

The Interaction of Labor Markets and Inflation: Analysis of Micro Data from the International Wage Flexibility Project

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The adoption of explicit or implicit inflation targets by many central banks, and the low stable rates of inflation that have ensued, raise the question of how inflation affects market efficiency. The goal of the International Wage Flexibility Project (IWFP)—a consortium of over forty researchers with access to micro level earnings data for 16 countries—is to provide microeconomic evidence on the costs and benefits of inflation in the labor market. We study three market imperfections that cause the rate of inflation to affect labor market efficiency. They are:

- The presence of substantial resistance to *nominal* wage cuts in a low inflation environment can slow the adjustment of relative wages to labor market shocks and thus result in a misallocation of resources (Keynes 1936; Slichter and Luedicke 1957; Tobin 1972; Akerlof, Dickens and Perry 1996). This distortion would not occur in a higher inflation environment.
- Alternatively, to the extent that the downward rigidity prevents real wage cuts, rather than nominal wage cuts, inflation will not improve efficiency. In this case only increases in real wages resulting from productivity growth can reduce the misallocation of resources caused by a real wage floor.
- Higher inflation is associated with more frequent wage and price changes, higher search costs for goods or jobs, and greater uncertainty about the future path of wages and prices (Sheshinski and Weiss 1977; Friedman 1977; Vining and Elwertowski 1976). These effects can lead to errors and adjustment lags in wage setting and diminish the information value of observed wages (Groshen and Schweitzer 1999, 2000). Thus, increased inflation may also cause a misallocation of resources.

In short, inflation can “grease” the wheels of economic adjustment in the labor market by relieving the constraint imposed by downward *nominal* wage rigidity, but not if there is also substantial downward *real* wage rigidity. At the same time, inflation can throw “sand” in the wheels of economic adjustment by degrading the value of price signals. Knowledge of which of these imperfections dominates at different levels of inflation, and under different institutional regimes can be valuable for choosing an

inflation target and for learning more about the economic environment in which monetary policy is conducted.

To investigate these imperfections, the IWFP convened thirteen country teams plus a central analysis team that devised a common protocol and analyzed the results jointly. Country teams have access to European or U.S. data with large samples of longitudinal data on individuals' wage or earnings for at least eight years. The countries include most of Europe (large and small, north and south, euro and non-euro areas).

The paper proceeds as follows. First, we briefly review the empirical literature in order to motivate the method we use to distinguish these three labor market imperfections. Next, we describe our data and empirical approach—which applies a common protocol to 31 distinct panels of workers' wage changes. Then we establish that wage changes show substantial dispersion that rises with the rate of wage inflation, as predicted by grease and sand effects. To identify the three imperfections under consideration, we examine histograms of wage changes (that are corrected for measurement errors) for the particular asymmetries and spikes that are characteristic of downward real and nominal wage rigidity. This process yields estimates of the prevalence of real and nominal wage rigidity for each data set and year that we then analyze for insight into the causes and consequences of wage rigidities. Finally, we examine the linkage between estimates of true wage change dispersion and inflation for evidence of sand effects.

I. Previous studies and the IWFP approach

The IWFP unites and advances three largely distinct strands of research that relate labor market imperfections to inflation and economic efficiency. Of these, only downward nominal wage rigidity has been extensively studied before.

a. Downward nominal wage rigidity, or grease effects

Taken at face value, the many studies of U.S. and Canadian wages show conflicting evidence of the extent of downward nominal wage rigidity (see reviews in Camba-Mendez, García and Rodríguez Palenzuela 2003 and Holden 2004). Yet, on closer examination, the subset of studies (including Altonji and Devereux 2000; Akerlof, Dickens and Perry 1996 and Gottschalk forthcoming) that focus on the base wage² and take proper account of reporting errors are much more consistent. Reporting error is likely to bias micro data measures of rigidity because it causes spurious variability in wage changes and false wage "cuts".³ Similarly, studies based on administrative data sets often include fluctuations in other parts of compensation (for example, overtime work paid at bonus rates) that can disguise the rigidity of the base wage. The papers that correct for these influences find a clear pattern of substantial resistance to nominal wage

² Some have argued that the more comprehensive measures of compensation are appropriate for studying rigidity. We believe a focus on base wages is appropriate because if base wages are very rigid (in either real and nominal terms), then circumventing those effects by varying other types of compensation is likely to be costly. Furthermore, many changes in other aspects of compensation are not voluntary, such as when a hike in insurance premiums raises employer costs for the same package of benefits. Such changes may occur, but they may not mean that employers have the ability to make such changes to what employees receive. Finally, Lebow *et al.*'s (2003) study of the U.S. Employment Cost Index, finds no evidence that firms circumvent rigidity in base wages by changing other types of compensation.

³ A validation study of the Current Population Survey, which uses questions very similar to those in the surveys analyzed for the IWFP, find that only about 55% of people correctly report their wages or earnings (Bound and Krueger, 1991).

cuts in the U.S.⁴ Specifically, they find a large number of people receiving no wage change in any particular year and very few wage cuts.

International comparisons offer a key route for investigation of the relative importance and causes and consequences of rigidity. However, differences in data and methods among independent micro studies can confound attempts to compare rigidities among countries. Three studies using high quality British data show considerably less resistance to nominal wage cuts in the U.K. than in the U.S. or Canada (Barwell and Schweitzer 2005, Nickel and Quintini 2003 and Smith 2000), while Fehr and Goette (2005) use error correction techniques on administrative and survey data, along with personnel data, and find considerable downward nominal wage rigidity in Switzerland. Two studies of cross-country variation during the mid 1990s using the European Community Household Panel (Dessy 2005; and Knoppik and Beissinger 2005) find that nominal rigidity varies considerably across countries. Using industry level data for 19 OECD countries, Holden and Wulfsberg (2005) also find significant downward nominal wage rigidity that is more prevalent when unemployment is low, union density is high, and employment protection is strict.

In a study using U.S. data, Akerlof, Dickens and Perry (1996) assess the impact of downward nominal wage rigidity on unemployment by estimating Phillips curves that include a term representing the wage effects of rigidity. The inclusion of the term reveals evidence of a long run trade-off between inflation and unemployment at very low rates of inflation (less than 3%). They find that only during the Great Depression was inflation

⁴ Other consistent support is found in studies that use interviews of market participants (see Kaufman 1984; Blinder and Choi 1990; and Bewley 1999) or analyze personnel files (see Altonji and Deveraux 2000 and Wilson 1999).

sufficiently low in the U.S. for downward nominal wage rigidity to increase unemployment by more than a percentage point. By contrast, Fortin (1996), Djoudad and Sargent (1997), and Dickens (2001) find large effects in Canada in the 1990s when inflation was low for an extended period. However, application of this method to several European countries (Dickens 2001) does not provide consistent evidence of a long-run trade-off between inflation and unemployment as would be expected if downward nominal rigidity was important in wage setting. One explanation for this result could be the presence of real rigidity in these countries.

b. Downward real wage rigidity

There has been much less study of downward real wage rigidity. Helping to fill this void, several recent micro data studies that use a methodology developed for an earlier phase of the IWFP find varying degrees of downward real rigidity in the U.K., Finland, Italy and other European countries.⁵

In addition, there have been several attempts to use macro data to assess the extent to which real and nominal wage changes are insensitive to economic circumstances (for example, see Alogoskoufis and Manning (1988) and Layard, Nickell, and Jackman (1991)). These studies, which measure concepts of rigidity different from ours, are less relevant to the question of what level of inflation to target.

⁵ See Barwell and Schweitzer (2004), Bauer, Bonin, and Sunde (2003), Böckerman, Laaksonen and Vainiomäki (2003), Dessy (2005), and Devicienti, Maida and Sestito (2005). The methodology used in these studies is not used here because some identifying assumptions proved invalid in some of the countries.

c. Sand effects

Few studies have examined the degree to which increased inflation distorts price signals in labor markets and leads to a misallocation of resources. Instead, studies have emphasized such problems in product markets. In the only exception that we know of, Groshen and Schweitzer (1999 and 2000) note that both sand and grease effects imply that the dispersion of wage changes should increase with inflation. Increasing inflation should reduce the concentration of wage changes at zero that is caused by downward nominal wage rigidity, while more errors in wage setting will raise the dispersion of wage changes regardless of the effects of rigidity. Groshen and Schweitzer find increasing variance of wage changes with increased inflation and implement a method for disentangling the roles of grease and sand effects in explaining that relationship.

d. The IWFP approach

In an early phase of the IWFP we attempted to replicate the Groshen and Schweitzer method across countries, but found that the identifying assumptions the authors used were not appropriate for European wage setting institutions. Thus, we develop a new approach that is based on the different ways that these three labor market imperfections are expected to affect the dispersion and symmetry of wage changes and takes careful account of the biases introduced by measurement error.

The features we test for are summarized Table 1. Each row lists a market imperfection, describes how it interacts with inflation, and lists how the presence of the imperfection is expected to affect the distribution of wage changes. The first key prediction (column 3) is that grease and sand effects both imply that the dispersion of wage changes rises with inflation (under the grease effect, firms are less constrained;

under the sand effect, they make more mistakes or have lagged adjustments). By contrast, if *real* wages are rigid downwards, higher inflation simply raises the mean wage change without affecting the dispersion of wages. Second, with regard to symmetry and spikes (column 4 of Table 1), both real and nominal downward rigidity should lead to high concentrations of workers with real or nominal wage freezes (that is, with wage changes equal to the rate of inflation or to zero, respectively) and correspondingly fewer workers with increases below those rates. By contrast, sand effects cause errors or lags in wage setting that will increase the variance of the observed wage change distribution, but there is no reason to believe that the errors or lags will affect the distribution's symmetry.

II. Empirical approach and data

a. Empirical approach

The empirical approach used by the IWFP (called “distributed micro analysis”) has country teams apply common analytical protocols to data sets for their country using their expertise with the relevant data, history and institutions, while observing the confidentiality restrictions under which they are granted access to the information. Statistics generated by the protocols for each data set are used by country teams as the basis of their analysis, and also collected into a data set for combined analysis.

This strategy has several virtues. First, heterogeneous country environments provide important variation for analysis of the impact of policy and institutions. Second, the application of common, flexible protocols allows for better comparison among countries than is typically available for meta-analysis of micro results. Finally, the use of

multiple data sources provides insight into the impact of data characteristics on estimated outcomes.

b. Data

The first goal of the IWFPP was to examine the relative importance of sand and grease effects across a number of European countries and the U.S., so availability of data appropriate to this task was the main determinant of participation in the study. The countries included, and the broad characteristics of the data sets used, are described in Table 2. To augment the data sets analyzed by country teams, the central analysis team obtained access to the European Community Household Panel (ECHP), which adds another 12 data sets and 3 additional countries.⁶

The 31 data sets analyzed for the project are diverse with respect to source, coverage, years, and definitions of variables of interest (see Table 2). The many differences among these types of data add richness as well as potentially confounding factors to the analysis below.

The three main sources of data are employment registers, household surveys and employer surveys. An employment register (maintained by a government for the administration of taxes and/or benefits) covers all workers in a specified universe and has minimal reporting error. Some country teams work with random samples drawn from the registers, while others analyze the entire census. Household surveys sample from the universe of all workers, but rely on respondent recall, and so they are subject to both

⁶ The ECHP is a longitudinal database drawn from a survey of households in 15 EU countries; it includes detailed information about individual characteristics, including earnings (for more information on the first three waves of ECHP data see Peracchi 2002). Wages are reported as net earnings (including overtime pay and bonuses) in the previous month (except for France and Finland, where net earnings are

sampling and reporting error. By contrast, employer salary surveys typically cover all workers in the occupations and firms in their purview and draw their data from payroll records, but vary considerably in how many occupations or firms they cover. The employer surveys in the IWFP are particularly comprehensive because they are conducted by national employer associations and are used extensively for policy and managerial purposes.

Data sets also vary in terms of the compensation measure available. Some data sets have base wages. However, most wage information in the IWFP is based on monthly or annual labor earnings (that is, including base wage, overtime pay, and bonuses). In those cases, we use a proxy for base wage: earnings divided by the best available measure of hours worked. Hours worked information is available for most of the data sources.

Samples are restricted to job stayers in order to concentrate on rigidity for ongoing employment relationships. In addition, large outliers in wage changes⁷ are excluded as they likely reflect wage reporting errors or unidentified job changes.

The time periods covered by the different data sets vary, with some starting in the early 1970s and others running through the beginnings of the 2000s. In total, there are 360 data set-year observations, including observations from multiple data sets for Austria, Belgium, Denmark, Finland, France, Germany, Italy, Norway, Portugal, Sweden, Switzerland, and the U.K.

derived from reported gross data using a net/gross ratio). We exclude the series for Spain, Luxembourg and Sweden due to data limitations.

III. The dispersion of wage changes

This section examines the dispersion of wage changes in the IWFP data sets for features consistent with the three interactions with inflation. Figure 1a shows a scatter plot of the standard deviation of log (percentage) wage changes against the rate of wage inflation for each year for each data set in our study. Both the magnitude of these standard deviations and the range are remarkable. To some extent, the magnitude and range are artifacts of a high average level of measurement error and of variation across data sets in the extent of error. But as Figure 1b shows, even when the data set mean is subtracted from the standard deviations (which should remove persistent differences due to data set measurement error characteristics), there is still substantial variation.

Further, the linear relationships plotted in the two graphs suggest that inflation plays a role in determining the extent of variation, as we would expect if either grease or sand effects were present. The magnitude of inflation's impact on wage change dispersion is modest. A two standard deviation rise in inflation (+5.7 percentage points) raises the dispersion of wage changes by about half of a standard deviation (or 2.1 percentage points). If grease or sand plays a role in the dispersion of wage changes, how can we assess their roles in labor market performance?

Histograms of wage changes offer a way to identify these effects more directly. For example, Figure 2a presents the histogram of percentage wage changes for the U.S. in 1988. It has four remarkable features:

- The histogram illustrates the substantial variation in wage changes among individuals that was shown to be common across all countries and years in Figure 1.

⁷ Increases of more than 60% in wage data or 100% in annual income data and cuts of more than 35% in wage data or 85% in income data were eliminated.

- There is a large concentration of workers at exactly zero wage change (that is, with wage freezes) suggesting the presence of downward nominal wage rigidity.
- The histogram reveals notable asymmetry; its mean is 1.2 percentage points greater than its median. This asymmetry is largely due to the absence of workers with wage cuts and the piling up of workers with wage freezes. If the workers with wage freezes are spread among the wage cut bins in proportion to the workers who actually received wage cuts, the difference between the mean and the median drops to only .4 percentage points. The boxes above the distribution to the left of the median show the reflection of the upper tail of the distribution. It is clear that a substantial number of workers are missing from the lower (wage cut) tail; they are concentrated in the wage-freeze spike at zero instead.
- The distribution of wage changes shown would not be Gaussian or normal even if the wage-freeze spike at zero and the missing wage-cuts were ignored. The distribution is notably more peaked and has somewhat fatter tails than does a normal distribution with the same median and standard deviation.

For a clear contrast, consider Figure 2b, which shows a wage change histogram for Belgium in 1979. While it initially looks similar to Figure 2a, a close look at the horizontal axis reveals that the spike is located in the range of 4 to 5%, rather than at zero in a year when price inflation was 4.5%. In this diagram, there are almost no “extra” wage freezes (that is, almost no spike at zero) and no evidence of a lack of wage cuts compared to low wage growth. If Figure 2a suggests downward nominal wage rigidity, Figure 2b suggests downward real wage rigidity and shows how the presence of strong downward real wage rigidity can make downward nominal rigidity irrelevant.

IV. Methodology

This section describes the methodology we use to assess histograms to determine the extent of various forms of wage rigidity and look for evidence of sand effects. We take two different approaches. First, we construct measures based directly on a few characteristics of the distribution. The relationship between these measures, the

histograms, and the concepts we are trying to measure is simple and clear. However, these estimates are potentially subject to a number of biases, the most important being bias due to measurement error. We thus present a second set of estimates derived using a two-stage method-of-moments procedure. In the first stage we estimate a model of the error distribution and use it to correct the histograms for error. In the second stage we fit a model of the wage change process which has as two parameters the fraction of the workforce subject to downward nominal and downward real wage rigidity.

a. Simple measures of downward nominal and downward real wage rigidity

We begin by positing an underlying notional wage change distribution. In the absence of downward nominal or downward real rigidity this would be the distribution of true wage changes. We assume the notional distribution is symmetric (which percentage wage change distributions appear to be when there is no rigidity distorting the distribution) so that in the absence of any rigidity the mean equals the median.

We then note that if a fraction n of workers are potentially subject to downward nominal rigidity, so that if their notional wage change is less than zero their wages will be frozen instead, that the fraction of workers who receive no wage change will be equal to the fraction of workers in the tail of the distribution below zero times n . We can thus estimate the fraction n as equal to the ratio of the number of workers receiving no wage change to the total receiving no wage change plus those receiving a cut.

Assuming the median of the wage change distribution is above zero (which it is in every country in every year in all our data sets) downward real wage rigidity will have no impact on the median of the wage change distribution, but it will increase the mean. Assuming that all workers who would have received wage cuts are equally likely to be

subject to downward nominal wage rigidity the mean will increase by an amount equal to the absolute value of the average wage below zero times the fraction of workers affected by downward nominal rigidity which is equal to the fraction of workers receiving no wage change. This has been called the wage sweep-up and we will use it below when we go to estimate the employment effects of wage rigidity.

Next note that if the expected rate of inflation is below the median of the notional wage change distribution, downward real wage rigidity will also have no effect on the median, but will increase the mean of the distribution. With only downward nominal and downward real wage rigidity affecting the mean their combined effect on the mean will be equal to the difference between the mean and the median. We can thus compute the wage impact of downward real wage rigidity by subtracting the impact of downward nominal rigidity from the difference between the mean and the median.

Finally, we want a measure of the fraction of the workforce susceptible to downward real wage rigidity that is analogous to our measure of downward nominal wage rigidity. Unfortunately, not all agents in the labor market share the same expected rate of inflation. Thus there is no single point at which all people subject to downward real wage rigidity pile up like the mass at exactly zero where those affected by downward nominal rigidity pile up. Rather, we expect those affected by downward real wage rigidity to pile up in the histogram cell containing the average expected rate of inflation and in the cells immediately around it. There is no easy way to distinguish those affected by downward nominal rigidity from those whose notional wage changes fall in that range. However, the method we used to derive the wage impact of downward wage rigidity

suggests an alternative way to determine the number of people affected by downward real wage rigidity.

The effect on the mean of downward real wage rigidity is equal to the fraction affected by downward real rigidity multiplied by the average difference between the expected rate of inflation and notional wage changes less than the expected rate of inflation. Thus by dividing our estimate of the wage effect of real rigidity by the difference between the expected rate of inflation and the average wage change below the expected rate of inflation we can recover an estimate of the fraction of people who are affected by downward real wage rigidity. We estimate the expected rate of inflation from a regression of the current year's inflation on the previous year's inflation. We assume that half the people affected by downward real wage rigidity will receive wage changes above this value and half will receive wage changes below this value. To compute the fraction of people susceptible to downward real wage rigidity (r) we divide our estimate of the fraction of people affected by the fraction that were at risk (those with notional wage changes below the expected rate of inflation). This will be equal to the fraction of people below our estimate of the expected rate of inflation plus half the people we believe to have been affected by downward real wage rigidity. We add only the half who we imagine receive wage changes above our estimate of the expected rate of inflation, because the other half are already counted among those with wage changes below the expected rate of inflation.

b. Model based estimates

Measurement error in base wages seriously impedes the assessment of wage rigidity because it creates spurious variance in wage changes. Errors can occur because surveys are subject to misreporting by those surveyed. By contrast, administrative data accurately report earnings, but if we lack an adequate measure of time worked and/or do not know the degree to which pay reflects components such as bonuses, overtime, and piece rates, earnings will be a very “noisy” gauge of base wage.

Our correction technique can be used on a variety of data sets and does not require strong assumptions about the distribution of wage changes.⁸ Consider two histograms: one showing the distribution of observed wage changes and one showing the distribution of true wage changes. Let us call the vector that records the share of workers in each cell of the true wage change histogram f^t and the corresponding vector of observed changes f^o . Now we can write

$$(1) \quad f^o = T f^t,$$

where T is a matrix whose columns are the percentage of observations in each cell of the true distribution that will end up in each cell of the observed distribution (since the cell columns sum to 1) because of measurement errors. If we invert T and multiply both sides of equation 1 by that inverse, we get

$$(2) \quad T^{-1} f^o = f^t.$$

Thus, if we know T we can recover the true distribution from the observed distribution. To construct T we assume that errors, when made, have a two-sided Weibull

⁸ Previous approaches to correcting for measurement error make strong functional form assumptions about the distribution of true wage changes (Altonji and Devereux, 2000; Fehr and Goette, 2005), or require high-frequency data on wage changes (Gottschalk, 2005) that are not available in most of the countries we study.

distribution.⁹ We use method-of-moments to estimate the parameters of the error distribution, the fraction of the population that is prone to errors, and the fraction of those who are prone to errors that make errors in that period. We assume that the errors are independent and that for the error-prone the probability of making an error is independent. Most important, we assume that the true wage change is not auto-correlated so that we can estimate the variance of the error from the negative auto-correlation of observed wage changes.¹⁰

Dickens and Goette (2005) discuss the method and its assumptions in detail. Here, we first note that Gottschalk's (forthcoming) method for estimating true wage changes also yields an error and a true wage series for each individual in his data set. We tested all of our assumptions on those two series and could not reject any of them.

Further, application of this approach to IWFP data sets provides convincing examples that the process works as intended. For example, the U.S. Panel Study of Income Dynamics (PSID) and the ECHP are survey data sets where wages are reported with a great deal of error. Our method detects a similar degree of error in the PSID and in each ECHP country we study. A few of our data sets (notably the Finnish employer survey data and the German and Portuguese administrative data) accurately measure a

⁹ A two sided Weibull distribution is defined by the following cumulative density function:

$$F(x) = \begin{cases} \text{for } x \leq \mu & .5e^{\left(\frac{x-\mu}{\beta}\right)^\alpha} \\ \text{otherwise} & 1 - .5e^{\left(\frac{\mu-x}{\beta}\right)^\alpha} \end{cases}.$$

The three parameters allow variation in the mean (μ), the dispersion (β), and the peakedness (α) of the distribution. The functional form provides a good fit to the empirical error distribution generated by Gottschalk's (2005) method applied to U.S. Survey of Income Program Participation data.

¹⁰ In fact, there is evidence that wage changes over long periods of time are positively auto-correlated (Baker, Gibbs and Holmstrom 1994). However this positive correlation is dwarfed by the

base wage concept. Our method yields very small estimates of the rate and variance of errors in these data.

Perhaps most convincingly, the Portuguese data set has two earnings variables: one measures the base wage and the other reports a more complete earnings concept (similar to that found in other administrative data sets, where we estimate higher error rates and variances). Our technique yields almost no correction to the one that measures the base wage, but makes substantial correction to the earnings measure. After the correction, the distinctive characteristics of the base wage distribution, which are lost in the distribution of earnings changes, are largely recovered (see Figures 3a and 3b). Using this correction makes our comparisons across data sets much more meaningful and reliable.¹¹

b. Estimation of rigidities and an indicator of sand effects

To measure rigidities, we use the generalized method of moments to fit a simple model of wage changes to the error-corrected wage-change histograms for each data set year.¹² We assume that, in the absence of rigidity, log wage changes have a symmetric

magnitude of the negative correlation induced by the measurement error in our typical data set so that at worst we slightly (less than 5%) under estimate the error variance.

¹¹ A second form of measurement error is specific to some administrative sources that record earnings for the whole year (annual earnings). In most countries, wage changes take place once a year for most workers. If wages are computed by dividing annual earnings by annual hours worked, a wage change that happens in the middle of one year will cause the computed wage to change in that year and the next year as earnings rises in both years as a result of a single wage change. This violates the assumptions of our error correction model and the wage change model used to estimate rigidity that is described in the next section. Dickens and Goette (2005) describe alternative versions of both the error correction and wage change models that are used with this type of data.

¹² The IWFPP has experimented with a number of other methods for identifying differing degrees of rigidity (Dickens and Goette 2004) requiring less restrictive assumptions. The other methods were judged inferior in that deviations from this more restrictive method seemed to result from shortcomings of the alternative methods rather than from failures of the distributional assumptions critical to the method used here.

two-sided Weibull distribution, referred to as the “notional” wage change distribution.

We estimate all three parameters of the notional distribution in each year.

We also assume that a fraction of the population is potentially subject to downward real wage rigidity. If their notional wage change is below their (or their firm’s) expected rate of inflation, they will receive a wage change equal to that expected rate of inflation rather than equal to their notional wage change. The mean and standard deviation of the expected rate of inflation for each country in each year are also parameters of the model and are estimated separately for each year.

We next assume that a fraction of the population is potentially subject to downward nominal wage rigidity. Such workers who have a notional wage change of less than zero, and who are not subject to downward real wage rigidity, receive a wage freeze instead of a wage cut.

Finally, since there is often a paucity of observations just above zero as well as just below it (see Figure 2a), we allow that some people are subject to symmetric nominal wage rigidity. This can be the result of the costs of revising pay schedules or a tendency to round off wage changes. If such people have a notional wage change close to zero and are not affected by downward real wage rigidity, they will receive a wage freeze rather than a small positive or negative change. We control for this possibility to avoid overestimating the role of downward nominal wage rigidity. However, because we doubt that this symmetric nominal rigidity is economically important, we do not address it in the analysis that follows.

This process yields estimates of the extent of downward nominal wage rigidity (n) and of downward real wage rigidity (r). These measures vary between 0 and 1, where 0

indicates perfect flexibility (no one is constrained) and 1 indicates full rigidity (all workers are potentially constrained). The protocol also yields estimates of the dispersion of notional wage changes that we can examine for evidence of sand effects.

V. Rigidity estimates

When we apply the method, described in the previous section, to estimating rigidity for each data set for each year, we find a great deal of variation across time and data sets. Before proceeding with further analysis, we examine the validity of this variation.

Focusing first on cross-country evidence, we find considerable variation across countries in the extent of both real and nominal rigidity when we average across all data sets and time for each country (see Figure 4). Estimates of the fraction of workers potentially affected by downward nominal wage rigidity range from 9% to 66%, while the comparable range for real rigidity is 3% to 52%. Countries with higher nominal rigidity tend to show less real rigidity, although the correlation of country averages is only a statistically insignificant -0.24. Regressions (not reported here) of our annual estimates of downward nominal wage rigidity and downward real wage rigidity on country effects show them to be jointly significant even after controlling for a full set of data set characteristics.¹³ In addition, the data set characteristics explain a minor part (less than 5 percent) of the variation in these regressions. This suggests that the error

¹³ The data set characteristic include indicator variables for the following: census vs. sample, survey vs. administrative records, earnings vs. wage data, whether the country team ran the annual earnings protocol vs. the wage protocol, whether the data was drawn from the ECHP, and whether hours worked were available.

correction procedure does a good job of removing the influence of data set characteristics on our rigidity measures.

Figures 5a, b, c and d compare these measures to those from other studies and between different data sets in our study. We find that n (our measure of the fraction of workers who might be affected by nominal rigidity) is strongly positively correlated with similar measures from two other cross-national studies that use different methodologies to estimate the extent of downward nominal wage rigidity (Figures 5a and b).¹⁴ Furthermore, in countries where we have r and n estimates from the ECHP and another data set the two are strongly positively correlated (0.54 for r and 0.60 for n) (Figures 5c and d). Since the paired estimates in all four figures cover different time periods, and in some cases different types of workers, we do not expect a perfect correlation. Overall, we consider these results to be very supportive of the reliability of our country average estimates.

Validation of the variation of our country estimates over time is difficult; nevertheless, they do receive support in a number of cases. Our ability to validate systematically is limited because we have no comparable alternative cross-country studies and because there is little or no overlap between the time periods covered by the different data sets for the same countries in our study.

However, some of the notable changes that we observe in smoothed measures of rigidity happen at the same time that important institutional changes in the country occur. For example, our estimate of the fraction of workers affected by real wage rigidity in the

¹⁴ We drop Holden and Wolfsburg's implausible estimate for the U.S. where they estimate the fraction subject to nominal rigidity to be negative. If we include it and set it to the lower bound of zero the correlation with our measure drops from .66 to .43.

U.S. declines from nearly 20% in the 1970s to zero in the mid-1980s and 1990s. This decline corresponds to the decline in the role of unions and pattern bargaining in U.S. wage setting (Blanchflower and Freeman, 1992). Declines in the importance of real rigidity in Germany and Italy also coincide with significant changes in the wage setting institutions in those countries (declining union power in Germany and the elimination of indexation in Italy--see Bauer *et al.* 2003 and Devicienti *et al.* 2005, respectively).

On the other hand, for several countries our rigidity measures show volatility over short periods of time that seems implausible. Examination of these cases suggests two causes that may be correctible in future research. First, our measure of symmetric nominal rigidity depends on the number of observations in the cells just around zero. This effect becomes difficult to disentangle from downward real rigidity when inflation rates are very low with consequences for the measurement of downward nominal rigidity as well.

Second, our concept of downward real wage rigidity may be difficult to distinguish empirically from another common feature of wage determination. Some important centralized wage bargains set a floor for wage changes while allowing decentralized changes above the floor (sometimes called “wage drift”). In those cases, the histogram for wage changes resembles that for downward real wage rigidity, although the spike will reflect not only the expected rate of inflation, but also the negotiated minimum real wage change. Our protocol restricts the expected rate of inflation to fall within what we consider reasonable bounds for such an expectation. For countries with this sort of wage drift, we can estimate considerable real wage rigidity in years when the floor falls within a preset range for expected inflation, but not in years when the floor is

above that range. This inconsistency will also have spillover effects on our estimates of nominal rigidity.

VI. Correlates of rigidity

We now explore whether our measures of wage rigidity are associated with labor market institutions that are suspected sources of wage rigidity. We consider the following six labor market institutions: strictness of employment protection legislation (EPL), union density, collective bargaining coverage, whether minimum wage or wage indexation legislation is in place, and the degree of corporatism (an index of bargaining coordination and centralization). Our most robust results are for measures of unionism.

Figures 6 and 7 show scatter plots of country averages of n and r against six measures of labor market institutions. We see that the index of EPL has a weak, statistically insignificant positive correlation with both our measures of wage rigidity. The corporatism index—which is a summary measure of centralization and coordination bargaining structures—has a statistically insignificant negatively correlated with both nominal and real rigidity, suggesting only weak support for the hypothesis that more centralized and coordinated unions exert less wage pressure. Our measure of indexation is weakly negatively correlated with nominal rigidity and positively correlated with real rigidity though neither relationship is statistically significant. The signs and relative magnitudes are what we would expect and the weakness of the result not surprising since all indexation regimes in our sample provide only partial coverage of the economy. Also, some countries such as Finland and France experience relatively high real rigidity without ever having had wage indexation clauses in place.

The figures also show that countries with higher ratios of minimum wages to average wages have modestly higher levels of nominal rigidity and lower levels of real rigidity. However, this result seems to be driven by the contrast between countries with substantial collective bargaining but no minimum wages, and those with substantial minimum wages. This suggests that the relationship is only a reflection of the much stronger and more robust correlation between union power and rigidity.

The strongest results are for union coverage and union density, though even with these variables the correlations are only statistically significant at the 10% level in a one-tailed test. We speculate that union representation raises worker awareness of what is happening to their real wages and gives them the bargaining power to protect their real wages. Accordingly, workers become less concerned with nominal wage changes. Alternatively, unions may achieve only partial real rigidity for some workers; that is, they may cause some wage freezes to become nominal wage increases, albeit real wage cuts. Under these circumstances we would estimate a lower rate of nominal rigidity.

VII. Does wage rigidity cause unemployment?

We now consider the consequences of wage rigidity. If *nominal* rigidity causes some unemployment at very low rates of inflation, small increases in inflation can reduce this joblessness, creating the grease effect. However, in cases where it is *real* rigidity that reduces employment, and many workers are potentially affected, the grease effect of inflation can be mitigated. To examine the relationship between wage rigidity and unemployment we turn to the general equilibrium model of Akerlof, Dickens and Perry (1996) that motivates a Phillips curve relation of the form

$$(3) \quad \pi_t = \pi_t^e + c - a U_t + b S_t + x_t.$$

In their model, π_t is the rate of price inflation at time t , π_t^e is the expected rate of price inflation, c is a constant, U_t is the unemployment rate, S_t represents the wage effects of rigidity, and x_t is the effects of supply shocks. This relationship implies that

$$(4) \quad U_t = S_t b/a + [c + x_t - (\pi_t - \pi_t^e)]/a.$$

The constant b is equal to 1 plus the average mark-up of prices over labor costs. Typical estimates of the coefficient a place it between 0.2 and 1.0, so we would expect the impact of S_t on unemployment to be greater than 1.0.

The wage rigidity variable (S_t) is the amount by which nominal wages of those constrained by downward rigidity are higher as a result of wage rigidity, multiplied by their share of the wage bill. For each data set for each year, we approximate S_t by estimating the extent to which wage rigidity raised average wage changes, as compared to the notional wage change distribution for that data-set-year.¹⁵

We estimate both equation (3) and equation (4). In estimating equation 3 we assume that the expected rate of inflation is equal to the previous period's inflation and that the effect of supply shocks can be captured by indicator variables for specific events

¹⁵ To be explicit, we compute a numerical estimate of the average notional log wage change conditional on the wage change being negative. We also compute the average log wage change conditional on it being less than our estimate of the average expected rate of inflation. The latter average wage change is multiplied by a smoothed estimate of the fraction of the workforce potentially subject to downward real wage rigidity (r). Similarly, the former average wage change is multiplied by a smoothed estimate of the fraction of the workforce potentially affected by downward nominal rigidity times one minus the fraction potentially subject to real rigidity ($[1-r]n$). These are summed to obtain an approximation of S_t . This is only an approximation because, as Akerlof, Dickens and Perry show, the effects of rigidity can accumulate over time if a large fraction of the workforce is affected by the rigidity. This normally is not

(oil shocks) or for all years. In estimating equation 4, we implicitly assume that expectation errors are orthogonal to S_t by excluding inflation or expectations from the regression.

We run two specifications for each equation. All specifications include data-set-specific intercepts and the second specification also includes year-specific intercepts to control for common supply shocks. Table 3 presents the results.

In all but one case, the estimate of the unemployment impact (b/a) is greater than 1 and in all cases is statistically significantly greater than zero at least at the 0.1 level. When we separately estimate the effects of real and nominal rigidity (regressions not reported), some of the estimates are not statistically significantly greater than zero, but we can never reject the hypothesis that the unemployment effect of a rigidity measure is greater than 1, nor can we reject the hypothesis that the coefficients on the two measures of the wage impact of rigidity are equal. Also, when we estimate b independent of a in equation 3 (regressions not reported), it is always less than the predicted minimum of 1, but we can never reject the hypothesis that it is greater than 1. Taken together, these results provide moderately strong evidence that inflation can lower unemployment in the presence of downward *nominal* wage rigidity (that is, of grease effects) as well as the inability of inflation to lower unemployment in the presence of downward *real* wage rigidity. The size of the grease effects is in the range predicted by the Akerlof, Dickens and Perry model.

important, but it can be during extended periods of low inflation or deflation such as the Great Depression in the U.S.

VIII. Sand estimates

We turn now to the sand effects of inflation and productivity growth. It is difficult to create a direct estimate of the sand that would focus on *unintended* variation in wage changes that could be applied to all of the IWFP datasets. Thus, we examine our measures of notional wage change variability (from which the impact of wage rigidities and reporting errors have been removed) for any remaining correlation with expected or unexpected inflation. We find weak evidence that high and variable inflation, in particular, distorts price signals in labor markets.

The first column of Table 4 reports the relationship between the standard deviation of changes in the log of notional wages and inflation expectations, inflation surprises, and productivity growth.¹⁶ Only the inflation surprise proxy is statistically significant at the 5 percent level (one-tail test), and there is little evidence that these effects taper off as the second order term is not statistically significant. The effects of expected inflation on the standard deviation of the notional wage are minimal; while productivity growth, which is difficult to predict, yields similar coefficients to inflation's surprises. This is consistent with the proposition that higher unexpected inflation distorts price signals.

To gauge the size of misallocation effects of inflation on the labor market, we estimate regressions similar to those reported in Table 3, including the standard deviation of notional wage change on the right hand side both with and without the measures of rigidity effects. Coefficients were small and statistically insignificant in nearly all

¹⁶ We measure expected inflation as a three year moving average of realized inflation rates. Several other expectations measures produced similar coefficient estimates, but the standard errors were larger

specifications, and in the few cases where they were significant they have the wrong sign.¹⁷ Of course, the costs and misallocation effects of inflation need not show up as unemployment, and it is difficult to assess the magnitude of the effects without a detailed structural model of firm wage setting.

IX. Conclusion

The International Wage Flexibility Project has investigated three ways in which labor market imperfections can interact with inflation. First, moderate inflation in the presence of resistance to *nominal* wage cuts can “grease” the wheels of relative wage adjustment to ongoing shocks and thus improve economic efficiency. Second, widespread resistance to *real* wage cuts can also raise unemployment rates, but in this case inflation provides no relief. Third, inflation can cause distortions in relative wages that lead to costly resource misallocations, thus throwing “sand” in the wheels of economic adjustment. While the first effect has been studied extensively, especially in the U.S., the other two have not.

The IWFP investigates these three effects simultaneously using 31 panel data sets covering individual workers in 16 European countries and the U.S., relying on the expertise, data access, and analysis contributed by 13 country teams. We find considerable variation in wage changes among workers in the same country and year. The variation increases with inflation, as we would expect if either downward nominal rigidity or sand effects were important.

¹⁷ The coefficients on the rigidity effects variables are virtually unaffected by the inclusion of this variable and no substantive conclusions are altered.

Applying a new estimator for the prevalence of these three effects, we also find evidence of both types of rigidity in nearly every country. Estimates of the fraction of workers potentially affected by downward nominal wage rigidity range from 9% to 66%, while the comparable range for real rigidity is 3% to 52%. Furthermore, there is some evidence that countries with higher nominal rigidity tend to show less real rigidity.

Our technique and wealth of data sets enable us to explore the impact of data features on empirical estimates of wage rigidity and compare our results with those of previous studies. Our method for correcting wage change histograms should make results comparable across different data sets. Indeed, the contribution of data set characteristics to explaining the variation in our rigidity measures across countries and across time is minimal, and results derived from the European Community Household Panel and those from country-specific data sets are strongly correlated. In addition, our measures of downward nominal rigidity are strongly correlated with other recent measures derived from industry or individual data.

Our examination of the causes of downward nominal and real wage rigidity suggests an important role for the extent of unionization and collective bargaining coverage. Both show a consistently positive relationship with the extent of downward real wage rigidity and a negative association with downward nominal wage rigidity. This finding suggests that collective bargaining focuses workers' attention on real wages and gives them some ability to resist real wage cuts. Other institutional variables that we examined had weaker relationships with our measures of rigidity.

These differences in rigidity across countries may translate into differences in unemployment. Measures of the “wage impact” of downward nominal and real wage

rigidity (that is, the extent to which workers' wages are affected in a particular year) are positively related to unemployment with statistically significant coefficients of about the size predicted by theory.

Finally, we find only suggestive evidence of potential sand effects in notional wage change distributions. The dispersion of these notional adjustments is positively correlated with unexpected inflation, consistent with the view that the increased dispersion reflects more, or more serious, errors in firm wage setting. In addition, we find no evidence of unemployment effects from degradation of price signals, so any costs imposed may be on productivity rather than on jobs.

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Figure 1a

Demeaned log standard deviation of log wage change
versus median observed log wage change for dataset-year (with linear fit)

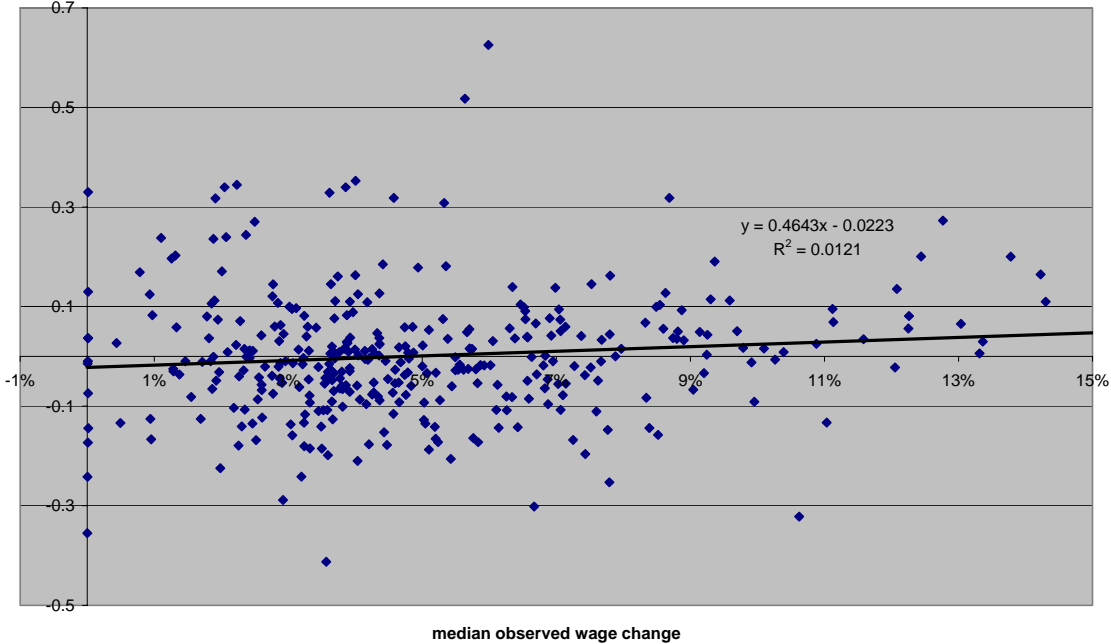


Figure 1b

Demeaned log standard deviation of log wage change
versus median observed log wage change for dataset-year (with linear fit)

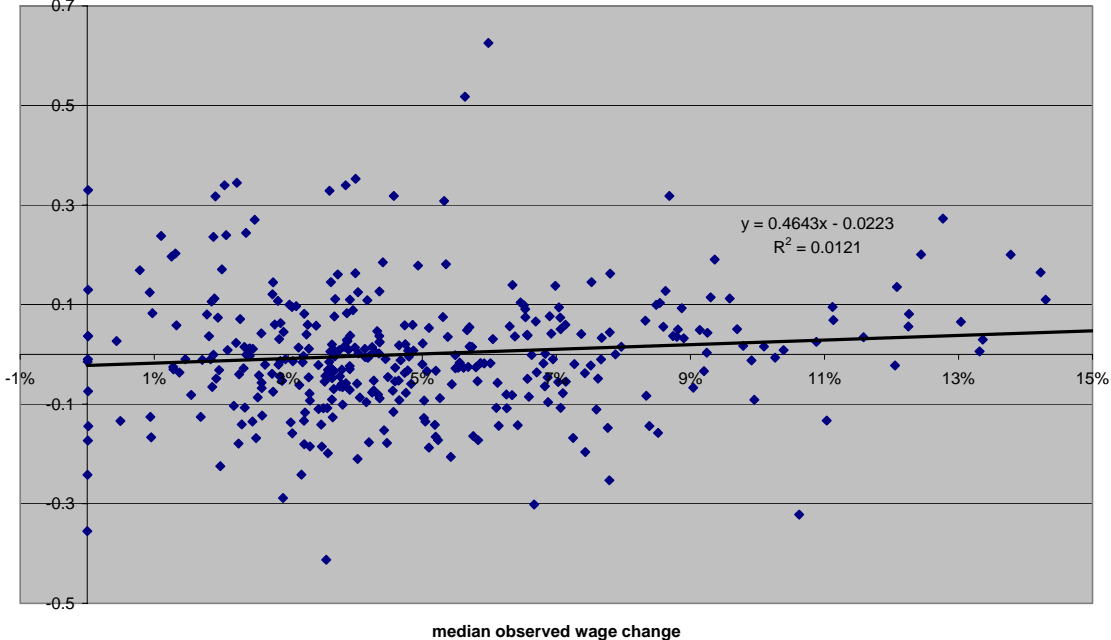


Figure 2a: Wage Change Distribution
USA 1988

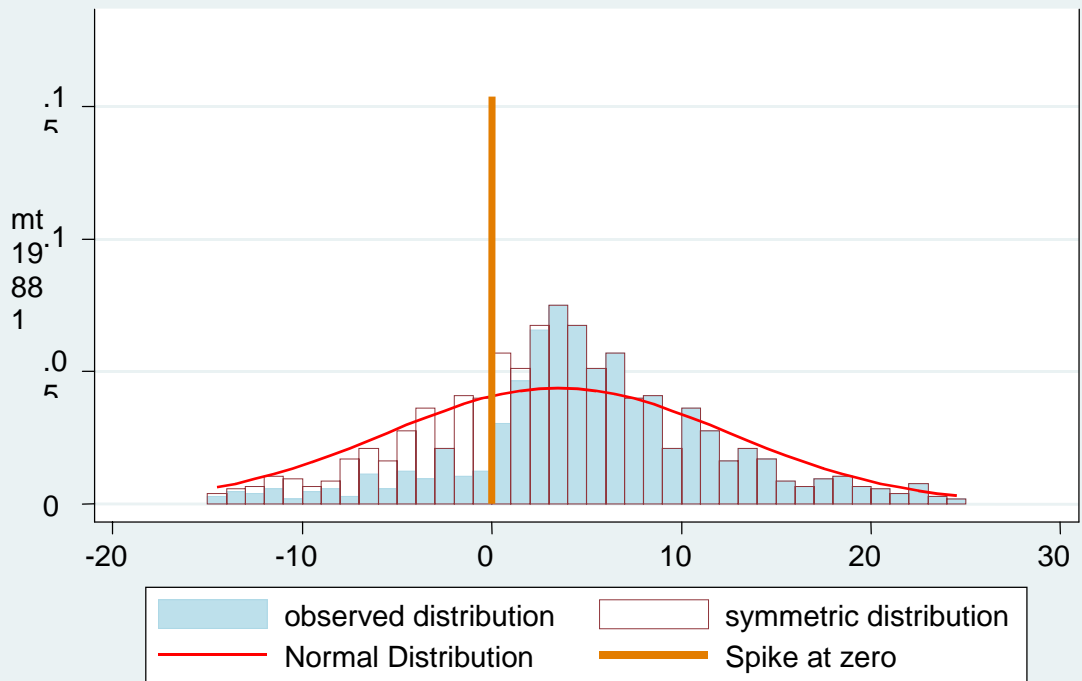


Figure 2b: Wage Change Distribution

Belgium 1979

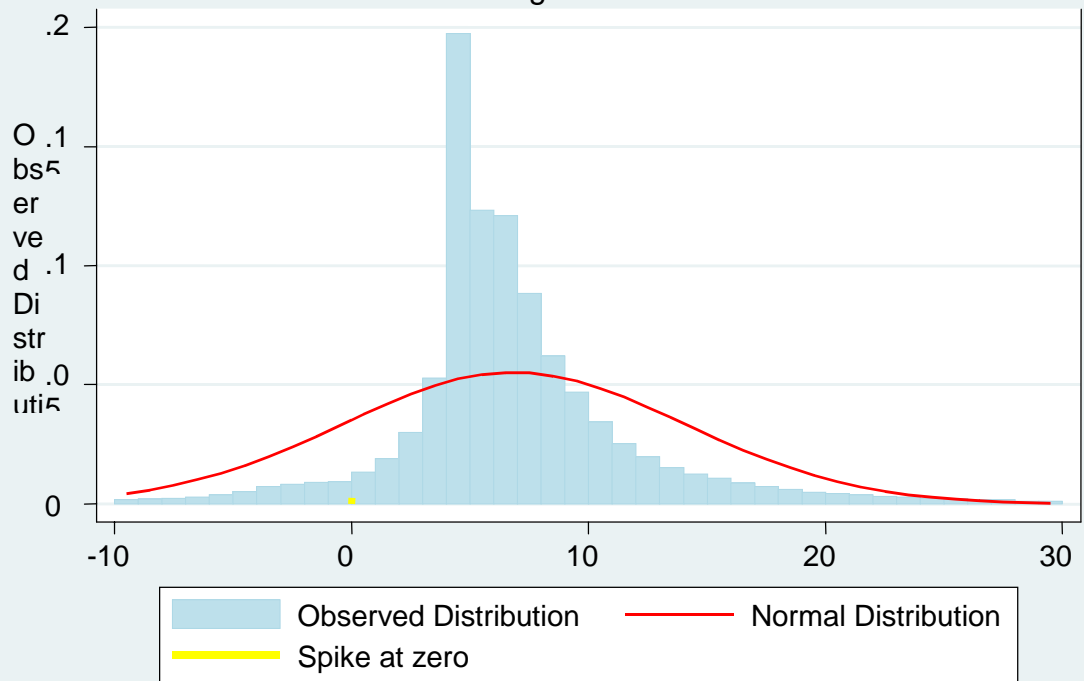


Figure 3a
Portuguese Wage Change Histograms 1994

Total Earnings

Base Wages

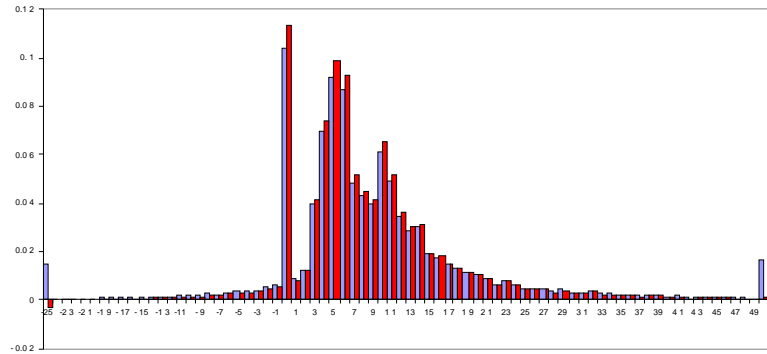
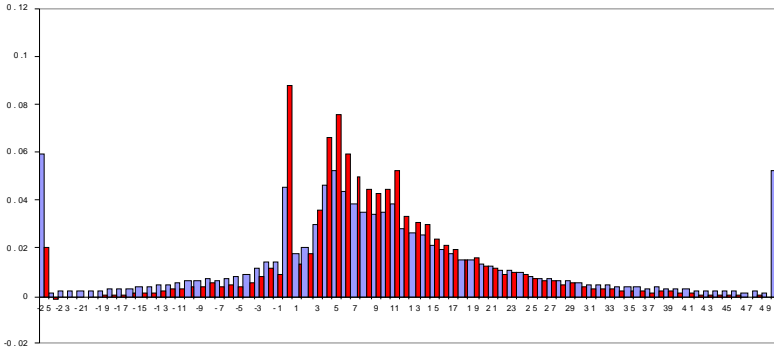
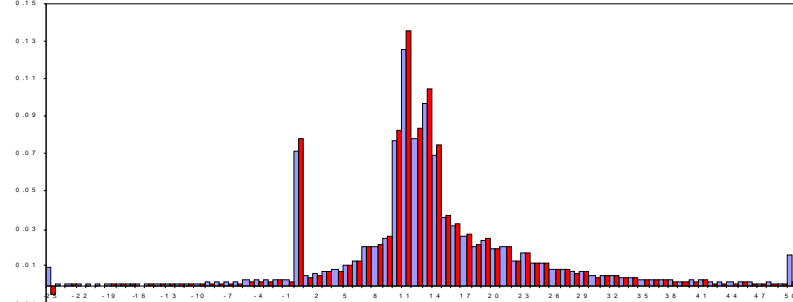
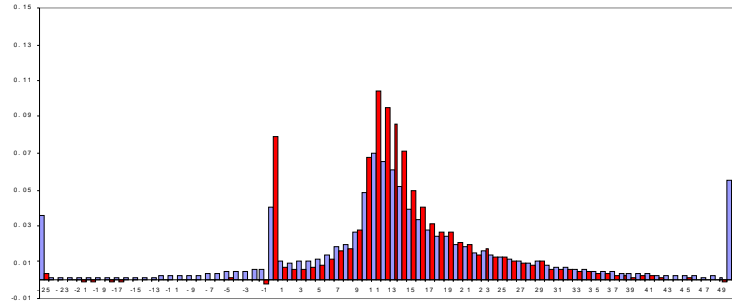


Figure 3b
Portuguese Wage Change Histograms 1992

Total Earnings

Base Wages



Blue bars are Observed Frequencies and red bars are the Corrected Frequencies.

Figure 4

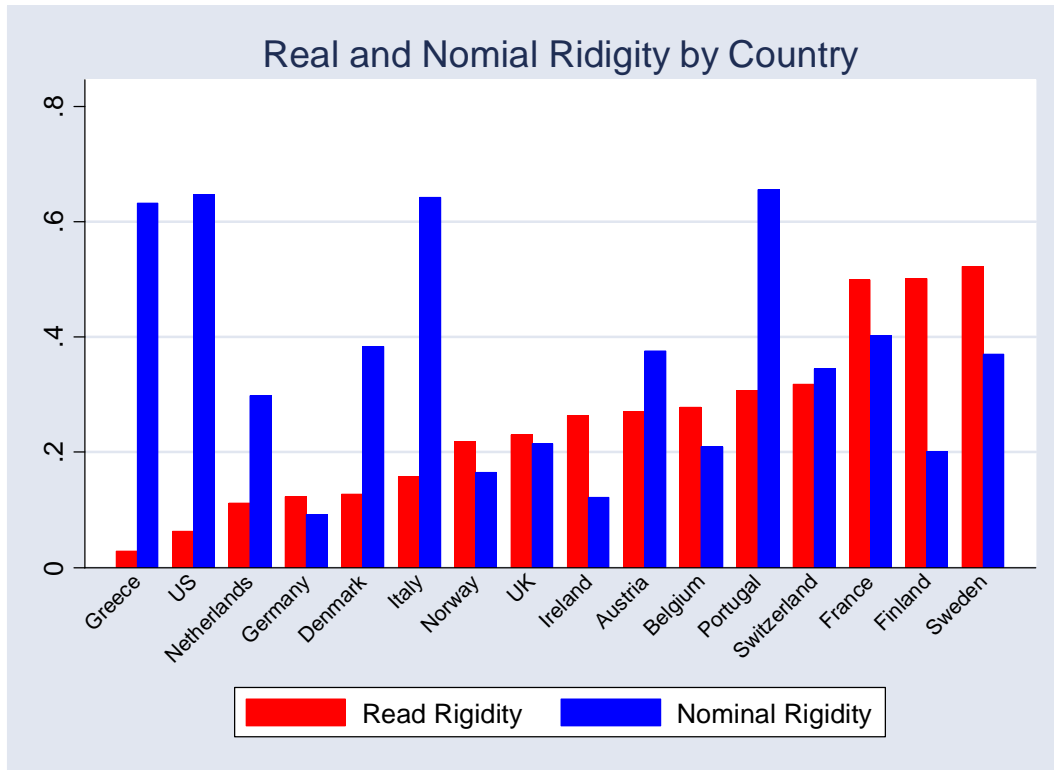
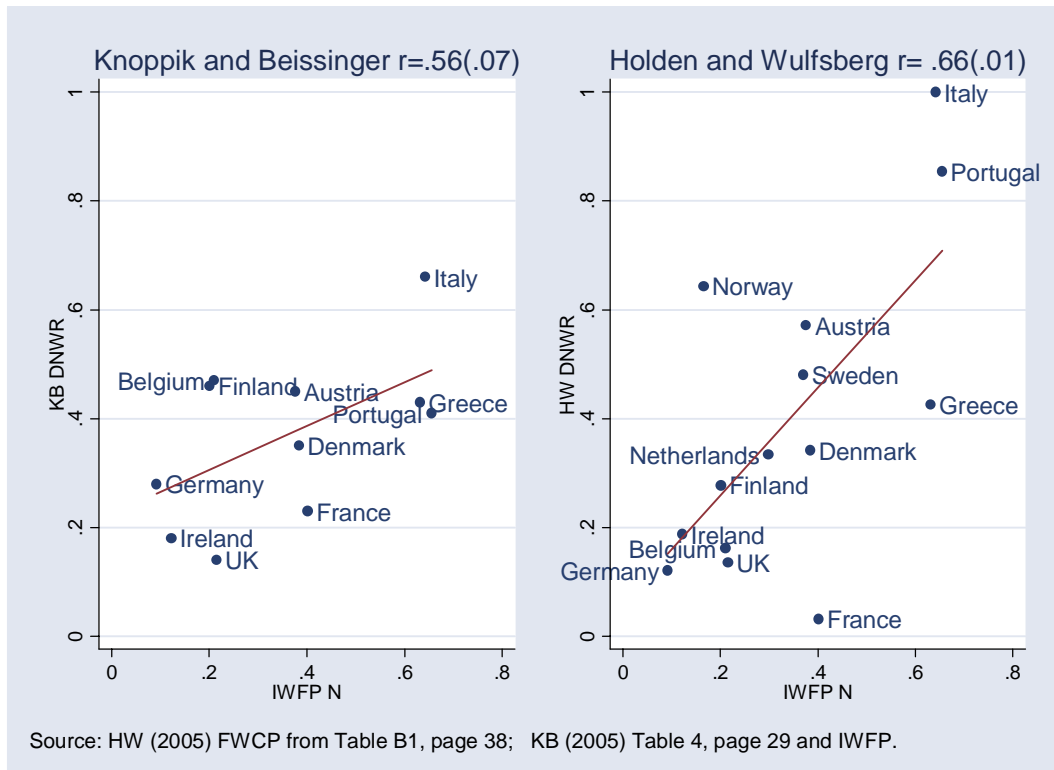
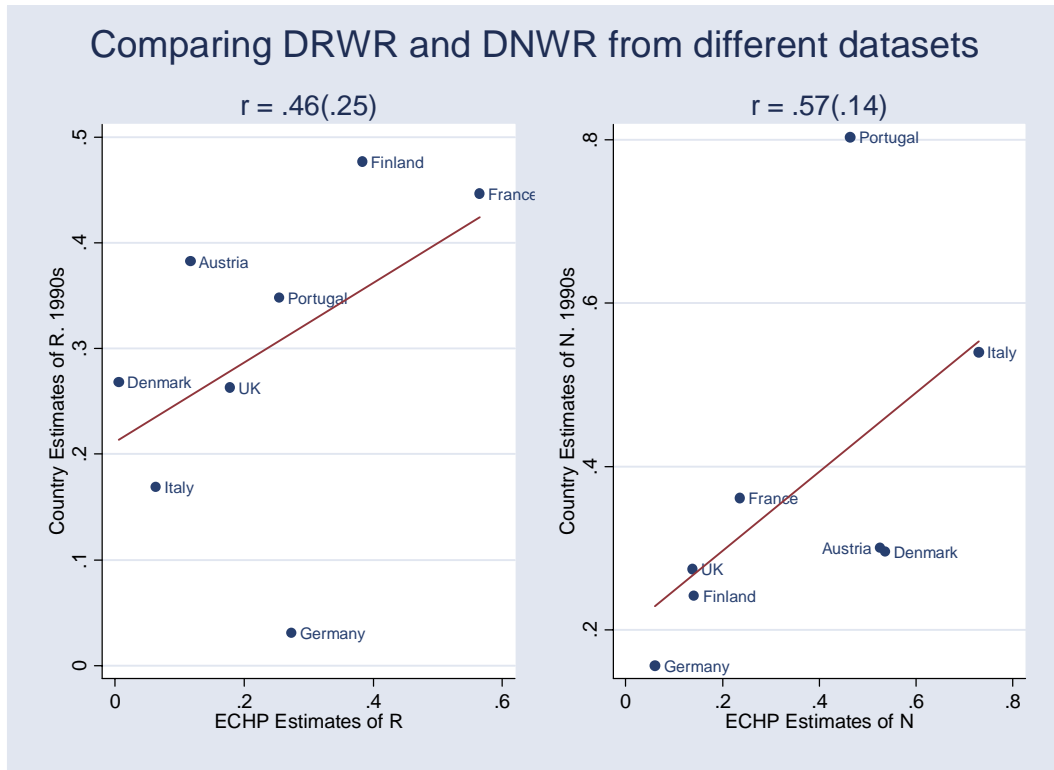


Figure 5a & b



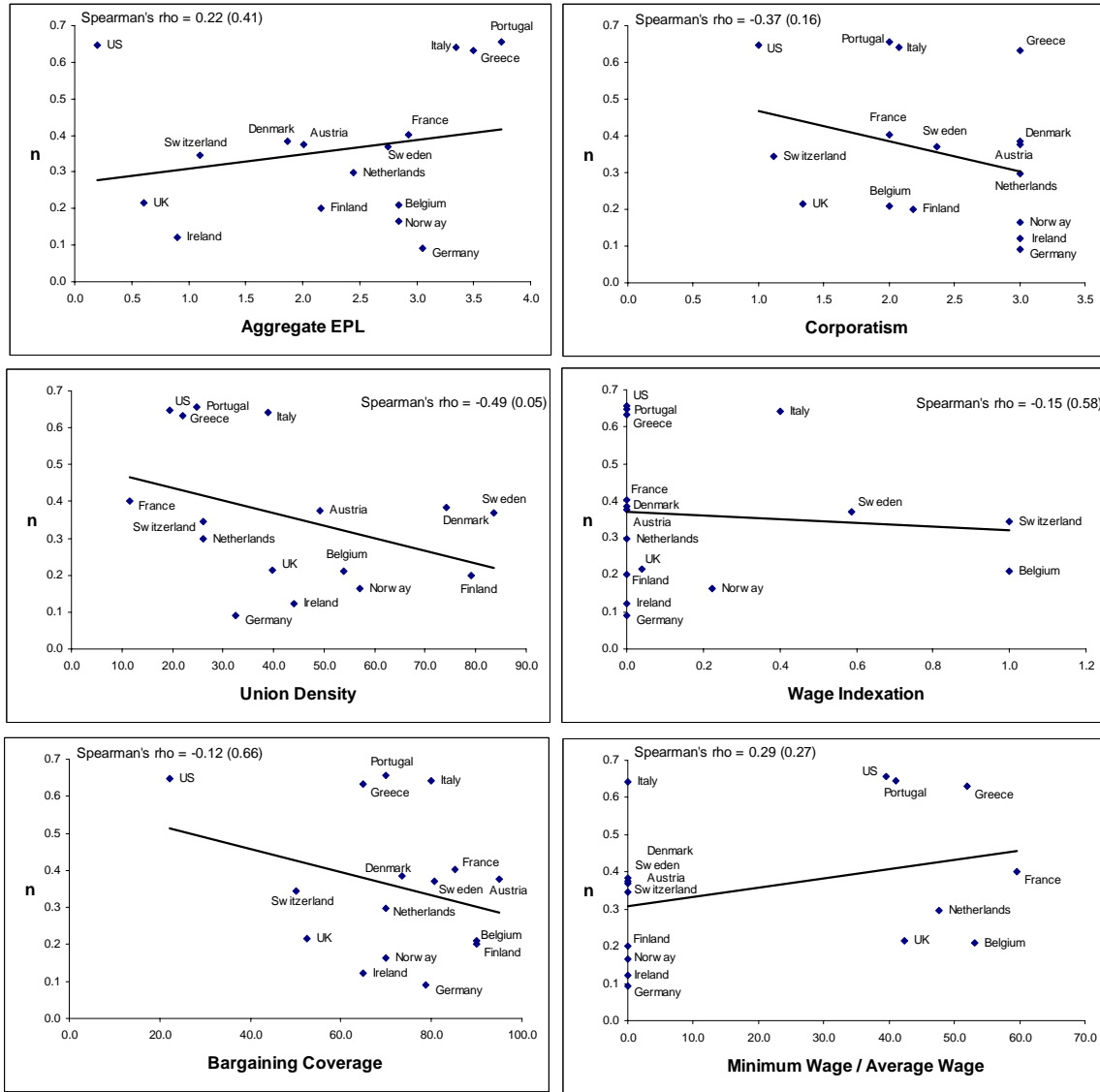
p values in parenthesis

Figures 5c & 5d



p values in parenthesis

Figure 6
Nominal Rigidity vs. Institutions



p values in parenthesis

Sources and definitions:

Aggregate EPL: OECD (2004), Index of the strictness of employment protection legislation, Categorical variable coded 0 to 6, where 6 is most restrictive

Corporatism: OECD (2004), Elmeskov, Martin and Scarpetta (1998), Wage-bargaining corporatism index, summary measure of collective bargaining structures of centralisation and coordination, Categorical variable coded 1=low to 3=high

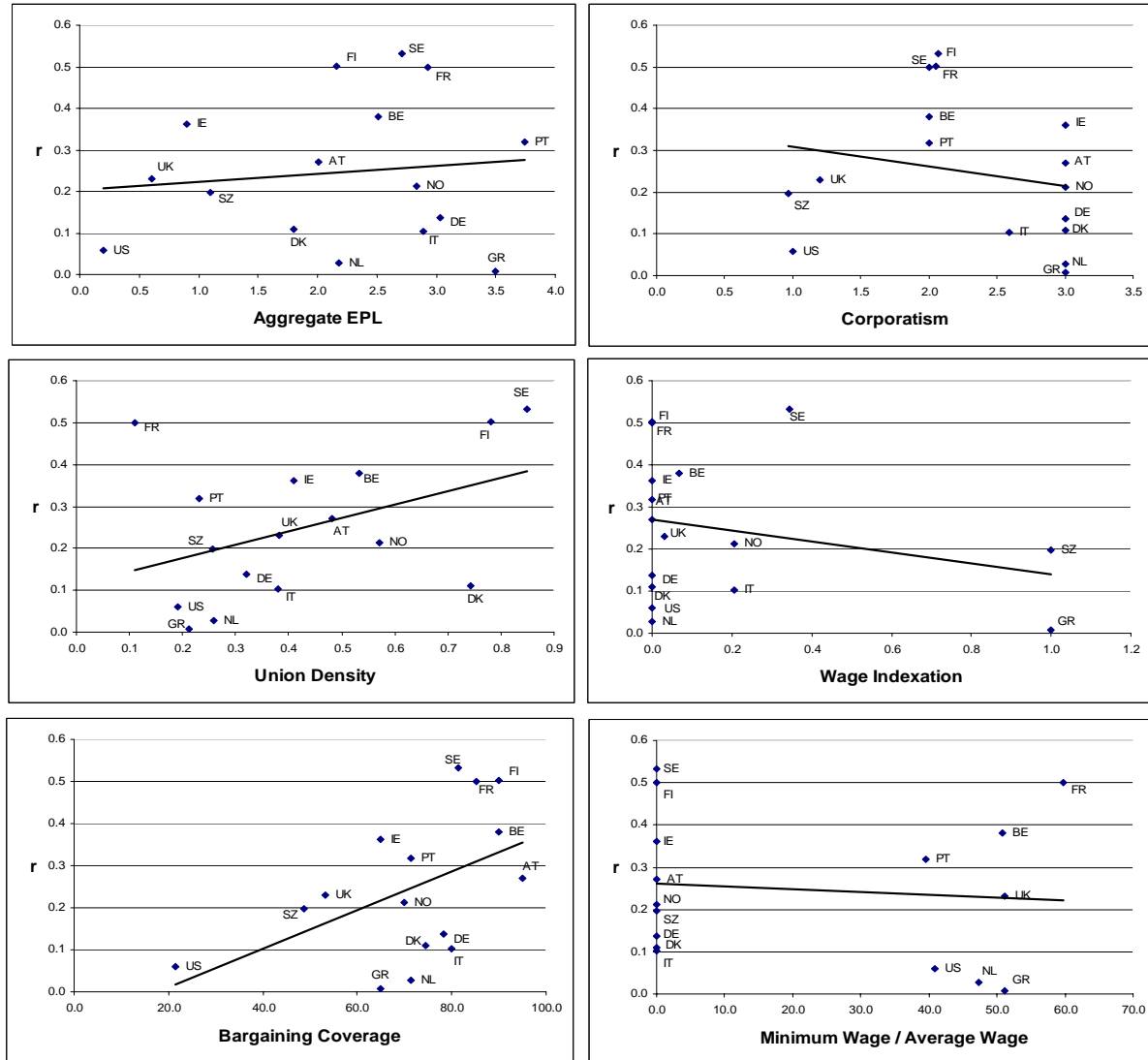
Union Density: OECD (2004), Elmeskov, Martin and Scarpetta (1998), The proportion of workers who are members of a trade union, in percent

Wage Indexation: Checchi and Lucifora (2002), Categorical variables coded 0 to 1, where 1 represents the presence of automatic wage indexation clauses for most sectors, updated for Belgium and extended to include Greece and the United States

Bargaining Coverage: OECD (2004), the extent to which salaried workers are subject to union-negotiated terms and conditions of employment, in percent, extended to include Greece and Ireland

Minimum Wage/Average Wage: OECD (2004), Elmeskov, Martin and Scarpetta (1998), Ratio of gross statutory minimum wage relative to average wage; supplemented by the Kaitz measure in Checchi and Lucifora (2002) for Greece, Portugal and the US; set equal to 0 for countries without minimum wage legislation

Figure 7
Real Rigidity vs. Institutions



p values in parenthesis

Sources and definitions:

Aggregate EPL: OECD (2004), Index of the strictness of employment protection legislation, Categorical variable coded 0 to 6, where 6 is most restrictive

Corporatism: OECD (2004), Elmeskov, Martin and Scarpetta (1998), Wage-bargaining corporatism index, summary measure of collective bargaining structures of centralisation and coordination, Categorical variable coded 1= low to 3 =high

Union Density: OECD (2004), Elmeskov, Martin and Scarpetta (1998), The proportion of workers who are members of a trade union, in percent

Wage Indexation: Checchi and Lucifora (2002), Categorical variables coded 0 to 1, where 1 represents the presence of automatic wage indexation clauses for most sectors, updated for Belgium and extended to include Greece and the United States

Bargaining Coverage: OECD (2004), the extent to which salaried workers are subject to union-negotiated terms and conditions of employment, in percent, extended to include Greece and Ireland

Minimum Wage/Average Wage: OECD (2004), Elmeskov, Martin and Scarpetta (1998), Ratio of gross statutory minimum wage relative to average wage; supplemented by the Kaitz measure in Checchi and Lucifora (2002) for Greece, Portugal and the US; set equal to 0 for countries without minimum wage legislation

Table 1
The Three Labor Market Interactions with Inflation Examined by the IWFP

Interaction	Underlying market imperfection	Predicted impact on the distribution of wage changes*	
		Dispersion rises with inflation?	Asymmetry and spikes?
Grease	Downward nominal wage rigidity	Yes: more inflation reduces the number of wage freezes and allows more wage changes below the mean wage change	Yes: skewed right, nominal wage freezes cause spike at zero wage change
Real wage inflexibility	Downward real wage rigidity	No: the entire distribution shifts with the inflation rate	Yes: skewed right, real wage freezes cause spike of wage changes around the inflation rate
Sand	Adjustment lags and errors	Yes: inflation adds more errors and lags to the variation in firms' wage changes	No: errors and lags are assumed to be symmetric

*These effects contrast with a fully flexible wage-setting regime that is assumed to produce wage changes that are symmetrical and unaffected by the rate of inflation.

Table 2

IWFP Data set characteristics

Country	Data set	Years	Source	Census or sample	Earnings or wages	Hours	Firm identifiers ?
1. Austria	Social Security	1972-1998	Register	Random sample	Earnings	No	Yes
2. Belgium	Social Security	1978-1985	Register	Census	Earnings	No	Yes
3. Denmark	Statistics Denmark	1981-1999	Register	Census	Wages	No	Yes
4. Finland	Finnish Service Sector Employers	1990-2001	Employer survey	Census	Wages	Yes	Yes
	The Confederation of Finnish Industry and Employers (Manual)	1985-2000	Employer survey	Census	Wages	Yes	Yes
	The Confederation of Finnish Industry and Employers (Non-manual)	1985-2000	Employer survey	Census	Wages	Yes	Yes
5. France	La Déclaration Automatisé des Salaires (DADS)	1976-1980, 1984-1989, 1991-2000	Register	Random sample	Earnings	Yes	No
	French Labor Survey	1994-2000	Household survey	Random sample	Earnings	Yes	No
6. Germany	Institut für Arbeitsmarkt und Berufsforschung (IAB)	1975-1996	Register	Random sample	Earnings *	No	No
7. Italy	Istituto Nazionale per la Previdenza Sociale (INPS)	1985-1996	Register	Random sample	Earnings	No	Yes
8. Norway	Norwegian Confederation of Business and Industry (Blue Collar)	1987-1998	Employer survey	Random sample	Wages	Yes	No
	Norwegian Confederation of Business and Industry (White Collar)	1981-1997	Employer survey	Census	Wages	Yes	No
9. Portugal	Quadros de Pessoal	1991-2000	Employer survey	Census	Wages	Yes	Yes
10. Sweden	Swedish Enterprises (Blue Collar)	1979-1990, 1995-2003	Employer survey	Census	Wages	Yes	Yes
	Swedish Enterprises (White Collar)	1995-2003	Employer survey	Census	Wages	Yes	Yes
11. Switzerland	Social Insurance Files (SIF)	1988-1999	Register	Random sample	Earnings	No	No
	Swiss Labor Force Survey	1992-1999	Household survey	Random sample	Wages	No	No

12. U.K.	National Employment Survey	1976-2000	Employer survey	Random sample	Earnings	Yes	No
13. U.S.	Employment Cost Index*	1981-2003	Employer survey	Random sample	Earnings	Yes	Yes
	Panel Study of Income Dynamics	1970-1997	Household survey	Random sample	Wages	Yes	No
14. Various**	European Community Household Panel	1993-2001	Household survey	Random sample	Earnings	Yes	No

Notes: *Not individual data. Not used in analysis of wage rigidity.

**Suitable ECHP data for the sand and grease analysis are available for Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Portugal, and the United Kingdom, available years vary somewhat by country. For Germany data wage data refer to earnings for most of the time period but to wages before 1984.

Table 3				
Effects of Rigidity on Unemployment				
	Dependent Variable			
	Unemployment		Change in Inflation	
Unemployment Effect (b/a)	1.26	.90	2.99	2.90
Standard Error	(.35)	(.32)	(1.28)	(2.09)
p for null hypothesis $0 \geq b/a$ (one tail test)	.00	.01	.01	.08
Controls for year	no	yes	no	yes

Table 4

Sand Effect Estimates Derived from the Notional Wage Change Results

	Estimated Underlying Standard Deviation of Wage Adjustments
Expected Inflation (MA3)	-.053 (-0.42)
Expected Inflation ²	.003 (0.28)
Inflation Surprise	.165 (1.74)
Inflation Surprise ²	-.009 (-0.20)
Productivity Growth	.170 (1.64)
Productivity Growth ²	-.020 (-1.18)
Within Group R ²	0.0202

Regressions include dataset specific intercepts.
T-statistics in parenthesis