How Strong is the Macroeconomic Case for Downward Real Wage Rigidity?*

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Abstract

We explore the existence of DRWR at the industry level, based on data from 19 OECD countries for the period 1973–99. We find that DRWR compresses the distributions of industry wage changes overall, as well as for specific geographical regions and time periods, but there are not many real wage cuts that are prevented. More important, however, DRWR attenuates larger real wage cuts, thus leading to higher real wages. We find stronger evidence for downward nominal wage rigidity than for DRWR. There is evidence that real wage cuts are less prevalent in countries with strict employment protection legislation and high union density.

JEL: J3, J5, C14, C15, E31
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1 Introduction

In recent years, real wage rigidity has become a key component of several contributions to the business cycle and monetary policy literature, see e.g. Blanchard and Gali (2007), Hall (2005), Krause and Lubik (2006), and Shimer (2005). However, there is considerable controversy about whether real wages really are rigid. We focus on one specific aspect of sluggish wages, namely to what extent real wages are rigid downwards. If present, downward real wage rigidity (DRWR) is particularly relevant for how the economy functions in a downturn, as DRWR affects how adverse shocks may lead to higher unemployment rather than lower wages.

Several recent studies have found evidence for considerable DRWR for job stayers in a number of OECD countries, (see Barwell and Schweitzer, 2004; Bauer et al., 2007; Christofides and Li, 2005; and Dickens et al., 2005), as well as in experimental work (Falk and Fehr, 2005) and in surveys of managers and firm owners (Bewley, 1999 and Agell and Lundborg, 2003). While these findings are useful for our understanding of individual wage setting, the effects on aggregate variables remain open. Even if individual wages are rigid in real terms, firms may respond by other means, like changing the composition of the work force. And even if wage rigidity binds in some firms, jobs may be shifted over to other firms with lower or more flexible wages. With annual job turnover rates above 20 percent, as is the case in many OECD countries (see Haltiwanger et al., 2008), and generally higher worker turnover rates, rigid wages for many individual job stayers need not imply the same rigidity of average wages. Consistent with this hypothesis, Farès and Lemieux (2001) find that in Canada most of the real wage adjustments over the business cycle are experienced by new entrants.

In contrast to the previous literature, we explore the existence of DRWR at the industry level, based on data from 19 OECD countries for the period 1973–99, covering in total 449 country-year samples. The key aim is to explore whether the effects of the wage rigidity found in micro data are entirely offset by compositional and other changes, or whether there remains an effect of individual downward rigidity on aggregate wage data.

In our view it is important to distinguish between these two alternatives. If we find no sign of DRWR in industry-level wage data, it seems hard to believe that the individual rigidity has a non-negligible effect on industry output or employment. On the other hand, if we detect DRWR in industry-level wage data, we know that the rigidity prevails in spite of varying compositional effects. In this case effects on industry output and employment also seem more likely.

We outline a simple theoretical model of DRWR, which we use as a framework for organizing the data and interpret our empirical findings. The empirical analysis is a variant of the wage change approach initiated by McLaughlin (1994), drawing upon our previous work on downward nominal wage rigidity (Holden and Wulfsberg, 2008). The key idea is to detect possible DRWR by comparing the empirical real wage change distribution with a constructed counterfactual or notional (as if no rigidity exists) wage change distribution. We construct the shape of the notional wage change distribution on the basis of country-year samples with high real and nominal wage growth, where downward rigidities are less likely to bind. If the empirical number of real wage cuts is significantly smaller than we would expect from the notional distributions, we conclude that wages are rigid downwards. Robustness checks in Holden and Wulfsberg (2008) indicate that this method has very good properties for detecting the downward wage rigidity that exists in the data.

The paper is organized as follows. In section 2 we present the theoretical model, while section 3 describes data and the empirical approach. In section 4, we present the main results. We find a fairly small but statistically significant DRWR for the OECD countries, and in particular the extent of large real wage cuts is reduced. In section 5 we make use of the broad scope of our data across countries and time, and explore whether the variation in DRWR can be explained by economic and institutional variables. The analysis shows that real wage cuts are less prevalent in countries with strict employment protection legislation and high union density. Section 6 concludes and discusses the relevance of our results for using wage rigidity in the context of business cycle analysis.

2 DRWR and the Distribution of Wage Changes

Recent studies have put forward two main explanations for DRWR. First, within a rational behavior framework, Ellingsen and Holden (1998) and Postlewaite et al. (2004) show that real wage resistance may follow if consumption patterns are costly to change. Second, a behavioral justification can be made from the existence of loss aversion, meaning 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).

We use a simple model of DRWR under firm-level wage bargaining, drawing upon Bhaskar (1990), Driscoll and Holden (2004), and McDonald and Sibly (2005). One motivation for the model is to make clear which empirical features we should look for in an investigation of DRWR. Furthermore, the model provides a framework for distinguishing between different types of real wage rigidity. Let the profits of the firm be a decreasing function of the real wage w,

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

and η is the elasticity of product demand.[1] A worker is assumed to have an indirect utility function which depends on the current and past real wages, w and w_{-1},

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

and where D is a dummy variable which is equal to unity if real wages fall, and is zero otherwise. As long as real wages do not fall, utility is simply linear in current real wages. However, we allow for the possibility that workers have loss aversion, in the sense that they compare their current wage with their past wage (if $\mu > 0$), incurring an additional utility loss if the real 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

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

utility function at the point where the wage is equal to its past value, implying that utility is non-differentiable from the left (when $w < w_{-1}$) at the point $w = w_{-1}$. In the limiting point when $\mu = 0$ there is no DRWR.

We model the wage setting by use of the (symmetric) Nash bargaining solution, where the bargaining outcome is the wage that maximizes the product of the firm's and the worker's gain from reaching an agreement, that is the payoffs as compared to the disagreement points, π_0 for the firm (for simplicity set to zero), and V_0 for the worker:[2]

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

If the bargainers fail to reach an agreement, the worker's disagreement point, $V_0 > 0$, will depend on variables that influence the workers' payoff, such as the rate of unemployment, unemployment benefits, and outside wages. As shown in appendix A in the supplemental material, the solution to (3) is given as follows:

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

where the two critical values for V_0 are given by

$$V_0^L = \frac{\eta-\mu-2}{\eta-1}w_{-1}, \quad \text{and} \quad V_0^H = \frac{\eta-2}{\eta-1}w_{-1} > V_0^L.$$

As in a standard wage bargaining model without a kink in the utility function (for

[2]One interpretation of this formulation is a union-firm setup, where the union represents the interests of the median worker who by seniority rules is sheltered from redundancies. In most OECD countries, the majority of the workforce is covered by collective bargaining agreements. However, the key features could also be derived in an efficiency wage framework, as long as the crucial assumption that workers experience a utility loss if their wages fall is maintained. We omit 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, 2009). However, this consideration will not prevent the effect of DRWR that binds, which is what we look for in the empirical analysis.

example Layard et al., 1991), the wage is a markup over the workers' disagreement point, and the markup depends on the elasticity of product demand η. However, due to the non-differentiability of the utility function, the negotiating outcome also depends on the past wage. If workers are in a weak bargaining position due to a low disagreement point, $V_0 < V_0^L$, their real wage will be cut. Yet their resistance towards a cut in the real wage will imply that they get a higher real wage than they otherwise would have received. 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 bargaining position, $V_0 > V_0^H$, they will get a real wage increase. Yet since 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.

Figure 2 provides a graphical illustration of the effect of DRWR on the wage-change distribution predicted by the bargaining model (4). There are many identical firms, and the workers' disagreement point is treated as a random variable with a normal distribution. The solid line represents the wage-change distribution when DRWR binds ($\mu > 0$), while the dotted line represents the wage-change distribution in the absence of rigidities ($\mu = 0$). The latter is referred to in the literature as the *notional* wage-change distribution (Akerlof et al., 1996). We observe that there is a deficit of negative real wage changes in the wage-change distribution when DRWR binds, compared to the the notional distribution, i.e. that the wage-change distribution is compressed from below. Furthermore, the deficit of wage cuts compared to the notional distribution is greater for large negative wage changes than for small. For example, while 22 percent of the notional wage cuts are pushed up above the zero threshold, 30 percent of the notional wage change below –2 percent are pushed up above the –2 percent threshold, and 46 percent of the notional wage changes below –5 percent are pushed up above the –5 percent threshold. The intuition for this effect is that while DRWR prevents some small wage cuts (when $V_0^L < V_0 < V_0^H$), DRWR also means that larger wage cuts are reduced to a smaller size (when $V_0 < V_0^L$). This feature, that the fraction of the notional wage changes that are pushed up above a lower threshold varies with the threshold, is a key prediction of the model that we shall explore further in the empirical analysis. While most of the previous literature on DRWR focusses on the existence of DRWR at zero wage growth, our model shows that it is also of interest to look at the effect of DRWR at negative thresholds.

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

3 Empirical Approach

We use an unbalanced panel of industry-level data for the annual percentage growth of gross hourly earnings for manual workers from the manufacturing, mining and quarrying, construction and electricity, gas and water supply sectors of 19 OECD countries in the period 1973–1999. The countries included in the sample are Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, the United Kingdom, and the United States. The main data sources for wages are harmonized hourly earnings from Eurostat and manufacturing wages from the International Labor Organization.

In line with the theoretical motivation, where DRWR is caused by worker preferences, we measure real wages by deflating the nominal wage with the average consumer price index over the year. One observation of real wage growth is denoted Δw_{jit}, where j is the industry index, i is the country index and t is the year index. In total, there are 9509 observations distributed across 449 country-year samples, with an average of 21 industries per country-year. We observe no less than $Y = 3092$ events of real wage cuts, which is 32.5 percent of all observations. Only 72 (16 percent) of the 449 country-year samples have no real wage cuts. More details on the data are provided in section B in the supplemental material.

The change in the average earnings in a given industry is affected both by the average change for job stayers, and by compositional effects due to differences in wages between new hires and the workers that leave the industry. Prevalent DRWR for individual workers will in general lead to a deficit of negative changes in average real wages at industry level. However, the compositional changes may blur this link.

Some compositional changes will be unrelated to the possible extent of DRWR at the individual level. Much of the turnover is caused by a number of idiosyncratic changes, like workers moving for family motives. Such unsystematic turnover may be considered as "noise" relative to individual wage rigidity, and make it more difficult to detect DRWR in our data. There will also be a systematic negative effect on average wages as older workers who leave the labor force on average have higher wages than younger newcomers to the labor market. One may also expect cyclical effects, as the share of low-skilled workers may increase in expansions (see Solon et al., 1994). This latter effect is likely to dampen fluctuations in wage growth, thus reducing the number of wage cuts. For instance, 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.

However, compositional changes may also come as a consequence of DRWR in individual wages. Firms may respond to downward rigidity at the individual level by cutting the wage of other workers, or by changing the composition of the workforce towards workers with lower wages. Furthermore, binding wage rigidity in some firms may lead to lower employment in these firms, at the benefit of higher employment in other firms in the same industry with lower or more flexible wages. Note that if such mechanisms are strong we would detect less or no DRWR in our data, but we would also expect there to be little or no effect on employment or output at the industry level. In contrast, if there are less such compositional effects, for example because employment protection legislation prevents firms from laying off workers with high wages, or because collective agreements at the industry level prevent jobs shifting from high wage to low wage firms, we would detect DRWR in our data. In this case, we would also expect to find effects of DRWR on industry employment and output.

In the empirical part, we consider the possible existence of downward rigidity at –2 and –5 percent (that is $\Delta w < -2$ and $\Delta w < -5$), preventing large real wage cuts, in addition to real wage rigidity at zero. One motivation for this is from the theory model, which predicts that the deficit of negative real wage changes is greater for large negative changes than for small. Compositional changes may also transform downward rigidity in individual wages at zero to downward rigidity in aggregate wages at a negative rate. For comparison, we also consider *nominal* wage rigidity, that is if $\Delta w + \pi < 0$, where π is the rate of inflation.

3.1 Constructing the notional distribution

Following the idea of previous literature (McLaughlin, 1994, and Kahn, 1997), we detect the possible existence of DRWR by comparing empirical wage change distributions with constructed notional (rigid-free) distributions, as illustrated in Figure 2. The notional distributions are derived from country-year samples with high median nominal and real wage growth, which are assumed to be unaffected by DRWR. We assume that absent any DRWR, the notional real wage growth in industry j in country i in year t is stochastic with an unknown distribution G, which is parameterized by $(\mu_{it}^N, \sigma_{it})$, where μ_{it}^N is the median real wage growth, and σ_{it} is a measure of the dispersion of G. Thus, we allow the location and dispersion of the notional industry wage growth to vary across countries and years, to capture variation across countries and time caused by differences in productivity growth, wage setting, inflation, industry structure, etc. However, we assume the same structural form (or shape) of G in all country-years. This gives us a larger data set to select high wage growth samples from, improving our possibility to find country-year samples that are not affected by downward wage rigidity. However, as this and other assumptions may seem strong, we also undertake extensive robustness checks.

Specifically, we construct an underlying distribution based on a subset H of the sample, with $S^H = 1,331$ observations from the country-year samples where both the median nominal and the median real wage growth are among their respective upper quartiles, 66 samples in total, implying that the median nominal wage growth is above 11.8 percent, and the median real wage growth is above 2.8 percent. To mitigate any effect of DRWR and outliers, we follow Nickell and Quintini (2003) and measure the location by the median, and the dispersion by the range between the 35th and the 75th percentiles. More precisely, the underlying distribution of wage changes is constructed by using 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 ($P75_{it} - P35_{it}$):

$$x_s \equiv \left(\frac{\Delta w_{jit} - \mu_{it}}{P75_{it} - P35_{it}} \right), \qquad \forall\, j, i, t \in H \text{ and } s = 1, \dots, S^H \tag{5}$$

where subscript s runs over all j, i, and t in the 66 country-year samples. x_s should thus be thought of as an observation of the stochastic variable X from the underlying distribution $G(0, 1)$. Figure 3 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 slightly skewed to the right, with a coefficient of skewness of 0.26, and with higher peak and fatter tails than the normal.

Then, for each of the 449 country-years in the full sample, we consider the notional country-year specific distribution of wage changes formed by adjusting the underlying distribution for the country-specific empirical median and inter-percentile range

$$Z_{it} \equiv X\Big(P75_{it} - P35_{it}\Big) + \mu_{it}, \qquad \forall\, i, t. \tag{6}$$

Thus, we have constructed 449 notional country-year distributions $Z_{it} \sim G(\mu_{it}, P75_{it} - P35_{it})$, each consisting of $S^H = 1,331$ wage-change observations. These notional country-year distributions have by construction a G distribution, i.e. the same shape across country-years, but their median and inter-percentile range are the same as their empirical country-year counterparts. In Figure 4 we have plotted the notional distribution for Austria in 1988, together with the empirical histogram. Figure 4 is the empirical counterpart of the theoretical distributions in Figure 2.

Our null hypothesis is that there is no DRWR, which corresponds to Z_{it} having the same distributions as Δw_{it}, while the alternative hypothesis of DRWR corresponds to $Prob(\Delta w_{it} < 0) < Prob(Z_{it} < 0)$. For all country-year samples it, an estimate for the probability of a notional real wage cut $\tilde{q}_{it} \equiv Prob(Z_{it} < 0)$ is given by the notional incidence rate of a real wage cut, i.e. the ratio of the number of notional real wage cuts $\#z_{it}^s < 0$ to total number of observations in the underlying distribution S^H

$$\tilde{q}_{it} = \frac{\#z_{it}^s < 0}{S^H}, \qquad s = 1, \ldots, S^H. \tag{7}$$

If we reject the null hypothesis of no DRWR, we can go on to estimate the extent of DRWR by comparing the incidence rate of wage cuts in the notional distributions with those of the empirical samples. The latter is given by

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

where $\#\Delta w_{jit} < 0$ is the number of empirical wage cuts and S_{it} is the number of observations, both in country-year it. 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 downward wage rigidity, namely the fraction of wage cuts prevented, FWCP,

$$\text{FWCP}_{it} = 1 - q_{it}/\tilde{q}_{it}. \tag{9}$$

For example, in Austria in 1988, the incidence rate of notional real wage cuts, $\tilde{q}_{it}$, is 0.11, while the empirical incidence rate, q_{it}, is 0.06, implying that the FWCP is 0.45.

As there are only on average 21 industries in each country-year sample, there may be considerable stochastic disturbances in μ_{it}, $P75_{it} - P35_{it}$, and q_{it}, which induce considerable disturbances in $\tilde{q}_{it}$ and FWCP_{it}. Thus, estimates of DRWR in single country-years will be imprecise. Therefore, we focus on incidence rates and FWCP's at more aggregated levels, like regions, periods, and the full sample, where the estimates will be more precise.

To test for the existence of DRWR, we explore whether there are "too few" empirical real wage cuts, as compared to the notional G distributions, i.e. without DRWR. This can be done by use of the formulae for binomial distributions, with the notional probabilities $\tilde{q}_{it}$. However, for the full sample of some 450 country-years, this is computationally infeasible. Therefore, we use the simulation method proposed in Holden and Wulfsberg (2008). Specifically, for each country-year it, we draw S_{it} times from a binomial distribution with the country-year specific notional probability $\tilde{q}_{it}$. We then count all the simulated notional real wage cuts $\widehat{Y}$ and compare with the total number of wage cuts Y in the corresponding empirical distribution. This procedure is repeated 5,000 times, counting the number of times where we simulate more notional wage cuts than the empirical counterpart, denoted $\#(\widehat{Y} > Y)$. The null hypothesis is rejected with a significance level of 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 DRWR.

A potential problem is that if DRWR binds in some country-years, and compresses the empirical wage change distribution from below to the extent that it affects the $35th$ percentile (and thus reduces the inter-percentile range) or increases the median, the associated notional country-year sample will also be compressed from below. This will involve a downward bias in the notional incidence rate of wage cuts, $\tilde{q}_{it}$, and thus to a downward bias in our estimate of DRWR, i.e. a downward bias in the estimated FWCP. This downward bias will also reduce the power of our test. However, if there is no DRWR, there will be no downward bias, so this will not affect the significance level of our test.

4 Results

Table 1 displays the main results. For the full sample, we find a FWCP of 0.037 which is highly significant. Thus, about 4 out of 100 notional real wage cuts in the overall sample do not result in an observed wage cut due to DRWR. Distinguishing between time periods, the DRWR appears stronger in the 1970s and the late 1990s, with FWCP of about 0.06, than in the 1980s and the early 1990s.

Table 1 also reports the FWCP across geographical regions: Anglo (Canada, Ireland, New Zealand, the United Kingdom, and the United States), Core (Austria, Belgium, France, Germany, Luxembourg, and the Netherlands), Nordic (Denmark, Finland, Norway, and Sweden), and South (Italy, Greece, Portugal, and Spain). The regional classification is largely based on geography and language, but typically, countries in the same region are fairly similar when it comes to labor market institutions. Generally, there is a tendency of high rates of unionization and fairly strict employment protection legislation (EPL) in the Nordic countries, moderate unionization and stricter EPL in the South, moderate unionization and moderate EPL in the Core, and lower unionization and weaker EPL in the Anglo countries. While the point estimates indicate some DRWR for all regions, this estimate is only significant at 5 percent for the Core region.

The middle columns display the results for DRWR at –2 and –5 percent. We observe that wages are more rigid at lower growth rates than at zero, with a FWCP in the full sample of 0.113 at –2, and 0.184 at –5. At –2 percent growth, DRWR is significant for all time periods and for all regions except the South. At –5 percent growth, the estimated FWCP is above 0.30 both in the Core and in the Nordic regions, while in the South, the FWCP is only 0.09, with a p-value of almost 6 percent.

The finding of higher FWCP for negative rates of change than at zero is consistent with the theoretical model given in section 2 ; DRWR pushes 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 to match the empirical results, $\eta = 3$ and $\mu = 0.033$, and drawing V_0 from the normalized underlying distribution as given by (5) (instead of using a normal distribution), we obtain FWCPs of 0.037, 0.126, and 0.162 at 0, –2, and –5 percent, as compared to the empirical results of 0.037, 0.113 and 0.184. This close fit strengthens the theoretical model's interpretation 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, more prevalent DRWR at –2 and –5 percent growth rates might also be caused by rigidity at constant real wages for individuals and possibly also for firms, combined with some downward flexibility due to compositional changes between types of workers.

The last column in Table 1 reports the results for 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 0.411.

When we combine time periods and regions, we find that DRWR at –2 and –5 percent was prevalent in the Anglo, Core, and Nordic regions in the 1970s and 1980s (see Table C1 in the supplemental material). In contrast, in the South, we do not find significant DRWR in any period. Testing for DRWR in individual countries, we find significant DRWR at the –2 percent level, with a FWCP of around 0.5, in Austria and Finland. The

FWCP is also significant, varying between 0.09 and 0.21 in Belgium, Ireland, Luxembourg, the Netherlands, New Zealand, Portugal, Sweden, the United Kingdom, and the United States (Table C2 in the supplemental material reports the results for individual countries). There is no indication of DRWR at –2 percent in Canada, Denmark, France, Germany, Greece, Italy, Norway, and Spain. At the country level, there is a positive correlation between the estimates of DNWR and DRWR at –2 percent.

The fraction of industry-years that are affected by downward rigidity can be calculated by multiplying the incidence rate of notional wage changes by the FWCP for the respective threshold. We find that 1.8 percent of all industry-year wage changes are pushed up above the –2 percent threshold, which is higher than for any of the other thresholds (see Table C3 in the supplemental material). This estimate is fairly stable across time periods, and the geographic variation is also limited, ranging from 1.0 percent in the South to 2.4 percent in the Anglo countries. This underscores that DRWR is a phenomenon that affects all regions and time periods, even if the extent is moderate.

Based on data for individual job stayers, Dickens et al. (2005) find evidence for DRWR at zero growth, with the FWCP ranging from around 0.05 percent in Greece and the United States to around 0.5 percent in Finland, France, and Sweden, with most countries in the $0.15 - 0.35$ range. However, these estimates are not directly comparable to ours, as our estimated FWCP are affected by aggregation and compositional effects.

4.1 Robustness

To explore the robustness of our results, we have varied the key assumptions concerning the shape, the location, and the dispersion of the notional G distributions. (The details are reported in section D of the supplemental material.) 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. Overall, there is clear evidence of DRWR, although the extent is moderate. Significance levels and FWCPs are higher at –2 and –5 percent than at zero, and also weaker and smaller in the South than in the other regions. Note that with country-specific or period-specific underlying distributions, all country-years are used in the construction of underlying distributions, implying that there is no selection of high wage or booming economy samples. When we nevertheless detect significant DRWR, it is because country-year samples with lower median real wage growth have more compressed left tail than other country-year samples.

We also perform the analysis with an entirely different identifying assumption that, following Card and Hyslop (1997), assumes symmetry within each country-year notional sample. Thus, we construct the notional distribution for each country-year sample by replacing the observations below the median by the mirror image of the observations above the median. Note that this approach does not assume a common shape of the notional distributions across country-years. As the symmetry method is based on orthogonal assumptions to our main approach, it constitutes a strong test of the robustness of our results. As shown in Table C4 in the supplemental material, the estimated FWCPs are somewhat lower, but the results are qualitatively similar to the main results. This finding strengthens our belief that our results are indeed caused by DRWR. The finding of asymmetric real wage rigidity is interesting, as it suggests that even if a shock is reversed, real wages need not revert to their original level.

To explore whether DRWR applies to *expected* real wages, rather than actual, we have re-simulated the results from the main procedure using expected real wage changes, where actual price level is replaced by the expected price level, and the latter is based on expected inflation being derived as country-specific AR1 processes of actual inflation. The results are qualitatively similar, even though the estimated FWCPs are somewhat smaller: 0.024, 0.066 and 0.165 at levels zero, –2, and –5 percent growth. The tendency towards weaker downward rigidity for expected, rather than for actual, real wages is the opposite of what one would expect if expectational errors regarding inflation are a key cause of real wage flexibility. This suggests that expectational errors are not important for real wage flexibility.

One possible alternative interpretation of our finding of DRWR at −2 and −5 growth levels is that the missing real wage cuts are in fact 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 inflation rates. The FWCP at the −2 percent level is indeed lower in country-years where inflation is above 10 percent (0.05) than if inflation is below 2 percent (0.16), suggesting that some of the downward real rigidity may be caused by downward nominal rigidity. However, the FWCP is even higher for country-years where inflation rates are between 4 and 6 percent (0.23). The FWCP is also high for country-years with inflation rates in the 8 to 10 percent interval (0.17), indicating that at least a large part of the DRWR we find is not caused by DNWR.

5 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 (2008), we find that employment protection legislation (EPL), union density, and unemployment are important determinants of DNWR. Table 2 reports results from Poisson regressions for the same variables, using the number of real wage changes below −2 percent in a country-year as the dependent variable. The first two columns report pooled and fixed effects estimates 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 pooled and fixed effects estimates 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. We also find that inflation has a negative impact on the FWCP. Note that this is not caused by the same mechanism as when inflation reduces the incidence of real wage cuts. If a positive inflation shock takes place, it will move the entire real wage-change distribution, and as we condition the notional distributions on the median real wage change, a positive inflation shock will not affect the FWCP unless there is a link between the inflation shock and the distributional shape of the real wage changes. One possible cause of such a link is if the DRWR applies to expected real wages, and then is eroded if a positive inflation shock takes place. However, our findings above do not support this interpretation. A more plausible interpretation, is that under low inflation, DNWR 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 effect on the incidence of wage cuts and the FWCP, but is only significant in one of the pooled regressions. The negative effect of EPL on the incidence of wage cuts is evidence against the hypothesis that the deficit of negative real wage changes is caused by low wage workers leaving the industry in downturns. EPL would help low wage workers keep their job in a recession, thus it will prevent the compositional effect that pushes up industry wages, and hence it would lead to more wage cuts. Hence this supports that our empirical findings are really evidence of DRWR.

Union density has the expected negative effect on the incidence of wage cuts when we control for fixed effects. Union density has a positive effect on the FWCP, although not significant. These results give some indication that DRWR is affected by labor market rigidity and unions, and that is it weakened by unemployment. We also tried other institutional variables like bargaining coverage, temporary employment, and indexes of centralization and coordination of wage setting, but they had lower explanatory power.

6 Conclusions

Using industry data for 19 OECD countries between 1973 and 1999, we find evidence of downward real wage rigidity (DRWR) in the core European countries, and in the Anglo group, but not for the southern European countries. The extent of DRWR is small, and in the full sample only 4 out of 100 notional wage cuts are prevented by DRWR. However, we find stronger evidence of downward rigidity at negative real wage changes. 11 percent of the real wage changes below –2 percent growth are prevented by DRWR, and 18 percent of changes below –5 percent real wage growth are prevented.

The stronger downward rigidity at negative real wage changes is a key finding of our study. It implies that one should not take frequent real wage cuts as indication that real wages are flexible downwards, as the downward resistance can bind also at lower levels. Possible effects on employment and output do not hinge on DRWR being binding at zero, it is sufficient that real wages are pushed up.

The stronger DRWR at negative growth rates is consistent with our theoretical model, where workers' resistance against wage cuts not only prevents smaller wage cuts, but also reduces the size of larger ones. Compositional changes in the work force, where e.g. older high-wage workers are replaced by younger low-wage workers, may also contribute to a limited reduction in average real wages, even if individual workers avoid real wage cuts.

Comparing the downward rigidity of nominal and real wages, we find that downward nominal rigidity in general is much more significant and of greater magnitude. The difference between DNWR and DRWR was, however, smaller in the late 1990s than in earlier periods, reflecting a reduction in the extent of DNWR. This suggests that nominal wages have become more flexible downwards, in line with the reduction in inflation, but there has not been the same increase in the flexibility of real wages. In periods of low inflation, DNWR will also involve DRWR, and it is indeed difficult to distinguish between the two types of rigidity. However, as we also find some DRWR in high inflation periods, it seems clear that the DRWR that we find is an independent phenomenon that is not only caused by DNWR combined with a low inflation rate.

In contrast to most previous studies of DRWR, which consider the wages of job stayers, we use data for average wages at the 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, we will not find DRWR in our data. Nor will our data capture downward wage rigidity in some firms, if many jobs are moved to other firms with lower wages in the same industry. However, in these cases it is questionable whether the wage rigidity at the worker- or the firm-level will have any impact at the aggregate level. In contrast, if the DRWR also prevails in industry wages, an effect on aggregate output and employment seems more likely.

Our finding of DRWR is based on a univariate framework, which only includes data for real wage growth. The univariate framework has the advantage of needing no assumptions on explanatory variables and functional forms. Thus, when we detect DRWR, we can be fairly confident that this finding is indeed a feature inherent in the data.

What is the effect of wage rigidity on employment and output? This is a matter of considerable controversy within recent macro-labor literature. Using a basic search model, Shimer (2005) argues that real wage rigidity is crucial for explaining the evolution of vacancies and unemployment over the business cycle. However, as pointed out by among others Shimer (2004) and Pissarides (2007), wage rigidity of job stayers is not important in the search model, it is the wages of new hires that matter. Furthermore, Pissarides (2007) argues that the evidence indicates that wages of new hires are flexible, and concludes that wage rigidity is not important for the cyclical movement of unemployment and vacancies. This view is, however, opposed by Gertler and Trigari (2009) who show that when controlling for compositional changes in job quality, the wages of new hires is no longer more flexible than that of job stayers. Furthermore, in many OECD countries, most workers have their wage set in a collective agreement, and these agreements typically also apply for new hires. Consistent with this, Card (1990) finds that wage rigidity in Canadian union contracts affect firms' employment decisions.

There is fairly strong evidence that the variation in unemployment rates across time and OECD countries is related to institutional labor market variables—like unemployment benefits, union density, and the degree of coordinated wage setting—which are likely to reflect differences in wage-setting behavior (see for example Nickell et al., 2003). Within this framework, one would expect increased wage pressure due to binding DRWR to induce higher unemployment, in line with the early explanations of the rise in European unemployment in the 1970s (see Bruno and Sachs, 1985 and Grubb et al., 1983). Testing this conjecture is an important task for future research.

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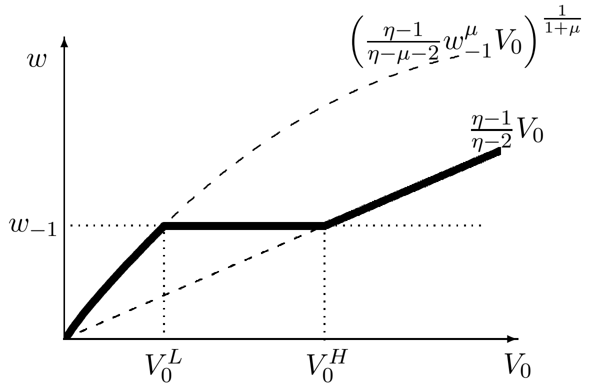

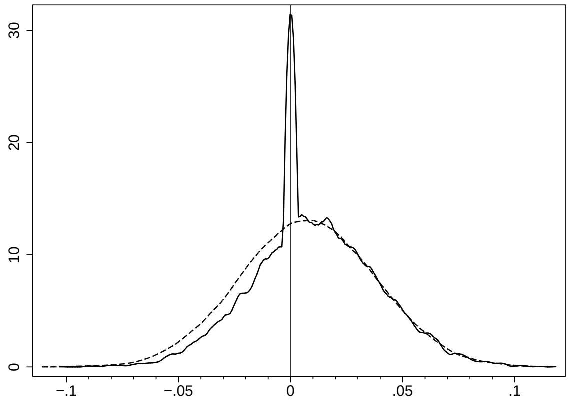

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Figure 1: The upper dashed line indicates the wage outcome conditional on a wage cut, while the lower dashed line is conditional on no wage cut. The solid line indicates the bargaining outcome, coinciding with the upper dashed line below V_0^L, and with the lower dashed line above V_0^H.

Figure 2: Kernel densities of a notional distribution of real wage changes (dotted line) and a distribution of real wage changes subject to DRWR *(solid line).* $\eta = 3$, $\mu = 0.007$, $V_0 \sim N(-0.6855, 0.003)$, $V_0^L \approx V^{P30}$, *and* $V_0^H \approx V_0^{P40}$.

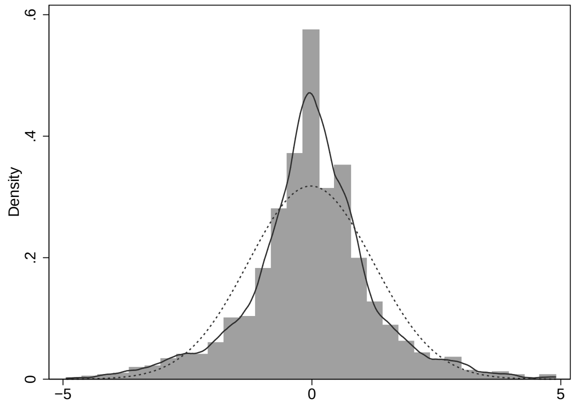

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Figure 3: Histogram and kernel density (solid line) of the normalized underlying distribution of wage changes compared to the normal density (dotted line). Fourteen extreme observations are omitted.

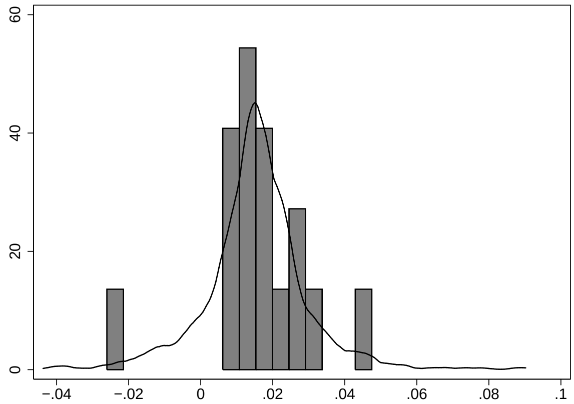

Figure 4: Histogram of observed real wage changes and the notional real wage-change distribution (solid line) in Austria, 1988.

Table 1: The FWCP *estimated at 0, –2, –5, and $-\pi$ percent real wage growth. p-values in parentheses.*

		DRWR evaluated below							
		0 percent		–2 percent		–5 percent		$-\pi$ percent	
Category	S	Y	FWCP	Y	FWCP	Y	FWCP	Y	FWCP
All observations	9505	3092	0.037	1372	0.113	449	0.184	324	0.260
			(0.000)		(0.000)		(0.000)		(0.000)
Periods									
1970–79	2224	453	0.067	214	0.162	59	0.309	5	0.612
			(0.016)		(0.000)		(0.000)		(0.011)
1980–89	3717	1545	0.028	755	0.096	270	0.157	74	0.399
			(0.024)		(0.000)		(0.000)		(0.000)
1990–94	1906	645	0.020	229	0.109	63	0.195	93	0.231
			(0.241)		(0.017)		(0.032)		(0.002)
1995–99	1662	449	0.058	174	0.129	57	0.146	152	0.159
			(0.041)		(0.016)		(0.105)		(0.005)
Regions									
Anglo	2961	1274	0.027	568	0.113	188	0.172	153	0.199
			(0.054)		(0.000)		(0.001)		(0.001)
Core	3110	788	0.063	248	0.188	48	0.347	125	0.234
			(0.004)		(0.000)		(0.000)		(0.000)
Nordic	1976	515	0.032	235	0.117	45	0.311	18	0.498
			(0.125)		(0.002)		(0.000)		(0.000)
South	1462	515	0.024	321	0.043	168	0.090	28	0.411
			(0.214)		(0.147)		(0.058)		(0.001)

Note: S is the number of observations, Y is the number of observed wage cuts below the relevant limit. DRWR evaluated below $-\pi$ percent is equivalent to evaluate DNWR at 0 percent.

Table 2: 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
$\text{Ln}(S_{it})$	1 (–)	1 (–)	–	–
Ln(Simulated cuts)	–	–	1 (–)	1 (–)
EPL	−0.195*	−0.078	0.005	0.146
	(0.063)	(0.090)	(0.022)	(0.173)
Union density	0.362	−1.596*	0.110	0.672
	(0.392)	(0.523)	(0.161)	(0.572)
Inflation	0.120*	0.111*	−0.014*	−0.026*
	(0.015)	(0.011)	(0.004)	(0.020)
Unemployment	0.102*	0.163*	−0.014	−0.029
	(0.022)	(0.020)	(0.008)	(0.016)
constant	−0.367	−1.576*	−0.297*	—
	(0.307)	(0.338)	(0.121)	
log-likelihood	–877.2	–755.7	–563.3	–563.9
Number of observations	422	422	392	392

Supplemental Material

A The Nash Solution

The first order condition for the Nash bargaining solution requires that the left-hand derivative (that is $w < w_{-1}$, 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 the 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 and V_0^L, are 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 $V_0 > V_0^H$. Finally, it is straightforward to establish 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 Data appendix

We have obtained wage data from Eurostat for all countries except Austria, Canada, Finland, New Zealand Norway, Sweden and the United States (see below). The precise source is Table HMWHOUR in the *Harmonized earnings* domain under the *Population and Social Conditions* theme in the NEWCRONOS database. Our wage variable (HMWHOUR) is labeled *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 deducting taxes 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 work-hours of work are those in a normal working week (that is a week that does not include public holidays) during the reference period (October or the last quarter). These hours are calculated based on the number of hours paid, including overtime hours paid. Furthermore, we use wage data denominated in the national currency, and wages for men and women are included in the data. The data for Germany does not include the German Democratic Republic 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 (for example D Manufacturing) to group levels (for example DA 159 Manufacturing of beverages), but use the most disaggregated 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 United States are from Table 5B "Wages in manufacturing" in LABORSTA, the Labor 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 sectors 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 is reported in Table B1.

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 Holden and Wulfsberg (2008, 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. For Greece, date for 1978 and 1979 are inter-

Table B1: 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

polated, while data before 1977 is extrapolated at the 1977 level.

Bargaining coverage data are from the OECD (2004, Table 3.5), which provides 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 are only available for 1994 from the 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 the OECD's Corporate Data Environment, Table *Employment by permanency of the (main) job*. Data for Finland in 1995 and 1996 and for 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.

C Tables

Table C1: The FWCP estimated at 0, –2, –5, and $-\pi$ percent real wage growth. p-values in parentheses.

			DRWR evaluated below							
			0 percent		–2 percent		–5 percent		$-\pi$ percent	
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)

Note: See Table 1

Table C2: The FWCP *estimated at 0, –2, –5, and $-\pi$ percent real wage growth. p-values in parentheses.*

		DRWR evaluated below							
		0 percent		–2 percent		–5 percent		$-\pi$ percent	
Category	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)
United Kingdom	615	199	0.033 (0.235)	98	0.110 (0.047)	35	0.274 (0.003)	18	0.217 (0.127)
United States	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 C3: The FIYA *estimated at 0, –2, –5, and $-\pi$ percent real wage growth. p-values in parentheses.*

		DRWR evaluated below							
		0 percent		–2 percent		–5 percent		$-\pi$ percent	
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: See Table 1

Table C4: The FWCP *estimated at 0, –2, –5, and $-\pi$ percent real wage growth. Symmetric and country-year specific notional distributions. p-values in parentheses.*

		DRWR evaluated below							
		0 percent		–2 percent		–5 percent		$-\pi$ percent	
Category	S	Y	FWCP	Y	FWCP	Y	FWCP	Y	FWCP
All	9505	3092	0.023 (0.020)	1372	0.070 (0.000)	449	0.127 (0.000)	324	0.200 (0.000)
Periods									
1970–79	2224	453	0.036 (0.128)	214	0.041 (0.207)	59	0.170 (0.040)	5	0.501 (0.061)
1980–89	3717	1545	0.018 (0.111)	755	0.035 (0.073)	270	0.100 (0.010)	74	0.230 (0.009)
1990–94	1906	645	0.008 (0.398)	229	0.120 (0.007)	63	0.102 (0.194)	93	0.213 (0.005)
1995–99	1662	449	0.047 (0.078)	174	0.167 (0.001)	57	0.219 (0.017)	152	0.159 (0.005)
Regions									
Anglo	2961	1274	0.003 (0.428)	568	0.067 (0.007)	188	0.134 (0.008)	153	0.124 (0.029)
Core	3110	788	0.073 (0.001)	248	0.152 (0.000)	48	0.334 (0.000)	125	0.220 (0.000)
Nordic	1976	515	−0.012 (0.693)	235	0.018 (0.349)	45	0.118 (0.151)	18	0.359 (0.018)
South	1462	515	0.023 (0.224)	321	0.040 (0.162)	168	0.036 (0.279)	28	0.333 (0.007)

Note: See Table 1

D Robustness

To explore the validity of assuming a common shape for 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 (19 tests), and one alternative where the underlying distribution is constructed separately for each of the four time periods (27 tests). The assumption of a common underlying distribution passes easily in all 46 tests with the lowest p-value of 0.211. (In principle, also binding DRWR should make the Kolmogorov-Smirnov be significant, but it seems that the test has too little power to detect this.)

To further explore 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 distributions that are country-specific and period-specific. In particular, 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, the motivation is 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 is quite often affected by DRWR, we also consider an alternative that does not rely on any specific percentile, 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 dispersion measures, inducing 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 provides information about the robustness of the broad picture. Taken together, to construct the notional distributions we use 18 different combinations of three distributional shapes (common, country-specific, or period-specific) $\times$ two measures of location (median or 80th percentile) $\times$ three dispersions (IPR, MDEV, or predicted IPR).

Figure D1 presents measures of the 18 estimates of the FWCP for each of the limits 0, –1, –2, –5 and $-\pi$ percent (that is nominal zero). The estimates from Table 1 are indicated with a dot, a cross indicates an estimate that is significant at the 5 percent level, while the plus signs indicate 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 in the overall sample, where 17 and 14 of the 18 FWCP estimates are significant. There is some evidence of

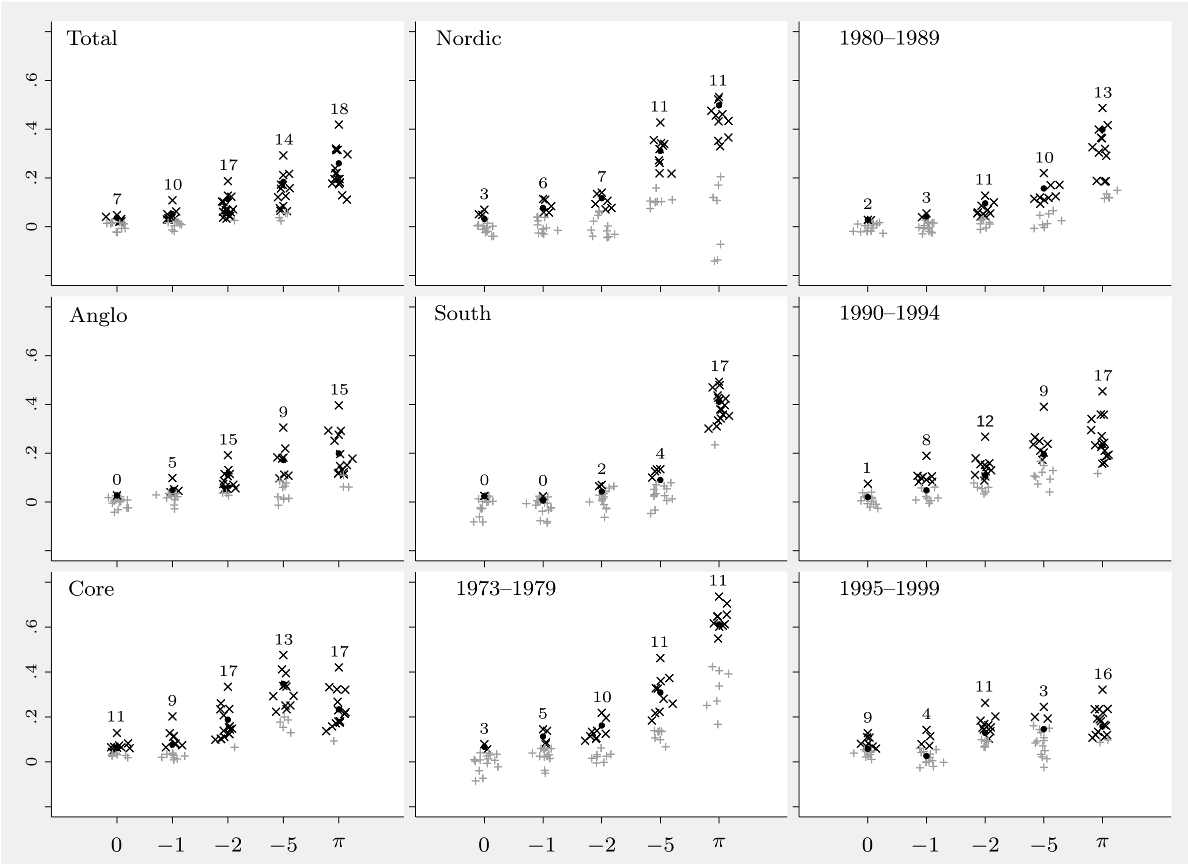

Figure D1: Estimates of the fraction of real wage cuts prevented evaluated at 0, –1, –2 and –5 percent, and the fraction of nominal wage cuts prevented. There are 18 estimates per evaluation criteria. A cross indicates a significant estimate at 5 percent while a plus sign indicates an insignificant estimate. The number of significant estimates reports are reported on top of each column.

DRWR at zero or –1 percent, but these point estimates are closer to zero, and few are significantly larger than zero. The evidence for DNWR is stronger than the evidence for DRWR, with higher FWCP estimates, where 18 are significant. In the other panels of Figure D1, we display similar charts for time periods and regions. There is considerable variation, yet the broad picture is not affected. Overall, there is clear evidence of DRWR, although the extent is moderate. Significance levels and FWCPs are higher at –2 and –5 percent than at zero, and also weaker and smaller in the South than in the other regions.

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