Real Wage Cyclicality of Newly Hired Workers in Germany

Heiko Stüber*†‡

January 14, 2014

Abstract

Several recent macroeconomic models rely on rigid wages. The wage rigidity of newly hired workers appears to play a crucial role in these models, as the decision of whether to open a vacancy is primarily influenced by their real wages. However, there is little empirical evidence on how the real wages of newly hired workers react to business cycle conditions. This paper analyzes the cyclical behavior of real entry wages in Germany while controlling for "cyclical up- and downgrading" in employer/employee matches. The results indicate that entry wages are not rigid but instead respond considerably to business cycle conditions.

JEL classification: E24, E32, J31

Keywords: real wage cyclicality, entry wages, search-and-matching model

*Institute for Employment Research (IAB); Heiko.Stueber@iab.de; +49 (0)911 - 179 3167.

†I would like to thank Gary Solon, Michael Elsby, Thomas Beißinger, Andrew Snell, Christian Merkl, and the participants of the 2012 EALE Conference, the 2012 EEA-ESEM Meeting, the 2012 ESAM, the 2012 SES Conference, and the 2013 SOLE Meeting for their helpful comments.

‡A substantial amount of the research was conducted during my research visit at the University of Edinburgh. I would like to thank the School of Economics, in particular Michael Elsby, for making this visit not only possible but also enjoyable. I would also like to thank the Graduate Program of the IAB and the School of Business and Economics of the University of Erlangen-Nuremberg for supporting and subsidizing my visit.

1 Introduction

Based on recent microeconometric evidence on wage cyclicality, some authors argue that the canonical Mortensen-Pissarides search-and-matching model (Mortensen and Pissarides, 1994) is unable to explain the cyclical volatility of unemployment (see, e.g., Shimer, 2005; Hall, 2005; Veracierto, 2008). One method of solving this so-called "Shimer-Puzzle" is suggested by Shimer (2005, p. 45): "An alternative wage determination mechanism that generates more rigid wages in new jobs, measured in present value terms, will amplify the effect of productivity shocks on the [.. vacancy-unemployment] ratio, helping to reconcile the evidence and theory." Shimer's (2004, 2005) suggestion that real wage rigidity is one method of generating more variability of unemployment in the search-and-matching model has been widely shared (see, e.g., Hall, 2005; Hall and Milgrom, 2008; Kennan, 2010).[1]

The recent literature argues that the real wage rigidity of newly hired workers in particular should play a crucial role, as the decision of whether to open a vacancy is primarily influenced by their real wages (see, e.g., Pissarides, 2009; Haefke et al., 2012). Pissarides (2009) argues that even if the wages of incumbent workers are rigid, the wages of newly hired employees could be highly procyclical and that the "Shimer-Puzzle" would remain with sufficiently procyclical entry wages. Haefke et al. (2012) demonstrate that the wages of entrants exiting non-employment in the USA respond one-to-one to changes in labor productivity. However, the wages of incumbents react only slightly to changes in productivity.

However, the recent literature also challenges the notion of introducing real wage rigidity into search-and-matching models to generate realistic unemployment volatility. For instance, Pissarides (2009, p. 1341) dismisses theories based on cyclically rigid wages because hiring wages are empirically procyclical: "I conclude that a good explanation of the unemployment volatility puzzle needs to be consistent with the observed proportionality [...] between wages in new matches and labor productivity. Models that imply nontrivial departures from unit elasticity between wages in new matches and productivity go against a large body of evidence." He bases his dismissal on microeconomic studies reporting that

[1] The existence of the "Shimer-Puzzle" in Germany is shown, e.g., by Gartner et al. (2012). Gartner et al. (2012, p. 106) reveal that average labor market flows in Germany are much smaller than in the USA and demonstrate that the "standard deviations of unemployment, vacancies, the job-finding rate and the separation rate are larger in Germany than in the United States, both in absolute terms and relative to productivity."

the real wage cyclicality for job movers is larger than that for incumbent workers (e.g., Bils, 1985; Shin, 1994; Devereux and Hart, 2006; Shin and Solon, 2007).[2]

However, there is an explanation for why the empirical evidence to which Pissarides (2009) refers does not preclude acyclical wage setting by firms. Gertler and Trigari (2009) argue that workers may switch between high- and low-wage jobs over the business cycle, whereas the wages of newly hired workers may be rigid or tied to the wages of incumbent workers within the same firm. Previous research has generally ignored the "cyclical upgrading" of workers to better employment opportunities in booms (i.e., from low- to high-wage jobs) and "cyclical downgrading" to worse employment opportunities in recessions.[3] Failure to control for the employer/employee match could lead to the conclusion that wages are procyclical over the business cycle when in fact the procyclical movements of wages actually result from job changes. Therefore, an empirical assessment of recent theories of the rigidity of entry wages requires an approach that identifies cyclical variation in hiring wages within employer/employee matches.

Thus far, to the best of my knowledge, there are only two studies on Portugal that control for cyclical up- and downgrading in employer/employee matches: Carneiro et al. (2012) and Martins et al. (2012). Carneiro et al.'s (2012) endogenous variable is the individual real wage. In their regressions, they control for worker characteristics and simultaneously for linearly separable worker, firm, and job fixed effects. Martins et al.'s (2012) endogenous variable is a slightly aggregated wage: they use the "typical" real wage of entry jobs, e.g., the modal real wage paid to entrants in a particular job at a particular firm. They define jobs within firms and use firm-job fixed effects instead of controlling for firm and job fixed effects separably.

Against the backdrop of these recent developments, this paper provides empirical evidence on the real wage cyclicality of newly hired workers in Germany. I use two-stage regressions to estimate how changes in the unemployment rate affect the wages of newly hired workers. In the regressions, I control for cyclical up- and downgrading in employer/employee matches through the use of firm-job fixed effects. For the empirical analysis, I

[2]Hagedorn and Manovskii (2013, p. 773) provide an explanation for this finding: "workers can sample from a larger pool of job offers in a boom than in a recession, and workers with lower quality of the current match benefit more from the expansion of the pool of offers in a boom." They include the match quality in regressions (using labor market tightness—measured as the ratio of aggregate stock of vacancies to the unemployment rate—as a proxy) to control for unobserved idiosyncratic productivity. Controlling for the match quality, Hagedorn and Manovskii (2013, p. 773) demonstrate that "wages of job stayers and switchers exhibit similar volatility".

[3]Cyclical up- and downgrading has long been discussed and documented—the literature goes back at least to Reynolds (1951, chapter 5). Recent analyses include, e.g., Devereux (2004); Bjelland et al. (2011), and Hart and Roberts (2011).

apply three statistical models—focusing on two different endogenous variables—to a large administrative, longitudinally matched employer-employee dataset for Germany over the 1977–2009 period. I focus on the typical real wage of newly hired workers following Martins et al.'s (2012) methodology and focus on the job entrants' individual real wage following Carneiro et al.'s (2012) methodology.

The paper's contribution to the literature is threefold. First, I present the first empirical evidence for a large economy, namely, Germany, on the cyclicality of real entry-wages while controlling for cyclical up- and downgrading in employer/employee matches. In light of the magnitude of the entry-wage cyclicality that I find for Germany, it appears that the notion of introducing wage rigidity into the Mortensen-Pissarides model—to amplify the volatility of unemployment to a realistic level—is not supported by the empirical evidence. Second, I argue that estimates obtained using typical real wages (cf. Martins et al., 2012) and individuals' real wages (cf. Carneiro et al., 2012) as the endogenous variable might be biased in different directions. By conducting separate regressions for the two endogenous variables, I obtain upper- and lower-bound estimates for the wage cyclicality of newly hired workers. I argue that the true parameter should lie within these limits. Third, I demonstarte that the procyclicality of the employment/population ratio in Germany is (nearly) identical to the procyclicality of the real wages of job entrants.

The remainder of the paper is structured as follows. The next section provides a brief literature review on the methods for measuring entry wage cyclicality and existing empirical evidence. The data description and data selection are presented in Section 3, and Section 4 presents the statistical models and empirical results. In Section 5, I discuss the results and their implications, and Section 6 concludes.

2 Previous Empirical Evidence and Measurement Methods

To the best of my knowledge, to date, only two papers identify the cyclical variation in hiring wages while controlling for cyclical up- and downgrading in employer/employee matches: Martins et al. (2012) and Carneiro et al. (2012). Both papers use the same matched employer-employee dataset for Portugal but consider time periods and different unemployment rates. Martins et al. (2012) use the 1982–2008 period, whereas Carneiro et al. (2012) use the shorter 1986–2007 period. Moreover, they employ different methodologies to identify the cyclical variation in wages.

Martins et al. (2012) identify entry jobs within firms, track the real wage paid to newly hired workers in those jobs, and measure how the entry wages vary over the business cycle. Their analysis employs a two-stage regression. In the first stage, they estimate a period fixed effect common to all entry jobs, where the endogenous variable is the log of the "typical" (e.g., modal) real wage of a job. In the second stage, they estimate the cyclicality of entry wages using regressions of the time series of the period fixed effect common to all entry jobs—from the first stage—on the unemployment rate and secular time trends as controls. Martins et al. (2012) find that a 1 percentage point (pp) increase in the unemployment rate leads to a 1.8% decrease in real wages for newly hired workers within given firm-jobs.

Carneiro et al. (2012) estimate the real wage cyclicality of newly hired workers and incumbent workers in a one-stage regression. They regress individual log real wages on the unemployment rate, a new-hire dummy variable, the unemployment rate interacted with the new-hire dummy variable, time-varying individual characteristics, and secular time trends as controls. They further control for worker, job title, and firm fixed effects.

Carneiro et al. (2012) find that a 1 pp increase in the unemployment rate leads to a 2.67% decrease in real wages for newly hired workers.[4]

3 Data Description and Data Selection

The empirical analysis on Germany is conducted for the 1977–2009 period using the IAB Beschäftigten-Historik (BeH), the Employee History File of the Institute for Employment Research (IAB) of the German Federal Employment Agency. The BeH comprises all workers who are gainfully employed and covered by the social security system. The BeH does not consider self-employed, family workers assisting in the operation of a family business, civil servants (Beamte), and regular students. The BeH covers approximately 80% of the German workforce. For the 1975–2009 period, the BeH contains data on 75 million workers in 9.11 million firms (IAB, 2011).[5] Workers from East Germany are included from 1992 onwards. Important advantages of the BeH are the enormous amount of information and high degree of reliability of the earnings data, which is due to plausibility checks performed by the social security institutions and the existence of legal sanctions for

[4]For incumbent workers, Carneiro et al. (2012) find that a 1 pp increase in the unemployment rate decreases real wages by approximately 2.2 percent.

[5]Because of certain selection criteria described in Sections 3.1 and a number of data inconsistencies in the first years of the BeH (see Appendix A.1) the analysis can only be conducted for the 1977–2009 period. Data from earlier years are used to identify newly hired workers.

misreporting. Measurement errors due to erroneous reporting should be much lower than in household surveys. Additionally, the BeH allows workers to be matched to firms, which is crucial to control for cyclical up- and downgrading in employer/employee matches, i.e., by controlling for firm-job fixed effects.

3.1 Data Selection and Identification Strategies

To create the dataset for the empirical analysis, I first identify all firms that employed at least seven workers[6] in at least one year in the 1975–2009 period. I then identify all full-time workers in those firms. For each identified worker, I draw all existing employment spells for the 1975–2009 period, including part-time employment and apprenticeships. The obtained dataset contains data on 59,711,757 workers in 1,635,679 firms.[7] It is used to identify newly hired workers (job entrants).

3.1.1 Definition of Jobs and Identification of Job Entrants

I define jobs within firms in terms of three-digit occupation codes[8] (such as bookkeeper, barber, and pharmacist) and further require that all workers in a job are at the same "job level" (cf. Martins et al., 2012). For job level, I use a four-category variable coded as blue-collar worker/no craftsman, craftsman/skilled laborer[9], master craftsman[10], and white-collar worker/salaried employee. Thus, I create unique job identifiers that consist of the firm identification number, occupation, and job level.

To identify newly hired workers, I use the individuals' employment spells. An individual is a job entrant if he/she worked at a different firm before (firm change)—and therefore in a different job—or if the individual has not worked (s.t. social security) at the same firm in the last 365 days. The second condition ensures that workers suspending their employment for a short period of time—for whatever reason—are not counted as job entrants when they return to the firm. Workers that change jobs within a firm are not identified as job entrants either.

[6] A worker must be subject to social security contributions without any specific tokens. The number of workers is evaluated at June 30 of each year.

[7] I checked the data for inconsistencies and excluded a small number of spells. The procedure and the inconsistencies found are provided in the Appendix A.1.

[8] The BeH covers 86 occupation groups containing 328 occupations. Spells without information on the occupation are dropped.

[9] This class also contains some master craftsmen and foremen, see Bender et al. (1996).

[10] Persons in this class can be employed as either blue- or white-collar workers.

3.1.2 Data Selection

After the identification of job entrants I select my estimation sample which is mostly defined by features of the BeH:

1. I use data for West Germany from 1977 onwards and for East Germany from 1993 onward.[11]

2. The BeH does not contain hourly wages. To minimize contamination with working-time effects, only full-time workers are considered in the analysis.[12]

3. As the earnings data are right censored at the contribution assessment ceiling[13] ("Beitragsbemessungsgrenze"), only non-censored wage spells are considered in the analysis. I apply consistent top-coding instead of simply dropping the censored wage spells.[14] Applying consistent top-coding has the advantage that the same fraction of the wage distribution is considered in the analysis throughout the sample period. I calculate the percentage of individuals subject to top-coded (censored) wages in every year. I identify the threshold for the top-coding separately for West Germany and East Germany, for which the highest percentage of spells are censored in the year 1992 (8.33%) and 2002 (6.99%), respectively. Therefore, in each year, I drop the 8.34%/7% highest wage spells for West/East Germany.[15]

4. I restrict the dataset to workers aged 16 to 65 years old.

[11] For the years 1975 for West Germany and 1992 for East Germany, I cannot apply the identification strategy for job entrants described above. Therefore, I cannot use the data in the empirical analysis. I also exclude observations for Berlin for all years before 1993 for the following reasons. First, West Berlin consistently had a special status prior to the reunification of Germany—West Berlin was highly subsidized and the labor market was not comparable to the labor market of the rest of West Germany. Second, in 1992 observations for Berlin cannot not be distinguished between East and West Berlin. Additionally, due to some data inconsistencies concerning firm assignment in 1976, the data for the year 1976 are not used for the empirical analysis but are used to identify job entrants.

[12] The BeH contains eight classes of workers. In the regressions I do not consider trainees, home workers, people with less than 18 hours of work per week, and people with 18 or more hours of work per week but who are not fully employed. Furthermore, the BeH contains 32 classifications for employment relationships, such as trainees, insured artists, and publicists, and employees in partial retirement. I only consider employees subject to social security contributions without particular tokens.

[13] The contribution assessment ceiling is annually adjusted to the changes in earnings (see Table 13 in Appendix A.3). Some employees—miners, mine-employees, sailors, and railroad employees—are insured by the so-called "knappschaftliche" pension insurance. The contribution assessment ceiling of this pension insurance is always higher than for the compulsory pension insurance scheme. Beginning in 1999, the BeH no longer indicates which pension insurance covers person. For this reason, I use the contribution assessment ceiling of the compulsory pension insurance scheme.

[14] See Burkhauser et al. (2004) for an introduction on consistent top-coding and Feng et al. (2006) for a discussion of the application on this method to labor earnings.

[15] Dropping top-coded spells leads to an underrepresentation of highly qualified (white-collar) workers, making the results somewhat less generalizable. For a quantitative evaluation of the effect of dropping censored spells see, e.g., Appendix A of Stüber and Beissinger (2012).

Furthermore, I only consider jobs in the dataset that could be observed in at least three years of the 1977–2009 period. This selection criterion is necessary to ensure that wages are observed in multiple years, which is essential for the empirical analysis.

As a robustness check I also apply much stricter sample selection criteria according to Martins et al. (2012). However, applying these further selection criteria (FSC) affects the regression results only minimally. The FSC are outlined in Appendix A.2, and the regression results obtained using this dataset are provided in Appendix A.4.

3.2 Description of Variables and Descriptive Overview of the Final Data Samples

In the empirical analysis, I analyze how changes in the unemployment rate affect the real wages of newly hired workers. As the endogenous variable, I use the typical real wages of entry jobs (following Martins et al., 2012) and alternatively individual real entry-wages (following Carneiro et al., 2012).

Employers have to report to the social security system annually. Therefore, the BeH data do not contain monthly or hourly wages but the wages[16] paid during the duration of an employment spell. Therefore, I cannot observe the wage of the first month of employment. However, because the exact duration of each employment spell is known, I can calculate the average daily wage for each spell. The first employment spell of a newly hired worker lasts for at most one year—January 1 to December 31. I also control for the different lengths of employment spells when using individuals' wages. I use the Consumer Price Index (CPI) to calculate the average daily real wage (in 2005 prices).[17]

I quantify the typical real entry-wage w_{jt} using either the modal or the mean average daily real wage paid to workers newly hired into job j in period t. By using the modal wage, some information is lost due to multiple modes. Summary statistics are provided in Table 1.

Alternatively, I use the individual average daily real wage w_{ijt} paid in period t to worker i newly hired for job j. Summary statistics are provided in Table 2. For the regressions I draw for each year a random 1% sample of the jobs (stratified by the number of entrants per job). For each drawn job, I retain all employment spells from the 1977–2009 period. With respect to the number of job entrants, this leads roughly to a bisection of the original

[16] Before 1984, including fringe benefits in disclosures was voluntary. Since 1984, one-time payments to employees have been subject to social security taxation and are therefore included in the data.

[17] Before I calculate the log real daily wage, I round the daily nominal wage to the second decimal place.

dataset: of the 122,180,828 job entrants, 59,863,251 are excluded, reducing the dataset to 62,317,577 employment spells of newly hired workers. Table 11 (see Appendix A.3) presents the sample sizes by year for this sub-sample.

Table 1: Number of entry jobs per year using the "typical" daily real entry-wage as endogenous variable

	Number of entry jobs per year using	
	Real mean wage	Real modal wage
Mean	1,122,075	631,226
Min	749,063	448,963
Max	1,377,595	775,498
Sum	37,029,491	20,830,454

Table 2: Number of job entrants per year using the individual daily real wage as endogenous variable

	Number of job entrants per year
Mean	3,702,449
Min	2,400,124
Max	4,745,060
Sum	122,180,828

Table 3: Exogenous variables used in regressions using individuals' wages

Qualification level of the employee (education)	This variable includes eight categories: no formal education, lower secondary school and intermediate (secondary) school without vocational qualification, lower secondary school and intermediate (secondary) school with vocational qualification, upper secondary school examination without vocational qualification, upper secondary school examination with vocational qualification, post-secondary technical college degree, university degree, and no classification applicable. Base category: lower secondary school and intermediate (secondary) school with vocational qualification. 14.8% (11.9%) of the spells of the dataset (with FSC, see Appendix A.2) have missing information on the qualification level of the employee. Therefore, I do not use the genuine variable but an imputed variable. I apply a slightly altered version of the imputation algorithm introduced by Fitzenberger et al. (2005) for the IAB employment sub-sample (IABS). Using the imputed variable only 0.9% of the spells have missing information on the qualification level of the employee.
Sex	Dummy for female workers. Base category: male worker.
Age, Age^2	Age a person is turning in the particular year.
Nationality	Dummy for worker with foreign nationality. Base category: German.
Length of the employment spell	Length of the first employment spell of a worker in a new job: 1 month $\leq$ length of employment spell $\leq$ 12 month.

The exogenous variables are presented in Table 3. I provide further information on the data in Appendix A.3. Table 12 provides statistics for the different years, information on the number of job entrants using the typical daily real entry-wage, and the number of entry jobs using the individual daily real wage as the endogenous variable. Table 13 (see Appendix A.3) provides the unemployment and inflation rates.

4 Empirical Analysis

4.1 Models

To estimate the cyclicality of real entry-wages over the business cycle, I identify particular jobs within firms. I track the wages paid to newly hired workers in firm-jobs and measure how the entry wages vary over the business cycle. By defining particular jobs within particular firms, each job is actually a firm-job combination (see Section 3.1.2). I follow Martins et al.'s (2012) methodology and apply two-stage regressions.[18] However, for the endogenous variable, I follow both Martins et al. (2012) and Carneiro et al. (2012), using both the typical real wages of entry jobs and the job entrants' individual real wages.

I apply three models to estimate the cyclicality of entry wages. Table 4 provides an overview of these models, which only differ with respect to the first-stage regressions.

Table 4: Overview of the regression models

Model	Endogenous variable	Job fixed effects	Worker fixed effects	Individual controls
1	"typical" real wages of entry jobs	yes	no	no
2	job entrants' real wages	yes	no	yes
3	job entrants' real wages	yes	yes	yes

4.1.1 Model 1

In model 1, I analyze how typical real wages are related to changes in the unemployment rate. I follow Martins et al. (2012) and estimate the cyclicality of entry wages with a two-stage regression. I estimate period fixed effects common to all entry jobs, β_t, in the first stage of the analysis, and I relate them to business cycle conditions in the second stage. The period fixed effects, β_t, are estimated by

$$\ln\left(w_{jt}\right) = \alpha_j + \beta_t + \varepsilon_{jt}, \tag{1}$$

[18] The unemployment rate—the regressor of interest—only varies between years. Regarding the estimation of the standard errors, I prefer a two-stage regression to a one-stage regression—even if one controls for year clusters in the one-stage regression. A discussion of clustering and serial correlation in panels can be found in, e.g., Angrist and Pischke (2009, chapter 8.2).

where w_{jt} denotes the typical real wage paid in period t to workers newly hired for job j, e.g., the modal real wage. α_j is a job fixed effect, and ε_{jt} is the error term with mean zero representing temporary job-specific departures from the general period effect.

To quantify the cyclicality of entry wages, in the second stage, I regress the estimated time series of β_t $\left(\hat{\beta}_t\right)$ on the unemployment rate u_t, controls for secular time trends, and a dummy that takes the value one for 1984 and every following year ($D_{\geq 1984}$):

$$\hat{\beta}_t = \delta u_t + \lambda_0 t + \lambda_1 t^2 + D_{\geq 1984} + \varepsilon_t. \tag{2}$$

The dummy $D_{\geq 1984}$ is introduced because the BeH does not allow fringe benefits to be distinguished from regular earnings. Before 1984, including fringe benefits in disclosures was voluntary. Since 1984, one-time payments to employees have been subject to social security taxation and are therefore included in the data.[19]

4.1.2 Models 2 and 3

In models 2 and 3, I analyze how the real wages of newly hired workers are affected by changes in the unemployment rate (following Carneiro et al., 2012). Using the individual wages as the endogenous variable makes it possible to control for individual worker characteristics and characteristics of the employment relationship, e.g., the length of the employment spell. As described in Section 3.2, the BeH does not provide monthly wages but wages for employment spells. The daily wage is calculated using the worker's first employment spell. The length of the worker's first employment spell can differ between one day and one year depending on the beginning of the period of employment. As the wage may include fringe benefits, this potential difference in length could cause some noise in the wage data. For example, the Christmas bonus is often only paid to workers that are employed at the end of the year and/or for at least a certain amount of the year. Model 2 (see Equation 3) makes it possible, inter alia, to control for this data issue by controlling for the length of the employment spell:

$$\ln\left(w_{ijt}\right) = \alpha_j + \beta_t + \gamma' \mathbf{x}_{it} + \varepsilon_{ijt}, \tag{3}$$

where w_{ijt} denotes the real wage paid in period t to worker i newly hired for job j and $\mathbf{x}_{it}$ is a vector of the individual characteristics of worker i in period t (see Table 3). To

[19] However, observations before 1984 should also be valid. If some employers reported fringe benefits before 1984 and others did not, it is very likely that employers were typically consistent in their reporting behavior. The obligation of reporting fringe benefits leads to a level effect on wages from 1984 onwards that I control for with the $D_{\geq 1984}$ dummy.

quantify the cyclicality of entry wages, I regress, as in model 1, the $\hat{\beta}_t$ time series on u_t, controls for secular time trends, and $D_{\geq 1984}$ (see Equation 2).

Several studies (e.g., Keane et al., 1988) demonstrate that a failure to control for unobserved heterogeneity leads to countercyclical biases. Therefore, the estimates of model 2 are likely biased countercyclically. Therefore, in model 3 (see Equation 4), I additionally introduce worker fixed effects. As I am only analyzing the wages of newly hired workers, I do not observe all workers frequently enough to introduce person fixed effects using the original sample (described in Section 3.2). This insufficiency is especially true for earlier birth cohorts where individuals often worked for only one employer in their working life. Therefore, I draw a sub-sample for the analysis that only includes workers that start at least five jobs during the observed time period. Furthermore, I require that these jobs be observed for at least five years.[20] The estimates of model 2 are used to demonstrate that the results of model 3 are not driven by the selection criteria used to obtain this sub-sample.

For model 3, I estimate linear, two-way fixed-effects, as in Abowd and Kramarz (1999):[21]

$$\ln(w_{ijt}) = \alpha_i + \alpha_j + \beta_t + \gamma^{'}\mathbf{x}_{it} + \varepsilon_{ijt}, \tag{4}$$

where α_i is a newly introduced worker fixed effect. To quantify the cyclicality of entry wages, I regress the estimates of the $\hat{\beta}_t$ time series on u_t, controls for secular time trends, and $D_{\geq 1984}$, as in the first two models (see Equation 2).

4.2 Results

The results for model 1 indicate, that the estimated coefficients of the unemployment rate differ only slightly depending on the typical real entry wage used in the analysis and the choice of regression model (see Table 5).[22] A 1 pp increase in the unemployment rate

[20] Further details on the sub-sample are provided in Section 4.2.

[21] I use the Stata ado file “a2reg” by Ouazad (2007).

[22] Martins et al. (2012, p. 44, Figure 3) present a sample distribution of differences between individual workers’ log wages and modal log wages per job/year. For the Portuguese data—with hourly wages—the modal wage appears to be a good measure. For Germany, the typical log wages differ more from the individual workers’ log wages than in Portugal, likely because the BeH provides daily and not hourly wages. Distributions of the differences between individual workers’ log wages and typical log wages are displayed in Figure 1 in Appendix A.5. The differences between typical wages and individual workers’ wages appears to be stronger for the dataset with FSC (right panel of Figure 1). This initial visual impression is also supported by the simple summary statistics (see Table 19 in Appendix A.5). The difference between individual workers’ log wages and the modal log wages for the dataset with FSC has a variance that is approximately twice as high as for the other measures.

decreases the wages of job entrants within given firm-jobs by between 0.92% to 1.03%.[23] The differences are not statistically significant at the 5% level.

Table 5: Model 1—estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using "typical" real entry-wages

	Modal wage	Mean wage
(1.0) 1st and 2nd stage unweighted OLS	-1.03^{***}	-0.94^{***}
	(0.35)	(0.34)
(1.1) 1st stage unweighted OLS, 2nd stage OLS weighted	-1.00^{***}	-0.92^{***}
by number of entry jobs per year	(0.34)	(0.33)

Note: *** Significant at 1% level; ** 5% level.

Robust standard errors in brackets. Jobs are observed at least 3 years of the 1977 to 2009 period. Further controls used: secular time trend controls (t and t^2) and a dummy for years $\geq$ 1984.

I estimate unweighted ordinary least squares (OLS) regression models in both stages in regression (1.0). I use weights according to Martins et al. (2012) in regression (1.1): whereas the first stage uses unweighted OLS, the second stage OLS is weighted by the number of observed entry jobs per year. Martins et al. (2012, p. 45) use these weights "in an effort to correct for the heteroskedasticity resulting from the wide variation in the per-year sample size". However, the per-year sample in the German BeH data varies only slightly (see Table 13 in Appendix A.3).[24] Thus, weighting the second-stage regressions appears unnecessary. A comparison of the estimates of model (1.0) and (1.1) demonstrates that, the weighting does not significantly affect the results.

The results of model 2 (see Table 6)—using individual real wages instead of typical real entry wages—are quite similar those of model 1. A 1 pp increase in the unemployment rate decreases the real entry wages of job entrants within given firm-jobs by 0.83–0.90%.[25]

[23]Moreover, whether FSC are used only slightly affects the estimated coefficients of the unemployment variable (see Appendix A.4). Therefore, the selection criteria from Martins et al. (2012) do not appear to influence the outcomes of the regressions.

[24]In Martins et al. (2012) the minimum number of entry jobs (newly hired workers) per year is 5.9 (11.1) times lower than the maximum. The differences in Germany are much smaller—the minimum number of entry jobs (newly hired workers) per year is 1.8 (2.0) times lower than the maximum.

[25]Some robustness checks for the regressions in Tables 5 and 6 are provided in Tables 16, 17, and 18, respectively, in Appendix A.4.

Table 6: Model 2——estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using individual real wages

(2.0) 1st stage unweighted OLS, 2nd stage OLS unweighted	-0.83^{***}
	(0.27)
(2.1) 1st stage unweighted OLS, 2nd stage OLS weighted by number job entrants per year	-0.90^{***}
	(0.28)

Note: *** Significant at 1% level.

Robust standard errors in brackets. Jobs are observed at least 3 years of the 1977 to 2009 period. Further controls used: secular time trend controls (t and t^2) and a dummy for years $\geq$ 1984. Individual controls used in the 1st stage regression: education, sex, nationality, age, age^2, and length of the employment spell.

As several papers demonstrate (e.g. Keane et al., 1988) that failure to control for unobserved heterogeneity produces a countercyclical bias, I introduce worker fixed effects in model 3. The introduction of the worker fixed effects makes it possible to better control for worker heterogeneity. As mentioned above, the dataset used for models 1 and 2 is not optimally suited for this type of regression.

Thus, I draw a sub-sample of employment spells for workers that begin at least five jobs during the observed time period. Furthermore, these jobs must be observed in at least five years in the 1977–2009 period. Due to this sampling decision, the dataset is reduced from 62,317,577 to 10,335,054 employment spells of job entrants.[26] To test whether the sampling affects the results, I re-run the regression in model 2 (see Table 6) using the sub-sample (see Table 7). The estimated coefficients of the control regressions (3.0 and 3.1) have approximately the same magnitudes as the estimated coefficients using the original sample (see Table 6). Therefore, it appears that using the sub-sample for the regressions does not meaningfully affect the results.

Controlling for worker fixed effects, a 1 pp increase in the unemployment rate decreases the real entry wages of job entrants within given firm-jobs by approximately 1.27%. Comparing the results of the control regressions (3.0 and 3.1) with the results of the linear,

[26] The dataset consists of 10,335,054 employment spells of 1,541,300 workers working in 230,722 different jobs.

two-way fixed-effects regressions (3.2 and 3.3) shows that failing to control for worker fixed effects leads to an underestimation of entry wage cyclicality, as expected.

Table 7: Model 3—estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using individual real wages

Control reg. w/o worker fixed effects (WFE)	(3.0) like (2.0): 1st stage unweighted OLS, controlling for job fixed effects (JFE), 2nd stage unweighted OLS	Ind. controls in 1st stage reg.: (a) and (b)	-0.82^{***} (0.24)
	(3.1) like (2.1): 1st stage unweighted OLS, controlling for JFE, 2nd stage OLS weighted by number job entrants per year		-0.84^{***} (0.22)
a2reg-reg. with WFE	(3.2) 1st stage unweighted linear two-way fixed-effects reg., controlling WFE and JFE, 2nd stage unweighted OLS	Ind. controls in 1st stage reg.: (b)	-1.26^{***} (0.25)
	(3.3) 1st stage unweighted linear two-way fixed-effects reg., controlling for WFE and JFE, 2nd stage OLS weighted by number job entrants per year		-1.27^{***} (0.23)

Note: *** Significant at 1% level.

Robust standard errors in brackets. Further controls used: secular time trend controls (t and t^2) and a dummy for years $\geq$ 1984. Individual controls used in the 1st stage regression: (a) education, sex, nationality, and (b) age, age^2, and length of the employment spell.

I only use wage spells of job entrants which I observe at least 5 times and the jobs must be observed in at least 5 years in the 1977 to 2009 period. Due to the sampling the dataset is reduced from 62,317,577 to 10,335,054 employment spells of job entrants.

In the next section, I discuss the regression results presented above and comment on the question of whether introducing wage rigidity in the Mortensen-Pissarides search-and-matching model to generate realistic unemployment volatility is a sound strategy in light of the empirical evidence.

5 Discussion of the Results

The estimated coefficients for the unemployment rate displayed in Tables 5 and 6 are in the general vicinity of -0.94 and the estimated coefficients are not significantly different from each other at the 5% level. Additionally, controlling for worker fixed effects results in a higher estimate for the wage cyclicality of approximately -1.27 (see Table 7).

5.1 Evaluation of the Regression Models

Using typical real wages has a disadvantage: it does not allow individual and employment characteristics to be controlled. However, given the German wage data, controlling for the length of the wage spell could be important, as the dataset provides average daily wages. For instance, the Christmas bonus is often only paid to workers that are employed for at least a certain portion of the year. Not controlling for the length of the wage spell could lead to biased results. Therefore, it appears that given the German data, the individual worker's log wage is typically better suited for the regressions.

However, using the individual real wage also has disadvantages. It implies that one is weighting by the hiring volume, which might be endogenous. If the wages of certain jobs are more rigid than those of others, then it could be that firms hire fewer workers for jobs with more rigid wages during recessions. This behavior would likely produce a procyclical bias in the wage analysis.[27]

As the estimates of model 3—using individual wages—are likely procyclically biased, one could argue that model 1 should be preferred over model 3.[28] However, the estimates of model 1 are also likely biased.

As Solon et al. (1994) note, using aggregate time-series data instead of longitudinal microdata leads to an underestimation of wage cyclicality due to the "compositional bias" in aggregated statistics. Skilled workers tend to retain their jobs during recessions; therefore, low-skilled workers account for a smaller share of employment in recessions than in booms (see, e.g., Bils, 1985; Mitchell et al., 1985; Keane et al., 1988; Solon et al., 1994), which causes a composition bias if aggregated wage data are used in the analysis: "the aggregated statistics are constructed in a way that gives more weight to low-skilled workers during expansions than during recessions." (Solon et al., 1994, p. 1) The general problem of using aggregated data is also mentioned by Bils (e.g., 1985, p. 667): "Aggregation also involves a loss of information and therefore of estimating efficiency." Typical wages are aggregated individual wages and information is lost. Using aggregated data instead of microdata should lead to an underestimation of wage cyclicality.

Moreover, using typical wages does not allow changes in the workforce and/or employment shares to be controlled. However, as Mitchell et al. (1985, p. 1162) note, the "composition of the labor force may change considerably over the course of the business cycle."[29] Using the typical wage further assumes that the number of hires in all jobs is identical and stable over time—it does not control for changes in the share of hires caused by, e.g., technological advances. For example, the share of less-trained workers within firms could decrease over time due to the introduction of new machines, whereas the share of engineers increases over time because more manpower is needed to maintain the machines. Furthermore, using the typical wage also does not allow cyclicality in job

[27] I would like to thank Gary Solon for identifying this issue.

[28] I do not discuss the quality of the results using model 2, as the results are primarily used for robustness checks.

[29] Moreover, human capital theory predicts (see, e.g., Becker, 1964) that the employment shares of different demographic groups will vary over the business cycle.

assignments to be controlled. However, Mitchell et al. (1985) find that the work force in the USA becomes younger over time. Mitchell et al.'s (1985, p. 1167) "results indicate that employment shares are not constant over time, but are influenced by the state of the economy, relative population growth, and time."

Moreover, cyclical upgrading may still cause an underestimation of the true procyclicality of entry wages, especially in model 1. An underestimation of the true procyclicality of entry wages could occur if employers were able to recruit more qualified workers at any given wage during a recession. Solon et al. (1997) state that firms might reduce hiring standards during a boom to increase employment while holding entry wages stable. "Such cyclicality in job assignments could cause the real wages of the firm's worker to be procyclical even if wages by job are sticky." (Solon et al., 1997, p. 403) Such behaviour would lead to a lower effective wage per efficiency unit of labor and an underestimation of wage cyclicality. Büttner et al. (2010) demonstrate that occupational upgrading and downgrading—occupations as units defining homogenous skill requirements—exist in West Germany. According to their results, the skill levels of new hires within occupations rise significantly during recessions and decreases during upturns, however, the effect only amounts to approximately 70% of the corresponding result for the USA.[30] Given the results of Büttner et al. (2010), the procyclicality of entry wages estimated in this paper should be only slightly underestimated. This insignificance should especially be true for model 3, where I control for the qualification level (education) of the employee.

In summary, considering typical wages and individual wages appears to produce biased estimates. Using individual wages likely produces a procyclical bias, whereas using typical wages likely produces a countercyclical bias. Therefore, I do not prefer any methodology to the other but suggest using both methodologies to obtain a range of estimates for the cyclicality of real entry wages.

The point estimate of model 1—regression (1.1)—provides a lower-bound estimate, and the point estimate of model 3—regression (3.3)—provides an upper-bound estimate. Thus, a 1 pp increase in the unemployment rate leads to a decrease in real wages of 0.92–1.27%. The true parameter should lie within this range. This assumption appears to be justified, as the estimates are not significantly different from each other at the 5% level.

[30] For an analysis of the heterogeneity of the cyclical sensitivity of job-to-job flows in Germany, see, e.g., Schaffner (2011).

5.2 Implications of the Results

The estimated coefficients for the unemployment rate—using individual wages and controlling for job and worker fixed effects—are in the general vicinity of -1.27 (see Table 7). Bearing in mind that if participation in the labor force is procyclical, "the negative of the change in the unemployment rate is an attenuated version of proportional changes in employment" (Martins et al., 2012, p. 48) implies that the cyclical elasticity of entry wages should have the same magnitude as the cyclical elasticity of employment. To determine whether this hypothesis can be confirmed empirically, I follow Martins et al. (2012) and estimate Okun's Law-style relationships for the 1977–2009 period. To control for the reunification of Germany, I introduce a dummy, $D_{\geq 1991}$, which takes the value of one for 1991 onward.

$$\Delta u = \alpha_1 + \beta_1 \log\left(\Delta GDP_{real}\right) + t + D_{\geq 1991} \tag{5}$$

$$\Delta \log\left(\frac{employment}{population}\right) = \alpha_2 + \beta_2\left(\Delta GDP_{real}\right) + t + D_{\geq 1991} \tag{6}$$

I find that a 1 pp increase in the unemployment rate is associated with a 1.27% decline ($\beta_2/\beta_1 = -1.27$) in the employment/population ratio. This procyclicality of employment is (nearly) identical to the procyclicality that I estimated for real entry wages using model 3 (see Table 7). However, compared to the results of model 1, employment is slightly more procyclical than the procyclicality estimated for real entry wages (see Tables 5).

Finally, I address the question of whether the Mortensen-Pissarides model can account for the cyclical variability of unemployment in light of the magnitude of the entry wage cyclicality found for Germany. I draw on the results of Kennan's (2010) model (cf. Martins et al., 2012) as a reference point for the real wage rigidity that is required in search-and-matching models to generate realistically large cyclical fluctuations in unemployment. When Kennan (2010) calibrates his modification of the Mortensen-Pissarides model (the informational rent model), most of his calibrations match the empirical variation in the unemployment rate if he assumes that the real hiring wage declines by less than 0.68% when the unemployment rate rises by 1 pp (see Table 8).

Table 8: Wage volatility in Kennan's (2010) informational rent model

	Wage change in percent—from life match begins in a bad state (w_1) to life match begins in a good state (w_2)—given a one percentage drop of the (long run) unemployment rates, assuming...	
	... symmetric Cobb-Douglas matching function ($\nu = 0.5$)	... labor share and matching elasticity parameter used by Shimer ($\alpha = \nu = 0.72$)
Wages: flat rates[a]	0.43	0.19
Wages: non-decreasing rates[b]	1.52	0.68

Notes: Source: Results are taken from Kennan (2010, Tab. 2, p. 650). Values converted to an unemployment change of one percentage point.

[a] The flat rate wage is given by $w_s = RW_s$. Where W_s is the present value of wages, and s represents the state: life match begins in a bad state ($s = 1$) or good state ($s = 2$). $R = r + \delta$, where r is the interest rate and δ is the (constant) job destruction hazard rate.

[b] The non-decreasing rate wage "is constant for the life of the match if the match begins in the good aggregate state, with a lower wage initially for matches that begin in a bad state [$s = 1$], followed by a wage increase when there is a transition to the good state [$s = 2$]." (Kennan, 2010, p. 648) The flow wages are given by $w_1 = w_2 - (R + \lambda_1)(W_2 - W_1) = RW_1 - \lambda_1(W_2 - W_1)$ and $w_2 = RW_2$. Where w_1 (w_2) represents the wage if a life match begins in a bad (good) state.

As my estimates (using model 3) show a 1.27% decline in real hiring wages when the unemployment rate rises by 1 pp, it apperas that the Mortensen-Pissarides model cannot account for the cyclical variability of unemployment in light of the magnitude of the entry wage cyclicality found for Germany. This result is also supported up by the lower-bound estimates of model 1: I still find a decline in real hiring wages of approximately 0.92% when the unemployment rate rises by 1 pp.

6 Conclusions and Outlook

Using longitudinal, matched employer-employee data from the IAB, I have tracked the cyclical behavior of the real wage paid to newly hired employees in over one million jobs. My results demonstrate that entry wages in Germany are not rigid but respond significantly to business cycle conditions. Furthermore, I demonstrate that the procyclicality of the employment/population ratio in Germany is (nearly) identical to the procyclicality of real entry wages.

Using the typical real wage of entry jobs, I obtain a lower-bound estimate for the cyclicality of real entry-wages: a 1pp increase in the unemployment rate leads an approximately 0.92% decrease in real entry wages. The regression results obtained using individual wages as the unit of observation and controlling for job and worker fixed effects suggest that a 1 pp increase in the unemployment rate leads to an approximately 1.27% decrease in real entry wages (upper bound). The true parameter should lie between the upper and lower bounds. This assumption appears to be justified, as the estimates are not significantly different from each other at the 5% level.

The results strengthen Pissarides' (2009) dismissal of theories based on cyclically rigid hiring wages. In light of the magnitude of the entry-wage cyclicality in Germany, it appears that introducing wage rigidity in the Mortensen-Pissarides model to generate realistic unemployment is not supported by the data. This lack of support challenges researchers to develop search-and-matching models that are able to generate realistic, e.g, unemployment, volatilities when considering the empirically documented real wage cyclicality.

However, it appears that real wages in Germany are less cyclical than in other countries. The two studies on Portugal that control for cyclical up- and downgrading in employer/employee matches find that a 1 pp increase in the unemployment rate decreases the real wages of job entrants by 1.8% (Martins et al., 2012) and 2.67% (Carneiro et al., 2012). Studies on the USA, which do not control for cyclical up- and downgrading in employer/employee matches, also find more procyclicality. For instance, Shin (1994) finds that a 1 pp increase in the unemployment rate decreases the real wages of white (black) job changers by 2.67% (3.80%).

As outlined in Section 4.2, controlling for worker fixed effects is problematic when analyzing job entrants alone. Therefore, future research on real wage cyclicality (in Germany) should analyze the real wage cyclicality of job entrants and incumbent workers simultaneously, as in Carneiro et al. (2012). However, this strategy is not without limitations. For example, Carneiro et al.'s (2012) model specification forces the wages of job entrants and the wages of incumbents to have an identical time trend, and as outlined in Section 5.1, the estimate is likely procyclically biased. Future research should also consider that the effect of a change in the unemployment rate on real wages might not be symmetric.[31] Thus, the results of regressions that do not allow for asymmetric reactions might be biased.

[31] For instance, Shin and Shin (2008, p. 13) demonstrate that for male job stayers in the USA, "wage growth in expansions [..] is much greater than wage reduction in recessions".

References

Abowd, J. and Kramarz, F. (1999). Econometric analyses of linked employer-employee data. *Labour Economics*, 6(1):53–74.

Angrist, J. and Pischke, J.-S. (2009). *Mostly harmless econometrics: an empiricist's companion*. Princeton University Press.

Becker, G. (1964). *Human Capital*. Columbia University Press.

Bender, S., Hilzendegen, J., Rohwer, G., and Rudolph, H. (1996). *Die IAB-Beschäftigtenstichprobe 1975-1990*, volume 197 of *Beiträge zur Arbeitsmarkt- und Berufsforschung (BeitrAB)*. Institut für Arbeitsmarkt- und Berufsforschung (IAB) der Bundesanstalt für Arbeit.

Bils, M. (1985). Real wages over the business cycle: evidence from panel data. *The Journal of Political Economy*, 93(4):666–689.

Bjelland, M., Fallick, B., Haltiwanger, J., and McEntarfer, E. (2011). Employer-to-employer flows in the United States: estimates using linked employer-employee data. *Journal of Business & Economic Statistics*, 29(4):493–505.

Büttner, T., Jacobebbinghaus, P., and Ludsteck, J. (2010). Occupational upgrading and the business cycle in West Germany. *Economics*, 4(2010-10).

Burkhauser, R., Butler, J., Feng, S., and Houtenville, A. (2004). Long term trends in earnings inequality: what the CPS can tell us. *Economics Letters*, 82(2):295–299.

Carneiro, A., Guimarães, P., and Portugal, P. (2012). Real wages and the business cycle: accounting for worker, firm, and job title heterogeneity. *American Economic Journal: Macroeconomics*, 4(2):133–152.

Devereux, P. (2004). Cyclical quality adjustment in the labor market. *Southern Economic Journal*, 70(3):600–615.

Devereux, P. and Hart, R. (2006). Real wage cyclicality of job stayers, within-company job movers, and between-company job-movers. *Industrial and Labor Relations Review*, 60(1):105–119.

Doeringer, P. and Piore, M. (1970). *Internal labor markets and manpower analysis*. U.S. Department of Health, Education & Welfare, Office of Education.

Feng, S., Burkhauser, R., and Butler, J. (2006). Levels and long-term trends in earnings inequality: overcoming current population survey censoring problems using the GB2 distribution. *Journal of Business and Economic Statistics*, 24(1):57–62.

Fitzenberger, B., Osikominu, A., and Völter, R. (2005). Imputation rules to improve the education variable in the IAB employment subsample. *FDZ Methodenreport*, 3.

Gartner, H., Merkl, C., and Rothe, T. (2012). Sclerosis and large volatilities: two sides of the same coin. *Economic Letters*, 117(1):106–109.

Gertler, M. and Trigari, A. (2009). Unemployment fluctuations with staggered nash wage bargaining. *The Journal of Political Economy*, 117(1):38–86.

Haefke, C., Sonntag, M., and Rens, v. (2012). Wage rigidity and job creation. *CEPR Discussion Paper*, 8968.

Hagedorn, M. and Manovskii, I. (2013). Job selection and wages over the business cycle. *The American Economic Review*, 103(2):771–803.

Hall, R. (2005). Employment fluctuations with equilibrium wage stickiness. *The American Economic Review*, 95(1):50–65.

Hall, R. and Milgrom, P. (2008). The limited influence of unemployment on the wage bargaining. *The American Economic Review*, 98(4):1653–1674.

Hart, R. and Roberts, J. (2011). Job re-grading, real wages, and the cycle. *IZA Discussion Paper*, 5912.

IAB (2011). *IAB Beschäftigtenhistorik (BeH) V08.05.00.* Institute for Employment Research (IAB).

Keane, M., Moffitt, R., and Runkle, D. (1988). Real wages over the business cycle: estimating the impact of heterogeneity with micro data. *Journal of Political Economy*, 96(6):1232–1266.

Kennan, J. (2010). Private information, wage bargaining and employment fluctuation. *The Review of Economic Studies*, 77(2):633–664.

Kerr, C. (1954). The balkanization of labor markets. In Bakke, E., Hauser, P., Palmer, G., and Myers, C., editors, *Labor mobility and economic opportunity*, pages 92–110. Technology Press of MIT, Cambridge.

Martins, P., Solon, G., and Thomas, J. (2012). Measuring what employers do about entry wages over the business cycle: a new approach. *American Economic Journal: Macroeconomics*, 4(4):36–55.

Mitchell, M., Wallace, M., and Warner, J. (1985). Real wages over the business cycle: some further evidence. *Southern Economic Journal*, 51(4):1162–1173.

Mortensen, D. and Pissarides, C. (1994). Job creation and job destruction in the theory of unemployment. *Review of Economic Studies*, 61(3):397–415.

Ouazad, A. (2007). Program for the estimation of two-way fixed effects. http://EconPapers.repec.org/RePEc:boc:bocode:s456942 (last accessed: June 4, 2012).

Pissarides, C. (2009). The unemployment volatility puzzle: is wage stickiness the answer? *Econometrica*, 77(5):1339–1369.

Reynolds, L. (1951). *The structure of labor markets*. Harper & Brother.

Schaffner, S. (2011). Heterogeneity in the cyclical sensitivity of job-to-job flows. *Zeitschrift für ArbeitsmarktForschung*, 43(4):263–275.

Shimer, R. (2004). The consequences of rigid wages in search models. *Journal of the European Economic Association*, 2(2-3):469–479.

Shimer, R. (2005). The cyclical behavior of equilibrium unemployment and vacancies. *The American Economic Review*, 95(1):25–49.

Shin, D. (1994). Cyclicality of real wages among young men. *Economics Letters*, 46(2):137–142.

Shin, D. and Shin, K. (2008). Why are the wages of job stayers procyclical? *Macroeconomic Dynamics*, 12(1):1–21.

Shin, D. and Solon, G. (2007). New evidence on real wage cyclicality within employer-employee matches. *Scottish Journal of Political Economy*, 54(5):648–660.

Solon, G., Barsky, R., and Parker, J. (1994). Measuring the cyclicality of real wages: How important is composition bias. *The Quarterly Journal of Economics*, 109(1):1–25.

Solon, G., Whatley, W., and Stevens, A. (1997). Wage changes and intrafirm job mobility over the business cycle: Two case studies. *Industrial and Labor Relations Review*, 50(3):402–415.

Stüber, H. and Beissinger, T. (2012). Does downward nominal wage rigidity dampen wage increases? *European Economic Review*, 56(4):870–887.

Veracierto, M. (2008). On the cyclical behavior of employment, unemployment and labor force participation. *Journal of Monetary Economics*, 55(6):1143–1157.

A Appendix

A.1 Data Preparation

Overall, I rarely identified inconsistencies in the dataset, and most of the inconsistencies were identified in spells of part-time workers or workers who were not employed subject to social security contributions without specific tokens. These spells are only used to identify job entrants and are not used in the regressions.

The most common inconsistency that I observed were spells that were identical except for the end date of the spell and/or wage. These inconsistencies can occur if an employment contract of a worker is supposed to end in the middle of a year. If the employment contract is extended, the human resources department may have already reported the information regarding the end of the original employment contract to the social security administration. However, at the end of the year, the human resources department will again report information to the social security administration, this time for the full period that the worker was employed at the firm in that year. Such reporting can lead to two spells for a certain worker that are identical except for the end date of the employment. Occasionally, I observed that the longer spell was associated with a higher average daily wage because the Christmas bonus is often only paid to workers that are employed at the end of the year and/or for at least a certain share of the year. However, even these inconsistencies are observed very rarely compared to the substantial number of spells that are observed every year.

In the following, I will describe some of the corrections that I used to overcome the inconsistencies and obtain the dataset that I used to identify job entrants:

1. If I observed two or more identical spells, I retained only one of these spells.

2. If I observed spells that were identical except for one variable, I used, e.g., the following rules to decide which spell to retain:

   (a) spell a with wage $\neq 0$ and spell b with wage $\mathrm{A} = 0 \rightarrow$ retain spell a

   (b) wage of spell $a >$ wage of spell $b \rightarrow$ retain spell a

   (c) spell a ends after spell $b \rightarrow$ retain spell a

3. If I observed spells that were identical except for two variables, I used, e.g., the following rule to decide which spell to keep:

   (a) wage of spell $a \neq$ wage of spell b & spell a ends after spell $b \rightarrow$ retain spell a

A.2 Data Selection using the Selection Criteria of Martins et al. (2012)

In addition to the sample selection criteria described in Section 3.1.2—only retaining particular jobs that are observed in at least three years of the 1977–2009 period—I also apply sample selection criteria according to Martins et al. (2012). These "further selection criteria" (FSC) are very restrictive.

For the FSC dataset, I only consider newly hired workers at firms that employed at least 50 full time workers on June 30 in at least five years of the 1977–2009 period. Additionally, I only include a particular job in the sample of entry jobs if the following requirements are satisfied in at least half of the years that the firm is in the dataset:

1. the job accounted for at least three new hires of full-time workers in that year, and
2. the particular job accounted for at least 10% of the firm's new hires of full-time workers in that year.

Due to the FSC, jobs are only included in the sample that are observed for at least three years[32]. Martins et al. (2012) apply the FSC because they focus on so called "port-of-entry" jobs (see, e.g., Kerr, 1954; Doeringer and Piore, 1970). Martins et al. (2012, p. 41) "do not mean, however, to subscribe to [.. the] stark description in which firms hire into only a limited number of such jobs, with other jobs filled almost exclusively by internal promotions and reassignments. [. . . The] focus on jobs that recurrently show new hires [. . .] is driven mainly by a pragmatic concern—to identify cyclical variation in hiring wages by job, we need those wages to be observed in multiple years spanning different business cycle conditions."

Due to the very restrictive FSC, many jobs and a number of firms from the original dataset are excluded. Table 9 provides summary statistics and shows the effects of the FSC on sample sizes.

[32] Strictly speaking, two and a half years would be sufficient—the firm has to exist for at least five years, and the job must be observed in at least half of the years the firm is in the dataset.

Table 9: Number of entry jobs per year using the "typical" real entry-wage as endogenous variable

	Real mean wage		Real modal wage	
	Dataset		Dataset	
	with FSC	w/o FSC	with FSC	w/o FSC
Mean	54,205	1,122,075	11,137	631.226
Min	42,020	749,063	9,080	448,963
Max	62,340	1,377,595	13,470	775,498
Sum	1,788,777	37,029,491	367,529	20,830,454

Alternatively, I use the daily real wage w_{ijt} paid in period t to worker i newly hired for job j. Table 10 again provides summary statistics and illustrates the effects of the FSC on sample sizes.

Table 10: Number of job entrants per year using the individual daily real wage as endogenous variable

	Daily real wage	
	Dataset	
	with FSC	w/o FSC
Mean	932,513	3,702,449
Min	578,294	2,400,124
Max	1,270,840	4,745,060
Sum	30,772,919	122,180,828

A.3 Data Description and Data Selection—Further Tables

Table 11: Number of entry jobs and job entrants by year for the dataset with individual real wages without FSC and the drawn sub-sample of this dataset

Year	Individual real wages, dataset without FSC			
	Number of job entrants		Number of entry jobs	
	Sub-sample	Original dataset	Sub-sample	Original dataset
1977	1,822,918	3,577,107	217,583	962,528
1978	1,843,047	3,644,717	228,657	1,019,450
1979	2,154,174	4,180,031	245,901	1,112,191
1980	2,046,373	4,012,189	252,777	1,134,087
1981	1,752,155	3,470,701	246,583	1,075,261
1982	1,390,748	2,832,966	232,736	976,068
1983	1,348,089	2,710,091	230,645	949,209
1984	1,560,836	3,026,232	241,060	994,372
1985	1,631,436	3,091,450	245,109	998,811
1986	1,767,417	3,430,838	261,615	1,106,821
1987	1,689,074	3,246,381	258,972	1,066,650
1988	1,807,335	3,441,390	267,887	1,108,947
1989	2,100,055	3,956,568	283,842	1,198,174
1990	2,391,281	4,484,235	297,592	1,284,954
1991	2,246,769	4,304,481	295,368	1,277,104
1992	1,927,238	3,848,049	288,015	1,234,042
1993	2,056,169	4,355,962	301,181	1,343,865
1994	2,132,882	4,393,695	300,874	1,333,431
1995	2,249,038	4,543,150	309,126	1,377,595
1996	2,026,732	4,125,827	292,528	1,282,525
1997	2,041,771	4,077,069	289,933	1,267,135
1998	2,215,217	4,354,929	297,880	1,329,964
1999	2,286,129	4,573,666	302,989	1,374,377
2000	2,480,050	4,745,060	298,422	1,345,393
2001	2,195,164	4,330,871	285,258	1,286,034
2002	1,857,721	3,692,327	258,271	1,149,262
2003	1,685,672	3,343,330	237,497	1,045,761
2004	1,562,565	3,069,068	219,533	958,107
2005	1,516,168	2,962,827	208,030	916,005
2006	1,765,947	3,323,631	210,366	938,147
2007	1,880,255	3,509,777	210,413	946,274
2008	1,650,361	3,122,089	199,284	886,884
2009	1,236,791	2,400,124	171,952	749,063
Mean	1,888,411	3,702,449	257,208	1,122,075
Min	1,236,791	2,400,124	171,952	749,063
Max	2,480,050	4,745,060	309,126	1,377,595
Sum	62,317,577	122,180,828	8,487,899	37,028,491

Notes: FSC: "further selection criteria" (see Appendix A.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.

Table 12: Number of entry jobs and job entrants by year for different samples

Year	Individual real wages and real mean wages				Real modal wages			
	Number of job entrants		Number of entry jobs		Number of job entrants		Number of entry jobs	
	Dataset		Dataset		Dataset		Dataset	
	with	w/o	with	w/o	with	w/o	with	w/o
	FSC		FSC		FSC		FSC	
1977	886,019	3,577,107	47,837	962,528	268,919	1,008,539	9,495	496,456
1978	894,609	3,644,717	49,114	1,019,450	272,156	1,038,035	9,575	529,977
1979	1,050,035	4,180,031	50,885	1,112,191	310,233	1,157,801	9,615	571,497
1980	1,012,511	4,012,189	52,031	1,134,087	293,375	1,122,416	9,445	594,675
1981	849,939	3,470,701	52,101	1,075,261	240,001	1,019,112	9,428	588,001
1982	662,769	2,832,966	50,775	976,068	180,329	875,739	9,912	559,749
1983	656,650	2,710,091	50,501	949,209	182,691	867,544	10,278	553,494
1984	756,423	3,026,232	51,426	994,372	221,216	961,644	10,212	573,108
1985	807,117	3,091,450	51,558	998,811	244,079	982,977	10,176	574,241
1986	860,956	3,430,838	52,647	1,106,821	251,062	1,057,838	9,584	625,188
1987	837,028	3,246,381	52,426	1,066,650	245,511	1,010,076	9,705	608,137
1988	904,067	3,441,390	53,124	1,108,947	270,533	1,066,980	9,668	628,335
1989	1,062,304	3,956,568	54,101	1,198,174	313,286	1,166,709	9,413	658,651
1990	1,214,943	4,484,235	54,897	1,284,954	372,947	1,308,744	9,538	690,343
1991	1,145,106	4,304,481	54,754	1,277,104	342,143	1,245,828	9,541	689,361
1992	953,085	3,848,049	54,199	1,234,042	259,790	1,123,255	9,080	679,246
1993	962,162	4,355,962	60,322	1,343,865	299,912	1,386,618	12,010	744,743
1994	1,001,916	4,393,695	61,010	1,333,431	321,871	1,410,525	12,176	740,015
1995	1,090,876	4,543,150	62,239	1,377,595	344,831	1,459,045	12,105	769,578
1996	976,505	4,125,827	60,993	1,282,525	316,160	1,370,161	12,700	733,884
1997	1,002,769	4,077,069	61,063	1,267,135	327,990	1,360,383	12,889	728,320
1998	1,139,079	4,354,929	62,140	1,329,964	392,045	1,458,422	12,723	761,967
1999	1,164,435	4,573,666	62,340	1,374,377	396,646	1,500,633	12,760	775,498
2000	1,270,840	4,745,060	62,238	1,345,393	410,450	1,528,862	12,557	753,306
2001	1,132,311	4,330,871	60,495	1,286,034	363,109	1,400,595	12,426	727,715
2002	960,419	3,692,327	57,439	1,149,262	313,366	1,261,486	12,654	669,674
2003	877,450	3,343,330	55,124	1,045,761	311,703	1,199,956	13,207	621,412
2004	811,292	3,069,068	52,909	958,107	292,498	1,134,828	13,470	577,913
2005	778,837	2,962,827	50,401	916,005	283,220	1,091,694	12,985	551,627
2006	844,207	3,323,631	49,739	938,147	328,840	1,230,806	12,987	550,745
2007	859,158	3,509,777	48,929	946,274	307,725	1,232,283	12,029	543,152
2008	768,808	3,122,089	47,000	886,884	267,849	1,082,093	11,576	511,483
2009	578,294	2,400,124	42,020	749,063	204,047	876,051	11,610	448,963
Mean	932,513	3,702,449	54,205	1,122,075	295,471	1,181,748	11,137	631,226
Min	578,294	2,400,124	42,020	749,063	180,329	867,544	9,080	448,963
Max	1,270,840	4,745,060	62,340	1,377,595	410,450	1,528,862	13,470	775,498
Sum	30,772,919	122,180,828	1,788,777	37,028,491	9,750,533	38,997,678	367,529	20,830,454

Notes: FSC: "further selection criteria" (see Appendix A.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.

Table 13: Contribution assessment ceiling for Germany, lower earnings limit, inflation, and unemployment rate

Year	Contribution assessment ceiling for Germany (€ per month)[a]				German CPI[b]		U rate[c]
	Compulsory pension insurance scheme		Lower earnings limit (§8, Social Code IV)		Index	Change to previous year (in %)	(in %)
	West	East	West	East			
	Germany		Germany				
1975	1,431.62		178.95		47.47	6.03	4.7
1976	1,585.01		198.13		49.48	4.22	4.6
1977	1,738.39		217.30[d]		51.31	3.70	4.5
1978	1,891.78		199.40		52.70	2.72	4.3
1979	2,045.17		199.40		54.88	4.13	3.8
1980	2,147.43		199.40		57.84	5.40	3.8
1981	2,249.68		199.40		61.50	6.33	5.5
1982	2,403.07		199.40		64.72	5.24	7.5
1983	2,556.46		199.40		66.81	3.23	9.1
1984	2,658.72		199.40		68.47	2.48	9.1
1985	2,760.98		204.52		69.86	2.04	9.3
1986	2,863.23		209.63		69.77	-0.12	9.0
1987	2,914.36		219.86		69.95	0.25	8.9
1988	3,067.75		224.97		70.82	1.25	8.7
1989	3,118.88		230.08		72.82	2.83	7.9
1990	3,221.14		240.31		74.74	2.63	7.2
1991	3,323.40		245.42		77.53	3.73	7.3
1992	3,476.79		255.65		80.57	3.93	8.5
1993	3,681.30	2709.85	270.98	199.40	83.45	3.57	9.8
1994	3,885.82	3016.62	286.32	224.97	85.71	2.71	10.6
1995	3,988.08	3272.27	296.55	240.31	87.11	1.63	10.4
1996	4,090.34	3476.78	301.66	255.65	88.31	1.38	11.5
1997	4,192.59	3630.17	311.89	265.87	90.01	1.93	12.7
1998	4,294.85	3579.04	317.00	265.87	90.91	1.00	12.3
1999	4,345.98	3681.30	322.11	322.11	91.41	0.55	11.7
2000	4,397.11	3630.17	322.11	322.11	92.71	1.42	10.7
2001	4,448.24	3732.43	322.11	322.11	94.51	1.94	10.3
2002	4,500.00	3750.00	325.00	325.00	95.91	1.48	10.8
2003	5,100.00	4250.00	325.00	400.00	96.91	1.04	11.6
2004	5,150.00	4350.00	400.00	400.00	98.51	1.65	11.7
2005	5,200.00	4400.00	400.00	400.00	100.01	1.52	13.0
2006	5,250.00	4400.00	400.00	400.00	101.61	1.60	12.0
2007	5,250.00	4550.00	400.00	400.00	103.91	2.26	10.1
2008	5,300.00	4500.00	400.00	400.00	106.61	2.60	8.7
2009	5,400.00	4550.00	400.00	400.00	107.01	0.38	9.1

[a] Values from 1975 until 2001 converted from DM into Euro. Source: Deutsch Rentenversicherung Knappschaft-Bahn-See; Hauptverwaltung Bochum.

[b] Consumer price index (CPI) for Germany (1995-2009) interlinked with the cost-of-living index of all private households for West Germany (1974-1994). Source: German Statistical Office (Statistisches Bundesamt).

[c] Unemployment rate in relation to dependent civilian labor force (abhängige zivile Erwerbspersonen) for West Germany (1976-1990) and Germany (1991-2009). Source: Statistic of the German Federal Employment Agency (Statistik der Bundesagentur für Arbeit).

[d] After July 1st, 1977: € 2,270.16.

A.4 Robustness Checks

To ensure the robustness of the results from Section 4, I run several additional regressions. Tables 14 and 15 present the estimated coefficients for the unemployment rate using the FSC dataset (see Appendix A.2).

Table 14: Model 1—estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using "typical" real entry-wages

	Modal wage		Mean wage	
	Dataset		Dataset	
	with FSC	w/o FSC	with FSC	w/o FSC
(1.0) 1st and 2nd stage unweighted OLS	−0.84**	−1.03***	−0.88**	−0.94***
	(0.38)	(0.35)	(0.33)	(0.34)
(1.1) 1st stage unweighted OLS, 2nd stage OLS weighted	−0.84***	−1.00***	−0.88**	−0.92***
by number of entry jobs	(0.37)	(0.34)	(0.32)	(0.33)

Note: *** Significant at 1% level; ** 5% level.

Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix A.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period. Further controls used: secular time trend controls (t and t^2) and a dummy for years $\geq$ 1984.

Table 15: Model 2—estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using individual real wages

	Dataset	
	with FSC	w/o FSC
(2.0) 1st stage unweighted OLS, 2nd stage OLS unweighted	−0.84***	−0.83***
	(0.27)	(0.27)
(2.1) 1st stage unweighted OLS, 2nd stage OLS weighted by	−0.92***	−0.90***
number job entrants	(0.29)	(0.28)

Note: *** Significant at 1% level.

Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix A.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period. Further controls used: secular time trend controls (t and t^2) and a dummy for years $\geq$ 1984. Individual controls used in the 1st stage regression: education, sex, nationality, age, age^2, and length of the employment spell.

Tables 16 and 17 present estimated coefficients for the unemployment rate in slightly altered versions of the baseline models (presented in Tables 5 and 6), and Table 16 shows estimated coefficients for the lagged unemployment rate.

To control for potential differences in the wage setting between West Germany and East Germany, I run regressions in which I introduce a dummy variable for East Germany ($East$). The dummy takes the value of one if the place of work is located in East Germany (base category: West Germany). Thus, the first-stage regressions (equations 1 and 3) become:

$$\ln(w_{jt}) = \alpha_j + \beta_t + East_{jt} + \varepsilon_{jt} \text{ and} \tag{7}$$

$$\ln(w_{ijt}) = \alpha_j + \beta_t + \gamma' \mathbf{x}_{it} + East_{jt} + \varepsilon_{jt}, \text{ respectively.} \tag{8}$$

However, introducing the East dummy has little effect on the coefficients for the unemployment rate. Moreover, all other robustness checks produce coefficients for the unemployment rate that are in the vicinity of the estimated coefficients from the baseline models. As expected, the coefficients for the lagged unemployment rate are higher than those for the unemployment rate and are therefore somewhat more procyclical.

Table 16: Robustness checks for model 1—estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using "typical" real entry-wages

	Estimated coefficients of the unemployment rate			
	Modal wage		Mean wage	
	Dataset		Dataset	
	with FSC	w/o FSC	with FSC	w/o FSC
Like (1.1) but 2nd reg. weighted by number of job entrants	-0.72**	-0.93***	-0.78**	-0.85**
	(0.35)	(0.33)	(0.30)	(0.32)
Like (1.1) but with a dummy for East Germany in the 1st reg	-0.84**	-1.00***	-0.88**	-0.92***
	(0.37)	(0.34)	(0.32)	(0.33)
Like (1.1) but 2nd reg. weighted by number of job entrants	-0.72**	-0.93***	-0.78**	-0.85**
and with a dummy for East Germany in the 1st reg.	(0.35)	(0.33)	(0.30)	(0.32)
Like (1.1) but 2nd reg. unweighted and with a dummy for	-0.84**	-1.03***	-0.88**	-0.94**
East Germany in the 1st reg.	(0.38)	(0.35)	(0.33)	(0.34)

Notes: OLS regression. Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix A.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period. Further controls used: secular time trend controls (t and t^2) and a dummy for years $\geq$ 1984. *** p<0.01, ** p<0.05, * p<0.1. Estimates for regressions (1.1) and (1.2) see Table 5.

Table 17: Robustness checks for model 1—estimated coefficients of the lagged unemployment rate $\left(\hat{\delta}\right)$ using "typical" real entry-wages

	Estimated coefficients of the lagged unemployment rate			
	Modal wage		Mean wage	
	Dataset		Dataset	
	with FSC	w/o FSC	with FSC	w/o FSC
Like (1.0)	-0.89**	-0.89**	-0.84**	-0.82**
	(0.34)	(0.34)	(0.30)	(0.32)
Like (1.1)	-0.89**	-0.87**	-0.84***	-0.80**
	(0.33)	(0.32)	(0.30)	(0.04)
Like (1.1) but 2nd reg. weighted by number job entrants	-0.80**	-0.82**	-0.77**	-0.75**
	(0.32)	(0.31)	(0.28)	(0.30)
Like (1.1) but with a dummy for East Germany in the 1st reg.	-0.89**	-0.87**	-0.84***	-0.80**
	(0.33)	(0.32)	(0.30)	(0.31)
Like (1.1) but 2nd reg. weighted by number job entrants and	-0.80**	-0.82**	-0.77**	-0.75**
with a dummy for East Germany in the 1st reg.	(0.32)	(0.31)	(0.28)	(0.30)
Like (1.1) but 2nd reg. unweighted and with a dummy for	-0.89**	-0.89**	-0.84**	-0.82**
East Germany in the 1st reg.	(0.34)	(0.34)	(0.30)	(0.32)

Notes: OLS regression. Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix A.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period. Further controls used: secular time trend controls (t and t^2) and a dummy for years $\geq$ 1984. *** p<0.01, ** p<0.05, * p<0.1. Estimates for regressions (1.1) and (1.2) see Table 5.

Table 18: Robustness checks for model 2—estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using individual real wages

	Estimated coefficients of the unemployment rate	
	Dataset	
	with FSC	w/o FSC
Like (2.0) but with a dummy for East Germany in the 1st reg.	-0.84***	-0.83***
	(0.27)	(0.27)
Like (2.0) but without individual controls in the 1st reg.	-0.84***	-0.83***
	(0.27)	(0.27)
Like (2.0) but without individual controls in the 1st reg. and	-0.78**	-0.76**
with a dummy for East Germany in the 1st reg.	(0.32)	(0.31)
Like (2.1) but with a dummy for East Germany in the 1st reg.	-0.92***	-0.90***
	(0.29)	(0.28)
Like (2.1) but without individual controls in the 1st reg.	-0.92***	-0.90***
	(0.29)	(0.28)
Like (2.1) but without individual controls in the 1st reg. and	-0.88**	-0.85**
with a dummy for East Germany in the 1st reg.	(0.34)	(0.33)

Notes: OLS regression. Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix A.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period. Further controls used: secular time trend controls (t and t^2) and a dummy for years $\geq$ 1984. *** p<0.01, ** p<0.05, * p<0.1. Estimates for regressions (1.1) and (1.2) see Table 6.

A.5 Evaluation of the Regression Models—Additional Tables

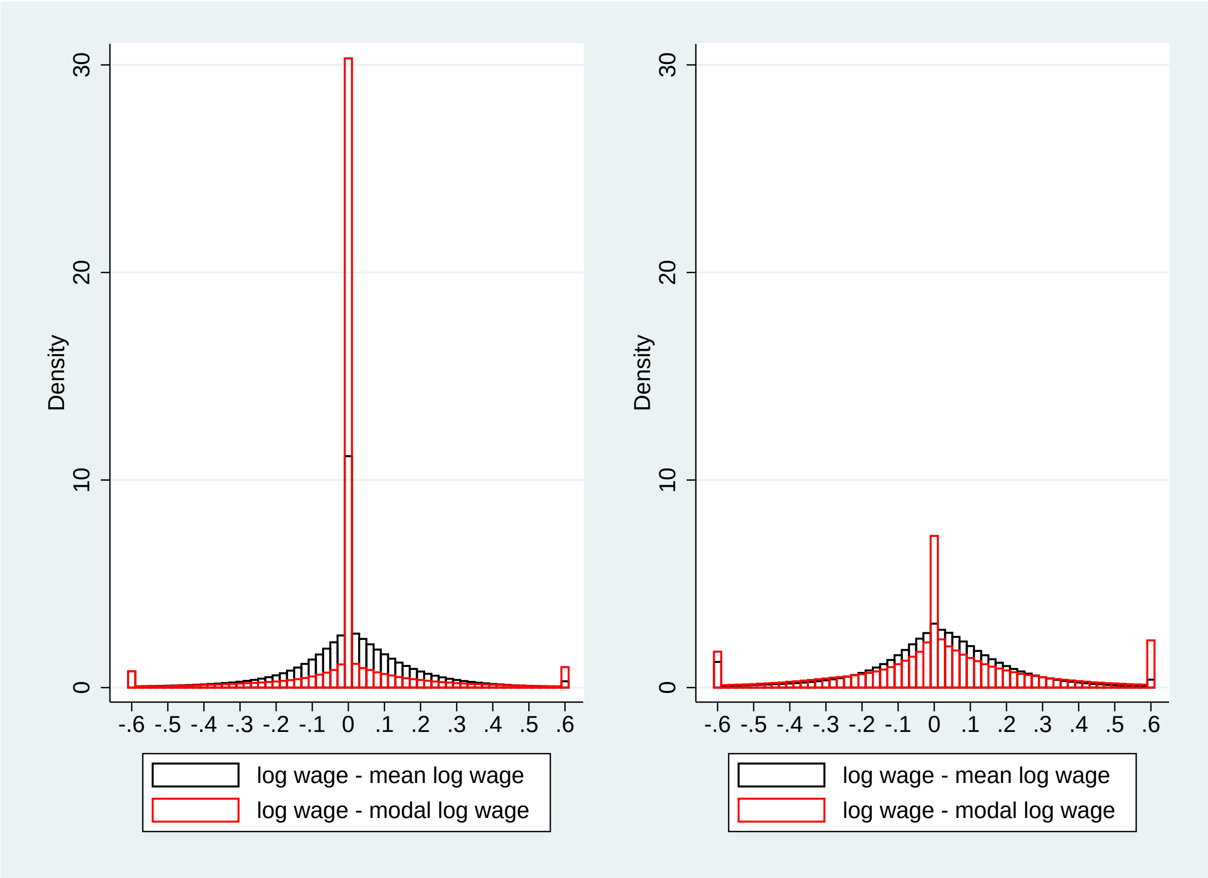

Figure 1: Distribution of differences between individual worker's log real wage and "typical" log real wage

Note: Distribution of differences between individual worker's log real wage and "typical" log real wage in job/year for the dataset w/o FSC (left Panel) and for the dataset with FSC (right Panel). FSC: "further selection criteria" (see Appendix A.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.

Table 19: Summary statistics for the differences between individual worker's log real wage and "typical" real wage in job/year

	Dataset w/o FSC		Dataset with FSC	
	Mean job wage	Modal job wage	Mean job wage	Modal job wage
Observations	122,180,828	38,997,678	30,772,919	9,750,533
Mean	0.000	0.010	0.000	0.025
Std. Dev.	0.202	0.227	0.241	0.343
Variance	0.0409	0.052	0.058	0.118
Skewness	-1.111	0.871	-1.271	0.514
Kurtosis	11.382	21.176	9.634	9.538

Note: *** Significant at 1% level; ** 5% level.

Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix A.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.