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Abstract

We estimate the degree of downward wage rigidity in Italy using a micro-econometric model in which wages may be subject to both nominal and real downward rigidities. We use the recently released Worker History Italian Panel (WHIP), an administrative individual-level data set covering both the high-inflation and automatic-indexation regime prevailing before the 1990s, and the regime that emerged after the indexation system was dismantled. Overall, we find a sizable amount of downward rigidities, downward real wage rigidity being much more relevant than downward nominal wage rigidity. Over time, downward rigidities have become less important, with the reduction in real rigidities more than offsetting the rise in nominal rigidities. This pattern is consistent with the labour market reforms Italy experienced and specifically with the abolition of the automatic price-indexation clause. In order to verify the robustness of these results we also explore an identification strategy in which the real rigidity threshold, instead of being centred around price inflation for all workers, is centred around the wage rise specifically dictated for each worker by the relevant industry-wide national collective contract. Our main results are broadly confirmed. Equipped with these more precisely identified measures of downward rigidities, we further explore their relationship with several labour market outcomes. We find that downward wage rigidities are positively related to firm turnover – which we interpret in terms of employment adjustments substituting for wage adjustments – and local unemployment rates – which hints at the macroeconomic relevance of our micro-based rigidity measures.

1. INTRODUCTION

Wage rigidity has traditionally attracted the attention of economists looking at macroeconomic phenomena and the functioning of the labour market. Regarded as a constraint that interferes with notional adjustments to demand or supply shocks, wage rigidities are standard culprits for high and persistent unemployment. For countries like Italy, characterized by large territorial imbalances, unresponsiveness of wages to local labour market conditions is often regarded among the factors impairing regional catching-up processes. More broadly, the disappearance of exchange rate variability within the euro area has brought back to the general attention the importance of labour market and wage flexibility to deal with regional shocks. Finally, the recent debate on monetary policy rules and the optimal rate of inflation has pointed out that a small but still positive rate of inflation may act like a "lubricant of the gears of the economy" (e.g., Akerlof, Dickens and Perry, 1996) in the presence of downward nominal wage rigidities.

But do we know that wages are really rigid? And, if they are, to what extent and what kind of rigidity can affect them? Traditionally, this question has been analyzed at a macroeconomic level considering aggregate wage behaviour. Nominal rigidities have been equated to a slow reaction of nominal wages to prices – a fully-fledged reaction being supposed to materialize in the long run ruling out monetary illusion – while the limited responsiveness of wages to (national and/or local) labour market conditions has been identified as real rigidity. Both types of rigidity were so elicited by looking at the coefficients of some simple macroeconomic relationships, like the Phillips curve or the so-called wage curve.[i]

Only recently, has the empirical analysis shifted to the microeconomic level. Here downward wage rigidity may be defined as the presence of some constraints causing the wage change distribution to be asymmetric and excessively concentrated around some thresholds. For downward nominal wage rigidities (DNWR) the relevant threshold impeding negative wage changes is the zero. For downward real wage rigidities (DRWR) the threshold, whose presence may impede negative changes as well as positive but " too small" wage increases, is usually identified around the inflation rate.

While inspection of the wage change distribution may provide some hints of the presence of wage rigidity, precise measurement requires the definition of a counterfactual – i.e. the notional wage change distribution which would prevail in the absence of rigidities – taking into account measurement errors, which may plague observed wage changes. Following the seminal contribution of Mc Laughin (1994), a burgeoning literature has recently focused on DNWR in a number of countries using a variety of micro level data [e.g., Card & Hyslop (1997), Kahn (1997), Mc Laughin (1994) for the US; Christofides and Leung (1988), Crawford and Harrison (1998) and Fortin (1996) for Canada; Smith (2000) and Nickell and Quintini (2003) for the

Great Britain; Fehr and Göette (2003) for Switzerland; Goux (1997) for France; Knoppik and Beissinger (2003) for Germany; Devicienti (2002) for Italy].

Such literature has however mostly neglected the presence of DRWR. Yet both visual inspection of the empirical wage change distributions and conventional wisdom suggest that in much of continental Europe, and most likely in Italy, real rigidities are at least as important as nominal rigidities. In general, such a conjecture may hold true in countries with centralized wage-setting institutions that aim at safeguarding workers purchasing power.

The novelty of the approach adopted here is that both DNWR and DRWR are jointly identified. The former would prevent wage adjustments whenever the occurred shocks “suggest" a nominal wage cut. It might arise because of coordination failures and asymmetric information producing a misperception of absolute and relative price movements and mistrust between the worker, unable to fully realize the presence of a negative shock, and the firm. It is the presence of this specific type of rigidity, and not simply a slow reaction of nominal wages to prices, that would suggest the optimality of a small but still positive inflation rate as a lubricant for the economy. In an economy with a positive inflation rate, however, it is likely that the above mentioned asymmetries would lead to a default “inactivity option”, in which wage changes are centred around the inflation rate. More broadly, an attraction point would be provided by statutory minimum wages and centralized union bargaining providing an exogenous institutional yardstick around which idiosyncratic shocks would be handled through bilateral bargaining between the individual firm and the individual worker. Just like DNWR prevents nominal wage cuts, DRWR would prevent wage changes that are “too small” vis-à-vis the positive wage norm associated with inflation and/or national contracts.

Notice that both types of rigidities, while affecting different segments of the wage change distribution (DNWR being confined to the left of zero, while DRWR being much more pervasive), would have a twofold impact on the wage change distribution. They would produce an “excessive" asymmetry in the shape of the wage change distribution and an excessive concentration around determined thresholds (one at zero wage-growth, another around inflation or the wage norm related to centralised unions’ bargaining). The joint presence of both such features of the wage change distribution is the effect of the downward wage rigidity we try to identify. By itself, an excessive concentration around the zero change threshold could be simply determined by the presence of transaction costs that prevent both small increases and small reductions in wages. Or it could be due to the fact that, lacking adequate information, inactivity (zero variation) or a variation equal to that prevailing in the rest of the economy could turn out to be the optimal solution. Transaction costs and lack of information of this type would however alter the shape of the distribution in a symmetric way.

This paper tries to implement the simple intuitions expressed above by adopting a parametric approach in which the observed wage change distribution is affected by both types of rigidities, measurement error and an underlying rigidity-free distribution capturing idiosyncratic shocks. More precisely, the extent of rigidity is identified by comparing the wage-change distribution observed in the data with a hypothetical, rigidity-free distribution. The latter is in turn estimated on the basis of "regularity" assumptions concerning its form and determinants. The empirical model also takes into account the possibility that the observed distribution may be affected by measurement error, which in our case mostly captures the fact that the administrative earnings data we use also include overtime payments. The parametric model is an extension of the Altonji and Deveraux (1999) model whose formalisation is presented in Dickens and Göette (2003) and fully described in the Appendix provided in the Bauer *et al*. paper in this same feature.

We implement that model using 15 years of Italian data spanning both a period with relatively high inflation and an automatic wage indexation clause and a period in which inflation was lower and the wage indexation clause had been dismantled. So, our first aim is to examine the changes over time in the extent of both types of rigidity, verifying whether those changes are consistent with macroeconomic and institutional evolution. Our first result is that in Italy DRWR is larger than DNWR. Over time we observe a reduction in wage rigidities, with a sizable decline in DRWR more than offsetting the small rise in DNWR, a pattern entirely consistent with the various labour market reforms Italy experienced, and more specifically with the abolition of the automatic price-indexation clause (*scala mobile*).

In the second half of our sample period, we are also able to complement that parametric model with the use of external information. One weakness of the parametric model is that the rigidity-free estimates and the two types of rigidities (and the measurement error) are all identified by looking at the same observed wage change distribution. In order to verify the robustness of our results we resort to external information concerning the hypothetical wage increases, which the relevant national unions contracts would dictate for each individual worker (taking into account the worker's industry and occupational grade). We centre the "real-rigidity threshold" around such a value instead of estimating it within the model. So, to facilitate comparisons with the results obtained for Britain and Germany, and over time within Italy, we start from the estimates of a benchmark case in which the "real-rigidity threshold" is treated as a parameter to be estimated alongside the other model's parameter, we also present these additional estimates which we believe are more robust.

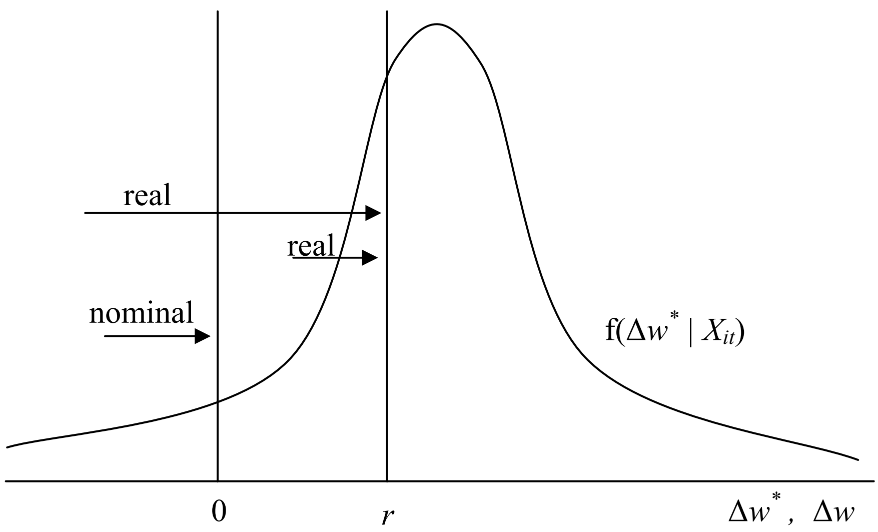
Equipped with these more refined estimates, which have the property of having a rich cross sectional variability as the real threshold varies a lot across workers and firms, we also explore some implications of wage rigidities. Theoretical arguments have been put forward that predict a negative relationship between wage and employment flexibility. In a nutshell, the argument says that firms, being constrained from cutting (nominal and/or real) wages whenever necessary, may resort to quantity adjustments. Such an argument has also been used in order to explain why significant workers and job turnover appears in countries with rather stringent employment protection legislation, which is actually the case in Italy, particularly in the period examined here (see Bertola and Rogerson, 1997). We therefore test the hypothesis that firms facing higher downward (nominal and real) wage rigidity experience higher job and worker turnover (and excess worker turnover, all defined *à la* Davis and Haltiwanger, 1999). On top of that, we also try to understand the aggregate implications of both nominal and real rigidities by averaging them over provinces and years and inserting them into a reduced-form regression for the evolution of the provincial unemployment rate. Both explorations produce interesting results supporting the economic significance of the rigidity measures constructed here: downward wage rigidities are conducive both to more turnover flows and to higher local unemployment rate.

The paper is organized as follows. In section 2 the econometric model is briefly presented, also explaining how the external contractual information is used in one of our additional exercises. Sections 3 and 4 provide details about the data used and a visual inspection of the observed wage change distribution, respectively. The benchmark estimates for the whole sample period are shown in section 5. Section 6 presents the results for the application in which the real threshold is identified using external contractual information and section 7 explores the implications of our rigidity measures. Section 8 briefly concludes.

2. THE ECONOMETRIC MODEL

Most micro-econometric studies on wage rigidity start with the assumption that (percentage) nominal wage-changes between year t and t-1, denoted by Δw_t*, follow a *notional* distribution. This is a counterfactual, unobservable, wage-change distribution that would prevail in the absence of downward rigidity. In figure 1 such a distribution, assumed to be approximately normal, is represented by $f(\Delta w_{it}* \mid X_{it\text{-}1})$: in the absence of institutional impediments or other obstacles, firms and workers would agree on varying nominal wages according to the distribution f, that depends on the characteristics $X_{it\text{-}1}$ of the worker i (and of the job he/she holds) in period t-1. Nominal and real downward rigidities act as constraints impeding some of these notional changes. Looking at figure 1, the arrow labelled "nominal" indicates that some negative notional variations $\Delta w_{it}*$ (below the zero-growth threshold) cannot be implemented (*DNWR*). In this case the *actual* wage change, Δw_{it}, would become equal to zero, since wage cuts are transformed into a *wage freeze*. Another type of rigidity, called *downward real wage rigidity* (DRWR), postulates instead that, sometimes, notional increases cannot be less than a threshold r>0. In this case, not only are some notional cuts prevented, and transformed into $\Delta w_{it} = r_t$ (as represented by the longer arrow called "real" in figure 1), but also some notional variations between 0 and r are forced to align to the real-threshold r (see the smaller arrow called "real"). The threshold r does not have a unique interpretation. It can represent the expected inflation rate, e.g., workers and firms have pre-determined the evolution of nominal wages to expected inflation and renegotiations based on the actual economic situation, macro or relative to specific situations, are costly or in some cases impossible. Or it can reflect the operation of a wage indexation mechanism to the actual inflation rate – mechanism that may fail to take into account that some consumer price changes, like those originating from supply shocks (e.g., an increase in the price of oil for a importing country), should lead to a notional decrease in the real wages. More generally, the threshold r can correspond to wage changes dictated by national contracts, which limit decentralized decisions at the firm level or at an individual worker level. One way or the other, this concept of DRWR tries to capture the idea that nominal wages, in some cases, cannot be increased by less than a specific threshold above zero.

Fig 1



The estimation of wage rigidities in this model consists of quantifying how many times the constraints represented by 0 and r are binding. To do this, we will use an econometric model to estimate: (a) the probability that the mechanism described as DNWR is operating, we denote this probability by p_n; (b) the probability that the mechanism described as DRWR is operating, we denote this probability by p_r; (c) the parameters of the notional distribution f.

As shown in figure 1, the two regimes are actually binding only for those individuals whose notional wage change lies in a particular range (left of zero for DNWR, and left of r for DRWR). So one can quantify the number of times the two regimes are binding by calculating the share of workers actually affected by the real rigidity regime (percentage that we will call "real wage freeze") and the percentage affected by the nominal rigidity regime (percentage that we will call "nominal wage freeze"). Furthermore, the estimates obtained also allow one to measure how much the actual wage distribution differs from the notional distribution because of the presence of rigidity. One summary measure used by the literature is the so-called "wage sweep- up". In particular, the "nominal wage sweep-up" measures how much the actual wage-change is expected to be higher than the notional change, because some wage cuts may be transformed into wage freezes. Similarly, the "real wage sweep-up" provides a measure of how much the actual wage-change is expected to be higher than the notional change, because some wage changes may be forced to align to the real rigidity threshold.

The model also allows for the possibility that some wage changes are observed with measurement errors. Assuming that errors are normally distributed, the model will estimate the percentage (denoted by *M*) of wage changes that are measured with error, and the variance of the error term ($\sigma_m^2(t)$).

More specifically, the model used is that specified by Dickens and Göette (2003) and also applied in the companion papers (in this same feature) by Barwell and Schweitzer to British data and Bauer *et al*. to German data. In such a model the threshold r is jointly estimated with the notional distribution f, allowing for some stochastic variability around the threshold. In other words, it is assumed that $r_t = \bar{r}_t + \varepsilon_t^r$, where both $\bar{r}_t$ and the variance of ε_t^r (denoted σ_r^2(t)) is estimated inside the model.

As better described in the Appendix to the paper by Bauer *et al*., this model generalizes the approach proposed by Altonji and Devereaux (1999), and permits a joint estimation of DNWR, DRWR, the notional wage distribution and the parameters of the measurement error process. Essentially, the estimates are carried out using the maximum likelihood method, assuming that all the stochastic components of the notional distribution f, the real threshold r and the measurement error are normally distributed.

The model's limitations should not be overlooked. First of all, the model distinguishes between DNWR and DRWR, but it is not able to directly analyze other factors possibly impacting upon the wage change distribution, for instance menu costs producing symmetric excessive concentration around some thresholds. More importantly, the joint identification of the notional distribution, the real threshold r, the probabilities of DNWR and of DRWR, and the error process is intrinsically difficult, because none of them is directly observable. Identification is necessarily impinging upon (a) the model's nonlinearities and (b) the observed heterogeneity contained in the X_{it} vector, with variables that could influence the notional distribution f while having no impact upon the rigidity probabilities p_r and p_n. In our case, these variables are assumed to be a few characteristics of workers (age, sex and qualification) and firms (size, occupational trend, sector and age) and broad regional dummies.

Given the potential weakness of this joint identification (and the paucity of workers' and firms' attributes available in our data) we also adopt an alternative strategy exploiting some external information. The basic idea stems from the fact that, taking into account the institutional features of the Italian labour market (with a widespread presence of national union contracts, within which the indexation clause operated), real rigidities in Italy should be related to industry level national contracts predetermining minimum wage rises. So, instead of assuming the presence of a real threshold equal (apart from some random variation) across all workers in a given year, we fix $\bar{r}_t$ at the level determined by the collective bargaining for each individual worker. Furthermore, we still maintain the possibility of *some* stochastic variability in the threshold, whose variance $\sigma_r^2(t)$ is estimated inside the model, but the role of such a component is (at least potentially) reduced. Such an alternative identification strategy – based on the external wage information available from national contracts – therefore provides a robustness check overcoming some of the weaknesses of the identification strategy. Unfortunately, the external information is available only for approximately half of the overall sample period and, even for those years, is not available for the whole sample, as we were unable to reconstruct detailed contractual information for some industries. So, while we believe such an alternative strategy is more precise and reliable, we start from the benchmark estimates in order to examine the changes over time of the rigidity measures in correspondence with institutional changes in the wage bargaining regime. Moreover, those benchmark estimates are more easily comparable to those presented in the companion papers in this same journal by Barwell and Schweitzer and Bauer *et al*.

We estimate rigidity measures for five sub-periods (by pooling yearly observations belonging to each sub-period). The rigidity parameters (p_r and p_n), as well as the parameters that govern the measurement error and the notional wage changes are assumed to be constant within each sub-period so as to acquire more precision in the estimates. Within each sub-period, year dummies, however, allow for some time variability in the notional wage changes and time variability is also allowed for in the real threshold. The sub-periods have been chosen so as to allow us to examine the trends in the rigidity estimates before and after the institutional reforms introduced by the 1992-1993 income policy agreements. More specifically, two sub-periods (1985-1988 and 1988-91) cover the regime in which the indexation clause known as the "*scala mobile"* was still in force. The sub-period from 1991 until 1994 is instead a phase of profound change determined by the abolition of the automatic indexation mechanism and by deliberate wage moderation. Finally the periods 1994-97 and 1997-99 are characterized by the full operation of the new institutional wage-setting regime as defined by the 1992-93 agreements.

3 DATA, DEFINITIONS AND SAMPLE SELECTION

The empirical analysis uses administrative data drawn from INPS (the Italian Institute for Social Security) and processed in a public-use file known as the Worker History Italian Panel (WHIP) by the researchers at the LABORatorio R. Revelli. WHIP is a longitudinal dataset reconstructing the working careers of a sample of workers in private firms. Each year from 1985 to 1999, about 140,000 job records are matched with data of the firm where the job is held, constituting a matched employer-employee database.[ii]

The analysis of wage rigidity is conducted on the basis of the changes in earnings of each individual worker between year t and the year $t+1$. We do not observe the number of hours worked by each employee. The "wage" of each worker in a given year is therefore obtained by dividing the total (gross) compensation received in the year – including firm and individual bonuses - by the number of days worked in the year.

The newly released version of data we use allows us to identify periods related to sick leave, maternity leave and temporary unemployment (*Cassa Integrazione Guadagni*), as well as to recognize the presence of arrears payments in a worker's compensation. All these events may cause spurious earnings changes. To reduce the distortions caused by them, the arrears payments are excluded from the calculation of actual yearly changes. We also exclude observations related to workers who, in year t or in year t-1, experienced a period of maternity leave, sick leave, or temporary unemployment (*Cassa Integrazione Guadagni*). For similar reasons, the sample has been restricted to full-time workers aged 20 to 64, who have been in the labour market for at least three months and with a minimum of 50 paid days in the year.

Yet it remains possible that some observed wage changes reflect changes in the labour input offered by the worker, rather than changes in his/her (unit) compensation. This is because the earnings that we observe are inclusive of overtime payments. We deal with this issue through the econometric model, which allows for statistical measurement error. In our case, given the administrative nature and the above-mentioned cleaning of the data, one may argue that measurement error mostly refers to changes in overtime.

The data cover the period 1985-1999, spanning before and after the 1992-1993 income policy agreements. In a difficult economic period (with a sharp currency devaluation and worrisome public debt figures), these agreements were instrumental to induce wage moderation and to curb inflationary pressures. While the *scala mobile* had ceased to operate since the beginning of 1992, unions finally accepted its complete dismantling when they signed a wage moderation agreement in July 1992. In July 1993 it was agreed to have the price inflation expected (and targeted) by the government as a common reference for national contracts (to be agreed upon every two years, against the 3 years of the previous set up), with firm level bargaining having to be geared to profit sharing considerations. Still the actual extent of innovation in wage behaviour is debated, as some observers have stressed the maintenance of a rather centralised system. So it is remarkable that the data allow us to provide some hints about the presence of structural breaks in the Italian wage determination process.

As said before, the WHIP data were integrated, for the second part of the period analysed, with the information on minimum wages dictated by national collective agreements for each sector and, within it, for each institutional job ladder. This external information is used to identify the "real-rigidity" threshold associated with each individual, freeing the parametric model from the burden of identifying it while also estimating the notional wage change distribution, the parameters of the measurement error process and the probability to fall into the nominal and real rigidity regimes. Furthermore, the rises dictated by the change in the minimum wages associated with national contracts vary a lot across individuals[iii], allowing more precise estimates of the relevant parameters and also of the wage sweep-ups for each individual worker.

We have been able to reconstruct contractual wage levels and dynamics for a total of 25 major nationwide contracts, for the years between 1990 and 1999. The 25 union contracts refer to the following sectors: the metal and mechanical engineering industries, trade, tourism, construction, textiles and clothing, the food industry, wood and furniture, and the services sector[iv]. For each job spell in the WHIP data, we can observe both the relevant national contract and the employee's position in the contractual ladder. Therefore, we can match with each worker the wage defined by the national contract. As a result, we are able to compute both the change in the total (observed) wage and the change attributable to the national contract. Notice that there is no mechanical relationship between the two variations, as, in the absence of rigidity, other wage components may contract, producing a total wage change that is lower than what is dictated by national contracts. Taking into account the Italian institutional set-up, it is the observed wage – whose *level* is always higher than the wage imposed by national collective bargaining (for brevity, "contractual wage") – that provides room for flexibility with respect to the exogenously determined national contracts. Broadly speaking in such an exercise we estimate how much the wage change attributable to national contracts (defined as the increase that would be observed if the only source of variation in the actual wage would have been the change imposed by the national contract, all other things being equal) affects the observed wage dynamics.

A drawback of such an exercise is that the sample for which this external information is available shrinks considerably (to about 51% of the observations available for the 1991-1999 period) for two reasons. First, this is because we lack information for the initial part of our sample (1985-1989) as well as for some (minor) national contracts. Second, because we can only consider contractual wage changes for those employees who, besides being a job stayer, also remain in the same job-ladder position between t and t+1. So, while the general analysis will be carried out considering those employees who have kept a job in the same firm between the two adjacent years in which the wage is being compared (job stayers), this additional exercise using contractual information is further restricted to those who did not change job-ladder position.

4. THE OBSERVED WAGE CHANGE DISTRIBUTION

A good start is simply to look at the observed yearly wage change distribution as depicted by Figure 2 in the Appendix. The main characteristics can be synthesized as follows.

1) The distributions are centred on a positive value very close to the current inflation rate. In the 1985-1991 and 1996-1999 periods there is a positive gap between the mean of the distribution and the inflation, while the gap is negative in the remaining years. This is consistent with the well-known aggregate real wage dynamics as the intermediate years were characterised by both high unemployment and institutional wage moderation. The attraction exerted by the current inflation rate is not by itself a sign of rigidity, because one would expect that, in the absence of monetary illusion, also notional wage changes be affected by the inflation rate (and by a positive drift term capturing aggregate productivity growth).

2) Even if positive variations prevail and the distribution is clearly asymmetric to the right, negative variations are not completely ruled out and DNWR is not absolute. Yet there is some evidence of an excessive concentration around zero wage growth. On average, wage changes exactly equal to zero represent 6% of the observations, with a weak increasing trend over time.

3) Such an "excessive" concentration around the zero, and also at a value close to the current inflation rate, does not seem to be the result of the presence of transaction costs that operate symmetrically on both sides of each of the two thresholds. In particular, there are no signs of a reduction in the probability mass to the right of zero.

Altogether, the visual inspection of the wage change distribution hints at the possible presence of downward rigidities, though the real rigidity is not easily discernible (possibly because it is unclear what is the relevant threshold) and nominal rigidity seems to have a limited impact. Measurement errors in the observed wages might be hiding part of the story and a more precise identification of the real threshold (within the model and/or using external information) might allow us to draw more precise inferences. So now we look at the econometric model.

5. BENCHMARK ESTIMATES OF DOWNWARD WAGE RIGIDITY

Table 1 displays in synthesis the main results of the benchmark estimates in which the real threshold is identified within the model and, apart from some randomness, varies only over time (and not across workers). These benchmark estimates cover the entire time span from 1985-86 to 1998-99, distinguishing among 5 sub-periods, within each of them a pooling of the yearly cross sections having been made.

In columns 2 and 3 the average actual wage-change and the average notional (estimated) wage-change are presented, respectively. The former is always larger than the latter because of the presence of DNWR and DRWR (and the related sweep-ups).

Overall, downward wage rigidities appear quite relevant, with a clear predominance of DRWR: on average, the probability of falling into the real rigidity regime (p_r) exceeds 50%, while the probability of falling into the nominal regime (p_n) exceeds 20%. Over time, there is clear evidence of a decrease in the incidence of DRWR – from around 65% in the first two sub-periods to around 45% in the last two sub-periods – only marginally compensated by an increase in the probability of DNWR – from 22% to 24% over the same time horizon.

In comparison with the results obtained by Devicienti (2002) using similar Italian data, the amount of DNWR we estimate is much lower. Besides the fact that the data he used were much less "cleaned-up," the difference is likely to reflect the fact that those estimates made no allowance for DRWR. So it is quite likely that those figures were somehow reflecting the presence of both types of rigidities (a similar claim is likely to hold with respect to the estimates for Germany presented by Knoppick and Beissenger, 2001). Our current estimates, with the predominance of DRWR, appear more in line with the standard characterisation of the Italian labour market. Furthermore, also the time evolution of both sources of downward rigidities evidenced by our estimates appears in line with what is known about the Italian labour market.

Apart from this plausibility check, some support for the results obtained here also comes from the ancillary estimates of the parameters governing the notional wage changes equation. The estimated β_t, shown in Table 2, while not the focus of our attention, appears economically plausible and statistically significant. Notice that the vector X_{it} includes year dummies within each sub-period, so as to capture the macroeconomic evolution. A quadratic polynomial for workers' age has been introduced in order to capture the wage dynamics related to tenure and experience effects: consistent with the typical concave age profile in wage *levels*, a convex profile in wage *variation* is shown by the estimated parameters. *Ceteris paribus*, women have received lower wage rises than men in all the sub-periods considered; however, this disadvantage decreases with age, as shown by the positive interaction coefficient age*women. Blue Collar workers (the excluded category) have received lower pay increases then white-collar workers and managers. In the South the wage increases were mostly lower than in the Centre of Italy, which in turn were lower than the ones recorded in the North. Younger firms offer higher notional increases than older firms during expansion periods (1988/91 and 1994/97). Notice that in a first attempt we also inserted the provincial unemployment rate in the X_{it} vector so as to capture the impact of the local labour market conditions. Consistent with the aggregate evidence of wage unresponsiveness to local labour market conditions (see for instance Casavola et al., 1995 and Lucifora and Origo, 1999), the estimated parameter turned out to be statistically insignificant and we dropped it.[v]

An encouraging signal also comes from measurement error. Indeed, the use of the latest version of WHIP, which has allowed us to use more "cleaned-up" data, produced a strong reduction in the variance of measurement error with respect to previous estimates we produced (see Devicienti *et al.*, 2003). Further, notice that to the extent that the measurement error captures time-varying overtime compensations, such an evolution is also implicitly picked up by the inclusion in the X_{it} vector of year dummies, sector dummies, geographical dummies and occupational dummies.[vi]

6. ALTERNATIVE ESTIMATES OF DOWNWARD WAGE RIGIDITY – THE USE OF CONTRACTUAL DATA

Table 3 presents an alternative set of estimates based upon the use of external information in order to identify the real threshold. In this alternative specification, the threshold $\bar{r}$ varies across individuals (and not only over time), being equal to $\bar{r}_i = (c_{it}-c_{it-1})/w_{it-1}$, where c_{it} represents the amount dictated by the national contract (and the contractual job ladder position) to which worker *i* belongs, while w_{it-1} is the same worker's observed wage in the previous year.

We have already explained the advantages created by an alternative specification. The use of external information may free the parametric model of the burden of identifying the real threshold while also estimating the notional wage change distribution, the parameters of the measurement error and the probability of falling into nominal and real rigidities. Furthermore, the real threshold so computed is very closely related to the institutional mechanism that in the Italian context is likely to be behind it[vii]. Finally, this approach allows for more variability in the real threshold across individuals.

The results reported in Table 3 are quite encouraging. The fact that the exercise was conducted upon a sub-sample, and according to a different (and more restrictive) definition of a job-stayer, does not permit us to examine very deeply the few differences in the estimates found with respect to the benchmark model of Table 1. It seems that in this alternative exercise DRWR are slightly more important, such a result possibly being consistent with the fact that the real threshold has been identified with greater precision. Along similar lines, it may be noticed that the randomness around the real threshold is somewhat lower in this case than in the previous estimates. This may signal that the assumption in the previous estimates of a common real threshold across individuals (apart from some randomness) was an undue simplification.

Nonetheless we prefer at this stage to interpret the alternative results as a broad confirmation of the benchmark estimates. This allows us to compare the benchmark results of section 5 to the results of the companion papers in this issue about Germany and Britain and, within Italy itself, to examine the time evolution of downward rigidities before and after the 1992-93 wage agreements.

It is, however, the results obtained through this alternative specification, which we deem more robust and reliable, and which we will use in the next section in order to explore some possible implications of downward rigidities.

7. LABOR REALLOCATION, UNEMPLOYMENT AND WAGE RIGIDITY

The impact of the estimated probability of falling into DRWR and DNWR depends, as already mentioned, upon the shape of the notional wage change distribution. An aggregate measure of such an impact is given by the amount of freezes and the size of the sweep-ups as already defined in section 2. According to Table 3, on average approximately 26% of the observations were bounded by DRWR (workers who belong to a real rigidity regime and whose wage is forced to align to a threshold). On the other hand, only 8% of the observations were actually affected by DNWR (workers whose negative notional wage changes are transformed into a zero wage change). Correspondingly, the wage sweep-up associated with DRWR was 1.45% per year, and the sweep-up associated with DNWR just 0.3% per year. While these results hint at the possible macroeconomic relevance of downward rigidities, some doubts may arise because of the instrumental nature and the first approximation of the notional wage-change estimates they are based upon. Moreover, simply measuring the amount of excessive aggregate wage pressure (due to downward rigidities) may be insufficient to derive the implications of those rigidities. So we prefer to resort to alternative ways in order to establish the economic significance of the measured downward rigidities.

In this section we present two different tests. First, we correlate the aggregate sweep-ups computed at provincial level to the provincial unemployment rate. More precisely, we verify whether the time evolution of provincial unemployment is related to the evolution over time of those sweep-ups, distinguishing between those due to DNWR and those due to DRWR. This is a simple and direct test of the overall macroeconomic implications of downward rigidities: do they matter in terms of creating “excess” unemployment?

The second test is more specific and attempts to understand whether firms whose wage adjustments are impeded by downward wage rigidity substitute them by adjusting more at the employment margin. More precisely, we explore the relationship between job and worker reallocation and downward wage rigidity taking advantage of the matched employer-employee nature of the WHIP data. The possible link between wage and employment adjustment is well known in theoretical terms and it has also been used as an argument for explaining the relative limited responsiveness of aggregate turnover measures to country differences in employment protection legislation regimes. More specifically, the argument is that European labour markets are often characterized by both greater quantity restrictions (e.g., restrictions on labour adjustment) and greater price restrictions (e.g., wage compression, downward wage rigidity) than US-style environments, the two features having offsetting effects on the observed labour market dynamics (see Bertola and Rogerson, 1997 and also Nickell, 1998). While such an argument has already been tested correlating job reallocation rates and within-firm wage dispersion measures at the firm level (e.g., Haltiwanger and Vodopivec, 2003), as far as we know our own is the first test directly conducted by looking at turnover and downward wage rigidity.

This has some advantages. The way we look at them, we believe that wage and employment adjustment margins are more directly and precisely compared. Indeed, the within-firm wage dispersion may depend on many technological factors (for instance, the range of skills a firm needs to cover) and not only on the presence of restrictions upon autonomous wage polices (for instance those posed by the existing wage bargaining system). Indeed, the notion of wage flexibility refers to the responsiveness of wages to changing circumstances more than to the average dimension of wage dispersion across individuals. So, the link between wage dispersion and turnover may well derive from other sources, as for instance the predominance of external versus internal markets, recruiting channels and the within-firm wage dispersion implications of internal market set-ups.

In any case, our data, while having the features of a matched employer-employee sample, are derived from a sample of employees. This means that we do not cover the whole workforce of the firms in our sample. For most of our firms we cannot observe a sufficiently large number of workers to reliably estimate the within-firm wage dispersion. On the contrary, we can estimate firms' measures of downward wage rigidity based on the estimates of the previous sections, as even one worker may already proxy for the average firm's measure of downward rigidity.

Notice that the turnover measures are instead computed by using the whole firm's workforce as a few firm characteristics are added to the WHIP data. Following the notation of Davis and Haltiwanger (see also Haltiwanger and Vodopivec, 2003), these additional variables allow us to define accession and separation rates:

$$ACC_{jt} = acc_{jt}/[0.5(E_{jt} + E_{jt-1})] ,$$

$$SEP_{jt} = sep_{jt}/[0.5(E_{jt} + E_{jt-1})].$$

where acc_{jt} is the number of worker accessions in firm j between t-1 and t and, similarly, sep_{jt} is the number of worker separations, while E_{jt} stands for firm j end-period employment at time t.[viii]

Using the firm's accession and separation rates the following three measures of turnover for firm j at time t can then be defined:

$$\text{worker reallocation} = ACC_{jt} + SEP_{jt} ,$$

$$\text{excess worker reallocation} = ACC_{jt} + SEP_{jt} - | ACC_{jt} - SEP_{jt} |,$$

$$\text{job reallocation} = | ACC_{jt} - SEP_{jt} |$$

Notice that we do not run regressions for the net change in employment, but only for its absolute value as a measure of job turnover. This is because we do not have adequate controls for labour demand shifts at the firm level (for the same reason no proper characterisation of firm demand shifts was available in the models estimated in the previous sections). What we actually look at is the possibility that a firm affected by downward rigidities is, *coeteris paribus,* induced to adjust at the employment margin. Actually we expect a stronger relationship in the case of the two worker turnover measures: for given jobs, firms are induced to substitute workers in order to circumvent downward rigidities. The relationship in the case of job reallocation, given the lack of adequate controls for labour demand shifts (as such affecting job turnover and its sign), is somewhat more ambiguous.

Table 4 presents our results for the three turnover measures. Downward rigidities are caught by the sweep-ups, separately those due to DNWR and those due to DRWR, computed for each worker and year and averaged at the firm level for each year. More specifically, we use the estimates presented in the previous section, i.e. those in which the real threshold varies across workers, so that there is a lot of cross-sectional as well as longitudinal variability in the estimated rigidities. Note that observations are weighted so as to account for the sample design of the WHIP data.[ix] We also include dummies for industry, year, provinces, firm size classes and firm age classes, as these are factors known to impinge upon turnover. To allow for a non-immediate reaction to downward rigidities we include both the current and the lagged sweep-up terms.

Column 2 of Table 4 shows that firms with higher downward rigidities tend to display higher job reallocation rates. The coefficients of the nominal sweep-ups are larger than that of real sweep-ups, being however less precisely estimated (actually the lagged nominal sweep-up is statistically insignificant). This does not account for the larger mean and standard deviation of the real sweep-ups (see Table 6). However, Table 6 and Table 4 together imply that the impact of a standard deviation increase in the nominal sweep-up would still be a bit larger than that of a standard deviation increase in the real nominal sweep-up.

As expected a stronger (and more precisely estimated) impact is shown for the two worker turnover measures (columns 3 and 4 of Table 4). Even more distinctively however it now appears that the coefficients of the real sweep-ups are smaller than those of the nominal sweep-ups. A one standard deviation increase in the nominal (real) sweep-up yields an increase in worker reallocation of about 0.09 (0.03) after two years, and of 0.08 (0.02) in the excess worker reallocation.

While we find a bit unclear the reasons underlying this predominance of the effects of DNWR, we read these results as a strong confirmation of the hypothesis that firms impeded to adjust wages tend to react adjusting quantities. Equipped with this evidence we now turn to the simple test of the impact upon aggregate unemployment performance.

The downward rigidity measures in this case are averaged for each province and year of our sample. We start (in Table 5) from a simple specification in which we regress the log of the provincial unemployment rate upon its own lag and the log of our (current and lagged) rigidity measures. The impact appears rather sizable, particularly that of the real sweep-ups.

We are well aware that there are a lot of missing factors that may explain geographical unemployment differentials, particularly in a country like Italy with stubbornly persistent regional differentials in unemployment. So in the next column we insert province dummies. The most important change in the estimate is the reduction in the size of the coefficient for the lagged unemployment rate, to some extent due to the fact that while in the previous specification it was somehow capturing permanent differences (so being upwardly biased) now it may be downwardly biased. Focusing upon the sweep-ups we are mostly interested in here, it appears that the overall picture is confirmed. While the coefficients somewhat change (the current nominal sweep-up disappears and the lagged real sweep-up has also a much reduced effect), the overall impact of our downward rigidities is still quite sizable. This is also confirmed in the last equation where we also insert the GDP growth rate in order to control for the aggregate business cycle evolution.

While the channel through which such a macroeconomic impact operates is still to be explored, as no direct jump may be made from the turnover effects previously shown at the firm level to aggregate unemployment at the local level, such evidence testifies to the macroeconomic significance of downward wage rigidities. Notice that in this case the impact of nominal and real sweep-ups do not differ very much from each other (and, if any, the sum of the current and lagged coefficients of the real sweep-up is larger than the corresponding sum for the nominal sweep-up).

8. CONCLUSIONS

This article has studied wage rigidity in Italy using a micro-econometric model, which allows us (i) to distinguish DNWR and DRWR, (ii) to take control for the observable determinants of the rigidity-free (notional) wage change distribution, and (iii) to control for the presence of measurement error in the data. Separately assessing nominal and real rigidities is important because the two may have different implications for the *policy maker* – for instance, while a positive inflation rate may overcome DNWR, DRWR cannot be relaxed by inflation, as more decentralized and flexible wage settings may be more important tools – and in order to take into account the many institutional factors shaping wage bargaining in a country like Italy. Our estimates are performed using administrative data from the Italian Institute for Social Security, recently released into a public-use data file known as *Worker History Italian Panel* (WHIP), which allowed us to rather effectively clean-up earnings data for irregular events in order to proxy for wages. The period of analysis is from 1985 to 1999, and thus permits us to examine the effect of important institutional changes in the Italian wage bargaining systems, more specifically the abolition of the indexation mechanism known as the *scala mobile*.

The results show the relevance of rigidities in the Italian labour market (more than two thirds of the observations turn out to be potentially influenced by rigidities), with a clear predominance of real ones. Over time, however, we confirm the impact of the institutional changes cited above, as DRWR diminished more than offsetting the small increase of DNWR.

On top of these benchmark estimates, for the later part of our sample period we were also able to test the robustness of our results using the external information drawn from national union contracts in order to identify more precisely (and in a much more detailed way) the real threshold attracting and constraining wage changes. We think of this second set of estimates as a robustness check for the benchmark econometric model used in this paper (and in the two companion papers of this issue on British and German data) that make use of non-testable functional form assumptions. Moreover, it corroborates the interpretation of the real rigidities as determined by the role of national union contracts, still rather pervasive in the Italian labour market. Finally, it allows us to construct very detailed measures of downward rigidities at the individual level, which we explore in order to identify the economic implications of those rigidities.

An additional contribution of this paper is the finding that downward rigidities at the firm level have a sizable impact upon worker turnover at the firm level. This appears as a strong, and novel, confirmation of the theoretical argument that firms substitute adjustments along the employment margin for (impeded) wage adjustments, replacing workers affected by downward rigidities. Furthermore, we also find a positive correlation between our (average) downward rigidity measures and the unemployment aggregate performance (at the provincial level), which is a strong signal of the macroeconomic relevance of downward rigidities and our measures of them.

Still there is a lot of analytical work to be done. As for measurement issues, we think that further consideration should be given to the econometric model, reducing its strong assumptions. Two non necessarily alternative routes seem worthwhile: a) extending the use of external information so as to reduce the identification burden born by functional form assumptions; b) evolving towards a non parametric approach specifically focused upon the extent of “excessive” attraction and “excess” asymmetry in the observed wage change distribution possibly caused by downward rigidities. As for the identification of the implications of downward rigidities, we believe that our results concerning turnover and unemployment performance are very preliminary steps, as many more dimensions should be taken into account, also by looking at the comparison of the wage dynamics of job movers and job stayers.

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APPENDIX:

A. TABLES AND FIGURES

Figure 2: Yearly wage changes distribution.

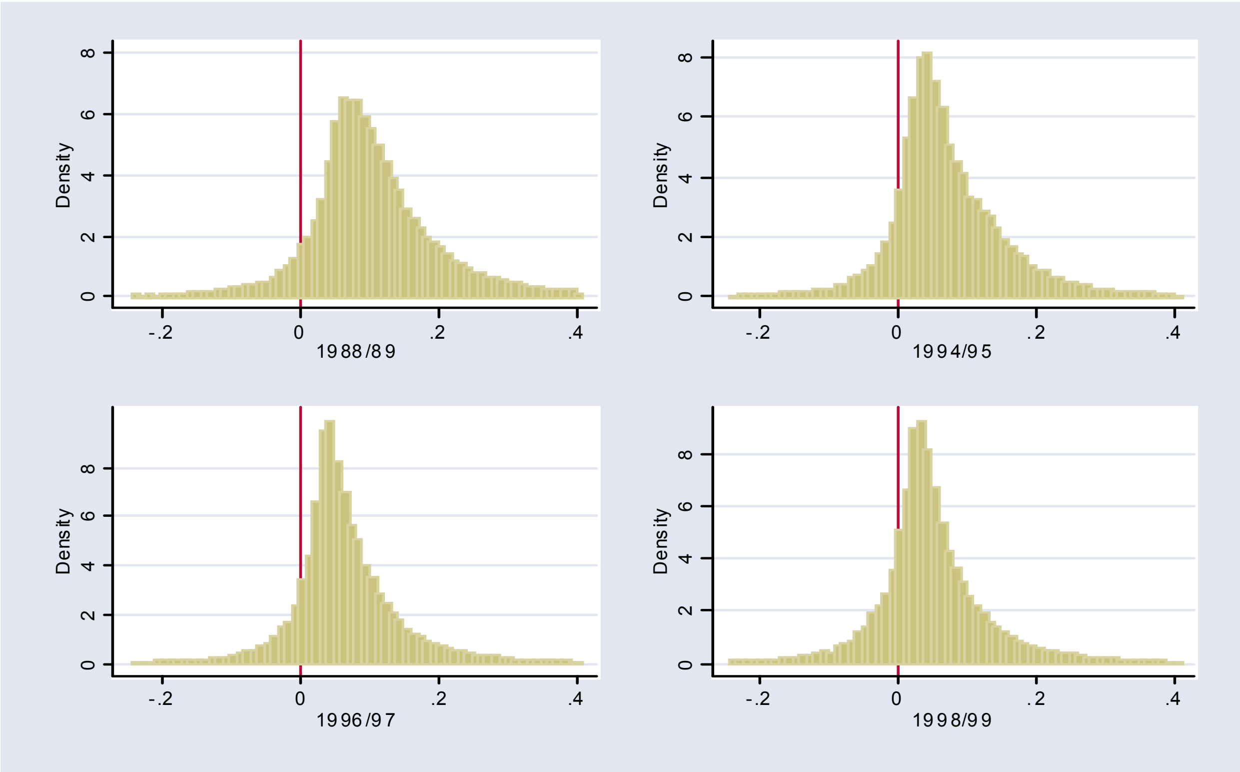

Table 1: Specification(A): ML benchmark estimates: r varies over time only being estimated within the model.

Period	Year	Mean observed wage change (Δw_t)	Mean notional wage change ($X_{it}'\beta_t$)	Mean real threshold (r)	Prob. real rigidity (p_r)	Prob. nominal rigidity (p_n)	Standard deviation of real threshold r (σ_r)	Standard deviation notional wage change (σ_e)	Standard deviation measurement error (σ_m)	% observation correctly measured (M)	% real wage freeze	% nominal wage freeze	Sweep-up real wage	Sweep-up nominal wage
85-88					0,58	0,21	0,03	0,10	0,20	0,91				
	85/86	0,072	0,025	0,061							0,37	0,09	0,035	0,007
	86/87	0,088	0,054	0,068							0,32	0,06	0,029	0,004
	87/88	0,073	0,032	0,060							0,35	0,08	0,034	0,006
88-91					0,69	0,23	0,03	0,08	0,18	0,89				
	88/89	0,105	0,078	0,059							0,29	0,04	0,019	0,002
	89/90	0,099	0,074	0,059							0,30	0,05	0,020	0,002
	90/91	0,114	0,095	0,069							0,27	0,03	0,017	0,001
91-94					0,49	0,24	0,02	0,08	0,19	0,92				
	91/92	0,075	0,056	0,039							0,20	0,06	0,012	0,003
	92/93	0,055	0,029	0,040							0,27	0,09	0,019	0,005
	93/94	0,052	0,023	0,036							0,27	0,09	0,019	0,005
94-97					0,52	0,22	0,02	0,08	0,19	0,93				
	94/95	0,068	0,048	0,034							0,23	0,06	0,013	0,003
	95/96	0,059	0,033	0,037							0,27	0,07	0,017	0,004
	96/97	0,065	0,040	0,038							0,26	0,07	0,015	0,003
97-99					0,39	0,26	0,01	0,07	0,18	0,91				
	97/98	0,053	0,031	0,031							0,20	0,09	0,010	0,004
	98/99	0,048	0,024	0,030							0,21	0,10	0,011	0,005

Note: sub-sample of employees that between t e t+1, are in the same firm (job stayers). The parameters p_r, p_n, σ_r, σ_e, M are invariant within each period, but varies across periods. The other parameters vary within and between periods. Number of observation (period): see table 3.

Table 2: Notional wage changes parameters – specification (A).

	1985-88		1988-91		1991-94		1994-97		1997-99	
	Coef.	**t**	**Coef.**	**t**	**Coef.**	**t**	**Coef.**	**t**	**Coef.**	**t**
Year 1	0.1189	18.28	0.2033	36.87	0.1799	34.70	0.1640	31.28	0.1616	26.12
Year 2	0.1473	22.66	0.1979	35.88	0.1523	29.70	0.1489	28.43	0.1548	24.95
Year 3	0.1251	19.27	0.2180	38.94	0.1474	28.64	0.1553	29.48	.	.
Age/1000	-3.8376	-14.15	-6.4166	-25.06	-5.6896	-24.91	-5.4888	-22.85	-6.1582	-21.73
Age^2/ 1000	0.0397	11.78	0.0661	21.07	0.0586	20.70	0.0553	18.57	0.0602	17.12
Energy, gas water	0.0063	2.88	0.0384	21.15	0.0075	4.43	0.0022	1.28	-0.0291	-14.28
Mining and chemical	0.0030	2.30	0.0121	9.95	0.0004	0.36	0.0041	3.57	0.0053	4.05
Metal products	0.0053	5.58	0.0089	10.25	-0.0104	-12.83	0.0075	9.74	0.0075	8.42
Construction	-0.0202	-13.35	0.0080	5.98	-0.0161	-12.89	-0.0167	-11.87	-0.0023	-1.44
Commerce	0.0021	1.96	0.0231	24.60	-0.0030	-3.69	0.0051	6.14	0.0240	25.46
Transport and Communication	0.0075	5.20	0.0093	7.47	0.0029	2.75	0.0038	3.07	-0.0063	-4.36
Nord East	-0.0070	-7.58	0.0000	-0.02	0.0004	0.55	0.0005	0.68	0.0012	1.39
Center	-0.0086	-8.75	-0.0012	-1.22	0.0007	0.81	-0.0048	-5.56	-0.0036	-3.35
South and islands	-0.0162	-11.27	-0.0051	-3.50	-0.0024	-1.82	-0.0121	-10.28	-0.0072	-5.02
Firm age	0.0444	4.95	-0.0387	-5.27	0.0170	2.40	-0.0247	-3.55	0.0242	2.72
Firm age ^{2}	-0.0351	-1.92	0.1192	7.97	-0.1467	-8.85	0.0867	5.79	-0.0729	-3.67
Female	-0.0275	-8.76	-0.0159	-5.94	-0.0211	-8.63	-0.0198	-7.78	-0.0244	-8.23
Female*age	0.2240	2.62	0.1423	1.91	0.3601	5.38	0.1463	2.13	0.4467	5.67
Manager	0.1014	38.90	0.0421	18.22	0.0532	27.98	0.0351	23.34	0.0429	27.93
White-collar	0.0411	52.72	0.0237	33.21	0.0203	33.21	0.0236	37.35	0.0234	32.33
σ_e	0.1034	153.26	0.0841	90.27	0.0766	154.91	0.0775	152.43	0.0685	124.86
N. observations	196,096		149,642		140,413		131,200		82,237	

Table 3: Specification (B): r varies across individuals as well over time and is identified according to national collective bargaining.

Period	Year	Mean observed wage change (Δw_t)	Mean notional wage change ($X_{it}'\beta_t$)	Mean real threshold (r)	Prob. real rigidity (p_r)	Prob. nominal rigidity (p_n)	Standard deviation of real threshold r (σ_r)	Standard deviation notional wage change (σ_e)	Standard deviation measurement error (σ_m)	% observation correctly measured (M)	% real wage freeze	% nominal wage freeze	Sweep-up real wage	Sweep-up nominal wage
91-94					0,47	0,29	0,01	0,05	0,15	0,88				
	91/92	0,075	0,038	0,054							0,236	0,088	0,013	0,003
	92/93	0,055	0,034	0,043							0,211	0,092	0,010	0,003
	93/94	0,052	0,038	0,030							0,196	0,106	0,010	0,004
94-97					0,59	0,22	0,01	0,07	0,17	0,92				
	94/95	0,068	0,031	0,038							0,318	0,071	0,020	0,003
	95/96	0,059	0,033	0,034							0,299	0,069	0,018	0,003
	96/97	0,065	0,037	0,038							0,296	0,065	0,017	0,003
97-99					0,43	0,27	0,01	0,06	0,16	0,90				
	97/98	0,053	0,033	0,033							0,296	0,065	0,017	0,003
	98/99	0,048	0,026	0,022							0,218	0,080	0,011	0,003

Note: sub-sample of employees that between t e t+1, are in the same firm (job stayers). The parameters p_r, p_n, σ_r, σ_e and M are invariant within each period, but vary across periods. The other parameters vary within and between periods.
Number of observations (period): 78,681 (1991-94), 62,122 (1994-97), 41,303 (1997-99).

Table 4: The relationship between firm wage rigidity and reallocation.

Explanatory variable	Dependent variable		
	Firm job reallocation	Firm worker reallocation	Firm excess worker reallocation
Nominal Sweep-up	5.2563	21.7082	19.7811
	(1.2063)	(5.3774)	(4.9745)
Nominal sweep-up (one year lag)	0.7137	40.7605	39.0316
	(1.2672)	(5.6464)	(5.2299)
Real Sweep	0.3058	0.4548	0.1920
	(0.0445)	(0.1986)	(0.1837)
Real sweep (one year lag)	0.0954	0.4820	0.3797
	(0.0359)	(0.1602)	(0.1500)
Controls	Dummies for: 8 industry, 7 year, 108 provinces, 6 firm size categories, 5 firm age categories.	Dummies for: 8 industry, 7 year, 108 provinces, 6 firm size categories, 5 firm age categories.	Dummies for: 8 industry, 7 year, 108 provinces, 6 firm size categories, 5 firm age categories.
R-squared	0.030	0.031	0.055
No. observations	67624	67624	67624

Notes: Standard errors in parenthesis. Weighted regressions (see footnote ix).

Table 5: The relationship between firm wage rigidity and local unemployment.

Explanatory variable	Dependent variable: local unemployment rate (log)		
(log) nominal sweep-up	0.0632	0.0018	0.0087
	(0.0309)	(0.0295)	(0.0282)
(log) nominal sweep-up (1 year lag)	0.0193	0.0876	0.0630
	(0.0185)	(0.0167)	(0.0162)
(log) real sweep-up	0.0632	0.0880	0.1271
	(0.0309)	(0.0144)	(0.0145)
(log) real sweep-up (1 year lag)	0.1327	0.0627	0.0769
	(0.0168)	(0.0170)	(0.0163)
(log) local unemployment (1 year lag)	0.9526	0.3059	0.3403
	(0.0098)	(0.0366)	(0.0351)
Controls	No	108 province dummies	108 province dummies, aggregate GDP growth (current and lagged).
Adjusted R-squared	0.933	0.951	0.955
No. observations	775	775	775

Notes: Standard errors in parenthesis. Regressions are employment-weighted.

Table 6: Summary statistics for reallocation and wage rigidity regressions.

Variable	Mean	Std. Dev.
Job reallocation	0.1812	0.3195
Gross worker realloc.	0.9712	1.2981
Excess worker realloc.	0.7929	1.2198
Real sweep	0.0176	0.0282
Nominal sweep	0.0029	0.0014

Notes: weighted statistics.

B. THE LIKELIHOOD FUNCTION.

In this appendix we provide the elements for the derivation of the likelihood function of the econometric model described in section 2. See Dickens and Goette (2001), Barwell and Schweitzer (2003) and Bauer et al. (2003) for further details.

We assume that the notional wage variation of individual i between t-1 and t is given by (we suppress the temporal index for simplicity):

$$d^n_i = X_i'\beta + e_i \qquad \text{with } e_i \sim N(0, \sigma_e^2). \tag{A.1}$$

The probabilities that individual i falls in the real rigidity regime (**R**), nominal rigidity regime (**N**) and unconstrained regime (**U**) are assumed to be constant, as follows:

$$\begin{aligned} &\Pr(i=\mathbf{R} \mid \chi_i) = p_r \\ &\Pr(i=\mathbf{N} \mid \chi_i) = p_n \\ &\Pr(i=\mathbf{U} \mid \chi_i) = 1 - p_r - p_n. \end{aligned} \tag{A.2}$$

The individual's actual wage variation, a_i, is then equal to:

$$a_i = \begin{cases} r_i & if \quad i = R, \quad and \quad d_i^n \le r_i \\ 0 & if \quad i = N, \quad and \quad d_i^n \le 0 \\ d_i^n & \qquad otherwise \end{cases} \tag{A.3}$$

where r_i is the real-rigidity threshold given by:

$$r_i = \bar{r} + e_{ri} \qquad \bar{r} \text{ constant, } e_{ri} \sim N(0, \sigma_r^2) \tag{A.4}$$

Due to the presence of measurement errors, a_i is not directly observed. The following cases are considered, which we represent with the indicator H: (1) wages are measured without errors (case that we indicate with H=1); (2) there are measurement errors but only in one period, t or t+1 (case that we denote indicate H=2); (3) measurement errors occur in both periods (H=3). For simplicity, we assume that the probability m of an individual being affect by measurement error is the same in each year, and is not correlated over time. Measurement error is given by $e_{mi} \sim N(0, \sigma_r^2)$, with identical distribution in t and t-1. Making use of the variable η_i to represent the three cases, we have:

$$\eta_i = \begin{cases} 0 & with \quad \Pr(H=1) = (1-m)^2 \\ e_{mi} & with \quad \Pr(H=2) = 2m(1-m) \\ 2e_{mi} & with \quad \Pr(H=3) = m^2 \end{cases} \tag{A.5}$$

As a consequence, the observed wage change, d_i, is given by:

$$d_i = \begin{cases} r_i + \eta_i & if \quad i = R, \quad and \quad d_i^n \le r_i \\ \eta_i & if \quad i = N, \quad and \quad d_i^n \le 0 \\ d_i^n + \eta_i & \qquad otherwise \end{cases} \tag{A.6}$$

The model's likelihood function refers to 5 categories of observations: (i) individuals that fall in the unconstrained regime (**U**); (ii) individuals that fall in the nominal rigidity regime (**N**) whose wage change is forced to align to zero threshold; (iii) individuals that fall in the nominal rigidity regime (**N**) whose wage change is not constrained; (iv) individuals that fall in the real rigidity regime (**R**) whose wage growth is forced to align to a threshold r_i; (v) individuals that fall in the real rigidity regime (**R**) whose wage growth is unconstrained.

Let $D=\{d_i\}$ be the observed nominal wage change. Given a vector of explicative variables x_i the likelihood function can be written as follows:

$$\begin{aligned}
\mathrm{L(D|\Theta)} = \prod_{i}^{N} & \Pr(i \in U \mid x_i) \times L(di \mid i \in U, x_i) \\
& + \Pr(i \in N \mid x_i) I\{a_i = 0\} \Pr(a_i = 0 \mid i \in N, x_i) L(di \mid i \in N, x_i) \\
& + \Pr(i \in N \mid x_i) I\{a_i > 0\} \Pr(a_i > 0 \mid i \in N, x_i) L(d_i \mid i \in N, x_i) \\
& + \Pr(i \in R \mid x_i) I\{a_i = r_i\} \Pr(a_i = r_i \mid i \in R, x_i) L(d_i \mid i \in R, x_i) \\
& + \Pr(i \in R \mid x_i) I\{a_i > r_i\} \Pr(a_i > r_i \mid i \in R, x_i) L(d_i \mid i \in R, x_i)
\end{aligned} \qquad [A.7]$$

where $I\{.\}$ is an indicator function equal to 1 when condition in parenthesis is true, equal to zero otherwise, and Θ is the set of parameters to estimate. The contribution to the likelihood function of each observation is given by three factors: the probability that each individual falls into a given regime, the probability that the regime is binding, the likelihood of the observation conditional on the regime and whether the regime is binding or not. The estimates of Θ are obtained by maximizing the likelihood function in [A.7], given the assumption of normality distribution of the error terms and r_i. Therefore, $\Theta = (\beta, r_i, \sigma_e, \sigma_r, m, \sigma_m)$.

So, for those observations that fall in the unconstrained regime, the contribution to the maximum likelihood function conditional on being in that regime can be written as follows:

$$L(di \mid i \in U, x_i) = \frac{1}{\sqrt{\sigma_e^2 + \sigma_\eta^2}} \times \phi\left(\frac{d_i - x_i\beta}{\sqrt{\sigma_e^2 + \sigma_\eta^2}}\right) \qquad [A.8]$$

where Φ (.) is the density function of the standard normal variable.

For the observations that fall into the regime **N**, the probability that this regime is binding depends upon the distribution of the notional error term e_i and the composite measurement error η_i. Therefore the contribution of these observations to the likelihood function is given by:

$$\Pr(a_i = 0 \mid i \in N, x_i) L(di \mid i \in N, x_i) = \left(\Phi\left(\frac{-x_i\beta}{\sigma_e}\right)\right) \times \frac{1}{\sigma_{\eta_i}} \Phi\left(\frac{d_i}{\sigma_\eta}\right) \qquad [A.9]$$

where Φ(.) refers to the cumulative distribution of the standard normal variable. It should be noticed that the error terms of the two expressions in [A.9] – expressions that define, respectively, the probability that a_i is constrained and the density of d_i – are independent. This does not hold anymore in case an observation falls into the regime **N**, but the regime is not actually binding: now the error tem e_i is present in both the expressions in [A.9]. The contribution to the likelihood function of these individuals is more complicated and can be generically written as follows:

$$\Pr(a_i > 0 \mid i \in N, x_i) L(d_i \mid i \in N, x_i) = \Pr\{e_i > -x_i\beta\} \times f_{e+\eta}(d_i - x_i\beta \mid e_i > -x_i\beta) \qquad [A.10]$$

which can be shown to be equal to:

$$\frac{\sqrt{\sigma^2{}_\eta + \sigma_e^2}}{\sigma_e \sigma_\eta} \phi\left(\frac{d_i - x_i\beta}{\sqrt{\sigma_e^2 + \sigma_\eta^2}}\right)\left(1 - \Phi\left(-x_i\beta\left(\frac{\sqrt{\sigma^2{}_\eta + \sigma_e^2}}{\sigma_e \sigma_\eta}\right) - \frac{\sigma_e \sigma_\eta (d_i - x_i\beta)}{\sigma^2{}_\eta + \sigma_e^2}\right)\right) \qquad [A.11]$$

Finally, the contribution to the likelihood function of the observations that fall into the regime **R** is similar to that described in [A.10-11], but in this case the relevant threshold is given by the expression for r in [A.4]. So for the observations actually constrained by the regime **R**, the contribution to the likelihood function can be written as follows:

$\Pr(a_i = r_i \mid i \in R, x_i) L(d_i \mid i \in R, x_i)=$

$$\left(\Phi\left(\left(r_i - x_i\beta + \frac{\sigma^2{}_r}{\sigma^2{}_\eta + \sigma^2{}_r}\right) \times \left(\frac{r_i - d_i}{\sqrt{\sigma^2{}_e + \frac{\sigma^2{}_\eta \sigma^2{}_r}{\sigma^2{}_\eta + \sigma^2{}_r}}}\right)\right)\right) \times \frac{1}{\sqrt{\sigma^2{}_\eta + \sigma^2{}_r}} \phi\left(\frac{d_i - r_i}{\sqrt{\sigma^2{}_\eta + \sigma^2{}_r}}\right) \qquad [A.12]$$

while for the unconstrained observations the contribution to the likelihood function is given by:

$\Pr(a_i > r_i \mid i \in R, x_i) L(d_i \mid i \in R, x_i) =$

$$\left(1 - \Phi\left(r - x_i\beta + \frac{\sigma^2{}_e}{\sigma^2{}_\eta + \sigma^2{}_e} \times \left(\frac{d_i - x_i\beta}{\sqrt{\sigma_r^2 + \frac{\sigma^2{}_\eta \sigma^2{}_e}{\sigma^2{}_\eta + \sigma^2{}_e}}}\right)\right)\right) \times \frac{1}{\sqrt{\sigma^2{}_\eta + \sigma^2{}_e}} \phi\left(\frac{d_i - x_i\beta}{\sqrt{\sigma^2{}_\eta + \sigma^2{}_e}}\right) \qquad [A.13]$$

Notes:

[i] In the case of Italy, several empirical macro-economic studies analysed the wage determination processes in the light of the institutional reforms of the early nineties: see among others, Fabiani *et al*. (1997 and 2001), Casadio *et al*. (the 2004) and De Stefanis *et al*. (2004).

[ii] See Contini (2002) for a detailed analysis of previous versions of the INPS source data, and the web site www.labor-torino.it for documentation on WHIP data, their availability and updating.

[iii] Notice that we construct the percentage change associated with the industry-specific contract and the job ladder that pertains to the individual by dividing the change in those minima, varying among contracts and ranks of the job ladder, and the total (observed) earnings that individual had the previous year. This implies that the real threshold is *ceteris paribus* lower for an individual who in the starting year had an observed wage much larger than the contractual minimum. This captures well the fact that, institutionally, wage flexibility may arise from wage components bargained for at the individual level, while downgrading a worker (within the contractual job ladder) is legally impossible (apart from very special cases).

[iv] The available contractual information includes: "minimum wages," cost of living allowances (the *scala mobile*, frozen since 1992) and other elements (special bonuses), each component differentiated according to the institutional job ladder specifically defined by each national contract.

[v] In principle such an issue deserves further investigation, as one would expect notional wage changes to react to local labour market conditions, downward rigidities impairing such an effect insofar as actual changes are concerned. At this stage, in order to avoid arbitrary assumptions about the link between notional wage changes and local unemployment we dropped such a term, whose inclusion or exclusion (given the statistical insignificance of the estimated parameters) would not change the results concerning downward rigidities.

[vi] In previous experiments (not shown here) we also introduced dummies for the firm's dimension, and whether the firm was either growing or shrinking. These variables captured some variability. However, they were not available in the WHIP data for the years 1985, 1986 and 1999 and in order to maintain an homogeneous specification, we excluded them here. Notice that their inclusion, while statistically valid, would not change in any substantial way the estimates of downward rigidities.

[vii] Notice also that when an automatic wage indexation mechanism was present, such an indexation clause did not affect equally all workers and, on average, the degree of coverage of nominal wages with respect to prices was (in the second half of the '80s) around 60%, not 100% that would be implied by simply equating the real threshold to price inflation. For evidence about the scala mobile mechanism see the Bank of Italy Annual Report issued during that specific period.

[viii] It should be noticed that we do not observe actual separations and accessions but only net employment changes at monthly frequencies. So we measure separations (accessions) by considering the net monthly changes that are negative (positive), assuming that there are no contemporaneous (i.e., in the same month) accessions and separations. In other words, we are assuming that net employment changes computed at monthly frequencies identify worker flows while net changes computed at yearly frequency identify job flows.

[ix] In the WHIP data, workers born on April 10th, May 10th, June 10th and July 10th are sampled from the universe of dependent workers, with a sampling probability of roughly 4/365 (see Contini 2002, for details). Workers are then matched to their firms. This entails that small firms have less probability of inclusion than large firms and that we need to weigh our firm-level records in the regressions. Note that the probability of inclusion in the WHIP data is given by a binomial random variable, B(k, s, n), indicating the probability of at least k successes in n trials, when the probability of success is s. In our case, n is the size of the firm, s is 4/365 and k is equal to 1. Our weights are the inverse of these probabilities.