while at the same time the workers inflict a cost on the firm by working less efficiently, the firm can be willing to raise nominal wages to ensure an agreement with the workers. Cramton and Tracy (1992) obtain empirical support for this idea on US wage contract data, and Holden (1989, 1998) do the same for wage setting in the Nordic countries.

However, we also note that the FWCP is also high for inflation rates in the interval 8 to 10 percent, and even some above 10 percent inflation. This suggest that part of our finding of DRWR is not caused by DNWR. Yet the overall negative relationship between the FWCP's and the rate of inflation clearly also indicates that DNWR does play a role.

6 The effect of institutional and economic variables

A key question is to what extent the DRWR we detect can be explained by differences in economic and institutional variables. In Holden and Wulfsberg (2007), we find that EPL, union density and unemployment are important determinants of DNWR. Table 5 reports results from Poisson regressions for the same variables, with the number of real wage changes below -2 percent in a country-year as the dependent variable. The first two columns report results for the incidence of real wage cuts (as we condition on the number of observations in the country-year), while the last two columns report results for the FWCP (as we condition on the simulated number of real wage cuts). Inflation is found to have a positive effect on the incidence of real wage cuts. This is not surprising, given that a positive inflation shock will reduce real wages.

Consistent with our findings above, inflation also has a negative impact on the FWCP. Note that this is not caused by the same effect as when inflation reduces the incidence of real wage cuts. If positive inflation shock takes place, it will move the whole real wage change distribution, and and as we condition on the mean real wage change, a positive inflation shock will not affect the FWCP unless there is a link between the inflation shock and the shape of the distribution of the real wage changes. One possible cause of a link like that is if the DRWR applies to expected real wages, and is then eroded if a positive inflation shock takes place. However, our findings in section 5.1 above does not support this interpretation. A more plausible interpretation, consistent with our findings in section 5.3 above, is that under low inflation, downward rigidity of nominal wages also contributes to DRWR.

Unemployment has a significant positive effect on the incidence of real wage cuts, and a negative effect, although not significant, on the FWCP. EPL has the expected sign in all regressions (negative effect on the number of wage cuts), but only significant in one of the pooled regressions. Union density has the expected negative effect on the number of wage cuts in three of the four

Table 5: Maximum likelihood estimates with standard errors in parenthesis from negative binomial regressions in columns one and two and from Poisson regressions in columns three and four. Significant estimates at 5% are indicated by an asterix.

	Incidence of real wage cuts below –2 percent		Fraction of real wage cuts prevented below –2 percent	
	Pooled	Fixed effects	Pooled	Fixed effects
Ln(S_{it})	1 (–)	1 (–)	–	–
Ln(Simulated cuts)	–	–	1 (–)	1 (–)
EPL	-0.191^* (0.061)	-0.056 (0.090)	0.005 (0.022)	0.146 (0.173)
Union density	0.316 (0.372)	-1.657^* (0.523)	0.110 (0.161)	0.672 (0.572)
Inflation	0.119^* (0.014)	0.118^* (0.011)	-0.014^* (0.004)	-0.026^* (0.020)
Unemployment	0.104^* (0.022)	0.165^* (0.020)	-0.014 (0.008)	-0.029 (0.016)
constant	-3.453^* (0.295)	-4.702^* (0.336)	-0.297^* (0.121)	—
log-likelihood	–866.5	–747.2	–563.3	–563.9
Number of observations	422	422	392	392

regressions, and significant in one of the fixed effect regressions. These results give some indication that DRWR is affected by labour market rigidity and unions, and that is it weakened by unemployment. As in our work on DNWR, we also tried other institutional variables in this regression: bargaining coverage, temporary employment, and indices of centralisation and coordination. However, these variables generally had even lower explanatory power than the variables that are included in Table 5.

7 Conclusions

This paper explores whether real wages are rigid downwards, based on data for gross hourly earnings for manual workers from the manufacturing, mining and quarrying, electricity, gas and water supply, and construction sectors of 19 OECD countries in the period 1973–1999. Distinguishing between groups of countries, we find evidence of downward real wage rigidity DRWR in the countries in the Core of Europe, and also some in the Anglo group, but no evidence of DRWR in the

Nordic and southern European countries. The extent of DRWR is small. In the Core, the FWCP is 6 percent, implying that 6 percent of all notional real wage cuts are prevented by downward rigidity. In the Anglo group, the FWCP is 3 percent. We find stronger evidence of downward rigidity at negative rates of change in the real wages, at -2 or -5 percent. In the Core, 19 percent of all notional wage cuts below -2 percent is prevented by DRWR, while in the Anglo and Nordic countries, the FWCP at -2 is around 11–12 percent. At -5 percent, the FWCP is above 30 percent in both the Core and the Nordic countries, 17 percent in the Anglo group, and 9 percent in the South (the latter with p-value of 6 percent). There is, however, considerable heterogeneity within the groups.

The higher FWCP at negative real wage changes is consistent with our theoretical model, where workers' resistance against a wage cut implies that DRWR pushes up the wage even when the real wage change is negative. Thus, even if small wage cuts are prevented by DRWR, larger wage cuts are made smaller by DRWR, dampening the reduction in the number of small real wage cuts. However, the greater FWCP for negative real wage changes than at zero may also reflect a negative effect on average wages from compositional changes, where old high-wage workers are replaced by young low-wage workers, increasing the incidence of small negative changes in average wages.

Comparing downward rigidity of nominal and real wages, we find that downward nominal wage rigidity is much more significant, and of greater magnitude, across regions and time periods, although the difference in the late 1990s is much smaller than it is in earlier periods. However, we also find some DRWR in high inflation periods, indicating that the DRWR that we find is an independent phenomenon that is not only caused by DNWR combined with low rate of inflation.

In contrast to most previous studies of DRWR, which consider wages of job stayers, we use data for average wages at industry level. Thus, if DRWR for job stayers is circumvented by firms that give lower wage increases to other workers, or hire new workers at lower wages, it will not be detected in our data. Nor will our data capture downward wage rigidity in some firms, if wages are flexible in other firms in the same industry, so that overall industry wages are flexible. However, in these cases it is questionable whether the wage rigidity at worker or firm level will have any impact at the aggregate level. In contrast, if the DRWR prevails also at industry level, an effect on aggregate output and employment seems more likely.

Our finding of DRWR is based on a univariate framework, including only data for real wage growth. The univariate framework involves the advantage that no assumptions on explanatory variables and functional forms are needed. Thus, when we detect DRWR, we can be fairly confident that this is indeed a feature of the data. On the other hand, by using a univariate framework, we clearly cannot directly explore how wages respond to changes in variables like unemployment and productivity. This would be an interesting extension of our work.

A further important extension of our work is to explore whether the wage rigidity we find has any effects on other relevant economic variables. From a theoretical perspective, this is not clear. Barro (1977) pointed out that short run real wage rigidity need not have any employment effects, as risk averse parties to a long term contract may avoid short run fluctuations in real remuneration, without letting it have any inefficient allocative effects. However, more recently Shimer (2005) and Hall (2005) have argued that real wage rigidity with allocative effects on vacancies and hirings is crucial to explain vacancies and recruitment behaviour over the cycle. Furthermore, there is fairly strong evidence that unemployment variation across time and OECD countries is related to institutional labour market variables, like unemployment benefits, union density and the degree of coordination of wage setting, that is likely to reflect differences in wage setting behaviour (see e.g. Nickell et al., 2003). Within this framework, one would expect increased wage pressure due to binding downward real wage rigidity to induce lower employment and higher unemployment, in line with the early explanations of the rise in European unemployment in the 1970s, cf e.g. Grubb et al. (1983) and Bruno and Sachs (1985). To test this conjecture seems an important task for future research. However, one should also bear in mind that we find only a limited amount of downward real wage rigidity, suggesting that the possible effects on employment and output in any case would be fairly small.

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A The Nash solution

The first order condition for the Nash bargain requires that the left-hand derivative (i.e. $w < w_{-1}$, so that $D = 1$) of the Nash maximand satisfies

$$\frac{d\,[.]^{-}}{dw} = (1-\eta)w^{-\eta}\left(w^{1+\mu}w_{-1}^{-\mu} - V_0\right) + w^{1-\eta}(1+\mu)w^{\mu}w_{-1}^{-\mu} \geq 0 \tag{A1}$$

while that right-hand derivative ($w \geq w_{-1}$) satisfies

$$\frac{d\,[.]^{+}}{dw} = (1-\eta)w^{-\eta}\left(w - V_0\right) + w^{1-\eta} \leq 0 \tag{A2}$$

Furthermore, we know that either $w = w_{-1}$, or one of (A1) or (A2) hold with equality. In the case where (A1) holds with equality, we obtain

$$w^{-} = \left(\frac{\eta-1}{\eta-\mu-2}w_{-1}^{\mu}V_0\right)^{\frac{1}{1+\mu}} \tag{A3}$$

while the case where (A2) holds with equality, we obtain

$$w^{+} = \frac{\eta-1}{\eta-2}V_0 \tag{A4}$$

The lower critical values for V_0, V_0^L, is found by imposing $w = w_{-1}$ in (A3), and then solving for V_0. As w^{-} is strictly increasing in V_0, it follows directly that $w^{-} < w_{-1}$ for $V_0 < V_0^L$. It is also straightforward to show that $w^{+} < w_{-1}$ for $V_0 < V_0^L$.

Correspondingly, V_0^H is found by imposing $w = w_{-1}$ in (A4), and then solving for V_0. As w^{+} is strictly increasing in V_0, it follows directly that $w^{+} > w_{-1}$ for $V_0 > V_0^H$. Furthermore, it is straightforward to show that $w^{-} > w_{-1}$ for $V_0 > V_0^H$. Thirdly, it is straightforward to show that in the interval $V_0 \in [V_0^L, V_0^H]$, we have $w^{+} < w_{-1} < w^{-}$.

It is then clear that for $V_0 < V_0^L$, the Nash maximand is maximized by equality in (A1), where $w = w^- < w_{-1}$. For $V_0 > V_0^H$, the Nash maximand is maximized by equality in (A2) and $w = w^+ > w_{-1}$. For $V_0 \in [V_0^L, V_0^H]$, the Nash maximand is maximized by $w = w_{-1} \in [w^+, w^-]$, where both (A1) and (A2) hold, with strict inequalities in the interior of the interval. QED

B Decomposing the change in industry wages

Consider a simple framework where we keep hours per worker constant, and where we assume that hiring and separations occur between, and not within, years (relaxing these assumptions is straightforward, but makes notation cumbersome and less transparent). Observe that the average hourly wages in an industry in year $t, \overline{w}_t$, can be decomposed into the average wage of all job stayers, $\overline{w}_t^s$, and that of all entrants, $\overline{w}_t^e$, i.e. $\overline{w}_t = \overline{w}_t^s(s/l_t) + \overline{w}_t^e(e/l_t)$, where $s + e = l_t$. (To minimise possible confusing notation, we let s refer to the number of workers who stay from year $t-1$ to year t, without time subscript, and likewise for entrants in period t, e, and quitters in period $t-1$.) Correspondingly, the average wage in year $t-1$ can be decomposed into the average wage of workers who stay on year t, $\overline{w}_{t-1}^s$, and that of all who quit or are laid off, $\overline{w}_{t-1}^q$, i.e. $\overline{w}_{t-1} = \overline{w}_{t-1}^s(s/l_{t-1}) + \overline{w}_{t-1}^q(q/l_{t-1})$, where $s + q = l_{t-1}$. Using that $\overline{w}_t^s \frac{s}{l_t} = \Delta\overline{w}_t^s + \overline{w}_{t-1}^s \frac{s}{l_t} - \Delta\overline{w}_t^s \frac{e}{l_t}$, the growth in average wages is thus

$$\begin{aligned}
\Delta\overline{w}_t = \overline{w}_t - \overline{w}_{t-1} &= \overline{w}_t^s \frac{s}{l_t} + \overline{w}_t^e \frac{e}{l_t} - \left(\overline{w}_{t-1}^s \frac{s}{l_{t-1}} + \overline{w}_{t-1}^q \frac{q}{l_{t-1}} \right) \\
&= \Delta\overline{w}_t^s + \left[\overline{w}_{t-1}^s \frac{s}{l_t} + \left(\overline{w}_t^e - \Delta\overline{w}_t^s \right) \frac{e}{l_t} - \left(\overline{w}_{t-1}^s \frac{s}{l_{t-1}} + \overline{w}_{t-1}^q \frac{q}{l_{t-1}} \right) \right]
\end{aligned}$$

where the first term is the wage increase for job stayers, while the term in the brackets is the composition effect, measured as the average wage in year $t-1$ of those who work in year t relative to those who work in year $t-1$. If the employment level is constant, $l_t = l_{t-1}$, and $e = q$, the expression reduces down to $\Delta\overline{w}_t^s(s/l_t) + (\overline{w}_t^e - \overline{w}_{t-1}^q)(q/l_t)$, i.e. the wage growth for job stayers times the ratio of stayers to total employment, plus the difference in wages for entrants relative to quitters, times the quit rate.

C Data appendix

We have obtained wage data from Eurostat for all countries except Austria, Canada, Finland, New Zealand Norway, Sweden and the US (see below). The precise source is Table HMWHOUR in the *Harmonized earnings* domain of under the *Population and Social Conditions* theme in the NEWCRONOS database. Our wage variable (HMWHOUR) is labelled *Gross hourly earnings* *of manual workers in industry.* Gross earnings cover remuneration in cash paid directly and regularly by the employer at the time of each wage payment, before tax deductions and social security contributions payable by wage earners and retained by the employer. Payments for leave, public holidays, and other paid individual absences, are included in principle, in so far as the corresponding days or hours are also taken into account to calculate earnings per unit of time. The weekly hours of work are those in a normal week's work (i.e. not including public holidays) during the reference period (October or last quarter). These hours are calculated on the basis of the number of hours paid, including overtime hours paid. Furthermore, we use data in national currency and males and females are both included in the data. The data for Germany does not include GDR before 1990 or new *Länder.*

The data are recorded by classification of economic activities (NACE Rev. 1). The sections represented are Mining and quarrying (C), Manufacturing (D), Electricity, gas and water supply (E) and Construction (F). We use data on various levels of aggregation from the section levels (e.g. D Manufacturing) to group levels (e.g. DA 159 Manufacturing of beverages), however, using the most disaggregate level available in order to maximize the number of observations. If for example, wage data are available for D, DA 158 and DA 159, we use the latter two only to avoid counting the same observations twice.

Wage data for Austria, Canada, Finland, New Zealand, Sweden and the US are from Table 5B 'Wages in manufacturing' in LABORSTA, the Labour Statistics Database, ILO. The data are recorded by ISIC, Three digit level covering the same sectors as the Eurostat data. Wage data for Norway are from Table 210 National Accounts 1970–2003, Statistics Norway, recorded by NACE Rev. 1. The sections represented are the same as for the Eurostat data.

The average number of observations per country-year sample is 20.5, with a standard error of 4.7. The distribution of the number of wage cuts relative to the number of observations on years and countries are reported in Table C1.

We have removed ten extreme observations from the sample.

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

The primary sources for the employment protection legislation (EPL) index, which is displayed in in Holden and Wulfsberg (2007, Table A.2), are OECD (2004) for the 1980–1999 period and Lazear (1990) for the years before 1980. We follow the same procedure as Blanchard and Wolfers (2000) to construct time-varying series which is to use the OECD summary measure in the 'Late 1980s' for 1980–89 and the 'Late 1990s' for 1995–99. For 1990-94 we interpolate the series. For 1973–79 the percentage change in Lazear's index is used to back-cast the OECD measure. However, we are not able to reconstruct the Blanchard and Wolfers data exactly.

Data for union density is from OECD. Data for Greece for 1978 and 1979 are interpolated while data before 1977 is extrapolated at the 1977 level.

Table C1: The distribution of real wage cuts relative to the number of observations by countries and years

Year	*Austria*	*Belgium*	*Canada*	*Germany*	*Denmark*	*Spain*	*Finland*	*France*	*Greece*	*Ireland*	*Italy*	*Luxembourg*	*Netherlands*	*New Zealand*	*Norway*	*Portugal*	*Sweden*	UK	US	*Total*
1973		0/20		0/23	0/19	–	0/16	0/20	1/12	–	1/24	0/14	0/19	2/24	1/28	–	–	1/21	8/20	14/260
1974	0/16	0/20	4/24	2/23	3/19	–	0/16	1/21	11/13	–	8/24	0/14	0/19	2/25	0/28	–	–	1/21	19/20	51/303
1975	0/16	1/20	0/24	7/24	3/19	–	1/16	2/22	0/13	–	1/24	1/15	1/19	16/25	0/28	–	–	5/21	8/18	46/304
1976	1/16	6/21	0/24	0/24	2/19	–	7/16	1/22	0/13	11/18	4/24	1/15	15/19	25/25	0/28	–	–	22/23	/18	95/325
1977	1/16	1/21	1/24	1/24	14/19	–	12/16	1/22	0/13	6/18	2/24	7/15	0/19	15/25	0/28	–	–	22/23	2/18	85/325
1978	0/16	3/21	23/24	0/24	5/19	–	8/16	1/22	0/13	1/18	1/24	8/15	2/20	2/25	4/28	–	4/26	1/23	4/18	67/352
1979	3/16	0/21	16/24	3/24	1/20	–	0/16	4/22	3/13	1/20	4/24	2/15	10/19	7/25	9/28	–	12/28	2/22	18/18	95/355
1980	4/16	1/21	9/24	0/24	20/20	–	5/16	3/22	4/13	15/19	15/24	3/15	15/19	23/25	18/28	–	14/28	11/22	17/18	177/354
1981	8/16	3/21	14/23	22/24	14/20	–	2/16	2/22	5/13	14/19	0/24	9/15	17/19	4/25	24/28	8/22	28/28	12/22	12/18	198/375
1982	5/16	18/21	11/20	19/24	11/20	–	4/16	5/21	0/13	15/20	10/24	13/16	3/18	9/25	13/28	8/22	27/28	6/22	4/18	181/372
1983	3/16	20/21	10/20	12/24	18/20	–	1/16	0/21	6/11	9/18	5/24	9/16	14/18	22/25	9/28	17/22	27/27	1/24	1/18	184/369
1984	12/16	21/21	6/28	15/27	18/20	–	0/16	21/22	1/17	6/18	21/24	10/16	15/16	27/25	1/28	21/22	1/27	2/24	13/18	211/385
1985	0/16	13/21	17/28	1/27	3/20	–	0/16	9/23	12/18	5/20	4/24	9/16	8/17	28/25	1/28	12/22	6/28	22/24	11/18	161/391
1986	0/16	15/21	19/28	0/27	8/20	–	0/16	5/23	18/18	2/21	–	0/14	2/18	3/25	2/28	3/22	1/28	2/24	7/18	87/367
1987	3/16	8/21	18/28	0/27	0/20	–	0/16	6/23	17/18	8/20	–	3/14	0/18	23/25	0/28	1/22	/28	/24	17/18	104/366
1988	1/16	6/21	18/28	0/27	3/20	–	0/16	14/23	1/18	3/20	–	3/14	3/18	7/25	21/28	8/21	1/28	1/25	17/18	107/367
1989	4/16	3/22	16/28	4/27	18/20	–	4/16	6/23	1/17	12/20	–	1/17	1/17	10/25	12/28	18/24	/28	6/26	19/20	135/371
1990	0/16	2/24	15/28	0/27	3/20	5/26	1/16	4/23	17/24	3/21	–	6/16	3/17	16/25	3/28	8/23	5/28	17/25	19/20	127/408
1991	1/16	2/24	18/28	1/27	3/20	1/26	5/16	4/23	17/25	8/21	–	3/16	7/17	9/25	0/28	6/23	–	5/25	18/20	108/380
1992	1/16	1/23	5/26	7/24	3/20	4/26	11/16	2/23	22/25	4/21	–	1/17	0/17	7/25	9/28	3/23	3/13	1/25	14/20	98/388
1993	8/16	4/22	11/26	15/24	4/20	7/26	7/16	12/24	16/25	2/21	–	3/17	4/14	17/25	4/28	8/23	14/14	12/25	17/20	165/386
1994	2/16	2/22	5/20	14/26	–	15/26	1/16	12/15	6/25	15/21	–	3/17	4/8	17/25	0/28	15/23	5/14	19/22	12/20	147/344
1995	1/16	21/22	13/20	0/26	–	9/26	0/16	1/10	9/25	12/20	–	5/17	0/10	17/25	2/28	10/23	2/14	4/21	13/20	119/339
1996	0/14	8/27	3/20	12/25	–	13/26	–	0/12	11/25	9/23	–	11/19	3/20	6/25	0/28	0/23	/14	3/26	7/20	86/347
1997	1/14	9/28	13/20	23/31	1/16	8/29	–	0/27	4/25	6/23	–	8/14	5/23	4/25	0/28	0/23	/15	10/27	5/18	97/386
1998	1/14	1/28	9/20	2/31	2/16	7/29	–	0/25	13/24	4/23	–	4/17	5/23	4/25	0/28	17/29	1/14	11/28	2/18	83/392
1999	0/14	–	15/20	–	4/16	12/30	–	–	–	–	–	2/17	21/22	6/25	0/22	–	1/14	–	3/18	64/198
Total	60/408	169/575	289/665	160/665	161/462	81/270	69/368	116/556	195/469	171/463	76/312	125/423	158/483	328/674	133/750	163/411	152/472	199/615	287/506	3092/9509

Data for bargaining coverage is from OECD (2004, Table 3.5) which provide data for 1980, 1990 and 2000. Data for the intervening years are calculated by interpolation while the observations for 1980 are extrapolated backwards. Data for Greece and Ireland is only available for 1994 from ILO (1997, Table 1.2). This observation is extrapolated for the entire period.

The incidence of temporary employment is defined as the fraction of temporary to total employment. Data from 1983 is from OECD's Corporate Data Environment, Table *Employment by permanency of the (main) job*. Data for Finland 1995 and 1996 and Norway are from Eurostat. Data for Sweden are provided by the Statistics Sweden (SCB). Lacking information prior to 1983, we have chosen not to extrapolate the data.

D Combined regions and periods

Table D1: The fraction of real wage cuts prevented (FWCP) estimated at 0, –2 and –5 percent and the fraction of nominal wage cuts prevented. See Table 1 for notes.

			Evaluation criteria							
			$\#\Delta w < 0$		$\#\Delta w < -0.02$		$\#\Delta w < -0.05$		$\#(\Delta w + \pi) < 0$	
Region	Period	S	Y	*FWCP*	Y	*FWCP*	Y	*FWCP*	Y	*FWCP*
Anglo	1970–79	698	245	0.048 (0.087)	143	0.103 (0.015)	38	0.248 (0.010)	0	1.000 (0.190)
Anglo	1980–89	1149	564	0.029 (0.118)	269	0.110 (0.003)	103	0.155 (0.020)	26	0.453 (0.001)
Anglo	1990–94	595	286	0.019 (0.322)	89	0.168 (0.022)	25	0.146 (0.235)	59	0.186 (0.039)
Anglo	1995–99	519	179	0.003 (0.500)	67	0.062 (0.303)	22	0.137 (0.271)	68	0.020 (0.452)
Core	1970–79	794	86	0.177 (0.014)	23	0.406 (0.003)	5	0.585 (0.019)	4	0.515 (0.083)
Core	1980–89	1183	430	0.033 (0.128)	136	0.163 (0.003)	18	0.434 (0.004)	40	0.305 (0.005)
Core	1990–94	587	128	0.073 (0.145)	29	0.204 (0.104)	5	0.402 (0.144)	18	0.244 (0.108)
Core	1995–99	546	144	0.063 (0.132)	60	0.114 (0.108)	20	0.062 (0.416)	63	0.144 (0.061)
Nordic	1970–79	474	86	0.026 (0.400)	27	0.228 (0.059)	3	0.724 (0.003)	1	0.374 (0.524)
Nordic	1980–89	888	335	0.017 (0.296)	182	0.068 (0.049)	39	0.189 (0.050)	3	0.665 (0.019)
Nordic	1990–94	354	81	0.037 (0.358)	23	0.204 (0.089)	3	0.301 (0.369)	12	0.294 (0.105)
Nordic	1995–99	260	13	0.310 (0.088)	3	0.573 (0.074)	0	1.000 (0.132)	2	0.759 (0.009)
South	1970–79	258	36	−0.020 (0.601)	21	0.058 (0.442)	13	−0.088 (0.695)	0	1.000 (0.244)
South	1980–89	497	216	0.034 (0.195)	168	0.038 (0.216)	110	0.072 (0.129)	5	0.446 (0.105)
South	1990–94	370	150	−0.040 (0.787)	88	−0.039 (0.709)	30	0.174 (0.134)	4	0.482 (0.115)
South	1995–99	337	113	0.093 (0.089)	44	0.180 (0.075)	15	0.161 (0.289)	19	0.353 (0.022)

E Robustness

As a further exploration of the robustness of our results, we perform an extensive sensitivity analysis of our main approach, by varying the key assumptions. More specifically, we try different assumptions along three dimensions of the underlying notional distribution, namely the shape, the location and the dispersion. As to the shape of the underlying distribution, in addition to the common distribution, we also try country-specific and period-specific distribution. Specifically, we construct the underlying notional distribution separately for each country (period), based on all observations from this country (period), and then proceed with the method as before. For the location of the distribution, we follow Knoppik and Beissinger (2003) by also trying the 80th percentile, with the motivation that in some country-years, the median wage change is potentially affected by DRWR, while this is rarely the case for the 80th percentile. For the dispersion of the distribution, we consider two alternatives to the inter percentile range. As the 35th percentile potentially quite often is affected by DRWR, we also consider an alternative that does not rely on any specific percentile, namely the mean deviation from the mean, MDEV. However, if DRWR is at work, it will compress the left part of the distribution and thus reduce both these measures of dispersion, involving a downward bias in our measure of downward rigidity. To avoid this, we also measure dispersion by the predicted inter percentile range, found in country-specific regressions of the actual inter percentile ranges on the lagged inter percentile range, inflation, the average inter percentile range in other countries in the same region, a trend and a squared trend. Note that several of these alternative measures are likely to involve considerably more random noise than the main measures (MDEV and the 80th percentile are sensitive to outliers, while the predicted IPR is sensitive to prediction error). Thus, we would expect considerable variation in the estimated FWCP. However, trying such diverse sets of measures nevertheless provide information about the robustness of the broad picture. All together, to construct the notional distributions we use 18 different combinations of three distributional shapes (common, country-specific, period-specific) $\times$ two measures of location (median or 80th percentile) $\times$ three dispersions (IPR, MDEV or predicted IPR).

Figure E1 presents measures of the 18 estimates of the FWCP for each of the limits 0, –1, –2, –5 and $-\pi$ percent (i.e. nominal zero). The estimates from Table 1 is indicated with a dot, a cross indicates an estimate that is significant at the five percent level, while the plus signs are FWCP estimates that are not significant. The number above the estimates is the number of significant estimates. We observe that while there is considerable variation in the estimates, the main features from the Table 1 still hold. There is clear evidence of DRWR at –2 and –5 percent growth rates, where almost all the FWCP estimates are significant. There is some evidence of downward real rigidity at zero or –1 percent, but these point estimates are closer to zero, and

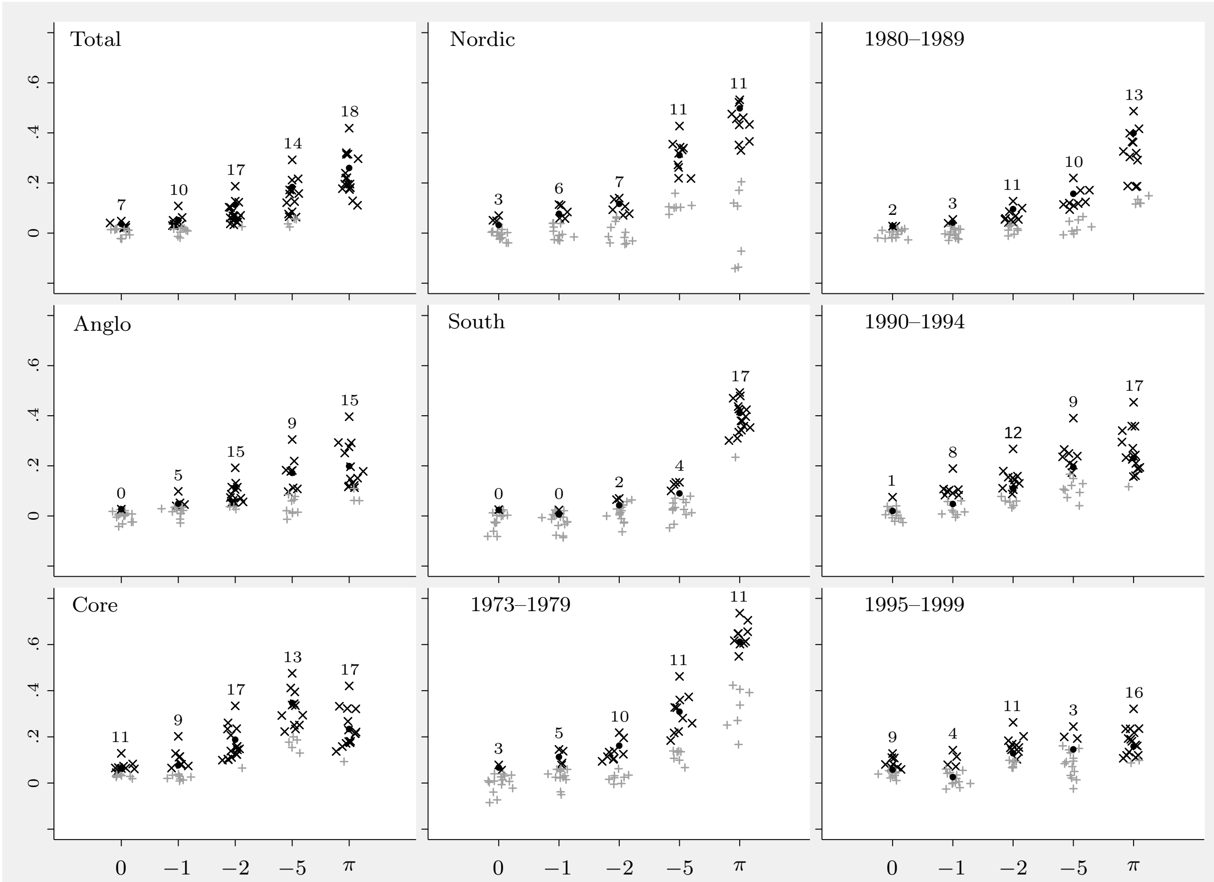

Figure E1: Estimates of the fraction of real wage cuts prevented evaluated at 0, –1, –2 and –5 percent, and the fraction of nominal wage cuts prevented. There are 18 estimates per evaluation criteria. A cross indicates a significant estimate at 5 percent while a plus sign indicates an insignificant estimate. The number of significant estimates reports are reported on top of each column.

fewer are significantly larger than zero. The evidence of DNWR is stronger than the evidence of DRWR, with higher FWCP estimates, where all are significant. In Figure E1, we also display similar charts for time periods and regions. There is considerable variation, yet the broad picture is not affected.

F Symmetric notional distributions

Table F1: The fraction of real wage cuts prevented (FWCP) estimated at 0, –2 and –5 percent and the fraction of nominal wage cuts prevented. Symmetric and country-year specific notional distributions. p-values in parenthesis.

		Evaluation criteria							
		$\#\Delta w < 0$		$\#\Delta w < -0.02$		$\#\Delta w < -0.05$		$\#(\Delta w - \pi) < 0$	
Category	S	Y	$FWCP$	Y	$FWCP$	Y	$FWCP$	Y	$FWCP$
All	9505	3092	0.023	1372	0.070	449	0.127	324	0.200
			(0.020)		(0.000)		(0.000)		(0.000)
Periods									
1970–79	2224	453	0.036	214	0.041	59	0.170	5	0.501
			(0.128)		(0.207)		(0.040)		(0.061)
1980–89	3717	1545	0.018	755	0.035	270	0.100	74	0.230
			(0.111)		(0.073)		(0.010)		(0.009)
1990–94	1906	645	0.008	229	0.120	63	0.102	93	0.213
			(0.398)		(0.007)		(0.194)		(0.005)
1995–99	1662	449	0.047	174	0.167	57	0.219	152	0.159
			(0.078)		(0.001)		(0.017)		(0.005)
Regions									
Anglo	2961	1274	0.003	568	0.067	188	0.134	153	0.124
			(0.428)		(0.007)		(0.008)		(0.029)
Core	3110	788	0.073	248	0.152	48	0.334	125	0.220
			(0.001)		(0.000)		(0.000)		(0.000)
Nordic	1976	515	−0.012	235	0.018	45	0.118	18	0.359
			(0.693)		(0.349)		(0.151)		(0.018)
South	1462	515	0.023	321	0.040	168	0.036	28	0.333
			(0.224)		(0.162)		(0.279)		(0.007)

Note: S is the number of observations, Y is the number of observed wage cuts below the relevant limit

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