

# THE DETERMINANTS OF DOWNWARD WAGE RIGIDITY: SOME METHODOLOGICAL CONSIDERATIONS AND NEW EMPIRICAL EVIDENCE\*

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## ASBSTRACT

This article discusses the identification of the determinants of downward wage rigidity and illustrates empirically its importance in Europe. It is shown that the models estimated so far in the literature suffer from econometric problems that prevent the contributions of those determinants to be correctly identified or precisely estimated. An empirical exercise, along the lines discussed in this article, using survey data for 15 European Union countries, shows that the results may significantly differ from the ones previously obtained in the literature. Together, the theoretical considerations and the estimated results suggest that new empirical evidence is required before definite conclusions on the determinants of downward nominal or real wage rigidity can be drawn.

## 1. Introduction

There now is an extensive empirical literature aiming at identifying the factors that explain why the importance of downward nominal and/or real wage rigidity may differ across firms, sectors or countries. The estimated models usually regress a measure of downward nominal or real wage rigidity, computed at the firm, sectoral or country level, on a number of variables the theory suggests as potentially important to explain such differences (see, among others, Dickens *et al.*, 2007, Holden and Wulfsberg, 2008, Caju *et al.*, 2009, Messina *et al.*, 2010 and Babecky *et al.*, 2010).

In this article, we pinpoint some methodological issues involving existing empirical literature on downward nominal or real wage rigidity and provide empirical evidence on the importance of downward nominal wage rigidity and its determinants in some European countries. Hereafter, we will denote downward nominal wage rigidity as DNWR and downward real wage rigidity as DRWR.

As regards the existing empirical literature, we argue that in some cases the regressors used may not be defined in a proper way (e.g., Dickens *et al.*, 2007, Holden and Wulfsberg, 2008, Caju *et al.*, 2009 and Messina *et al.*, 2010), and that, in some others, the estimated models may not be specified correctly (e.g., Babecky *et al.*, 2010). The first situation may imply potential important biases for the estimated parameters. The second has the implication that the model parameters cannot be interpreted as measuring the importance of the regressors for downward wage rigidity.

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\*The authors would like to thank Cláudia Duarte and Pedro Portugal for helpful discussions and useful suggestions. The opinions expressed in the article are those of the authors and do not necessarily coincide with those of Banco de Portugal or the Eurosystem. Any errors or omissions are the sole responsibility of the authors.

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Using survey data for 15 European Union countries, we perform an empirical exercise, taking into account some of the remarks made in the article. It is shown that downward rigidity in nominal base wages is pervasive in Europe.

A probit model, estimated over the firms scheduled for a base-wage cut, suggests that the degree of downward nominal base-wage rigidity increases with the proportion of “high-skilled white-collar workers” and the importance of “wage agreements negotiated outside the firm”, and decreases with the “degree of competition” faced by the firm. The “incidence of permanent contracts”, the “labour share”, the “tenure” or the “proportion of workers covered by collective agreements”, suggested by the economic theory as potential relevant factors, do not emerge as having a significant impact on downward nominal base-wage rigidity. These results differ significantly from the ones previously obtained in the literature, suggesting that, at least, some of the methodological considerations raised in this article have important practical implications.

The article is organised as follows. Section 2 briefly reviews the empirical literature on downward wage rigidity. Section 3 discusses some methodological issues involving the literature that has tried to identify the relevant determinants of wage rigidity. Section 4 illustrates these issues by estimating a model using survey data for several European countries. Section 5 concludes.

## 2. Empirical evidence on the determinants of downward wage rigidity

The literature on wage rigidity has suggested several statistics to gauge the importance of DNWR and DRWR. Here, we focus on the measures used, for instance, in Dickens and Goette (2006), Dickens *et al.*, (2007), Holden and Wulfsberg (2008), Caju *et al.*, (2007), Caju *et al.* (2009), Messina *et al.*, (2010), and Babecky *et al.*, (2010).

The measures of DNWR or DRWR suggested in Dickens *et al.* (2007), attempt to capture the fraction of workers who would receive a (nominal or real) wage freeze when they were scheduled for a (nominal or real) wage cut, whether due to individual performance or to external conditions. More specifically, for DNWR it is assumed that everyone who had a nominal wage freeze would have had a nominal wage cut in the absence of downward nominal rigidity and the authors suggest using the following statistic:

$$dnwr_1 = \frac{A}{A+B} \quad (1)$$

where  $A$  is the number (or fraction) of workers whose wages have been frozen and  $B$  the number (or fraction) of workers whose wages have been cut.

This measure of DNWR differs from the one discussed in Dickens and Goette (2006), and used for instance in Caju *et al.*, (2007), Caju *et al.* (2009), and Messina *et al.* (2010), in that it does not exclude the number of wage freezes that would have taken place in the absence of any DNWR. This alternative measure may be written as:

$$dnwr_2 = \frac{A-C}{A-C+B} \quad (2)$$

where  $C$  stands for the number (or fraction) of workers whose wages would have been frozen in the absence of DNWR. This is usually estimated by assuming an underlying “counterfactual or notional distribution” that would have been observed under fully flexible wages.<sup>1</sup> Taken together,  $A-C+B$  stand

<sup>1</sup> In order to identify the notional or counterfactual distribution, it is usually assumed that such distribution is symmetric and that the upper half of the distribution of observed wage changes is unaffected by wage rigidities (see, for instance, Card and Hyslop, 1997, Altonji and Devereux, 2000, Fehr and Goette, 2005, Goette *et al.*, 2007, and Dickens *et al.* 2007). However, the assumption that the upper half of the distribution of observed wage changes is unaffected by wage rigidities, *i.e.*, that DNWR (or DRWR) operates only at zero nominal (or real) wage growth has been challenged in the most recent literature. See Holden and Wulfsberg (2009), Elsby (2009) and Stüber and Beissinger (2012).

for the number (or fraction) of workers for whom there was the intent of reducing the base wages. The use of  $dnwr_2$  addresses one important limitation of  $dnwr_1$ , given that this last measure assumes that all the firms that freeze their wages would have cut them in the absence of DNWR, *i.e.*  $C=0$ . Henceforth, for simplification, we will refer to  $A-C+B$  or only to  $A+B$  as the workers scheduled for a wage cut.

These two measures of DNWR are usually computed at the sectoral, industry or country level by aggregating data on wages at the worker level (see, Messina *et al.*, 2010, Caju *et al.*, 2009, Holden and Wulfsberg, 2008, and Dickens *et al.*, 2007) and may be interpreted as measuring the fraction of wage cuts prevented by downward nominal wage rigidity.

Statistics similar to  $dnwr_1$  and  $dnwr_2$ , denoted below as  $drwr_1$  and  $drwr_2$ , have been constructed to gauge the importance of DRWR, where  $A$  now stands for the fraction of workers with real wage freezes (wage changes equal to inflation or expected inflation),  $B$  for the fraction of workers with real wage cuts and  $C$  for the fraction of workers who would have received a real wage freeze in the absence of any downward real wage rigidity (again computed by assuming a counterfactual or notional distribution for the real wage changes distribution).<sup>2</sup>

The bulk of the literature on wage rigidity has tried to identify the factors that may explain why some sectors, countries or firms display higher downward wage rigidity than others, based on the previous measures of wage rigidity. Examples for DNWR are Dickens *et al.* (2007) who use  $dnwr_1$ , and Holden and Wulfsberg (2008) and Messina *et al.* (2010), who use  $dnwr_2$ . Examples for DRWR are Dickens *et al.*, (2007), who use  $drwr_1$ , and Caju *et al.*, (2009) and Messina *et al.*, (2010), who use  $drwr_2$ .

Using data at the country level, Dickens *et al.*, (2007) compute the correlation between  $dnwr_1$  (and  $drwr_1$ ) and a very large set of factors that may potentially explain the differences in the degree of wage rigidity across countries. Such factors include union density, union coverage, degree of coordination in bargaining, the fraction of temporary workers, index of employment protection legislation, a corporatism index, etc. As regards  $dnwr_1$ , for none of the regressors was the relationship statistically significant at the 5 percent level, while for  $drwr_1$  only the relationship with union density was significant at the 5 percent level. Strangely enough, however, the union density and the union coverage emerged as negatively correlated with nominal wage rigidity.

A similar exercise was performed in Holden and Wulfsberg (2008). The authors compute a  $dnwr_2$ -type measure for 19 countries and test whether inflation, unemployment, union density and the EPL (employment protection legislation index) help explaining differences on DNWR across countries. Messina *et al.*, (2010) for Belgium, Denmark, Spain and Portugal and Caju *et al.*, (2009) for Belgium, using sectoral data, also evaluate whether the workforce and firms' characteristics (as measured by firm size, the proportion of high-skilled white and blue collar workers, the incidence of firm-level wage agreements, the degree of competition, etc.) may help explaining why DNWR or DRWR is higher in some countries or in some sectors than others.

An important feature common to all these empirical contributions is that the regressors defined, either at the sectoral or country level, are computed using all the workers in the sample (*i.e.*, in the corresponding sector or country) and not just the workers scheduled for a wage cut (*i.e.*, workers whose wages were frozen or cut). This, as we shall argue below, may have important implications for the estimates of the parameters of the regressions used in those papers.

Recently, Babecky *et al.*, (2010) used the proportion of firms in the economy that have frozen base wages as a statistic to gauge the importance of DNWR and identify its determinants. We denote such a measure by:

<sup>2</sup> See, for instance, Dickens and Goette (2006), and Dickens *et al.* (2007).

$$dnwr_3 = \frac{D}{N} \quad (3)$$

where  $D$  is the number of firms whose wages have been frozen and  $N$  is the total number of firms in the sample. This measure differs from  $dnwr_1$  in that it uses the firm as the relevant unit (not workers) and, more importantly, in that it compares the number of firms that have frozen their base wages with the total number of firms rather than with the firms that have frozen or cut their wages, as the previous statistics do.

Babecky *et al.*, (2010) use firm-level survey data for 15 European countries to identify the factors that might explain why some firms display higher DNWR or DRWR than others. These authors consider that firms that froze their base wages show evidence of DNWR, while firms that have an automatic indexation mechanism linking base wages to (past or expected) inflation are subject to DRWR. Some issues concerning the contribution by Babecky *et al.*, (2010) will be discussed further below.

### 3. Identifying the determinants of downward wage rigidity: Problems with the empirical literature

As we have seen above, the definitions of DNWR and DRWR, as well as the statistics suggested in the literature, namely  $dnwr_1$ ,  $dnwr_2$ ,  $drwr_1$  and  $drwr_2$ , involve only the workers whose wages have been scheduled for a wage cut, i.e., have been frozen or cut, leaving aside the remaining workers whose wages have increased. However, this important fact seems to have been overlooked by the models estimated in the literature aimed at identifying the factors that may explain why DNWR or DRWR is higher in some countries, sectors or firms than others. In fact, for those statistics, all the regressions (including simple correlations) estimated in the literature use regressors that involve the full set of workers and not just the workers scheduled for a wage cut. Similarly, in the case of  $dnwr_3$ , used in Babecky *et al.*, (2010), the model is estimated using all the firms in the sample and not just the firms for which wages have been frozen or cut.

This section discusses the implications for the parameters of the estimated models stemming from these facts. We distinguish between the use of  $dnwr_1$ ,  $dnwr_2$ ,  $drwr_1$  and  $drwr_2$  on the one side, and of  $dnwr_3$  on the other side.

#### 3.1 Use of $dnwr_1$ , $dnwr_2$ , $drwr_1$ and $drwr_2$

In order to make the presentation more intuitive, let us take the union coverage (proportion of workers covered by collective wage agreements) as an example of a regressor, which is commonly used in the regressions that involve  $dnwr_1$ ,  $dnwr_2$ ,  $drwr_1$  and  $drwr_2$  as dependent variables.

According to the literature, it is expected that unionised workers will exhibit higher downward (nominal and real) wage rigidity (see, for instance, Holden, 2004, Dickens *et al.*, 2007, Holden and Wulfsberg, 2008 and 2009). Thus, for sector (or country)  $j$  let us define:

$S_{1j}$  = Number of workers covered by collective agreements whose wages have been frozen;

$S_{2j}$  = Number of workers covered by collective agreements whose wages have been cut;

$S_{3j}$  = Number of workers covered by collective agreements whose wages have increased;

$S_j = S_{1j} + S_{2j} + S_{3j}$  = Total number of workers covered by collective agreements;

$N_{1j}$  = Total number of workers whose wages have been frozen;

$N_{2j}$  = Total number of workers whose wages have been cut;

$N_{3j}$  = Total number of workers whose wages have increased;

$N_j = N_{1j} + N_{2j} + N_{3j}$  = Total number of workers.

As regards measure  $dnwr_{1j}$  for sector (or country)  $j$ , which we denote by  $dnwr_{1j}$ , we note that from equation (1) we have

$$dnwr_{1j} = \frac{A_j}{A_j + B_j} = \frac{N_{1j}}{N_{1j} + N_{2j}} \quad (4)$$

so that the identification of  $dnwr_{1j}$  in sector (or country)  $j$  involves the workers that have their wages frozen or cut, but not the workers in the sample that have their wages increased. The same goes for  $dnwr_{2j}$ ,  $dnwr_{1j}$  and  $dnwr_{2j}$ .<sup>3</sup>

However, in the empirical literature, union coverage in sector or country  $j$  is computed using all the workers in the sector or country  $j$ :

$$\text{coverage}_j = \frac{S_j}{N_j} = \frac{S_{1j} + S_{2j} + S_{3j}}{N_{1j} + N_{2j} + N_{3j}} \quad (5)$$

where the correct measure, that follows directly from the definitions of DNWR and DRWR, should be given by:

$$\text{coverage}_j^* = \frac{S_{1j} + S_{2j}}{N_{1j} + N_{2j}} \quad (6)$$

that is, the proportion of workers covered by collective agreements among those scheduled for a wage a cut (i.e., those who had their wages frozen or cut).

According to the theory,  $\text{coverage}_j^*$  is expected to have a positive impact on  $dnwr_{1j}$  because the higher the proportion of workers covered by collective agreements, among those scheduled for a wage a cut, the higher the degree of DNWR, i.e., the higher the number of workers whose wages are frozen (as opposed to being cut). To put it slightly different: out of the population of workers scheduled for a wage a cut, for two otherwise identical workers, the worker covered by a collective agreement will display higher probability of having his wage frozen (or a lower probability of having his wage cut).

But what about  $\text{coverage}_j$ , the measure used so far in the empirical literature? We notice that we may decompose  $\text{coverage}_j$  as:

$$\text{coverage}_j = \alpha_j \cdot \text{coverage}_j^* + \beta_j \quad (7)$$

where  $\alpha_j$  stands for the fraction of workers scheduled for a wage cut and  $\beta_j$  for the coverage of workers not scheduled for a wage cut in sector (or country)  $j$ , i.e.:

$$\alpha_j = \frac{N_{1j} + N_{2j}}{N_{1j} + N_{2j} + N_{3j}}, \quad \beta_j = \frac{S_{3j}}{N_{1j} + N_{2j} + N_{3j}} \quad (8)$$

According to equation (7), by using  $\text{coverage}_j$ , instead of  $\text{coverage}_j^*$ , we are permitting that variations in coverage in other parts of the economy as captured by  $\beta_j$  (workers of the same sector or country not scheduled for a wage cut) affect the coverage variable without affecting the fraction of wage cuts prevented by downward wage rigidity, i.e., without affecting the measures of DNWR or of DRWR. The same goes for  $\alpha_j$ , the fraction of workers scheduled for a wage cut, which is expected to vary across sectors or across countries. Thus, the use of  $\text{coverage}_j$  instead of  $\text{coverage}_j^*$ , is likely to have important consequences for the estimate of the parameter aimed at measuring the impact of coverage on DNWR or DRWR.

<sup>3</sup> More precisely, in the case of the statistics  $dnwr_{2j}$  and  $dnwr_{3j}$ ,  $N_{1j}$  should be computed by excluding the fraction of workers whose wages would have been frozen in the absence of downward wage rigidity.

This problem applies to all the regressors considered in the empirical literature that has used the statistics  $dnwr_1$ ,  $dnwr_2$ ,  $drwr_1$  and  $drwr_2$  at the sectoral or country level to identify the relevant determinants of DNWR and DRWR, and may explain why such studies have found it so difficult to get significant correlations with the expected sign. This is an issue deserving further empirical investigation, as soon as appropriate worker-level data become available.

### 3.2 Use of $dnwr_3$

We now consider the case of  $dnwr_3$  used in Babecky *et al.* (2010). These authors use survey data to investigate the causes of DNWR and DRWR at the firm level.<sup>4</sup> In order to identify the factors that may explain why some firms are subject to DNWR while others are not, Babecky *et al.* (2010) estimate a probit model where the dependent variable,  $y_i$ , is a dummy variable which equals one, if the firm answers in the survey that the base wages of their workers have been frozen ( $\Delta w_i = 0$ ), and is zero otherwise. If we denote this model by Model A, we have:

Model A:

$$y_i = 1 \text{ if } \Delta w_i = 0, \quad y_i = 0 \text{ if } \Delta w_i \neq 0$$

We notice that this model is estimated over the full sample, *i.e.*, including not only the firms that have frozen or cut their wages, but also the firms whose wages have increased.<sup>5</sup>

To better understand the implications of model A for the estimated parameters we start by introducing two additional models which we denote by models B and C:

Model B:

$$y_i = 1 \text{ if } \Delta w_i = 0, \quad y_i = 0 \text{ if } \Delta w_i < 0$$

Model C:

$$y_i = 1 \text{ if } \Delta w_i = 0, \quad y_i = 0 \text{ if } \Delta w_i > 0$$

We notice that model B is obtained by restricting model A to firms whose wages have been frozen or cut, *i.e.*, to firms whose workers were scheduled for a wage cut, and that model C is obtained by restricting model A to firms whose nominal base wages were frozen or raised.

From the discussion above it should now be clear that Model B is the model to be estimated if one wants to correctly identify the factors that explain why some firms are subject to DNWR while others are not. Notice that it implies estimating a model using just the firms whose workers were scheduled for a wage cut. In this model, it is expected that the parameter associated with “coverage” will have a positive impact on the probability of a firm having their wages frozen as opposed to have them cut, *i.e.*, of being subject to DNWR.<sup>6</sup>

In contrast, the estimates for the parameter associated with “coverage” in model C may be expected to

<sup>4</sup> Further details on the approach by Babecky *et al.*, (2010) are provided in the next section.

<sup>5</sup> In rigour, the authors estimate a bivariate probit model to account for the interdependence between their measures of DNWR and DRWR which are investigated together in their paper. This, however, is not relevant for the point made in this section which concerns the sample used and not the type of model estimated. Moreover, as we shall show below, the empirical results are basically the same when a univariate or a bivariate model is used.

<sup>6</sup> Notice that now “coverage” is a firm-level variable defined as the proportion of workers covered by collective wage agreements, because the endogenous variable is a binary variable which assumes that either all the workers in the firm have their wages frozen or none have.

be either negative or zero depending on the assumptions about the shocks that might have hit the firms in the sample. If we assume that all the firms in the sample were hit by similar negative shocks, one may expect the estimate of the coefficient associated to coverage to be negative: the higher the coverage in the firm, the lower the probability of having its wages frozen (as opposed to have them increased). If we assume that firms not scheduled for a wage cut were not hit by (large enough idiosyncratic) negative shocks and that such shocks were randomly distributed across firms (the implicit identifying assumption that underlies the statistics  $dnwr_1$ ,  $dnwr_2$ ,  $dwr_1$  and  $dwr_2$  discussed above), then the parameter associated to “coverage” in model C is expected to be equal to zero.

The estimates for the parameters of model A, used in Babecky *et al.*, (2010), are a weighted average of those estimated for models B and C. Ultimately, the sign and the magnitude of the estimated parameters in model A would depend on the proportion of firms with positive and negative wage changes, as well as on the distribution of shocks across the firms in the sample. In most samples, the proportion of wage increases is much higher than the proportion of wage cuts, so that in model A one should not be surprised if some parameters turn out not to be significantly different from zero or even wrong signed. More importantly, however, parameters in model A cannot be interpreted as measuring the impact of DNWR on wages, *i.e.*, they do not measure the importance of DNWR in preventing wage cuts.

#### 4. Evidence on the importance of DNWR and its determinants

We now investigate the extent and the determinants of DNWR in Europe, taking into account the discussions of the previous section concerning the use of  $dnwr_3$ . The dataset used is based on the results of a survey of firms conducted by the National Central Banks of 15 European Union countries between 2007 and 2008. The full sample covers around 14,600 firms from different sectors of activity (manufacturing, energy, construction, market services, non-market services, trade, and financial intermediation).

Our dataset matches closely the one used in Babecky *et al.*, (2010), though they differ in some respects. Ours is an updated version of the original dataset, whose major difference is the inclusion of the information for Cyprus, which was not available to be used in Babecky *et al.* (2010). In turn, in contrast with Babecky *et al.*, (2010), our dataset excludes the Greek data, given that the survey conducted in Greece has no information on base-wage cuts, which is a variable of interest for us. Finally, we have also excluded from our original sample the firms that have not answered one of the two questions on wage freezes or wage cuts. The final set of countries includes Austria, Belgium, Cyprus, Czech Republic, Estonia, France, Hungary, Ireland, Italy, Lithuania, Netherlands, Poland, Portugal, Slovenia and Spain.<sup>7</sup>

In the survey, firms were asked the following two questions pertaining to downward nominal wage rigidity: a) “Over the last five years, has the base wage of some employees in your firm ever been frozen?” and b) “Over the last five years, has the base wage of some employees in your firm ever been cut?”

Besides these questions on base-wage freezes and base-wage cuts, the survey also contained information on a large number of worker and firms’ characteristics. These include information on the composition of the labour force (tenure, share of white collar vs. blue collar workers, share of low skilled vs. high skilled workers, share of workers with permanent contracts), the percentage of workers covered by collective wage agreements, the type of collective wage agreement prevailing at the firm (firm-level or outside agreement), the degree of competition faced by the firm, the number of employees, the labour cost share, etc.

The responses to the two questions on wage freezes and wage cuts are used to define the endogenous variable in the model to be estimated below, while the remaining information is used to construct the corresponding exogenous regressors.

<sup>7</sup> For further details on the design of the survey, see Druant *et al.*, (2012) and Babecky *et al.*, (2010).



#### 4.1 Wage freezes, wage cuts and DNWR

A summary of the responses to the two questions pertaining to downward nominal wage rigidity is presented in Table 1. Columns (2) and (3) present the fraction of firms that froze or cut base wages and column (4) presents an estimate of  $dnwr_t$ , i.e., the fraction of base wage cuts prevented by DNWR (see equation (1)).<sup>8</sup>

From the table, we see that the prevalence of base-wage cuts is extremely rare. On average, for the 15 countries only 1.6 percent of the firms cut base wages of some employees over the last five years. In turn, 7.1 percent of the firms froze base wages. The Czech Republic, Netherlands and Estonia stand out with the highest incidence of base-wage freezes (around 20 percent of the firms).<sup>9</sup>

From Table 1, we also see that our aggregate measure of DNWR, as defined in equation (1), is about 82 percent. This means that, on average, downward wage rigidity prevented scheduled base-wage cuts from taking place in about 82 percent of the firms in the 15 European Union countries in our sample.

**Table 1**

BASE-WAGE FREEZES, BASE-WAGE CUTS AND DNWR   COUNTRY BREAKDOWN			
	Base-wage freezes	Base-wage cuts	DNWR
	(2)	(3)	(4)
Austria	0.079	0.036	0.689
Belgium	0.053	0.017	0.758
Cyprus	0.147	0.041	0.784
Czech Republic	0.221	0.076	0.744
Estonia	0.195	0.025	0.887
France	0.066	0.018	0.787
Hungary	0.057	0.022	0.722
Ireland	0.074	0.016	0.818
Italy	0.038	0.008	0.833
Lithuania	0.149	0.073	0.671
Netherlands	0.209	0.014	0.936
Polan	0.078	0.042	0.647
Portugal	0.147	0.012	0.924
Slovenia	0.023	0.032	0.417
Spain	0.020	0.001	0.946
Total <sup>(a)</sup>	0.071	0.016	0.823

**Source:** Author's calculations.

**Notes:** Columns (2) and (3) are the proportions of firms that froze or cut their base wages. DNWR corresponds to the measure  $dnwr_1$  for base wages as defined in equation (1). With the exception of the last line, it is obtained by dividing column (2) by the sum of columns (2) and (3). **(a)** Weighted average for the 15 countries (GDP weights).

- 8** Some of our figures on the incidence of wage freezes differ from the ones presented in Babecky *et al.* (2010). We believe that the main source of divergence stems from the fact that figures in Babecky *et al.* (2010), are employment-weighted while our figures in Table 1 are not. The fact that we are using a slightly different sample may also help explaining the differences.
- 9** The use of the fraction of wage freezes as a measure of DNWR has been criticized in the literature. Dickens *et al.*, (2007) pointed out that “the fraction of workers with nominal wage freezes in a year varies with the expected rate of inflation and so could be a misleading basis for thinking about the extent of wage rigidity”. More generally, the incidence of wage freezes observed in a given year depends on the sign and magnitude of the shocks that hit the firms in the sample in that particular year, so that there might be firms potentially subject to downward wage rigidity that did not freeze or cut wages because they were not hit by large enough negative shocks. This limitation does not necessarily apply to  $dnwr_t$  in column (4) of Table 1. Even though the fraction of workers scheduled for a wage cut is expected to depend on the sign and magnitude of the shocks, the fraction of wage freezes prevented by downward nominal wage rigidity may be largely independent of economic conditions.



From the table, we also see that Spain, the Netherlands and Portugal rank among the countries with the highest degree of DNWR (base-wage cuts prevented in between 92 and 95 percent of the firms), while Slovenia, Poland, Lithuania and Austria stand out as the countries with the lowest DNWR (base-wage cuts prevented in between 41 and 69 percent of the firms).

Despite the differences in the method and in the datasets used, it might be interesting to compare the figures in Table 1 with the estimates of DNWR computed in Dickens *et al.* (2007) for the 7 countries common to the two datasets: Austria, Belgium, France, Ireland, Italy, Netherlands and Portugal.<sup>10</sup> With the exception of Ireland which shows up with the lowest DNWR in Dickens *et al.* (2007), the rankings for the other six countries closely match in the two datasets: Portugal, Netherlands and Italy define the group with the highest DNWR, while France, Belgium and Austria the group with the lowest DNWR.

It is important to stress that, these estimates should be read with particular care because, as referred before, they are based on a measure that assumes that wage freezes would not exist in the absence of DNWR. In addition, these estimates are based on firm-level data and not on worker-level data, *i.e.*, it is implicitly assumed that all the workers in the firms involved are scheduled for a wage cut, regardless of the share of workers covered by the wage cuts or freezes.

## 4.2 An econometric model for the determinants of downward nominal wage rigidity

In order to identify the determinants of DNWR, we estimate a probit model restricting the original sample to firms scheduled for a wage cut. Thus, our dependent variable,  $y_i$  is defined such that  $y_i=1$  if the firm has frozen wages and  $y_i=0$  if the firm has cut wages. For comparability reasons, we also present the results when all the firms in the sample are used as in Babecky *et al.* (2010). In this case, the dependent variable is defined such that  $y_i=1$ , if the firm has frozen wages, and  $y_i=0$ , otherwise, thus including firms where base wages were cut, as well as firms where base wages were not frozen nor cut.

The choice of the exogenous regressors used in the empirical model was guided by the literature on downward wage rigidity. These include firm-level regressors aimed at measuring the importance of workers' and firms' attributes such as the tenure, the proportion of high- and low-skilled white- and blue-collar workers, the importance of labour costs, the proportion of permanent employees, the proportion of employees covered by collective wage agreements (coverage), the type of union contracts (firm-level or outside agreement), the degree of competition and the size of the firm. The Appendix describes how these regressors were constructed.<sup>11</sup>

Table 2 presents the results of the estimated models and Table 3 the average marginal effects of each of the covariates on the probability of a firm freezing wages. As data for the full set of regressors is not available for the 15 countries we estimate two variants of the model. The first variant, in columns (2) and (3), includes the regressors available for the full sample composed of 15 countries. The variant in columns (4) and (5) includes 4 additional regressors (coverage, tenure between 1 and 5 years, tenure above 5 years and competition) which are available for 8 countries only (Austria, Czech Republic, Estonia, Hungary, Ireland, Lithuania, Poland and Portugal).<sup>12</sup>

The first important point to notice is that the estimates for the average marginal effects in column (2) of

<sup>10</sup> The estimates of DNWR in Dickens *et al.*, (2007) use the statistic  $dnwr_i$  (see equation (1)) based on worker-level data taken from households surveys or administrative data on individuals.

<sup>11</sup> For a review of the literature underlying such regressors, see Babecky *et al.*, (2010).

<sup>12</sup> The four regressions include country dummies to account for fixed effects whose estimated coefficients are not reported in Tables 2 and 3. These country dummies enable us to control for variations in any country-specific omitted factor, such as differences in the survey design across countries, different degrees of employment protection legislation, different inflation rates, etc.

Table 2

PROBIT MODEL   ESTIMATED COEFFICIENTS				
Regressors	Full sample (2)	Restricted sample (3)	Full sample (4)	Restricted sample (5)
Low-skilled blue collar	-0.2875*** (0.0693)	-0.0102 (0.1808)	-0.2191*** (0.1031)	-0.0466 (0.2461)
High-skilled blue collar	-0.1326* (0.0773)	-0.3403* (0.2080)	-0.1804* (0.1097)	-0.4465 (0.2858)
Low-skilled white collar	-0.1347 (0.0966)	0.0376 (0.2670)	-0.0538 (0.1392)	-0.0987 (0.3523)
Labour cost share	0.2175*** (0.0844)	-0.1347 (0.2060)	0.2558** (0.1270)	-0.0421 (0.2766)
Permanent workers	0.1761 (0.1086)	0.0940 (0.2783)	0.1740 (0.1410)	0.0424 (0.3482)
Only firm level agreement	0.0121 (0.0633)	0.0039 (0.1449)	0.0563 (0.1809)	0.5525 (0.4884)
Only outside agreement	-0.0443 (0.0541)	-0.0357 (0.1489)	0.3044 (0.1976)	0.9117* (0.5611)
Both agreements	-0.0833 (0.0722)	-0.2044 (0.1792)	-0.0473 (0.1977)	0.5054 (0.5387)
Coverage	-	-	-0.1813 (0.1911)	-0.6452 (0.5173)
Tenure 1-5 years	-	-	0.3636** (0.1737)	0.4552 (0.4215)
Tenure above 5 years	-	-	0.4635*** (0.1523)	0.3162 (0.3535)
High competition	-	-	0.0125 (0.0519)	-0.2715** (0.1253)
Size=20-49	0.1019** (0.0510)	0.0106 (0.1246)	0.0913 (0.0791)	0.0611 (0.1745)
Size=50-199	0.1818*** (0.0489)	-0.1460 (0.1173)	0.1654** (0.0755)	-0.2021 (0.1734)
Size=200+	0.1521*** (0.0575)	-0.1920 (0.1420)	0.1777* (0.0922)	-0.2361 (0.2118)
Construction	-0.2255*** (0.0686)	-0.1406 (0.1610)	-0.2042** (0.0893)	-0.0533 (0.2177)
Trade	-0.0873* (0.0504)	-0.0820 (0.1206)	-0.0673 (0.0719)	-0.0365 (0.1576)
Other services	-0.0660 (0.0441)	-0.0512 (0.1096)	-0.1057 (0.0692)	-0.1599 (0.1555)
Number of observations	12855	1381	4799	696
Number of countries	15	15	8	8
	$X^2=573.68$ p-value=0.00	$X^2=120.87$ p-value=0.00	$X^2=165.83$ p-value=0.00	$X^2=61.81$ p-value=0.00
	$R^2=0.0815$	$R^2=0.100$	$R^2=0.0511$	$R^2=0.0939$

Source: Author's calculations.

Notes: Robust standard errors in parentheses.  $R_p$  stands for the Pseudo  $R_p$  and  $X_p^2$  for the test statistic on the overall significance of the estimated coefficients. \*\*\*, \*\* and \* stands for significance at 1, 5 and 10 percent levels, respectively.

Table 3 do not significantly differ from the estimates presented in Babecky *et al.*, (2010). The observed differences seem compatible with the differences in the two datasets and the model used (bivariate vs. univariate probit model). The second point to notice regards the models for the restricted sample in columns (3) and (5). In these models, the number of observations is drastically reduced because the sample is constrained to firms whose workers were scheduled for a wage cut, and wage cuts in the sample are extremely rare, as we have seen.

In the model for the full sample (columns (2) and (4)), there are several regressors whose coefficients are significantly different from zero namely the proportion of low-skilled and high-skilled blue-collar workers, the labour cost share, the tenure and the size.

However, as we argued before, these coefficients cannot be interpreted as gauging the probability of a firm being subject to DNWR, *i.e.*, they do not measure the importance of DNWR in preventing base-wage cuts. The fact that in the full sample we are comparing firms that have frozen base wages with firms that have either cut or increased base wages, makes the estimated parameters uninterpretable.<sup>13</sup>

For the model with the restricted sample, given the relatively small number of wage cuts, one should not expect to find many regressors with statistically significant coefficients. Indeed, it is well known that the estimators of the parameters in probit or logit models are biased in finite samples. Moreover, King and Zeng (2001) show that these biases become especially acute and the conventional variance estimators significantly magnified in the presence of rare events, *i.e.*, when  $\text{Prob}(y_i = 1)$  (or  $\text{Prob}(y_i = 0)$ ) is very small. In our case, the small proportion of wage cuts in the population of firms scheduled for a wage cut (around 20 percent), is likely also to make it more difficult to get unbiased and statistically significant coefficients for the parameters of the model.

If we look at the model with the full set of regressors (column (5) in Tables 2 and 3) we see that two regressors emerge with a significant impact on the probability of a firm being subject to DNWR: the existence of (only) outside agreements and high competition. According to Table 3, the probability of a firm being subject to DNWR is about 24 percentage points higher if their wages are negotiated with unions at a level outside the firm (and there are no firm-level agreements). In turn, for a firm operating in a highly competitive environment the probability of being subject to DNWR is about 7 percentage points lower than for an otherwise identical firm. These results are in line with the theory. Cutting base-wages when these are negotiated outside the firm with unions is a difficult task because wages may be changed only by mutual consent (Holden, 2004). In contrast, wages in firms with no union contracts (the reference or baseline group) are expected to be easier to cut in bad times. In turn, firms operating in a highly competitive environment are likely to feel stronger pressure to reduce costs and thus one may expect a more intense adjustment of wages in reaction to shocks.<sup>14</sup>

The estimated results also suggest that the workforce composition is related to downward wage rigidity. The proportion of high-skilled blue-collar workers emerges as a significant regressor in the model with the restricted sample in column (3) and closely to being statistically significant in the model in column (5), despite the strong reduction in the number of observations. In general, according to Table 3, we may say that firms with a larger proportion of high-skilled white-collar workers (the baseline category) are more likely to be subject to DNWR, in line with the efficiency wage theory (the effort of high-skilled workers is more valuable and more difficult to monitor so that firms may be more reluctant to cut their wages).

Interestingly, the share of workers covered by collective wage agreements and the proportion of permanent contracts are not significant in any regression. In contrast to what is found when the full sample is used, the degree of DNWR does not also seem to vary significantly with the labour cost share, the tenure or the size of the firm.

Overall, the estimations in this article show that the empirical evidence on the determinants of downward wage rigidity changes significantly according to whether or not the sample used in the estimation is

<sup>13</sup> Babecky *et al.* (2010), explicitly assume that there are three types of firms in the dataset: 1) firms that have frozen wages are considered as subject to DNWR; 2) firms that apply an automatic wage indexation mechanism are considered as subject to DRWR; 3) firms that do not show signs of DNWR or DRWR are considered as firms with flexible wages. In our view, by bunching together in the third group firms that have cut wages and firms that have increased wages the authors end-up estimating a model where the parameters do not seem to have any obvious interpretation.

<sup>14</sup> We consider that a firm is subject to “high competition” if it answered in the survey that it will likely or very likely decrease its price in reaction to a decrease in the price of its main competitor.

Table 3

PROBIT MODEL   AVERAGE MARGINAL EFFECTS				
Regressors	Full sample	Restricted sample	Full sample	Restricted sample
	(2)	(3)	(4)	(5)
Low-skilled blue collar	-0.0417*** (0.0100)	-0.0026 (0.0454)	-0.0400*** (0.0188)	-0.0124 (0.0657)
High-skilled blue collar	-0.0192* (0.0112)	-0.0855* (0.0520)	-0.0330* (0.0201)	-0.1192 (0.0758)
Low-skilled white collar	-0.0195 (0.0140)	0.0094 (0.0671)	-0.0098 (0.0254)	-0.0264 (0.0941)
Labour cost share	0.0315*** (0.0123)	-0.0338 (0.0518)	0.0467** (0.0232)	-0.0112 (0.0739)
Permanent workers	0.0255 (0.0157)	0.0236 (0.0699)	0.0318 (0.0258)	0.0113 (0.0929)
Only firm level agreement	0.0018 (0.0092)	0.0010 (0.0364)	0.0103 (0.0331)	0.1475 (0.1300)
Only outside agreement	-0.0064 (0.0078)	-0.0090 (0.0374)	0.0556 (0.0361)	0.2435* (0.1492)
Both agreements	-0.0121 (0.0105)	-0.0513 (0.0450)	-0.0086 (0.0361)	0.1349 (0.1436)
Coverage	-	-	-0.0331 (0.0349)	-0.1723 (0.1376)
Tenure 1-5 years	-	-	0.0664** (0.0317)	0.1215 (0.1122)
Tenure above 5 years	-	-	0.0847*** (0.0278)	0.0844 (0.0943)
High competition	-	-	0.0023 (0.0095)	-0.0725** (0.0332)
Size=20-49	0.0153* (0.0079)	0.0027 (0.0312)	0.0172 (0.0153)	0.0161 (0.0454)
Size=50-199	0.0276*** (0.0077)	-0.0372 (0.0303)	0.0312** (0.0147)	-0.0549 (0.0478)
Size=200+	0.0234*** (0.0093)	-0.0502 (0.0385)	0.0346* (0.0191)	-0.0657 (0.0611)
Construction	-0.0290*** (0.0077)	-0.0369 (0.0440)	-0.0340** (0.0134)	-0.0150 (0.0599)
Trade	-0.0122* (0.0068)	-0.0210 (0.0314)	-0.0120 (0.0126)	-0.0098 (0.0426)
Other services	-0.0094 (0.0062)	-0.0129 (0.0278)	-0.0189 (0.0121)	-0.0436 (0.0433)
Number of observations	12855	1381	4799	696
Number of countries	15	15	8	8

Source: Author's calculations.

Note: \*\*\*, \*\* and \* stands for significance at 1, 5 and 10 percent levels, respectively.

restricted to firms that would have their wages cut in the absence of DNWR. Most of the regressors that emerge with a significant impact when the full sample is used are not significant when the analysis is restricted to firms scheduled for a base-wage cut (labour cost share, tenure and size) while the coefficients that emerge with a significant impact in the restricted sample, with the exception of the skill distribution, are not important when the full sample is used (type of wage level agreement and competition). These results suggest that the methodological considerations raised in this article can have important empirical implications.

## 5. Conclusions

This article discusses some methodological issues involving the existing empirical literature on downward nominal and real wage rigidity and presents empirical evidence that illustrates its importance and identifies its determinants in some European countries.

As regards the existing empirical literature, it is shown that in some cases the regressors used may not be correctly defined, and that, in some others, the estimated models may be specified properly. The first situation may imply potential important biases for the estimated parameters. The second implies that the model parameters cannot be interpreted as measuring the importance of the regressors for downward wage rigidity.

Using survey data for 15 European Union countries, we perform an empirical exercise, taking into account the remarks made in the article. It is shown that downward rigidity in nominal base wages has played an important role in those countries: on average, it has prevented scheduled base-wage cuts from taking place in about 82 percent of the firms. Nominal base-wage rigidity emerges as especially important in Spain, Netherlands and Portugal and less significant in some eastern countries (Slovenia, Poland and Lithuania).

However, these estimates should be read with particular care because they are based on a measure that assumes, on the one hand, that wage freezes would not exist in the absence of DNWR and, on the other hand, that all the workers in the firms involved are scheduled for a wage cut, regardless of the share of workers affected by the wage cuts or freezes.

A probit model, restricted to firms that would have their base wages cut in the absence of downward nominal wage rigidity, suggests that the importance of downward nominal base-wage rigidity increases with the proportion of “high-skilled white-collar” workers and the importance of “wage agreements negotiated outside the firm”, and decreases with the “degree of competition” faced by the firm. The incidence of “permanent workers”, the “labour cost share”, the “tenure” or the “proportion of workers covered by collective agreements”, suggested by the economic theory as potential relevant factors, do not emerge as having a significant impact on downward nominal base-wage rigidity in those countries. These results differ significantly from the ones previously obtained in the literature, suggesting that, at least, some of the methodological considerations raised in this article have important practical implications.

## Appendix - Variable Definitions

In this Appendix, we describe the covariates used in the probit models whose results are presented in section 4. The details are as follows:

*Low-skilled blue-collar* – Proportion of low-skilled blue-collar workers on firm’s total employment;

*High-skilled blue-collar* – Proportion of high-skilled blue-collar workers on firm’s total employment;

*Low-skilled white-collar* – Proportion of low-skilled white-collar workers on firm’s total employment;

*Labour cost share* – Proportion of labour costs on total costs;

*Permanent workers* – Proportion of workers with permanent contracts on the firm’s total workforce;

*Only firm level agreement* – Dummy variable which equals 1 if the firm applies only an agreement concluded inside the firm;

*Only outside agreement* – Dummy variable which equals 1 if the firm applies only an agreement concluded outside the firm;

*Both agreements* – Dummy variable which equals 1 if the firm applies both firm-level and outside wage agreements;

*Coverage* – Proportion of workers covered by collective wage agreements;

*Tenure 1-5 years* – Proportion of workers with tenure between one and five years;

*Tenure above 5 years* – Proportion of workers with tenure above five years;

*High competition* – Dummy variable equal to one if the firm answers in the survey that it will likely or very likely decrease its price in reaction to a decrease in the price of its main competitor;

*Size=20-49* – Dummy variable equal to one if the number of employees is between 20 and 49;

*Size=50-199* – Dummy variable equal to one if the number of employees is between 50 and 199.

*Size=200+* – Dummy variable equal to one if the number of employees is equal or larger than 200;

*Construction* – Dummy variable equal to one if the firm operates in the Construction sector;

*Trade* – Dummy variable equal to one if the firm operates in the Trade sector;

*Other services* – Dummy variable equal to one if the firm operates in any other services.

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