Real Wage Cyclicality of Newly Hired Workers*

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Abstract

Several recent macroeconomic models rely on rigid wages. Especially wage rigidity of newly hired workers seems to play a crucial role, since the decision of opening a vacancy or not is mainly influenced by their real wages. However, so far little empirical evidence exists on how real wages of newly hired workers react to business cycle conditions. This paper aims at filling this gap for a large economy, namely Germany, by analyzing the cyclical behavior of real wages of newly hired workers while controlling for "cyclical up- and downgrading" in employer/employee matches. For the analysis two endogenous variables are used: either the "typical" (e.g. modal) real wage paid to entrants into a particular job of a particular firm or the entrants' individual real wage. The results show that entry-wages are not rigid, but considerably respond to business cycle conditions. This finding strengthens Pissarides' (2009) dismissal of theories based on cyclically rigid hiring wages and challenges researchers to develop models that are able to generate realistic volatilities of, e.g., unemployment when considering the empirically documented real wage cyclicality. Furthermore, I show that the procyclicality of the employment/population ratio is (nearly) identical to the procyclicality of real entry-wages.

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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 not able to explain the cyclical volatility of unemployment (see, e.g., Shimer, 2005; Hall, 2005; Veracierto, 2008). One way 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." So far, Shimer's (2004, 2005) suggestion that real wage rigidity is one way to generate more variability of unemployment within the search and matching model has been widely shared (see, e.g., Hall, 2005; Hall and Milgrom, 2008; Kennan, 2010).[1] Recent literature argues that especially the real wage rigidity of newly hired workers should play a crucial role, since the decision of opening a vacancy or not is mainly influenced by their real wages (see, e.g., Pissarides, 2009; Haefke et al., 2012). Pissarides (2009) argues that even if wages of incumbent workers are rigid, the wages of newly hired employees could be highly procyclical, and, with sufficiently procyclical entry-wages, the "Shimer-Puzzle" would remain. Haefke et al. (2012) show that in the USA wages of entrants out of non-employment respond one-to-one to changes in labor productivity. The wages of incumbents however react very little to changes in productivity.

However, recent literature also challenges the idea of introducing real wage rigidity into search and matching models in order to generate a realistic volatility of unemployment. Pissarides (2009, p. 1341), e.g., dismisses theories based on cyclically rigid wages because, empirically, hiring wages are 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 the real wage cyclicality for job movers is larger than for incumbent workers (e.g., Bils, 1985; Shin, 1994; Devereux and Hart, 2006; Shin and Solon, 2007).

[1] The existence of "Shimer-Puzzle" for 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 show 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."

But there is an explanation why the empirical evidence, to which Pissarides (2009) refers to, 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, while wages of newly hired workers may be rigid or tied to the wages of incumbent workers within the same firm. Previous research has mostly ignored "cyclical upgrading" of workers to better employment opportunities in booms (i.e. from low-wage jobs to high-wage jobs) and "cyclical downgrading" to worse employment opportunities in recessions. Not controlling for the employer/employee match could lead to the conclusion that the wage is procyclical over the business cycle when in fact the procyclical movements of the wage actually result only from the job changes. Hence, the empirical assessment of recent theories of the rigidity of entry-wages requires an approach that identifies cyclical variation in hiring wages within particular jobs (employer/employee matches).

So far, to the best of my knowledge, only two studies for Portugal exist that control for "cyclical upgrading" and "cyclical 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-specific fixed effects. Martins et al.'s (2012b) endogenous variable is a slightly aggregated wage: they use the "typical" real wage of entry jobs, e.g. the modal wage of a certain job within a certain firm. They define jobs within firms and use firm-job fixed effects instead of controlling separably for firm and job fixed effects.

Against the backdrop of the 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 upgrading" and "cyclical downgrading" in employer/employee matches through the use of firm-job fixed effects. For the empirical analysis I apply three statistical models—focusing on two different endogenous variables—to a huge administrative longitudinal matched employer-employee dataset for Germany over the 1977 to 2009 period. I focus on the "typical" real wage of newly hired workers—e.g. the modal real wage paid to entrants into a particular job of a particular firm—following Martins et al.'s (2012b) methodology and I focus on the job entrants' individual real wages following Carneiro et al.'s (2012) methodology.

This 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 firm-job fixed effects. In light of the magnitude of the entry-wage cyclicality that I find for Germany, it seems that the idea of introducing wage rigidity into the Mortensen-Pissarides model in order to amplify realistic volatility of unemployment 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), respectively, as the endogenous variable might be biased in different directions. By running separate regressions for both endogenous variables I obtain an upper and a lower bound estimate for the wage cyclicality of newly hired workers. I argue that the true parameter should lie within these limits. Third, I show 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 gives a brief literature review on methods of measuring entry-wage cyclicality and on existing empirical evidence. The data description and the data selection are presented in Section 3, while Section 4 presents the statistical models and the empirical results. In Section 5 I discuss the results and their implications, while Section 6 concludes.

2 Previous Empirical Evidence and Methods of Measuring

To the best of my knowledge, so far only two papers exist which identify cyclical variation in hiring wages while controlling for "cyclical upgrading" and "cyclical 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 different time periods and different unemployment rates. Martins et al. (2012) use the 1982 to 2008 period, while Carneiro et al. (2012) use the shorter 1986 to 2007 period. Also, they use 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. For their analysis they use 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" real wage of a job—e.g. the modal wage. In the second stage they estimate the cyclicality of entry-wages by 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 an increase in the unemployment rate by one percentage point leads to 1.8 percent lower 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 the 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 fixed effects, job title fixed effects, and firm fixed effects. Carneiro et al. (2012) find that an increase in the unemployment rate by one percentage point leads to 2.67 percent lower real wages for newly hired workers.[2]

3 Data Description and Data Selection

The empirical analysis is undertaken for Germany for the 1977 to 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 the total population gainfully employed and covered by the social security system. Not covered are the self-employed, family workers assisting in the operation of a family business, civil servants (Beamte) and regular students. The BeH covers roughly 80 percent of the German workforce. From 1975 to 2009, the BeH contains data of 75 million workers in 9.11 million firms (IAB, 2011).[3] Workers from East Germany are included from 1992 onwards. Important advantages of the BeH are the enormous amount of information and the high reliability of the earnings data, which is due to plausibility checks performed by the social security institutions and the existence of legal sanctions for misreporting. In contrast to household surveys, measurement errors due to erroneous reporting should

[2]For incumbent workers Carneiro et al. (2012) find that an increase in the unemployment rate of one percentage point decreases real wages by around 2.2 percent.

[3]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 run for the 1977 to 2009 period. Data from earlier years is used for identifying job entrants.

be much weaker. Also, the BeH allows a matching of workers with firms, which is crucial to control for "cyclical upgrading" and "cyclical 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 which employed at least seven workers[4] in at least one year in the 1975 to 2009 period. In those firms I identify all full-time workers. For each identified worker I draw all existing employment spells for the 1975 to 2009 period—including part-time employment, apprenticeships etc. The obtained dataset contains data of 59.711.757 workers in 1.635.679 firms.[5] 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[6] (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). As "job level" I use a four-category variable coded as blue-collar worker / no craftsman, craftsman / skilled laboror[7], master craftsman[8], and white-collar worker / salaried employee. Hence, I create unique job identifiers that consist of the firm identification number, the occupation, and the job level.

To identify newly hired workers I use the individuals' employment spells. An individual is a newly hired worker (job entrant) if he/she has worked in a different firm before (firm change)—and therefore in a different job—or if the individual has not worked (s.t. social security) in the same firm in the last 365 days. The second condition makes sure that workers adjourning 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.

[4] 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.

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

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

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

[8] Persons in this class can be employed either as blue-collar or as 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 onwards.[9]

2. The BeH does not contain hourly wages. To minimize contamination with working-time effects, only full-time workers are considered in the analysis.[10]

3. Since earnings data are right censored at the contribution assessment ceiling[11] ("Beitragsbemessungsgrenze"), only non-censored wage spells are considered in the analysis. I apply consistent top-coding instead of just dropping the censored wage spells.[12] Applying consistent top-coding has the advantage that over the whole sample period the same fraction of the wage distribution is considered in the analysis. 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 West Germany the highest percentage of spells (8.33%) are censored in the year 1992, for East Germany this is the case in 2002 (6.99%). Therefore, in each year I drop the 8.34% / 7% highest wage spells for West / East Germany.[13]

[9] For the years 1975 for West Germany and 1992 for East Germany, respectively, I cannot apply the identification strategy for job entrants described above. Therefore I cannot use the data for the empirical analysis. I also drop observations for Berlin for all years before 1993 for the following reasons. First, West Berlin always had a special status before 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 distinguished between East Berlin and West Berlin. Also, due to some data inconsistencies concerning the firm assignment in 1976 the data for the year 1976 are not used for the empirical analysis, but for identifying job entrants.

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

[11] 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 in the so called "knappschaftliche" pension insurance. The contribution assessment ceiling of this pension insurance is always higher than for the compulsory pension insurance scheme. Since 1999, the BeH does not indicate anymore in which pension insurance a person is insured. For this reason, I use only the contribution assessment ceiling of the compulsory pension insurance scheme.

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

[13] 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)

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

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

As a robustness check I also apply the much stricter sample selection criteria according to Martins et al. (2012). However, applying these further selection criteria (FSC) hardly affects the regression results. The FSC are outlined in Appendix A.2 and regression results using this dataset are displayed 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 wages of newly hired workers. As the endogenous variable I use "typical" real wages of entry jobs (following Martins et al., 2012) and alternatively individual entry-wages (following Carneiro et al., 2012).

Employers have to report to the social security system on a yearly base. Therefore, the BeH data does not contain monthly wages or hourly wages, but the wages[14] paid during the duration of an employment spell. Hence, I cannot observe the wage of the first month of employment. But since 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. When using individuals' wages I also control for the different lengths of employment spells. To calculate the average daily real wage (in 2005 prices) I use the Consumer Price Index (CPI).[15]

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

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

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

Tabelle 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

Alternatively, I use the individual average daily real wage w_{ijt} paid in period t to worker i newly hired into job j. Summary statistics are provided in Table 2. For the regressions I draw for each year a random 1 percent sample of the jobs (stratified by the number of entrants per job). For each drawn job, I keep all employment spells of the 1977 to 2009 period. Concerning the number of job entrants this leads roughly to a bisection of the original dataset: of the 122,180,828 job entrants 59,863,251 are dropped, reducing the dataset to 62,317,577 employment spells of newly hired workers. Table 11 (see Appendix A.3) shows the sample sizes by year for this sub-sample.

Tabelle 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

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

Tabelle 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.

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 (2012b) methodology and apply two stage regressions.[16] However, concerning the endogenous variable I follow both, Martins et al. (2012) and Carneiro et al. (2012), and use both the "typical" real wages of entry jobs and the job entrants individual real wages.

[16] The unemployment rate—the regressor of interest—varies only between years. When it comes to the estimation of the standard errors I prefer a two stage regression over a single 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, e.g., in Angrist and Pischke (2009, chapter 8.2).

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

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

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

where w_{jt} denotes the "typical" real wage paid in period t to workers newly hired into job j, e.g. the log 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 I regress in the second stage 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 is 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 separating fringe benefits from regular earnings. Before 1984, the inclusion of fringe benefits to notification was voluntary. Since 1984, one-time payments to employees have been subject to social security taxation and are therefore included in the data.[17]

[17] However, observations before 1984 should be valid as well. If some employers reported fringe benefits before 1984 and others did not, it is very likely that employers were usually consistent in their reporting

4.1.2 Models 2 and 3

In models 2 and 3 I analyze how 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 allows to control for individual worker characteristics and for 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 employment. Since the wage may include fringe benefits this 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 time of the year. Model 2 (see Equation 3) allows, inter alia, to control for this data issue by controlling for the length of the employment spell:

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

where w_{ijt} denotes the real wage paid in period t to worker i newly hired into job j and $\mathbf{x}_{it}$ is a vector with individual characteristics of the worker i for period t (see Table 3). To 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) show that the failure to control for unobserved heterogeneity leads to countercyclical biases. Hence the estimates of model 2 are probably 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 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 which start at least 5 jobs during the observed time period. Furthermore, I require that these jobs are observed for at least

behavior. The obligation of fringe benefits to notification leads to a level effect on wages from 1984 onwards for which I control with the $D_{\geq 1984}$ dummy.

5 years.[18] The estimates of model 2 are used to show 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):[19]

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

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

4.2 Results

The results for model 1 show, 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 the regression model (see Table 5).[20] An increase in the unemployment rate of one percentage point decreases the real wage of job entrants within given firm-jobs by between 0.92 to 1.03 percent.[21] The differences are not statistically significant at the five percent level. In regression (1.0) I estimate unweighted OLS regression models in both stages. In regression (1.1) I use weights according to Martins et al. (2012): while the 1*st* stage uses unweighted OLS, in the 2*nd* stage OLS is used 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 hardly varies (see

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

[19] I use the stata ado file "a2reg" by Ouazad (2007).

[20] Martins et al. (2012, p. 44, Figure 3) show a sample distribution of differences between individual workers' log wage and modal log wages per job/year. For the Portuguese data—with hourly wages—the modal wage seems to be a good measure. For Germany the "typical" log wages differ more from the individual workers' log wages than in Portugal. This is probably due to the fact that the BeH provides daily and not hourly wages. Distributions of differences between individual workers' log wage and "typical" log wages are displayed in Figure 1 in Appendix A.5. The differences between "typical" wages and individual workers' wages seems to be stronger for the dataset with FSC (right panel of Figure 1). This first visual impression is also supported by 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 roughly twice as high as for the other measures.

[21] Also, whether FSC are used or not only slightly affects the estimated coefficients of the unemployment variable (see Appendix A.4). Hence, the selection criteria from Martins et al. (2012) do not seem to influence the outcome of the regressions.

Table 13 of Appendix A.3).[22] Hence a weighting in the second stage regressions seems not to be necessary. A comparison of the estimates of model (1.0) and (1.1) shows that indeed, the weighting hardly affects the results.

Tabelle 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 by number of entry jobs per year	-1.00^{***}	-0.92^{***}
	(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.

The results of model 2 (see Table 6)—using individual wages instead of "typical" real entry-wages—are quite similar those of model 1. An increase in the unemployment rate by one percentage point decreases the real entry-wages of job entrants within given firm-jobs by between 0.83 and 0.90 percent.[23]

Tabelle 6: Model 2—–estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using individual 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 and nationality, and age, age^2 and length of the employment spell.

Since several papers show (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 allows to better control for worker heterogeneity. As mentioned above, the dataset used for models 1 and 2 is not optimally suited for this kind of regression.

[22] 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 one. 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 one.

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

Thus, I draw a sub-sample of employment spells of workers which enter at least 5 jobs during their working life and all those jobs must be observed in at least 5 years in the 1977 to 2009 period. Due to this sampling, the dataset is reduced from 62,317,577 to 10,335,054 employment spells of job entrants.[24] To test whether the sampling affects the results, I re-run the regression shown in Table 6 using the sub-sample as a robustness check (see Table 7). The estimated coefficients of the control regressions (3.0 an 3.1) have about the same magnitudes as the estimated coefficients using the original sample (see Table 6). Hence, it seems that using the sub-sample for the regressions hardly affects the results.

If one controls for worker fixed effects, an increase in the unemployment rate by one percentage point decreases the real entry-wages of job entrants within given firm-jobs by about 1.27 percent. Comparing the results of the control regressions (3.0 and 3.1) with the results of the linear two-way fixed-effects regressions (3.2 and 3.3) shows, that not controlling for worker fixed effects leads—as expected—to an underestimation of entry-wage cyclicality.

Tabelle 7: Model 3—estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using individual 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 and 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 just presented and I comment on the question whether or not introducing wage rigidity in the Mortensen-Pissarides search and matching model—in order to amplify realistic volatility of unemployment—is a sound strategy in light of the empirical evidence.

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

5 Discussion of the Results

The estimated coefficients of 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 percent level. Additionally controlling for worker fixed effects results in a higher estimate for the wage cyclicality of about -1.27 (see Table 7).

5.1 Evaluation of the Regression Models

Using "typical" wages has a disadvantages: it does not allow to control for individual and employment characteristics. However, given the German wage data controlling for the length of the wage spell could be important since the dataset provides average daily wages. The Christmas bonus, e.g., is often only paid to workers that are employed during at least a certain time of the year. Not controlling for the length of the wage spell could lead to biased results. Therefore it seems that, given the German data, in general the individual worker's log wage is better suited for the regressions.

However, using the individual worker's log wage has disadvantages too. It implies that one weights by the amount of hiring, which might be endogenous. If the wages of some jobs are more rigid than the wages of other jobs, then it could be that during a recession firms hire less workers into the jobs with more rigid wages. This probably would produce a procyclically bias in the wage analysis.[25]

Since the estimates of model 3—using individual wages—are probably procyclical biased, one could argue that model 1 should be preferred over model 3.[26] However, the estimates of model 1 are probably biased as well.

As, e.g., Solon et al. (1994) point out, 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). This causes a "composition bias" if aggregated wage are used in the analysis: "[...] the aggregated statistics are constructed in a way that gives more weight

[25] I would like to thank Gary Solon for pointing out this issue.

[26] I do not discuss the quality of the results using model 2, since the results are mainly used for robustness checks.

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 the wage cyclicality.

Moreover, using "typical" wages does not allow to control for changes in the workforce and/or employment shares. However, as Mitchell et al. (1985, p. 1162) point out, the "[...] composition of the labor force may change considerably over the course of the business cycle."[27] 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 advance. For example the share of less trained workers within firms could decrease over time due to the introduction of new machines, while the share of engineers increases over time because more manpower is needed to maintain the machines. Further, using the "typical" wage does not allow to control for cyclicality in job assignments either. However, Mitchell et al. (1985), e.g., find for the USA that the work force 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."

Also, "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 in a recession employers would be able to recruit better qualified workers at any given wage. Solon et al. (1997), e.g., state that firms might lower hiring standards in 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). This would lead to a lower effective wage per efficiency unit of labor and to an underestimation of the wage cyclicality. Büttner et al. (2010) show for West Germany that occupational upgrading and downgrading—occupations as units defining homogenous skill requirements—exist in Germany. According to their results, the skill level of new hires within occupations rises significantly in recessions and decreases in upturns—however the effect amounts only to

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

about 70 percent of the corresponding USA result.[28] Given the results of Büttner et al. (2010), the procyclicality of entry-wages estimated in this paper should be underestimated only slightly. This should especially be true for model 3, where I control for the qualification level of the employee (education).

To sum up, looking at "typical" wages as well as looking at individual wages seems to produce biased estimates. Using individual wages probably produces a procyclical bias, while using "typical" wages probably produces a countercyclical bias. Therefore, I do not prefer any methodology over the other, but suggest to use 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 estimates of model 3—regression (3.3)—provides an upper bound estimate. Thus an increase of the unemployment rate by one percentage point leads to a decrease of real wages by between 0.92 and 1.27 percent. The true parameter should lie within this range. This assumption seems to be justified since the estimates are not statistically different from each other at the 5% level.

5.2 Implications of the Results

The estimated coefficients of the unemployment rate—using individuals' wages, controlling for job and worker fixed effects—are in the general vicinity of -1.27 (see Table 7). Being aware that if labor-force participation 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. In order to see whether this can be confirmed empirically, I follow Martins et al. (2012) and estimate Okun's Law-style relationships for the 1977 to 2009 period. In order to control for the reunification of Germany I introduce a dummy, $D_{\geq 1991}$, that is equal to one for years from 1991 onwards.

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

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

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

I find that a one-point increase in the unemployment rate is associated with a 1.27 percent reduction ($\beta_2/\beta_1 = -1.27$) in the employment/population ratio. This procyclicality of employment is (nearly) identical to the procyclicality I have 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 I have estimated for real entry-wages (see Tables 5).

Finally, I address the 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. 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, I draw on results of Kennan's (2010) model (cf. Martins et al., 2012). 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 percent when the unemployment rate rises by one percentage point (see Table 8).

Tabelle 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 an 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.

Since my estimates (using model 3) show a decline of real hiring wages of 1.27 percent when the unemployment rate rises by one percentage point it seems 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 backed up by the lower bound estimates of model 1: I still find a decline of real hiring wages of about 0.92 percent when the unemployment rate rises by 1 percentage point.

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 show that entry-wages in Germany are not rigid, but considerably respond to business cycle conditions. Furthermore, I show 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: an increase of the unemployment rate of one percentage point leads to about 0.92 percent lower 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 an increase of the unemployment rate of one percentage point leads to about 1.27 percent lower real entry-wages (upper bound). The true parameter should lie between the upper and the lower bound. This assumption seems to be justified since the estimates are not statistically 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 seems that introducing wage rigidity in the Mortensen-Pissarides model in order to generate realistic volatility of unemployment is not supported by the data. This challenges researchers to develop search and matching models that are able to generate realistic volatility of, e.g, unemployment when considering the empirically documented real wage cyclicality.

However, it seems that real wages in Germany are less cyclical than in other countries. The two studies for Portugal that control for "cyclical upgrading" and "cyclical downgrading" in employer/employee matches find that a one percentage point rise in the unemployment rate decreases real wages of job entrants by 1.8 percent (Martins et al., 2012) and 2.67 percent (Carneiro et al., 2012), respectively. Studies for the USA—that do not control for "cyclical upgrading" and "cyclical downgrading" in employer/employee matches—find more procyclicality as well. Shin (1994), e.g., finds that a one percentage point rise in the unemployment rate decreases real wages of white (black) job changers by 2.67 (3.80) percent.

As outlined in Section 4.2, controlling for worker fixed effects is problematic when analyzing job entrants only. Therefore, future research on real wage cyclicality (in Germany) should analyze the real wage cyclicality of job entrants and incumbent workers simultaneously as done by Carneiro et al. (2012). However, this is not without drawbacks. 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 probably procyclically biased. Future research should also consider that the effect of a change in the unemployment rate on real wages might not be symmetric.[29] Hence results of regressions not allowing for asymmetric reactions might be biased.

[29] Shin and Shin (2008, p. 13), e.g., show that for male job stayers in the US "[...] wage growth in expansions [..] is much greater than wage reduction in recessions [..]."

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A Appendix

A.1 Data Preparation

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

The most common inconsistency I observed were spells that were identical except for the end date of the spell and/or the 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, it can happen that the human resources department has already sent out the information about 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 sent out information to the social security administration, this time for the full period the worker was employed at the firm in that year. This can lead to two spells for a certain worker that are identical except for the end date of the employment. Sometimes I observed that the longer spell stated a higher average daily wage. This is caused by the fact, that 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 time of the year. However, even these inconsistencies are observed very rarely compared to the huge amount of spells that are observed every year.

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

1. If I observed two or more identical spells I only kept 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 keep:
   (a) spell a with wage $\neq 0$ and spell b with wage $\mathrm{A} = 0 \rightarrow$ keep spell a
   (b) wage of spell $a >$ wage of spell $b \rightarrow$ keep spell a
   (c) spell a ends after spell $b \rightarrow$ keep 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 $b \rightarrow$ keep spell a

A.2 Data Description and Regression Results with the Selection Criteria of Martins et al. (2012)

In addition to the sample selection criteria described in Section 3.1.2—keeping only particular jobs that are observed in at least 3 years of the 1977 to 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 consider only newly hired workers of firms which employed at least 50 full time workers at 30th of June in at least five years of the 1977 to 2009 period. Additionally, I only include a particular job in the sample of entry jobs if for at least half the years that the firm is in the dataset the two following requirements are met:

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 percent of the firm's new hires of full-time workers in that year.

Due to the FSC only jobs are included in the sample which are observed for at least three years[30]. Martins et al. (2012) apply the FSC because they are focusing 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."

[30] 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 the years the firm is in the dataset.

Due to the very restrictive FSC not only a lot of jobs but also a lot of firms are dropped from the original dataset. Table 9 provides summary statistics and shows the effects of the FSC on sample sizes.

Tabelle 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 into job j. Table 10 again provides summary statistics and shows the effects of the FSC on sample sizes.

Tabelle 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

Table 12 provides statistics for single years for both datasets (with and without FSC). The Table additionally provides information on the number of job entrants using the “typical” real entry-wage and the number of entry jobs using individual daily real wage as the endogenous variable, respectively.

As Appendix A.4 makes clear, using the data set with the FSC hardly affects the regression results.

A.3 Data Description and Data Selection—Further Tables

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

Year	Real individual 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	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.

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

Year	Real individual wages and 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.

Tabelle 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 assure the robustness of the results from Section 4, I run several additional regressions. Tables 14 and 15 show estimated coefficients of the unemployment rate using the FSC dataset (see Appendix A.2). Tables 16 and 17 show estimated coefficients of the unemployment rate in slightly altered versions of the baseline models (presented in Tables 5 and 6), and Table 16 shows estimated coefficients of the lagged unemployment rate.

Tabelle 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.

Tabelle 15: Model 2—–estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using individual 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 and nationality, and age, age2 and length of the employment spell.

To control for possible differences in the wage setting between West Germany and East Germany, I run some regressions in which I introduce a dummy variable for East Germany (*East*). The Dummy is equal to one if the place of work is located in East Germany (base category: West Germany). Hence the first stage regressions (equations 1 and 3) change to:

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

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

However, introducing the East Dummy hardly affects the coefficients of the unemployment rate. Also, all other robustness checks show coefficients of the unemployment rate which are in the vicinity of the estimated coefficients of the baseline models. As expected, the coefficients of the lagged unemployment rate are higher than the coefficients of the unemployment rate and are therefore somewhat more procyclical.

Tabelle 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.

Tabelle 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.

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

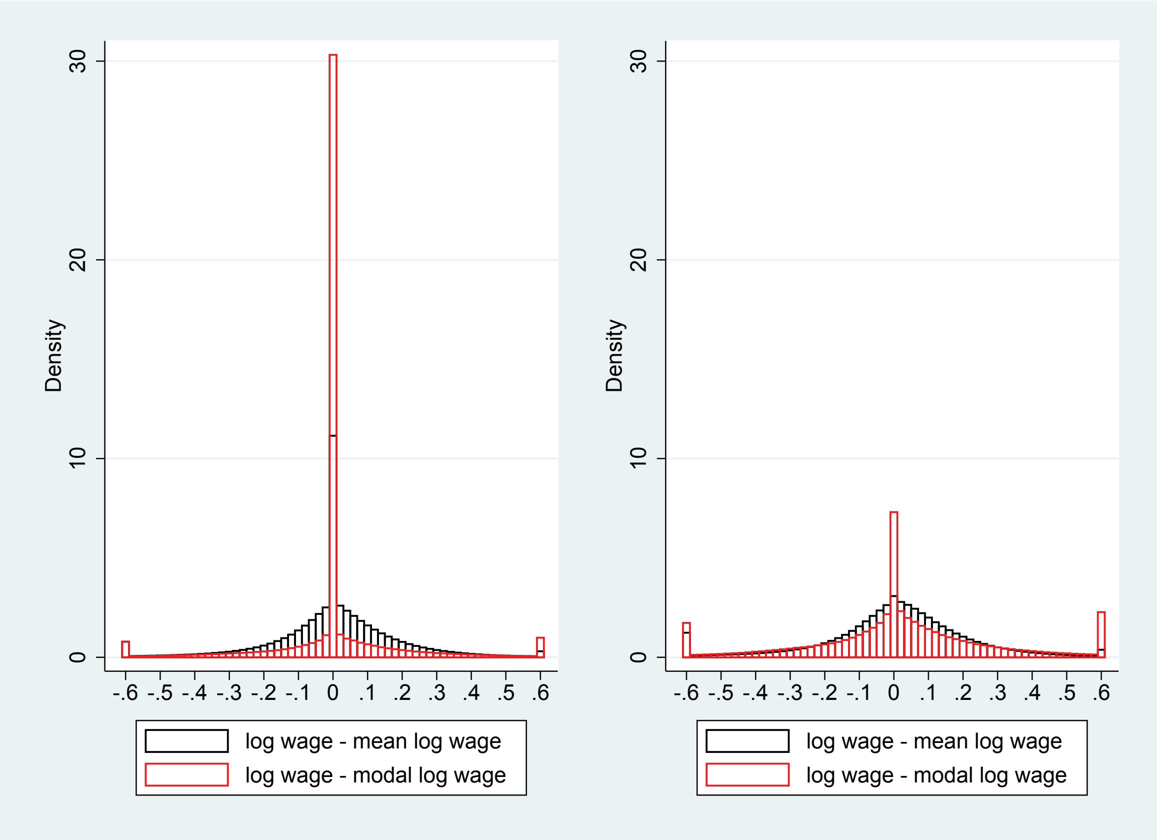

Abbildung 1: Distribution of differences between individual worker's log wage and "typical" log wage

Note: Distribution of differences between individual worker's log wage and "typical" log 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.

Tabelle 19: Summary statistics for the differences between individual worker's log wage and "typical 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.