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Are real entry wages rigid over the business cycle?

Empirical evidence for Germany from 1977 to 2009

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Abstract

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 Germany by analyzing the cyclical behavior of real wages of newly hired workers while controlling for ‘cyclical upgrading’ and ‘cyclical downgrading’ in employee/employer matches over the cycle. The analysis is undertaken for the 1977 to 2009 period using administrative longitudinal matched employer-employee wage data. I find that an increase in the unemployment rate of one percentage point decreases the real wages of job entries within given firm-jobs by about 1.27 percent. In light of the magnitude of the entry-wage cyclicality it seems that introducing wage rigidity in the Mortensen-Pissarides model in order to amplify realistic volatility of unemployment is not supported by the data. Further I show that the procyclicality of the employment/population ratio is identical to the procyclicality of real entry wages. This counters the view of many macroeconomists that wages are much less cyclical than employment and unemployment.

Zusammenfassung

Bisher gibt es wenig empirische Evidenz darüber, wie die Reallöhne neu eingestellter Arbeitnehmer auf den Konjunkturzyklus reagieren. Dieses Papier analysiert für Deutschland das zyklischen Verhaltens realer Einstiegslöhne unter Kontrolle von Arbeitnehmer/Arbeitgeber–Paarungen. Es zeigt sich, dass ein Anstieg der Arbeitslosenquote um einen Prozentpunkt zu etwa 1,27 Prozent niedrigeren realen Einstiegslöhnen führt. In Anbetracht dieser Volatilität scheint es, dass die Einführung von Lohnrigidität in das Mortensen-Pissarides-Modell, um realistische Volatilitäten der Arbeitslosigkeit zu erzeugen, nicht durch die empirischen Befunde gestützt wird.

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1 Introduction

Because of recent microeconometric evidence on wage cyclicality some authors argue that the standard Mortensen-Pissarides search and matching model (Mortensen/Pissarides 1994) is not able to explain the cyclical volatility of unemployment (see, e.g., Shimer 2005, Hall 2005, Veracierto 2008). However, by introducing the hypothesis of rigid wages into the Mortensen-Pissarides model, it is much better in matching fluctuations in unemployment. Especially the real wage of newly hired workers seems to play a crucial role in generating realistically cyclical fluctuations in the unemployment rate. Pissarides (2009), for example, shows that the decision of opening a vacancy or not is mainly influenced by the real wage of newly hired workers.

However, there is remarkably little empirical evidence on how real wages of newly hired workers react to business cycle conditions. Also, previous research on cyclical behavior of real entry wages has mostly ignored 'cyclical upgrading' of workers to better employment opportunities in booms (i.e. from low wage jobs to high wage jobs)—and the 'cyclical downgrading' to worse employment opportunities in recessions—in employee/employer matches as emphasized by Gertler and Trigari (2009).[1] This paper aims at filling the gap by applying the methodology proposed by Martins et al. (2011) to German data, i.e. by controlling for firm-job fixed effects.

For the empirical analysis I apply three statistical models—focusing on two different units of observation—to a huge administrative longitudinal matched employer-employee data set for Germany over the 1977 to 2009 period. I focus on the 'typical real wage'—e.g. the modal real wage paid to entries in a particular job—(unit of observation: entry jobs) following Martins et al. (2011), and I focus on the job entries individual real wages (unit of observation: job entries) following Carneiro et al. (2011). Using the entry jobs as the unit of observation is more in line with the search and matching model (one worker per firm), while using the job entries as the unit of observation is more in line with the empirical question whether or not real entry wages are rigid. Further the latter allows controlling for individual characteristics of the workers.

The contribution of the paper to the literature is threefold. First, it presents the first empirical evidence for a large economy, namely for Germany, on the cyclicality of real entry-wages while controlling for firm-job fixed effects.[2] In light of the magnitude of the entry-wage cyclicality it seems that introducing wage rigidity in the Mortensen-Pissarides model in order to amplify realistic volatility of unemployment is not supported by the data. Second, the paper shows that the unit of observation—job en-

[1] 'Cyclical upgrading' and 'cyclical downgrading' has long been discussed and documented—it goes back at least to Reynolds (1951, Chapter 5). Recent analyses include e.g. Devereux (2004), Bjelland et al. (2011), and Hart/Roberts (2011).

[2] So far comparable empirical evidence exists, to the best of the authors' knowledge, only for Portugal (see Martins et al. (2011) and Carneiro et al. (2011).

tries vs. entry jobs—hardly affects the regression results. However, not controlling for worker fixed effects, which is only possible using the job entries as the unit of observation, leads to an underestimation of the wage cyclicality. Third, I show that the procyclicality of the employment/population ratio is identical to the procyclicality of the real wages of job entries.

The remainder of the paper is structured as follows. The next Section gives a brief literature review on the macroeconomic importance of entry-wage cyclicality, 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 macroeconomic implications, while Section 6 concludes.

2 Literature Review

2.1 Macroeconomic Importance of Real Entry-Wage Cyclicality

The recent interest in real wage rigidity is driven by the ongoing debate on the ability of the canonical Mortensen-Pissarides search and matching model (Mortensen/Pissarides 1994) to generate realistically large cyclical fluctuations in unemployment (see, e.g., Shimer 2005, Hall 2005, Veracierto 2008). Shimer (2005, p. 45) for example shows “that a search and matching model in which wages are determined by Nash bargaining cannot generate substantial movements along a downward-sloping Beveridge curve in response to shocks of a plausible magnitude”. Gartner et al. (2011) show the “Shimer-Puzzle” also exists for Germany. They show that average labor market flows in Germany are much smaller than in the USA. Gartner et al. (2011, p. 9) show “that the standard deviations of labor market variables (unemployment, vacancies, market tightness, job-finding rate) in Germany are larger than in the United States (both in absolute terms and relative to productivity).” Shimer (2005, p. 45) states that "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 stickiness is one way to generate more variability of unemployment within the model, is widely shared (see, e.g., Hall 2005, Hall/Milgrom 2008, Gertler/Trigari 2009, Kennan 2010).

Especially the real wage of newly hired workers seems to play a crucial role in generating realistic cyclical fluctuations in the unemployment rate. Pissarides (2009) shows that the decision of opening a vacancy or not is mainly influenced by the real wage of newly hired workers. Pissarides (2009, p. 1341-1342) argues that “wages in continuing jobs may be completely fixed, and yet, if wages in new matches satisfy the Nash wage equation, the volatility of job creation will be unaffected by their wage stickiness. The wage stickiness that matters in [.. the search and matching] model is therefore wage stickiness in new matches, and the model’s Nash wage equation should be compared with the empirical evidence relating only to wages in new matches.” This point of view is also shared by Haefke et al. (2009). They show that wages of job entries out of non-employment in the USA respond one-to one to changes in labor productivity. The wages of incumbents however react very little to changes in productivity.

However, not all economists share the opinion that introducing rigid wages into the canonical model is a plausible way to solve the “Shimer-Puzzle”. Pissarides (2009, p. 1341), for example, dismisses theories based on cyclically rigid wages because empirically hiring wages are quite 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.” Pissarides 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/Hart 2006, Shin/Solon 2007).

However, there is an explanation why the empirical evidence—to which Pissarides (2009) refers—does not preclude acyclical wage setting by firms. Gertler and Trigari (2009, p. 71) argue that “While workers may transition between high- and low-wage jobs over the [business] cycle, the wage of new hires may still be tied to those of existing workers within the same firm.” Therefore, one has to control for the so-called ‘cyclical upgrading’ in booms and the ‘cyclical downgrading’ in recessions: “Suppose, for example, that a highly skilled machinist takes a job as a low-paid cab-driver in a recession [‘cyclical downgrading’] and then is reemployed as a high-paid machinist in a boom [‘cyclical upgrading’].” (Gertler/Trigari 2009, p. 73) Not controlling for the job could lead to the result that the wage is procyclical with the business cycle—however, the procyclical movement of the wage actually results only from the job change. Hence, empirical assessment of recent theories of hiring-wage rigidity requires an approach that identifies cyclical variation in hiring wages within particular jobs.

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. But whether introducing wage rigidity into the canonical model is the right way should be subject to empirical investigation: How rigid are real wages—and especially real entry wages—over the business cycle? New empirical evidence is needed on the cyclicality of real entry-wages which controls for ‘cyclical upgrading’ and ‘cyclical downgrading’ in employee/employer matches as pointed out by Gertler and Trigari (2009).

2.2 Entry-Wage Cyclicality: Previous Empirical Evidence and Methods of Measuring

To the best of the author's knowledge so far only two papers exist, which identify cyclical variation in hiring wages while controlling for 'cyclical upgrading' and 'cyclical downgrading' in employee/employer matches: Martins et al. (2011) and Carneiro et al. (2011). Both papers use the same matched employer-employee dataset for Portugal, but different time periods. Martins et al. use the 1982 to 2008 period, while Carneiro et al. use the shorter 1986 to 2007 period. Also, they use different approaches to identify the cyclical variation in wages.

Martins et al. (2011) identify entry jobs within firm, and track the real wage paid to newly hired workers in those jobs, and measure how the entry wages vary over the business cycle. For this 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 '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 trend controls. Martins et al. 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 a given firm-job.

Carneiro et al. (2011) estimate the cyclicality of entry wages and the cyclicality of the wage of incumbent workers in a one stage regression. They regress the individual log real wages of workers on the unemployment rate, a new-hire dummy variable, the unemployment rate interacted with the new-hire dummy variable, time-varying individual characteristics, and a time trend and its square. Further they control for worker fixed effects, job title fixed effects, and firm fixed effects. Carneiro et al. find that an increase in the unemployment rate by one percentage point leads to 2.67 percent lower real wages for newly hired workers.[3]

Despite the fact that the same data set is used in both papers, the results are quite different. Looking at the methodologies four reasons for the different results come to mind. First, Martins et al. (2011) use data of the 1982 to 2008 period, while Carneiro et al. (2011) use data of the 1986 to 2007 period. However, as a robustness check Martins at al. run their regression for the 1986 to 2008 period, which is nearly identical to the time period used in the paper of Carneiro et al. The estimated coefficient of the unemployment rate for the shorter time period is even lower than for longer time period: -1.59 vs. -1.8. Second, the difference in the results could steam from the fact that the papers control for different kinds of heterogeneity. Martins et al. (2011) control for firm-job fixed effects, while Carneiro et al. (2011) separately control, inter alia, for firm fixed effects and job (title) fixed effects. However, Carneiro et

[3] For incumbent workers Carneiro et al. (2011) find that an increase in the unemployment rate of one percentage point decreases wages by around 2.2 percent.

al. (2012, p. 13) show that estimates using firm-job fixed effects or separate firm fixed effects and job fixed effects hardly differ. Their results imply “that the inclusion of a match specific effect between firm and job title does not affect the results.” Third, Carneiro’s et al. (2012) units of observation are job entries and incumbents. This allows them to control for worker fixed effects and individual characteristics. Martins et al. (2011) are not able to do so, since their unit of observation is the entry job. Especially the introduction of the worker fixed effect could lead to differences in the results. As a robustness check Martins et al. (2011, Table 3) run regressions in which they control for worker fixed effects but not for jobs within firms.[4] For the longitudinally matched workers, restricted to workers changing employers, they estimate a coefficient of change in unemployment rate of -2.55—which is pretty close to the result of Carneiro et al. (2011). Hence, it seems that not controlling for worker fixed effects leads to an underestimation of wage cyclicality. Fourth, the different results could also be driven by the different units of observation. Martins et al. (2011) focus on the entry job, while Carneiro et al. (2011) focus on the job entry. None of the papers switches the unit of observation as a robustness check to show whether or not the unit of observation affects the results.

The relatively huge difference of the results of Martins et al. (2011) and Carneiro et al. (2011) for Portugal is the reason that I use different models to estimate the cyclicality of entry wages. Hence I am able to show whether the unit of observation affects the result and to show whether controlling for worker fixed effects, while simultaneously controlling for firm-job fixed effects, affects the results.

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 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 Beschäftigten-Historik, 2011).[5] 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

4 According to Gary Solon, Martins et al. (2011) apply a longitudinal first-difference regression—differencing out the worker fixed effects—in the worker-level regressions of Table 3.

5 Because of certain selection criteria—described in Sections 3.1.2—and a number of data inconsistencies in the first years of the BeH (see Section 8.1) the analyses can only be run for the 1977 to 2009 period. But data from earlier years is used for identifying job entries.

household surveys, measurement error due to erroneous reporting should be much weaker. Also, the BeH allows matching workers with firms, which is crucial to control for 'cyclical upgrading' and 'cyclical downgrading' in employee/employer matches, i.e. by controlling for firm-job fixed effects, as outlined in Section 2.

3.1 Data Selection and Identification Strategies

To create the data set for the empirical analysis I first identify all firms which employed at least seven workers[6] in at least one year in the 1975 to 2009 period. In those firms I identify any full-time worker. For each identified worker I then draw all existing employment spells for the 1975 to 2009 period—including part-time employment spell, apprenticeship spells etc. The obtained data set contains data of 59.711.757 workers in 1.635.679 firms.[7] This data set is used to identify job entries. After identifying job entries further data selection is necessary.

3.1.1 Identifying job entries

I try to stick as much as possible to Martins' et al. (2011) specifications for identifying jobs into which employers are observed to hire new workers. 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". As "job level" I use a four-category variable coded as blue-collar worker / no craftsman, craftsman / skilled labor[9], master craftsman[10,] and white-collar worker / salaried employee. Hence, I create unique job numbers that consist of the firm identification number, the occupation, and the job level.

To identify newly hired workers I use the individual's employment spells. An individual is a newly hired worker (job entrance) if the individual 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 last 365 days. The second condition makes sure that workers adjourning the employment for a short period of time—for whatever reason—are not counted as job entries when they return to the firm.

3.1.2 Data Selection

After the identification of job entries I run a first data selection, which is mostly defined by some features of the BeH.

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6 Worker must be subject to social security without any specific tokens. Number of workers evaluated at 30th June of each year.

7 I checked the data for inconsistencies and I dropped a small number of spells. The procedure and the inconsistencies found are provided in the Appendix (see Section 8.1).

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

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

10 Persons in this class are employed as blue-collar or white-collar workers.

(1) I use data for West Germany from 1977 onwards and for East Germany from 1993 onwards.[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) Since earnings data are right censored at the contribution assessment ceiling[13] (Beitragsbemessungsgrenze), only non-censored wage spells are considered in the analysis. In the original data set—which is used to identify job entries—between 4.03% (East Germany, 1997) and 8.33% (West Germany, 1992) of the wage spells in a year are censored over the sample period. I apply consistent top-coding instead of just dropping the censored wage spells.[14] 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. For West Germany the most spells (8.33%) are censored in the year 1992, for East Germany the most spells (6.99%) are censored in 2002. Therefore, I drop in each year the 8.34% / 7% highest wage spells for West / East Germany.[15]

(4) I restrict the data set to workers aged 16 to 65.

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11 For the years 1975 for West Germany and 1992 for East Germany, respectively, I cannot apply the identification strategy for job entries described above. Therefore I cannot use the data for the empirical analysis. Out of the following reasons I also decided to drop observations from Berlin for all years before 1993. 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 are not distinguished between East Berlin and West Berlin. Also, due to some data inconsistencies concerning the firm assignment in 1976 the data for the years 1976 are not used for the empirical analysis, but only for identifying job entries.

12 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 and more weekly hours of work but not fully employed. Further, the BeH contains 32 classifications for employment relationships, such as trainees, insured artistes and publicists and employees in partial retirement. I only keep employees subject to social security without particular tokens.

13 The contribution assessment ceiling is annually adjusted to the changes of earnings (see Table A3 in Section 8.2). 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.

14 I calculate the percentage of individuals subject to top-coded (censored) wages in every year. I determine the year in which the greatest percentage of the individuals was affected by the top code for the wage and then top code the wages for every year to yield the same percentage. I identify the threshold for the top-coding separately for West Germany and East Germany. 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.

15 Dropping top-coded spells leads to an underrepresentation of highly qualified (white collar) workers, making the results somewhat less generalizable. For a quantitative valuation of the effect of dropping censored spells see, e.g., Tables A3 and A4 of Stüber and Beissinger (forthcoming).

The obtained data set is used to create two data sets for the empirical analysis. To figure out whether the data selection process of Martins et al. (2011) affects the results I first create a data set applying their sample selection criteria. These "further selection criteria" (FSC) are very restrictive. For the second data set I relax the sample selection criteria.

For the FSC data set I consider only newly hired workers of firms which employed at least 50 full time workers at 30th 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 the firm is in the data set 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[16]. Martins et al. (2011) apply the FSC because they are focusing on so called "port-of-entry" jobs (see, e.g., Kerr 1954 or Doeringer/Piore 1970). However, Martins at al. (2011, p. 9) do not mean "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 not only a lot of jobs but also a lot of firms are dropped from the first data set. Therefore I relax the FSC for the creation of the second data set. I only keep the restriction that particular jobs have to 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.

Hence I obtain two data sets. Both data sets consist out of individual employment spells. They differ, however, in the way how I select the jobs for which I included the individual employment spells in the data sets. For the creation of the first data set in apply the FSC, for the creation of the second data set I keep only the restriction that particular jobs have to be observed in at least three years.

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[16] 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 data set.

3.2 Variables Descriptions and Descriptive Overview of the Final Data Samples

As the endogenous variable I use the 'typical real wage' of entry jobs and alternatively the individual entry wage. This allows running regressions in which the unit of observation is the entry job—following Martins et al. (2011)—and regressions in which the unit of observation is the job entry—following Carneiro et al. (2011).

Unfortunately, the BeH data does not contain monthly wages or hourly wages, but the wage[17] paid during the duration of the employment spell. Therefore, I cannot observe the wage of the first month of employment. However, since the exact duration of each employment spell is known, I can calculate the daily wage for each spell. An employment spell accounts for at most one year—1st of January to 31st of December. Since normally workers do not receive a wage increase within the first year, I am confident that the error caused by this calculation is not substantial. However, in one of the models I control for the different lengths of the employment spells. To calculate the daily real wage (in 2005 prices) I use the Consumer Price Index (CPI).[18]

As the 'typical real entry wage' w_{jt} I use either the modal or the mean daily real wage paid to workers newly hired into job *j* in period *t*. Table 1a provides summary statistics and shows the effects of the "further selection criteria" on the sample sizes. Using the modal wage some information is lost due to multiple modes (see Table 1a).

Table 1a
Number of entry jobs using the 'typical real entry wage' as endogenous variable

	Real mean wages		Real modal wages	
	Data set with FSC	Data set w/o FSC	Data set with FSC	Data set 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	37,028,491	367,529	20,830,454

FSC: "further selection criteria" (see Section 3.1.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.

Alternatively I use the daily real wage w_{ijt} paid in period *t* to worker *i* newly hired into job *j*. Table 1b provides summary statistics and shows the effects of the "further selection criteria" on the sample sizes. For the regressions using the individual wage data without the FSC I draw for each year a random 1 percent sample of the jobs

[17] 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.

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

(stratified by the number of entries per job). For each drawn job, I keep all employment spells of the 1977 to 2009 period. Concerning the number of job entries this leads roughly to a bisection of the original data set: of the 122,180,828 job entries 59,863,251 are dropped. Table A1 (see Section 8.2) shows the sample sizes statistics by year for the drawn subsample.

Table 1b
Number of job entries using the individual daily real wage as endogenous variable

	Real individual wages	
	Data set with FSC	Data set 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

FSC: "further selection criteria" (see Section 3.1.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.

The exogenous variables are presented in Table 2. In the Appendix (see Section 8.2) I provide some further information on the data. Table A2 provides statistics for single years for both data sets (with and w/o FSC). Additionally the Table provides information on the number of job entries if using the 'typical real entry wage' and the number of entry jobs if using individual daily real wage as the endogenous variable, respectively. Table A3 provides the unemployment rates and the inflation rates.

Table 2
Exogenous variables used in some model specifications

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 data set w/o FSC (with FSC) 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 subsample (IABS). Using the imputed variable, in both data sets 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 ≤ length of employment spell ≤ 12 month.

4 Empirical Analysis—Cyclicality of Real Entry Wages in Germany

4.1 Models

To estimate the cyclicality of real entry wages over the business cycle I follow Martins et al. (2011) and identify particular entry jobs within firms—controlling for heterogeneity in jobs within firms. I track the wage 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. I also stick to Martins et al. (2011) methodology and apply two stage regressions.[19] However, as to the unit of observation, I follow both—Martins et al. (2011) and Carneiro et al. (2011)—in using entry jobs and job entries. Because I do not use wage data from incumbent workers, I do not observe a specific worker frequently enough to introduce person fixed effects. This is especially true for earlier birth cohorts where individuals often worked for only one employer in their working life. Using the data sets described in Section 3.2 I am able to show whether the unit of observation affects the results, but I am not able to show whether the introduction of person fixed effects affects the results. Therefore, I draw a subsample of the data set without FSC that only includes workers which enter at least ten jobs during the observed time period. Using this subsample I can control whether or not the introduction of worker fixed effects—while simultaneously controlling for jobs within firm—affects the results. Hence I apply three models to estimate the cyclicality of entry wages. Table 3 provides an overview of the three models. The models only differ on the first stage regression, while the second stage regression is identical for all three models.

Table 3
Overview of the regression models

Model	Unit of observation	Job fixed effects	Worker fixed effects	Individual controls
1	entry jobs	yes	no	no
2	job entry	yes	no	yes
3	job entry	yes	yes	yes

Model 1. I follow Martins et al. (2011) and estimate the cyclicality of entry wages applying a two stage regression. The object of the analysis is to estimate period fixed effects common to all entry jobs, β_t, and to relate them to business cycle conditions. In the first stage the period fixed effects β_t are estimated by:[20]

$$(1a) \qquad \ln(w_{jt}) = \alpha_j + \beta_t + \varepsilon_{jt} ,$$

[19] The unemployment rate—the regressor of interest—varies only between years. If 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 on clustering and serial correlation in panels can be found, e.g., in Angrist and Pischke (2009, Chapter 8.2).

[20] To control for the job fixed effects I use the stata® 11.1 command '**areg** *depvar [indepvars] [weight]*'. The command fits a linear regression absorbing one categorical variable.

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 paid for a job. The variable α_j is a job fixed effect and ε_{jt} is the zero-mean error term representing temporary job-specific departures from the general period effect. To quantify the cyclicality of entry wages I regress—in the second stage regression—the $\hat{\beta}_t$ time series on the unemployment rate u_t, secular time trend controls, and a dummy that is one for 1984 and later years $(D_{\geq 1984})$:[21]

$$(1b) \qquad \hat{\beta}_t = \delta u_t + \lambda_0 t + \lambda_1 t^2 + D_{\geq 1984} + \varepsilon_t .$$

I introduce the $D_{\geq 1984}$ dummy 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.[22]

Model 2. For two reasons I introduce a altered statistical model to estimate a time series for β_t. First, the unit of observation in model 1 is the entry job itself, the unit of observation in model 2 is the job entry as in Carneiro et al. (2011). Therefore, I can check whether the differences in Martins et al. and Carneiro et al. results are driven by the unit of observation. The second reason is motivated by the data. As described in detail in Section 3.2, the data used does not provide monthly wages but wages for employment spells. Employers have to report to the social security system on a yearly base. Therefore, the wage is based on an employment period up to one year—depending on the date of employment. The model 2 allows controlling for this shortcoming of the data by controlling for the length of the employment spell. It also allows controlling for wage differences in hiring wages which are due to individual characteristics:

$$(2) \qquad \ln(w_{ijt}) = \alpha_j + \beta_t + \gamma' X_{it} + \varepsilon_{ijt} ,$$

where w_{ijt} denotes the real wage paid in period *t* to worker *i* newly hired into job j and X_{it} is vector with individual characteristics of the worker *i* for period *t* (see Table 1). To quantify the cyclicality of entry wages I regress, as in the first model, the $\hat{\beta}_t$ time series on u_t, secular time trend controls, and $D_{\geq 1984}$ (see Equation 1b).

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21 I use the stata® 11.1 command '**reg** *depvar [indepvars] [weight], vce(robust)*' and estimate the robust (or sandwich) estimator of the variance.

22 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 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.

Model 3. To control whether introducing worker fixed effects affect the results I run regression models that estimate linear two-way fixed-effects:[23]

$$(3) \qquad \ln(w_{ijt}) = \alpha_i + \alpha_j + \beta_t + \gamma' X_{it} + \varepsilon_{ijt},$$

where α_i is a newly introduced worker fixed effect. To quantify the cyclicality of entry wages I estimate, as in the first two models, the regressions of the $\hat{\beta}_t$ time series on u_t, secular time trend controls, and $D_{\geq 1984}$ (see Equation 1b).

4.2 Results

This section presents the results of all three models using data sets with and without FSC, respectively. The estimated coefficients of the unemployment rate, for two specifications of the models 1 and 2 are displayed in Tables 4a and 4b, respectively.

Table 4a
Model 1—estimated coefficients of the unemployment rate $(\hat{\delta})$ using 'typical' real entry wages

	Estimated coefficients of the unemployment rate using model 1			
	Modal wage		Mean wage	
	Data set with FSC	Data set w/o FSC	Data set with FSC	Data set w/o FSC
(1.1) according to Martins et al. (2011): 1st stage unweighted OLS, 2nd stage OLS weighted by number of entry jobs	-0.8430** (0.3679)	-0.9981*** (0.3367)	-0.8772** (0.3194)	-0.9160*** (0.3253)
(1.2) 1st stage OLS weighted by number of job entries, 2nd stage unweighted OLS	-1.2926*** (0.3824)	-1.1052*** (0.3605)	-0.8846** (0.3397)	-0.8563** (0.3327)

Robust standard errors in brackets. FSC: "further selection criteria" (see Section 3.1.2). 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 ≥1984. *** p<0.01, ** p<0.05, * p<0.1.

The results for model 1 show, that the estimated coefficients of the unemployment rate differ depending on the 'typical real entry wage', the data set, and the regression model (see Table 4a). However, the differences are not statistically significant at the five percent level. An increase in the unemployment rate of one percentage point decreases the real entry wages of job entries within given firm-jobs by between 0.84 to 1.29 percent. The mean of the eight estimated coefficients is -0.97, the mean of the four estimated coefficients for the first (second) specification of model 1 is 0.91 (1.03). Also the used data set—with or without FSC—only slightly affects the outcome. Hence, the selection criteria from Martins et al. (2011) do not seem to influence the outcome of the regressions. However, the choice of the 'typical real entry wages' seems to be critical to some degree. The 'typical' wage leads—within a

[23] I use the stata® ado file 'a2reg' by Ouazad (2007).

given data set and a given regression model—to a maximal difference in the estimated coefficients of 0.41. Also the choice of the model version affects the outcome. In the first version of model 1—regression (1.1)—I use weights according to Martins et al. (2011): 1st stage unweighted OLS, 2nd stage OLS weighted by the number of observed entry jobs per year.[24] In regression (1.2) I control for the fact that a 'typical real wage' is representing different number of job entries by weighting the 1st stage OLS by the number of job entries and using an unweighted 2nd stage regression. The maximal (minimal) difference in the estimated coefficients caused by the model version is 0.45 (0.01).

The results of model 2—using individual wages instead of 'typical real entry wage'—are quite similar (see Table 4b). An increase in the unemployment rate of one percentage point decreases the real entry wages of job entries within given firm-jobs by between 0.83 to 0.92 percent.

Some robustness checks for the regressions of the Tables 4a and 4b are provided in the Appendix (see Section 8.3).

Table 4b
Model 2—estimated coefficients of the unemployment rate $(\hat{\delta})$ using individual wages

	Estimated coefficients of the unemployment rate using model 2	
	Data set with FSC	Data set w/o FSC
(2.1) 1st stage unweighted OLS, 2nd stage OLS weighted by number job entries	-0.8369*** (0.2667)	-0.8269*** (0.2666)
(2.2) 1st stage unweighted OLS, 2nd stage OLS unweighted	-0.9215*** (0.2867)	-0.9023*** (0.2846)

Robust standard errors in brackets. FSC: "further selection criteria" (see Section 3.1.2). 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 ≥1984. *** p<0.01, ** p<0.05, * p<0.1. Individual controls used in the 1st stage regression: education, sex and nationality, and age, age2 and length of the employment spell.

Interestingly, neither the restrictive sample selection criteria from Martins et al. (2011) nor the use of the 'typical' wage instead of individual wages strongly affect the results. Only the use of the modal wage as the 'typical' wage—in combination with regression model (2.2)—results in higher estimates for the wage cyclicality compared to the other estimates. In the next Section I discuss why the modal wage, especially for the data set with FSC, seems not to be suitable for the analysis.

To control whether introducing worker fixed effects affect the results I estimate model 3 which employs linear two-way fixed-effects. As mentioned above, the data set is not optimally suited for this kind of regression, since I only observe job entries.

[24] Martins et al. (2011, p. 13) use the weights "in an effort to correct for the heteroskedasticity resulting from the wide variation in the per-year sample size." 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.

Hence I often do not observe a specific worker frequently enough to introduce person fixed effects. This is especially true for earlier birth cohorts where individuals often worked for only one employer in their working life. Thus, for the linear two-way fixed-effects regressions I only use wage spells of entries which I observe at least ten times in the 1977 to 2009 period. Due to the sampling the data set is reduced from 62,317,577 to 8,120,618 employment spells of job entries. To test whether the sampling affects the results, I re-run the regression shown in Table 4b as a robustness check (see Table 4c). The estimated coefficients of the control regressions (3.1 to 3.4) have about the same magnitudes as the estimated regressions using the larger data set (see right panel of Table 4b). Hence, using the subsample for the regressions hardly affects the results.

Table 4c
Model 3—estimated coefficients of the unemployment rate $\left(\hat{\delta}\right)$ using individual wages

			Estimated coefficients of the unemployment rate using model 3
Control regressions w/o worker fixed effects (wfe)	(3.1) like (2.1): 1^{st} stage unweighted **OLS**, controlling for job fixed effects (jfe), 2^{nd} stage OLS weighted by number job entries	Ind. controls in 1^{st} stage reg.: (a) and (b)	-.8491*** (.2021)
	(3.2) like (2.2): 1^{st} stage unweighted **OLS**, controlling for jfe, 2^{nd} stage unweighted OLS		-.8363*** (.2180)
	(3.3) 1^{st} stage unweighted **OLS**, controlling for jfe, 2^{nd} stage OLS weighted by number job entries	Ind. controls in 1^{st} stage reg.: (b)	-0.8566*** (0.2054)
	(3.4) 1^{st} stage unweighted **OLS**, controlling for jfe, 2^{nd} stage unweighted OLS		-0.8457*** (0.2205)
a2reg-regressions with wfe	(3.5) 1^{st} stage unweighted **linear two-way fixed-effects regressions**, controlling for wfe and jfe, 2^{nd} stage OLS weighted by number job entries		-1.2714*** (0.2196)
	(3.6) 1^{st} stage unweighted **linear two-way fixed-effects regressions**, controlling wfe and jfe, 2^{nd} stage unweighted OLS		-1.2658*** (0.2322)

Robust standard errors in brackets. FSC: "further selection criteria" (see Section 3.1.2). 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 ≥1984. *** p<0.01, ** p<0.05, * p<0.1. Individual controls used in the 1st stage regression: (a) education, sex and nationality, and (b) age, age2 and length of the employment spell.

Comparing the results of the control regressions (3.1 to 3.4) with the results of the linear two-way fixed-effects regressions (3.5 and 3.6) shows, that not controlling for worker fixed effects seems to lead to an underestimation of entry wage cyclicality. This seems to explain the different results from Martins et al. (2011) and Carneiro et al. (2011): Carneiro et al.—controlling for worker fixed effects—estimate higher wage cyclicality than Martins et al.—not controlling for worker fixed effects.[25]

In the next Section I discuss the results of the different regressions. Further I focus on the question which model seems to be suited best to analyze whether introducing wage rigidity in the Mortensen-Pissarides model in order to amplify realistic volatility of unemployment is a sound strategy in the light of the empirical evidence.

5 Discussion of the Results

Most of the estimated coefficients of the unemployment rate displayed in Tables 4a and 4b are in the general vicinity of -0.88 and the estimated coefficients are not significantly different from each other on the 5% level. Controlling for worker fixed effects results in a higher estimate for the wage cyclicality of about -1.27 (see Table 4c).

5.1 Evaluation of the Regression Models

Due to the different estimates for the wage cyclicality, two questions arise: (1) what are the implications of the different models and (2) which model is most suitable to answer the question whether the Mortensen-Pissarides model can account for the cyclical variability of unemployment in light of the magnitude of the entry-wage cyclicality.

Martins et al. (2011, Figure 3) show a sample distribution of differences between individual worker's log wage and modal log wage in job/year. For the data of Portugal with hourly wages this measure seems to be quite good. For Germany, however, the modal log wage of jobs within firms as well as the mean log wage of jobs within firms differs strongly from the individual worker's log wage (see Figure 1).

Table 5
Summary statistics for the differences between individual worker's log wage and 'typical wage' in job/year

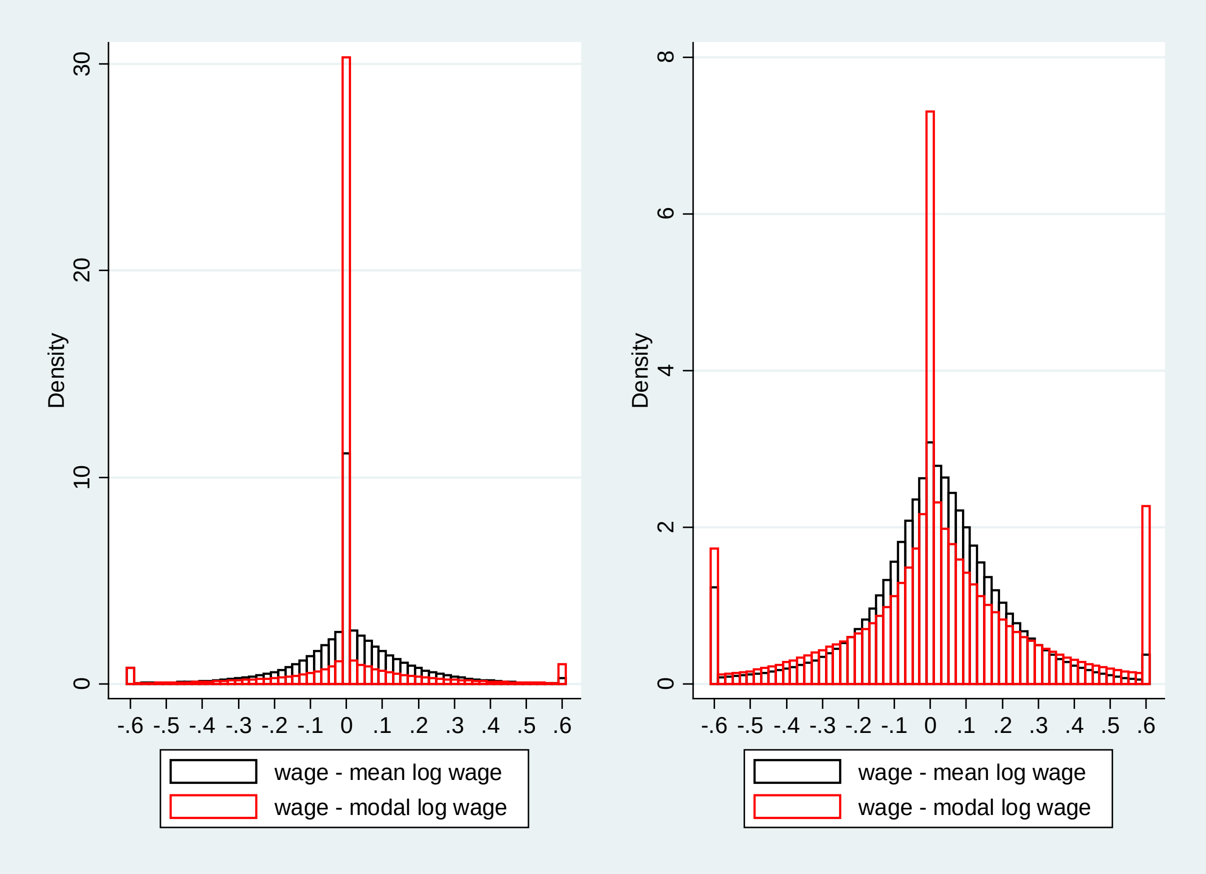
	Mean job wage	Modal job wage	Mean job wage	Modal job wage
	Data set w/o FSC		Data set with FSC	
Observations	122,180,828	38,997,678	30,772,919	9,750,533
Mean	0.000	0.010	0.000	0.025
Std. Dev.	0.202	0.227	0.241	0.343
Variance	0.0409	0.052	0.058	0.118
Skewness	-1.111	0.871	-1.271	0.514
Kurtosis	11.382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

FSC: "further selection criteria" (see Section 3.1.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.

[25] As a robustness check Martins et al. (2011, Table 3) run regressions in which they control for worker fixed effects but not for jobs within firm. For the longitudinally matched workers, restricted to workers changing employers, they estimate a coefficient of change in unemployment rate of -2.55—which is pretty close to the estimate of -2.67 from Carneiro et al. (2011).

This seems to be especially true for the data set without FSC (right panel of Figure 1). This first visual impression is also supported by simple summary statistics (see Table 5). The difference between individual worker's log wage and modal log wage in job/year for the data set with FSC has a variance that is approximately twice as high as for the other measures. A further disadvantage of using the modal wage as the 'typical' wage is the fact that a lot of observations are lost because for some job/years the mode cannot be calculated due to multiple modes. Hence, the modal wage for jobs seems not to be a suited for empirically analysis.

Figure 1
Distribution of differences between individual worker's log wage and 'typical' log wage in job/year for the data set w/o FSC (left Panel) and for the Data Set with FSC (right Panel)



FSC: "further selection criteria" (see Section 3.1.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.

The results of Tables 4a and 4b show, that the application of the FSC in general hardly affects the results. The exceptions are the estimates using the modal wage as the 'typical' wage. For the modal wage the difference in the estimated coefficients—due to the application of the FSC—are +0.16 and -0.19, respectively.

Concerning the weighting, in the regressions using the 'typical wage' (model 1) I prefer the first stage regressions weighted by the number of observed workers in the job, because jobs within firms are observed with different frequencies. Martins et al. (2010) only weight the second stage regression by the number of entry jobs to control for heteroskedasticity resulting from the wide variation in the per-year sample size. Table A3 shows that in the German data the per-year sample size hardly varies. Hence a weighting in the second stage regressions seems not to be necessary. Also, it is not obvious whether a weighting by the number of entry jobs or weighting by number job entries is better suited to control for heteroskedasticity.

Hence, in general the individual worker's log wage seems to be better suited for the regressions. If one wants to use a 'typical' wage it seems that—at least for the used data set—the mean wage should be favored over the modal wage. Though, the use of a 'typical' wage has further disadvantages. Using the 'typical' wage does not allow to control for individual characteristics, and—more important—it does not allow to control for worker fixed effect. Not controlling for worker fixed effects leads to an under estimation of the wage cyclicality—in the case of Germany—of around 23.6 percent or 0.3 percentage points.

To sum up, a model using individual worker's log wage and controlling for job and worker fixed effects simultaneously seem to be best suited for the analysis of wage cyclicality. Whether or not I weight the second stage regression by the number of job entries does not affect the results. However, since controlling for worker fixed effects for job entries is problematic if workers do not the change jobs at least a few times, the idea of Carneiro et al. (2011)—looking at job entries and incumbents—seems to be a promising alternative. However this has some cons as well. Due to their model specification the wages of entries and the wages of incumbents are forced to have, e.g., an identical time trend.

5.2 Implications of the Results

The estimated coefficients of the unemployment rate—controlling for job and worker fixed effects—are in the general vicinity of -1.27 (see Table 4c). "Recognizing that, with procyclical labor force participation, the negative of the change in the unemployment rate is an attenuated version of proportional changes in employment, these estimates imply that the cyclical elasticity of entry wages and the cyclical elasticity of employment are of similar magnitude." (Martins et al. 2011, p. 16) Therefore, I follow Martins et al. (2011) 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.

$$\Delta u = \alpha_1 + \beta_1 \log(\Delta GPD_{real}) + t + D_{\geq 1991}$$

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

I find that a one-point increase in the unemployment rate is associated with a 1.27 percent reduction $\left(\beta_2/\beta_1 = -1.27\right)$ in the employment/population ratio. This procyclicality of employment is identical to the procyclicality I have estimated for real entry wages. Martins et al. (2011, p. 22) find a similar result for Portugal: The "procyclicality in entry wages is substantial in the sense that the cyclical elasticity of this price variable is of approximately the same magnitude as the cyclical elasticity of employment."[26] The finding that the procyclicality of employment is (nearly) identical to the procyclicality of real entry wages, "like practically all the longitudinal evidence on workers' wage cyclicality, counters a view often stated by influential macroeconomists that wages are much less cyclical than employment and unemployment." (Martins et al. 2011, p. 22)

'Cyclical upgrading' may cause an underestimation of the true procyclicality of entry wages. This would be the case if in a recession employers would be able to recruit higher qualified workers at any given wage. This would lead to a lower effective wage per efficiency unit of labor. 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 about 70 percent of the corresponding U.S. result. Given the results of Büttner et al. (2010) the procyclicality of entry wages estimated in this paper may only be slightly underestimated.

Finally, returning to the question 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. 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 by assuming that the real hiring wage declines by less than 0.68 percent when the unemployment rate rises by one percentage point (see Table 6).

Table 6
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†	0.43	0.19
Wages: non-decreasing rates‡	1.52	0.68

† 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 + δ, where r is the interest rate and δ is the (constant) job destruction hazard rate.

‡ The 'non-decreasing rate' wage "is constant for the life of the match if the match begins in a 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.

Source Kennan 2010, Tab 2, p. 650, values converted to an unemployment change of one percentage point.

[26] Martins et al. (2011, p. 17) show that in Portugal "a one-point increase in the unemployment rate is associated with a 1.6 percent reduction in the employment/population ratio."

Since my estimates show a decline of real hiring wage 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.

6 Conclusions

Using longitudinally matched data from the German Institute for Employment Research (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 rather considerably respond to business cycle conditions.

I show that the unit of observation—job entries vs. entry jobs—hardly affects the regression results. However, not controlling for worker fixed effects, which is only possible using the job entries as the unit of observation, leads to an underestimation of the wage cyclicality—in the case of Germany—of around 23.6 percent or 0.3 percentage points. The estimates—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. This strengthens 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 amplify realistic volatility of unemployment is not supported by the data.

The estimates indicate 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.

Furthermore I show that the procyclicality of the employment/population ratio is identical to the procyclicality of real entry wages. This counters the view of many macroeconomists that wages are much less cyclical than employment and unemployment.

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8 Appendix

8.1 Data Preparation—Inconsistency the Data Sets

I rarely identified inconsistencies in the data set. In total I found approximately 20 different types of inconsistencies. However, most types of inconsistencies were identified in spells of part-time worker or spells of workers that were not employed subject to social security without specific token. These spells are only used to identify job entries and are not used in the regression.

Most common I observed spells that were identical except of 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, it can happen that the human resources department has already sent out the information to the retirement pension institution about the end of the original employment contract. However, at the end of the year the human resources department will again sent out information to the retirement pension institute, this time for the full period the worker was employed at the firm in that year. This can leads to two spells for a certain worker that are identical except for the end date of the employment. 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. Therefore, one sometimes observes that the above mentioned spells show a higher average daily wage for the longer spell. 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 data set that I used to identify job entries:

1. If I observed identical spells I only kept one of the spells.

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

   a. spell *a* with wage ≠ 0 and spell *b* with wage A = 0 → keep spell *a*

   b. wage of spell *a* > wage of spell *b* → keep spell *a*

   c. spell *a* ends after spell *b* → keep spell *a*

3. If I observed spells that were identical except for two variable I used, e.g., the following rules to decide which spell I kept:

   d. wage of spell *a* ≠ wage of spell *b* & spell *a* ends after spell *b* → keep spell *a*

8.2 Data Description and Data Selection—Further Tables

Table A1
Number of entry jobs and job entries by year for the data set with real individual wages without FSC and the drawn subsample of this data set

Year	Real individual wages, data set without FSC			
	Number of job entries		Number of entry jobs	
	subsample used for regressions	Original data set	subsample used for regressions	Original data set
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

FSC: “further selection criteria” (see Section 3.1.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.

Table A2
Number of entry jobs and job entries by year for different samples

Year	Real individual wages and mean wages				Real modal wages			
	Number of job entries		Number of entry jobs		Number of job entries		Number of entry jobs	
	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

FSC: “further selection criteria” (see Section 3.1.2). Jobs in the sample without FSC are observed at least 3 years of the 1977 to 2009 period.

Table A3
Contribution assessment ceiling for Western Germany, lower earnings limit, inflation, and unemployment rate

Year	Contribution assessment ceiling for Western Germany (Euro per month)[a]				German Consumer Price Index[b]		Unempl. rate[c] (in %)
	Compulsory pension insurance scheme		Lower earnings limit (§8, Social Code IV)		Index	Change to previous year (in %)	
	West Germany	East Germany	West Germany	East 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		[d]217.30		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: Deutsche Rentenversicherung Knappschaft-Bahn-See; Hauptverwaltung Bochum.

[b] Consumer Price Index 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.

8.3 Robustness Checks

To assure the robustness of the results from Section 4, I run several regressions. Tables A1a and A1b shows estimated coefficients of the unemployment rate of slightly altered versions of the baseline models (presented in Tables 3a and 3b), and Table A1a also shows estimated coefficients of the lagged unemployment rate.

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 (1a) and (2)) changes to:

$(1a^*)$ $\ln(w_{jt}) = \alpha_j + \beta_t + East_{jt} + \varepsilon_{jt}$ and

(2^*) $\ln(w_{ijt}) = \alpha_j + \beta_t + \gamma' X_{it} + East_{it} + \varepsilon_{ijt}$.

However, introducing the *East* Dummy does not affect 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.

Table A4a

Robustness checks for model 1—estimated coefficients of the (lagged) unemployment rate $(\hat{\delta})$ using 'typical' real entry wages

	Equation (1) - typical real entry wage			
	Modal wage		Mean wage	
	with FSC	w/o FSC	with FSC	w/o FSC
Estimated coefficients of the **unemployment rate**				
Like (1.1) but 2nd reg. weighted by number of job entries	-0.7243** (0.3525)	-0.9338*** (0.3334)	-0.7806** (0.3049)	-0.8545** (0.3222)
Like (1.1) but 2nd reg. unweighted	-0.8441** (0.3777)	-1.0287*** (0.3516)	-0.8765** (0.3282)	-0.9440** (0.3402)
Like (1.1) but with a dummy for East Germany in the 1st reg	-0.8429** (0.3680)	-0.9980*** (0.3367)	-0.8771** (0.3195)	-0.9160*** (0.3253)
Like (1.1) but 2nd reg. weighted by number of job entries and with a dummy for East Germany in the 1st reg.	-0.7242** (0.3526)	-0.9337*** (0.3334)	-0.7804** (0.3049)	-0.8545** (0.3222)
Like (1.1) but 2nd reg. unweighted and with a dummy for East Germany in the 1st reg.	-0.8439** (0.3777)	-1.0286*** (0.3516)	-0.8763** (0.3282)	-0.9440** (0.3402)
Like (1.2) but 2nd reg. weighted by number job entries	-1.1586*** (0.3617)	-1.0028*** (0.3434)	-0.7781** (0.3164)	-0.7723** (0.3147)
Like (1.2) but 2nd reg. weighted by number entry jobs	-1.2855*** (0.3745)	-1.0728*** (0.3452)	-0.8804** (0.3310)	-0.8333** (0.3170)
Like (1.2) but 2nd reg. weighted by number entry jobs and with a dummy for East Germany in the 1st reg.	-1.2859*** (0.3744)	-1.0728*** (0.3452)	-0.8803** (0.3311)	-0.8332** (0.3170)
Like (1.2) but 2nd reg. weighted by number job entries and with a dummy for East Germany in the 1st reg.	-1.1590*** (0.3616)	-1.0028*** (0.3434)	-0.7780** (0.3164)	-0.7722** (0.3147)
Like (1.2) but with a dummy for East Germany in the 1st reg.	-1.2930*** (0.3822)	-1.1052*** (0.3605)	-0.8845** (0.3397)	-0.8563** (0.3327)
Estimated coefficients of the **lagged unemployment** rate				
Like (1.1)	-0.8926** (0.3291)	-0.8692** (0.3202)	-0.8440*** (0.2956)	-0.7998** (0.0363)
Like (1.1) but 2nd reg. weighted by number job entries	-0.8030** (0.3151)	-0.8156** (0.3130)	-0.7710** (0.2817)	-0.7483** (2997)
Like (1.1) but 2nd reg. unweighted	-0.8902** (0.3385)	-0.8936** (0.3386)	-0.8435** (0.3037)	-0.8219** (0.3240)
Like (1.1) but with a dummy for East Germany in the 1st reg.	-0.8925** (0.3291)	-0.8692** (0.3202)	-0.8439*** (0.2956)	-0.7997** (0.3063)
Like (1.1) but 2nd reg. weighted by number job entries and with a dummy for East Germany in the 1st reg.	-0.8028** (0.3151)	-0.8155** (0.3130)	-0.7709** (0.2817)	-0.7483** (0.2997)
Like (1.1) but 2nd reg. unweighted and with a dummy for East Germany in the 1st reg.	-0.8901** (0.3385)	-0.8936** (0.3386)	-0.8434** (0.3037)	-0.8219** (0.3240)
Like (1.2)	-1.3292*** (0.3494)	-1.0217*** (0.3414)	-0.8688*** (0.3063)	-0.7669** (0.3093)
Like (1.2) but 2nd reg. weighted by number job entries	-1.2510*** (0.3314)	-0.9435*** (0.3179)	-0.7864** (0.2874)	-0.7001** (0.2873)
Like (1.2) but 2nd reg. weighted by number entry jobs	-1.3356*** (0.3401)	-1.0001*** (0.3228)	-0.8641*** (0.2989)	-0.7511** (0.2922)
Like (1.2) but with a dummy for East Germany in the 1st reg.	-1.3291*** (0.3496)	-1.0217*** (0.3414)	-0.8688*** (0.3063)	-0.7669** (0.3093)
Like (1.2) but 2nd reg. weighted by number job entries and with a dummy for East Germany in the 1st reg.	-1.2510*** (0.3315)	-0.9435*** (0.3179)	-0.7864** (0.2874)	-0.7001** (0.2873)
Like (1.2) but 2nd reg. weighted by number entry jobs and with a dummy for East Germany in the 1st reg.	-1.3355*** (0.3402)	-1.0001*** (0.3228)	-0.8641*** (0.2989)	-0.7510** (0.2922)

OLS Regression. Robust standard errors in brackets. FSC: "further selection criteria" (see Section 3.1.2). 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 ≥1984. *** p<0.01, ** p<0.05, * p<0.1. Estimates for regressions (1.1) and (1.2) see Table 4a.

Table A4b
Robustness checks for model 2—estimated coefficients of the unemployment rate $(\hat{\delta})$ using individual wages

Estimated coefficients of the unemployment rate	Using Equation (2) with individual real wages	
	with FSC	w/o FSC
Like (2.1) but with a dummy for East Germany in the 1st reg.	-0.8369*** (0.2667)	-0.8269*** (0.2666)
Like (2.1) but without individual controls in the 1st reg.	-0.8369*** (0.2667)	-0.8269*** (0.2666)
Like (2.1) but without individual controls in the 1st reg. and with for East Germany in the 1st reg.	-0.7781** (0.3164)	-0.7571** (0.3114)
Like (2.2) but with a dummy for East Germany in the 1st reg.	-0.9215*** (0.2867)	-0.9023*** (0.2846)
Like (2.2) but without individual controls in the 1st reg.	-0.9214*** (0.2867)	-0.9023*** (0.2846)
Like (2.2) but without individual controls in the 1st reg. and with for East Germany in the 1st reg.	-0.8846** (0.3397)	-0.8514** (0.3320)

Robust standard errors in brackets. FSC: “further selection criteria” (see Section 3.1.2). 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 ≥1984. *** p<0.01, ** p<0.05, * p<0.1. Estimates for regressions (2.1) and (2.2) see Table 4b.

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