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A replication note on downward nominal and real wage rigidity: survey evidence from European firms

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Abstract This note shows that the models estimated in Babecký et al. (Scand J Econ 112(4):884–910, 2010) do not allow identifying the determinants of downward nominal wage rigidity and provides new empirical evidence on the importance of downward nominal wage rigidity and its determinants in several European countries.

Keywords Base-wage rigidity · Wage freezes · Wage cuts · Probit model

JEL Classification C35 · J31 · J50

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1 Introduction

In a recent contribution, Babecký et al. (2010) try to identify the factors that explain why the importance of downward nominal and real wage rigidity may differ across firms. The authors estimate probit models using firm-level data for 15 European countries and conclude that factors like the proportion of high-skilled white-collar workers, the share of labour cost, the share of permanent workers, or the average tenure increase the likelihood of a firm being subject to downward nominal wage rigidity.

In Babecký et al. (2010), the models were estimated using all the firms in the sample. In this note, we argue that for estimation purposes the relevant sample must be constrained to firms with workers scheduled for a wage cut. Otherwise, the parameters of the estimated models cannot be interpreted as measuring the importance of the regressors for downward nominal wage rigidity. We also provide empirical evidence on the importance of downward nominal wage rigidity and its determinants for 15 European countries by using a dataset that closely matches the one used in Babecký et al. (2010).

The note is organised as follows. Section 2 discusses the problem with the models estimated in Babecký et al. (2010). Section 3 investigates the extent and the determinants of downward nominal wage rigidity by estimating a model for 15 European countries. Section 4 concludes.

2 Identifying the determinants of downward wage rigidity

The literature on wage rigidity has suggested several statistics to gauge the importance of downward nominal wage rigidity (DNWR) and downward real wage rigidity (DRWR). For instance, the measures 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 DNWR and the authors suggest using the statistic:

$$dnwr1 = \frac{A}{A + B} \tag{1}$$

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

This measure 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 DNWR. This alternative measure may be written as

$$dnwr2 = \frac{A - C}{A - C + B} \tag{2}$$

where C stands for the number (or fraction) of workers whose wages would have been frozen in the absence of DNWR. C is usually estimated by assuming an underlying counterfactual or notional distribution that would have been observed under fully

flexible wages.[1] Statistics similar to *dnwr*1 and *dnwr*2 have been constructed to gauge the importance of DRWR.[2]

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

Recently, Babecký et al. (2010) followed a different approach. These authors use the proportion of firms in the economy that have frozen nominal wages as a statistic to gauge the importance of DNWR. We may denote such statistics as

$$dnwr3 = \frac{D}{N} \tag{3}$$

where D is the number of firms whose wages have been frozen and N the total number of firms in the sample. This measure of DNWR differs from *dnwr*1 in that it uses the firm as the relevant unit (not workers) and, more important to the point, in that it compares the number of firms that have frozen their wages with the total number of firms in the sample rather than with the firms whose workers have been scheduled for a wage cut, as the previous statistics do. This difference, as we now show, has important consequences for the interpretation of the parameters of the estimated models. As in Babecký et al. (2010), define y_i as a dummy variable which equals one if the firm i froze nominal wages ($\Delta w_i = 0$), and consider the 3 alternative models:

Model A:

$y_i = 1$ if $\Delta w_i = 0$, $\quad y_i = 0$ if $\Delta w_i \neq 0$.

Model B:

$y_i = 1$ if $\Delta w_i = 0$, $\quad y_i = 0$ if $\Delta w_i < 0$.

Model C:

$y_i = 1$ if $\Delta w_i = 0$, $\quad y_i = 0$ if $\Delta w_i > 0$.

Model A corresponds to the approach followed by Babecký et al. (2010) which was estimated using the full sample, i.e. including not only the firms that have frozen or cut their wages, but also the firms whose wages were increased.[3]

Model B is obtained by restricting model A to firms whose workers were scheduled for a wage cut and model C is obtained by restricting model A to firms whose wages were frozen or raised.

Since the goal is to identify the factors that explain why some firms are subject to DNWR while others are not, it is obvious that model B is the right model to use as it is the one that restricts the analysis to firms whose workers were scheduled for a wage cut.

[1] See, for instance, Card and Hyslop (1997), Kahn (1997), Altonji and Devereux (2000), Fehr and Goette (2005), Goette et al. (2007), Dickens et al. (2007) and Knoppik and Beissinger (2009).

[2] See, for instance, Dickens and Goette (2006) and Dickens et al. (2007).

[3] 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 note 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.

In order to get the intuition of the problem let us take, for instance, the union coverage as an example of a regressor, which is commonly used in empirical applications.[4] In model B, 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. In contrast, the estimates for the parameter associated with this regressor in model C may be expected to 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 with 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 whose workers were not scheduled for a wage cut were not hit by large enough negative shocks (the implicit identifying assumption that underlies the statistics *dnwr*1 and *dnwr*2 discussed above), then the parameter associated with coverage in model C is expected to be equal to zero.

Thus, the estimates for the parameters of model A, used in Babecký et al. (2010), are a weighted average of those 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 wrongly 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.

3 New evidence on the importance of DNWR and its determinants

Following our discussion of the previous section, we now investigate the extent and the determinants of DNWR in Europe. 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).[5]

[4] As in Babecký et al. (2010), in this note the union coverage is computed as the proportion of workers in the firm covered by collective wage agreements. In rigour, however, for each firm, the union coverage should be defined as the proportion of workers covered by collective wage agreements out of those scheduled for a wage cut. This limitation stemming from the lack of appropriate data may be expected to have implications for the parameters of the models estimated below. For a discussion, see Dias et al. (2012).

[5] Our dataset matches closely the one used in Babecký 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 Cyprus, which was not available to be used in Babecký et al. (2010), and the exclusion of Greece, because the survey conducted in this country has no information on base-wage cuts, which is a variable of interest for us. We also excluded 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. For further details on the design of the survey, see Druant et al. (2009) or Babecký et al. (2010).

Table 1 Base-wage freezes, base-wage cuts and DNWR

	Base-wage freezes (2)	Base-wage cuts (3)	DNWR (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
Poland	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[a]	0.071	0.016	0.823

The entries in column (2) correspond to *dnwr*3 as defined in Eq. (3), i.e. are the proportions of firms that froze their base wages. The entries in column (3) are the proportions of firms that cut their base wages. DNWR in column (4) corresponds to measure *dnwr*1 based on firm-level data. 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)

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?".

The responses to these two questions on base wage freezes and base-wage cuts are used to define the endogenous variable in the model to be estimated below, while the information in the survey regarding workers and firms' characteristics is used to construct the exogenous regressors.

3.1 Wage freezes, wage cuts and DNWR

A summary of the responses to the two questions pertaining to DNWR is presented in Table 1. Column (2) presents an estimation of *dnwr*3 (proportion of firms that froze base wages) and column (3) records the fraction of firms that cut base wages. Column (4) presents an estimate of *dnwr*1 based on firm-level data, i.e. the fraction of firms in which scheduled base-wage cuts were prevented from taking place due to DNWR.[6] We see that 7.1 % of the firms froze base wages, but the prevalence of base-wage cuts is extremely rare: on average, for the 15 countries only 1.6 % of the firms cut base wages of some employees.

[6] Some of our figures on the incidence of wage freezes differ from the ones presented in Babecký et al. (2010). We believe that the main source of divergence stems from the fact that figures in Babecký 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.

From Table 1, we also see that the aggregate measure of DNWR, as defined in Eq. (1), is about 82 %, which means that, on average, over a period of 5 years, downward nominal wage rigidity has frozen base wages in about 82 % of the firms with scheduled nominal base-wage cuts.[7] Spain, the Netherlands and Portugal rank among the countries with the highest degree of DNWR, while Slovenia, Poland, Lithuania and Austria stand out as the countries with the lowest DNWR.

Despite the differences in the method and in the datasets used, it seems interesting to compare the figures in Table 1 with the estimates of DNWR computed in Dickens et al. (2007) for the seven countries common to the two datasets: Austria, Belgium, France, Ireland, Italy, Netherlands and Portugal.[8] 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.

3.2 An econometric model for the determinants of downward nominal wage rigidity

In order to identify the determinants of DNWR, we now estimate a probit model restricting the original sample to firms for which some workers were scheduled for a nominal base-wage cut as in model B above. For comparability reasons, we also present the results when all the firms in the sample are used as in model A above.

The choice of the exogenous regressors used in the empirical model was guided by the literature on downward wage rigidity.[9] The "Appendix" describes how these regressors were constructed.

Table 2 presents 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).[10]

The first important point to note is that the estimates for the average marginal effects in column (2) do not significantly differ from the estimates presented in Table

[7] Note that the estimates of DNWR 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. Moreover, because they are based on *dnwr*1, i.e. do not exclude the workers whose wages would have been frozen in the absence of DNWR, they are likely to overestimate the importance of DNWR.

[8] The estimates of DNWR in Dickens et al. (2007) use the statistic *dnwr*1 [see Eq. (1)] based on worker-level data taken from households surveys or administrative data on individuals.

[9] For a review of the literature underlying such regressors, see Babecký et al. (2010).

[10] The four regressions include country dummies to account for fixed effects whose estimated coefficients are not reported in Table 2. 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—average marginal effects

Regressors	Full sample (2)	Restricted sample (3)	Full sample (4)	Restricted sample (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 (0.0.0361)	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	12,855	1,381	4,799	696
Number of countries	15	15	8	8

"Full sample" refers to model A and "Restricted sample" to model B; the four estimated models also include country dummies

***,**,* Significance at 1, 5 and 10 % level, respectively

4 of Babecký et al. (2010), despite the differences in the two datasets and in the type of model used (bivariate probit model). The second point to note 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 for which some workers were scheduled for a wage cut.[11]

[11] In the survey about 0.7 % of the firms answered that they had both frozen and cut wages over the last 5 years. For estimation purposes, we assumed that wages in such firms are flexible, so that they enter in the estimated probit models with $y_i = 0$.

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 shown above, 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.

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.[12] If we look at the model with the full set of regressors [column (5)], 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 firm-level agreements and high competition. According to Table 2, 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. 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 nominal 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 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.

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). 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 share, the tenure or the size of the firm.

4 Conclusions

This note shows that the parameters of the models estimated in Babecký et al. (2010) cannot be interpreted as measuring the importance of the regressors for DNWR and

[12] 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 whose workers were scheduled for a wage cut (around 20 %), is likely also to make it more difficult to get unbiased and statistically significant coefficients for the parameters of the model.

provides new empirical evidence on the importance of downward nominal wage rigidity and its determinants.

Using survey data for 15 European Union countries, we show that downward rigidity in nominal base wages is pervasive in Europe: on average, over a period of 5 years, it has frozen base wages in about 82 % of the firms with scheduled nominal base-wage cuts. Nominal base-wage rigidity emerges as especially important in Spain, Netherlands and Portugal and less significant in some eastern countries like Slovenia, Poland or Lithuania.

A probit model, restricted to firms that would have their base wages cut in the absence of downward nominal wage rigidity, suggests that the degree of downward nominal 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 wage rigidity. These results differ significantly from the ones previously obtained in Babecký et al. (2010), suggesting that the methodological considerations raised in this note have important practical implications.

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Appendix: Variable definitions

In this appendix, we describe the covariates used in the probit models whose results are presented in Sect. 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 or workers covered by collective wage agreements.
Tenure 1–5 years—Proportion of workers with tenure between 1 and 5 years.
Tenure above 5 years—Proportion of workers with tenure above 5 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 sector.

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