Wage Rigidity in Germany

Stüber, Heiko

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Die Forschungseinrichtung der Bundesagentur für Arbeit

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Contents

List of Tables

List of Figures

List of Abbreviations

A	Austria
ARIMA	auto-regressive integrated moving average
B	Belgium
BeH	Beschäftigten-Historik (the employee history file of the IAB)
BSWS	Basic Survey on Wage Structure
CDN	Canada
CH	Switzerland
coef.	coefficient
CPI	consumer price index
CPS	Current Population Survey
D	Germany
DFL	method of DiNardo, Fortin, and Lemieux (1996) (see Section 2.3)
DK	Denmark
DNWR	downward nominal wage rigidity
E	Spain
ECHP	European Community Household Panel
ECI	Employment Cost Index
EFA	earnings-function approach
EU	European Union
EU-10	The EU-10, the group of ten countries that have joined the EU in 2004.
F	France
FIN	Finland
FSC	further selection criteria (see Appendix D.2)
GB	Great Britain
GR	Greece
GradAB	Graduate Program of the IAB and the School of Business and Economics of the University Erlangen-Nuremberg
GSOEP	German Socio-Economic Panel
HICP	harmonised index of consumer prices
HLA	histogram-location approach
HNA	hyperbolic-notional approach
I	Italy
IAB	Institute for Employment Research (Institut für Arbeitsmarkt- und Berufsforschung)
IABS	IAB-Beschäftigtenstichprobe

IMSS	Administrative records of the Instituto Mexicano del Seguro Social
IRL	Ireland
J	Japan
JFE	job fixed effects
L	Luxembourg
LIAB	linked employer-employee dataset from the IAB
LSDV	least squares dummy variable
MEX	Mexico
N	Norway
NAIRU	non-accelerating-inflation rate of unemployment
NES	New Earnings Survey
NL	the Netherlands
NZ	New Zealand
OECD	Organization for Economic Co-operation and Development
OLS	ordinary least squares
P	Portugal
PPI	producer price index
PSID	Panel Study of Income Dynamics
RIF	recentered influence function
S	Sweden
SLFS	Swiss Labor Force Survey
SSIF	Sample of the Social Insurance Files
std.	standard
std. dev.	standard deviation
std. err.	standard error
SUR	seemingly unrelated regression
UK	United Kingdom
UQR	unconditional quantile regressions
USA	United States of America
w/o	without
WFE	worker fixed effects
WHIP	Worker History Italian Panel

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Heiko Stüber
Nuremberg, April 2013

Preamble and Motivation

This book contributes to two facets of the discussion on wage rigidity: downward nominal wage rigidity (DNWR) and real wage rigidity of newly hired workers over the business cycle. It is divided into three parts.

The first and major part of the book focuses on DNWR. It provides an overview on its causes and degree, deals with the macroeconomic consequences of DNWR, and analyzes whether or not DNWR affects workers differently, conditional on their characteristics, their position in the wage change distribution, and their employers' characteristics.

The second part of the book focuses on real wage rigidity of newly hired workers over the business cycle. One way of generating realistically cyclical fluctuations in the unemployment rate in the Mortensen-Pissarides model is the introduction of rigid wages into the model. This part of the book contributes to the discussion on whether or not this assumption can be confirmed empirically.

The third part of the book summarizes the results and offers an outlook for future research.

Motivation for Part I: Downward Nominal Wage Rigidity

Price stability adds to economic wealth and is hence favored by many economists. Advantages of price stability include, e.g., a reduced inflation risk premium in interest rates and the reduction of uncertainties about the general development of prices – which leads to more transparency of relative prices. Since the early 1990s the prior goal of central banks' monetary policy has therefore been the assertion and maintenance of low inflation rates (i.e. inflation rates below but close to 2 %) in order to assure price stability.

However, some economists call attention to possible negative effects of low inflation rates. The most prominent critique can be traced back to Tobin (1972). He advances the view that DNWR in combination with low inflation rates could hamper necessary real wage adjustments of firms that have economic difficulties. This would lead to a higher level of real wages and to an increase of the equilibrium unemployment rate. Tobin (1972) argues that "inflation greases the wheels of the labor market": a certain positive inflation rate is necessary in order to enable employers to decrease the real wages of employees without being forced to decrease nominal wages – which is often not accepted by employees.

Concerns about potentially adverse employment effects of low inflation have given rise to a plethora of studies on the extent of DNWR, such as the microeconometric multicountry studies of Behr and Pötter (2010), Knoppik and

Beissinger (2009), and Dickens et al. (2007a) or the survey evidence provided by Bewley (1999). Focusing on the compression of wage cuts, microeconometric studies usually find a high degree of DNWR. In Germany, e.g., approximately 28 percent of wage cuts desired by employers are avoided because of DNWR (Knoppik and Beissinger, 2009). Several studies also show that certain workers frequently experience nominal wage freezes, while other workers experience nominal wage cuts (see, e.g., Kahn, 1997; Beissinger and Knoppik, 2001; Anspal and Järve, 2011), and that firm characteristics play a crucial role as well (see, e.g., Babecký et al., 2010). Given the microeconometric evidence, the observed macroeconomic effects on aggregate real wages and employment seem to be surprisingly weak, leading Lebow et al. (1999) to speak of a "micro-macro puzzle".

Chapter 1

provides an overview on causes, extent, and implications of DNWR with a focus on Germany. The chapter is a translated and slightly revised compendium of the book article "Ursachen, Ausmaß und Implikationen von Abwärtsnominallohnstarrheit" [Causes, extent and implications of downward nominal wage rigidity] (Beissinger and Stüber, 2010) and the IAB Brief Report "Geldpolitik und Beschäftigung: Ist niedrige Inflation Gift für den Arbeitsmarkt?" [Monetary policy and employment: is low inflation the labor market's poison?] (Stüber and Beissinger, 2011).

Chapter 2

deals with the macroeconomic consequences of DNWR and offers a solution to the "micro-macro puzzle": I show that wage cuts as well as wage increases are compressed in the presence of DNWR. Because of the compression of wage increases, the macroeconomic effects on aggregate real wages are weak. I find that a decrease in inflation of one percentage point only causes an average increase of real wage growth between 0.013 and 0.060 percent. The results indicate that DNWR does not provide a strong argument against low inflation targets. The chapter is based upon the article "Does downward nominal wage rigidity dampen wage increases?" (Stüber and Beissinger, 2012) and the corresponding discussion paper (Stüber and Beissinger, 2010).

Chapter 3

analyzes whether or not DNWR affects workers differently, conditional on their characteristics, their position in the wage change distribution, and their employers' characteristics. The results show that some workers are "discriminated" against by DNWR. Previous results are confirmed, e.g., that women resist nominal wage cuts less than men (see, e.g., Anspal and Järve, 2011), and new insights are gained, e.g., that blue-collar workers in particular are affected by the compression of wage increases. The chapter is based upon the article "Downward nominal wage rigidity in a cross section: an analysis of linked employer-employee data for the years 1995 to 2007" (Stüber, 2012b).

Motivation for Part II: Real Wage Rigidity

The second part of the book deals with real wage rigidity. More precisely, it discusses and analyzes the real wage rigidity of job entrants over the business cycle. So far, little empirical evidence exists on how real wages of newly hired workers react to the business cycle.

The recent interest in real wage rigidity has been driven by the ongoing debate on the ability of the canonical Mortensen-Pissarides search and matching model (Mortensen and 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." This fact is usually referred to as the "Shimer-Puzzle". So far, Shimer's (2004, 2005) suggestion to generate more variability of unemployment within the model by introducing wage rigidity has been widely shared (see, e.g., Hall, 2005; Hall and Milgrom, 2008; Kennan, 2010). Since the decision of opening a vacancy or not is mainly influenced by the real wage of newly hired workers (see, e.g., Pissarides, 2009; Haefke et al., 2012), research has recently often focused on the real wage cyclicality of job entrants.

Recently the idea of introducing real wage stickiness into the search and matching model – in order to amplify realistic volatility of unemployment – has also been challenged. Pissarides (2009), e.g., dismisses theories based on cyclically rigid wages because empirical research shows that hiring wages are in fact procyclical. His dismissal is based 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). These studies, however, do not control for "cyclical upgrading" in booms and "cyclical downgrading" in recessions: workers may move from high- to low-wage jobs over the business cycle and vice versa, while the wage of newly hired workers within these jobs may be rigid (cf. Gertler and Trigari, 2009). Hence, 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.

Whether or not introducing wage rigidity into search and matching models is justified should be subject to empirical investigation: how rigid are real wages – especially real entry-wages – over the business cycle? So far, to the best of my knowledge, only two studies for Portugal exist that control for "cyclical upgrading" and "cyclical downgrading" in employee/employer matches: Carneiro et al. (2012) and Martins et al. (2012b).

Chapter 4
provides a brief overview on previous research and recent developments concerning the real wage rigidity of newly hired workers over the business cycle.

Chapter 5
presents the first empirical evidence for a large economy (Germany) on the cyclicality of real entry-wages while controlling for "cyclical upgrading" and "cyclical downgrading" in employee/employer matches, by introducing firm-job fixed effects in the regressions. The results show that entry-wages in Germany are not rigid, but respond considerably to business cycle conditions. An increase in the unemployment rate of one percentage point leads to about 0.92 to 1.27 percent lower real entry-wages. The results strengthen Pissarides' (2009) dismissal of theories based on cyclically rigid hiring wages. The chapter is based upon the discussion paper "Are real entry wages rigid over the business cycle? Empirical evidence for Germany from 1977 to 2009" (Stüber, 2012a).

Part I
Downward Nominal Wage Rigidity

1 Causes, Extent, and Implications of Downward Nominal Wage Rigidity

Since around two decades, central banks of western industrialized countries have been able to stabilize the inflation rate at a low level. Although price stability is a central target of economic policy, some economists see a threat in it: downwardly rigid nominal wages in combination with low inflation rates could lead to a higher unemployment rate.

The interaction between inflation and unemployment has concerned researchers and politicians for a long time. For instance, the former German chancellor Helmut Schmidt admitted in 1972: "It appears to me, that the German nation – poignantly formulated – rather tolerates a 5 percent price increase than 5 percent unemployment."[1] But is there really a compulsory trade-off between inflation and unemployment, and vice versa?

Price stability adds to the economic wealth and is hence favored by many economists. Advantages of price stability include, e.g., a reduced inflation risk premium in interest rates and the reduction of uncertainties about the general development of prices – which leads to more transparency of relative prices.[2] Since the early 1990s the prior goal of monetary policy of central banks of western industrialized countries has therefore been the assertion and maintenance of low inflation rates in order to assure vast price stability. The Governing Council of the European Central Bank, e.g., defined price stability in 1998 "[…] as a year-on-year increase in the Harmonised Index of Consumer Prices (HICP) for the euro area of below 2 %." (European Central Bank, 2011, p. 8) In May 2003, the Governing Council made clear that, within this definition, it aims at keeping inflation rates below but '[…] "close to 2 % over the medium term".' (European Central Bank, 2011, p. 8)

However, some economists call attention to possible negative effects of low inflation rates. The most prominent critique can be traced back to Tobin (1972). He advances the view that downward nominal wage rigidity (DNWR)[3] in combination with low inflation rates could hamper necessary real wage adjustments of firms that have economic difficulties. This would lead to a higher real wage level and to an increase of the equilibrium unemployment rate.[4] Tobin (1972) argues that "inflation greases the wheels of the labor market": a certain positive inflation rate

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1 Own translation; original statement: "Mir scheint, daß das deutsche Volk – zugespitzt – 5 Prozent Preisanstieg eher vertragen kann, als 5 Prozent Arbeitslosigkeit." (Süddeutsche Zeitung, 1972)

2 A detailed overview of the advantages of price stability is provided by the European Central Bank (2011).

3 DNWR describes the fact that firms cannot cut nominal wages, or that they cannot cut the nominal wage in the desired extent. Reasons for DNWR are introduced in Section 1.2.

4 The equilibrium unemployment rate is defined as the unemployment rate that would exist if the labor market was in equilibrium.

is necessary in order to enable employers to decrease the real wages of employees without being forced to decrease the nominal wage – which is not accepted by employees.

Since the 1990*s* there has been a lively academic debate on the existence and the extent of DNWR. This debate has not only been carried out in academic journals but also in the media.[5]

This chapter provides an overview of the debate on DNWR and inflation. First, I illustrate the theoretical view on how an inflation that is too low in combination with DNWR can increase the equilibrium unemployment rate. In Section 1.2 I address possible reasons for DNWR. Section 1.3 introduces strategies for the identification of DNWR in microdata. Section 1.4 looks at the existence and the extent of DNWR, as well as macroeconomic consequences, and Section 1.5 concludes.

1.1 The Connection between Inflation, Unemployment, and Downward Nominal Wage Rigidity

Since the 1970*s* the majority of economists has held the view that a long-term relationship between inflation and unemployment does not exist. This opinion is based on the work of Phelps (1967) and Friedman (1968). Independent from each other, they stress that rational economic agents should orient their behavior towards real variables and not towards nominal variables. Hence, employees should be interested in their real wage and not their nominal wage. In wage bargaining usually the nominal wage is fixed for the next period; therefore the expected inflation rate is of great importance. If (in the long-run) the expected inflation rate is anticipated correctly, nominal wage increases should, ceteris paribus, exactly correct for the inflation rate. Hence monetary policy should be neutral in the long-run – i.e. it should not affect real variables in the economy. An enduring relationship between monetary policy and the real wage can only exist if money illusion exists – i.e. if economic agents base their decisions on nominal variables (e.g., prices in monetary units) and not on real variables (e.g., relative prices). Such a behavior, however, contradicts the usual assumption of rational behavior of economic agents.

The hypothesis of the neutrality of monetary policy in the long-run can graphically be displayed as a vertical Phillips-curve (see Figure 1.1). The equilibrium unemployment rate u^* depends – if anticipations are correct – only on structural factors like the amount of unemployment benefit. At this equilibrium unemployment

5 See, e.g., International Monetary Fund (1999), the contributions in European Central Bank (2001, 2003), Organisation for Economic Co-operation and Development (2002), and the articles of the Economist (2000a,b, 2002) and the NNZexecutive (2009).

rate there is neither pressure for rising nor for falling inflation rates from the labor market – i.e. u^* is consistent with a constant inflation rate. Therefore, the acronym NAIRU (Non-Accelerating-Inflation Rate of Unemployment) is used for the equilibrium unemployment rate. According to the hypothesis of the neutrality of monetary policy in the long-run, central banks can use their monetary policy to "choose" an inflation rate that is optimal in their opinion – e.g. point A or point B in Figure 1.1a – without affecting the NAIRU.

But some economists criticize the hypothesis of the neutrality of monetary policy in the long-run. Tobin (1972), e.g., points out possible threats for the labor market that could result from low inflation rates. He hypothesizes that the combination of DNWR and low inflation rates could increase the equilibrium unemployment rate. Tobin (1972) constitutes that firms, which are hit by an adverse (negative) shock, might not be able to implement necessary real wage cuts if the inflation rate is too low and DNWR binds. Instead of cutting real wages these firms resort to dismissing employees. Therefore Tobin (1972) concludes that a certain positive inflation rate is preferable from a macroeconomic perspective.

Figure 1.1: Phillips-curve by flexible and downward rigid nominal wages

a) without downward nominal wage rigidity (DNWR) b) with DNWR

It took nearly 20 years to include Tobin's (1972) hypothesis in a general equilibrium model. Akerlof et al. (1996) use a modification of the standard NAIRU model to prove Tobin's postulated long-run Phillips-curve trade-off in times of low inflation. Their result ignited a lively debate on the existence and the extent of DNWR. Akerlof et al. (1996) show that in times of low inflation rates the long-run Phillips-curve is not vertical but inclining. Figure 1.1b shows how the equilibrium unemployment rate can then depend on the inflation rate. In point A the inflation rate is sufficiently high in order to keep the minimal equilibrium unemployment rate u^*_{min} – the value is equivalent to the value of u^* from Figure 1.1a. With a lower inflation rate, e.g. in point B, the corresponding unemployment rate u^*_B is higher than u^*_{min}. The interaction of low inflation and DNWR leads to an excess-unemployment rate of $u^*_B - u^*_{min}$. Akerlof's et al. (1996) simulations for the USA show that in the case of zero inflation the excess-unemployment rate can reach several percentage points. However, already a inflation rate of around three percent is sufficient to assure u^*_{min}. The following paragraph examines the modeled processes of the labor market.

The model of Akerlof et al. (1996) is based on plausible assumptions: goods and labor market are characterized by imperfect competition, and firms and labor unions are heterogeneous. Because of their market power, firms and labor unions possess price and wage setting powers. In combination with the assumption of heterogeneity this can lead to different real wages between firms. If a firm is confronted with a declining demand for its goods, it will react – under imperfect competition – by reducing employment and/or by lowering real wages. In times with relatively high inflation, firms can decrease the real wage by increasing nominal wages by less than prices rise. The lower the inflation rate, the higher is the probability that the desired real wage decrease can only be reached by decreasing nominal wages. In an economy with zero inflation, every real wage decrease will result in a nominal wage decrease. If DNWR exists, and hence firms are not able to decrease nominal wages freely, it is not possible to decrease the real wage as desired. Therefore firms, which are affected by an adverse development of demand, are forced to reduce employment by a higher degree than they would with flexible nominal wages or in times of higher inflation. However, one should also keep in mind that an inflexibility of real wages cannot only be absorbed by employment adjustments. During Germany's most recent recession working time accounts proved to be of value to preserve employment (Zapf and Brehmer, 2010). However, the more rigid real wages are, the higher is the probability that firms are forced to reduce employment. In contrast, if a decrease of labor cost is possible, then the reduction of employment can be attenuated or it can even be avoided. Hence, on the macroeconomic level the combination of low inflation and DNWR can lead to an increase of the equilibrium unemployment rate. This explains the inclining trend of the Phillips-curve in times of low inflation rates (see Figure 1.1b).

1.2 Causes for Downward Nominal Wage Rigidity

In the literature the existence of DNWR is explained either by psychologically founded patterns of behavior or by institutional conditions.

1.2.1 Psychological Causes

In a well-known study Bewley (1999) looks into firms' wage setting during the American recession in the beginning of the 1990s. Because of the tremendous decrease in demand and the associated decrease in production the firms had strong economic incentives to lower their production costs by real wage decreases. Since the inflation rate was relatively low, desired real wage decreases should have resulted in nominal wage cuts in many firms. However, nominal wage cuts could be observed only in very few firms. Bewley's (1999) analysis is based on over 300 interviews with human resources managers, union leaders, managers of temporary employment/help agencies and employment agencies. He discovers that firms try to avoid nominal wage cuts because management is afraid that nominal wage cuts would damage workers' morale.

If a firm decides to decrease the nominal wage two effects appear. First, with given prices the real wages of the workers decrease. Second, workers feel discredited – they are used to yearly nominal wage increases or that their nominal wages at least stay stable. A sudden nominal wage cut is interpreted as unfair behavior; the workers have the feeling that their work effort is not sufficiently recognized. This leads to a decrease of workers' morale and hence workers eventually provide less work effort and engagement which in turn leads to lower productivity of the firm. The concept of work morale describes a situation in which employees adopt the firm's aims as their own and are therefore willing to provide a considerable work effort to achieve these aims. A good morale within the firm is of great relevance to the firm's management, since usually labor contracts are imperfect contracts – the nominal wage is fixed but not the worker's consideration. Certain aspects of the worker's consideration simply cannot be fixed by a contract – e.g., the level of his or her initiative or independent judgment. Given imperfect contracts, it is all the more important for employers to create and preserve a cooperative working atmosphere. If firms are not able to decrease real wages they are forced to reduce employment by a higher degree compared to a situation where real wages are flexible. In contrast to nominal wage cuts, dismissals have a weaker effect on worker morale and affect the morale only temporarily – since the dismissed persons are not able to express their dissatisfaction within the firm. Further, if a firm dismisses employees its management has some control over which persons to dismiss. In contrast, if a firm cuts wages in particular the more productive employees could decide to leave the firm in order to start working for a better-paying firm. Only under exceptional circumstances employees seem to be willing to accept nominal wage cuts – e.g., if the survival of their employer is threatened by financial problems (see, e.g., Stephan, 2006).

The observation that employees particularly dislike real wage cuts if they are achieved by nominal wage cuts points to the existence of money illusion. Money illusion is contradictory to the concept of the *homo oeconomicus*, but the psychology literature shows that it is an important and common phenomenon (Sharfir et al., 1997).[6] A further important result from the field of cognitive psychology is that alternative representations of one and the same situation (so-called frames) can influence individuals' behavior in these situations in a systematic way (Tversky and Kahneman, 1986). Economic transactions, e.g., can either be formulated in nominal or in real values. The way they are presented affects how individuals rate the transaction. Sharfir et al. (1997) attribute money illusion to the use of multiple frames – i.e. the simultaneous use of a real and a nominal frame during the rating of economic transactions. I would like to illustrate the idea of multiple frames using an example where individuals are confronted with two scenarios. In scenario A they are offered a two percent nominal wage increase in a time with four percent inflation. In scenario B they are offered a two percent nominal wage decrease in a time with stable prices (zero inflation). The individuals are indifferent between scenario A and B if the scenarios are presented using the real frame – in each scenario they would experience a two percent real wage decrease. However, the individuals prefer scenario A over scenario B if the scenarios are presented using the nominal frame. Confronted with both frames simultaneously, the individuals perceive scenario B as more negative as scenario A. Hence the phenomena of money illusion can be traced back to the simultaneous use of the nominal and the real frame, and the fact that the evaluation of the real frame is adulterated by the simultaneous evaluation of the nominal frame. The fallback to the nominal frame happens because it is more convenient than the use of the real frame – that first demands a translation. Kahneman et al. (1986) demonstrate that money illusion also affects fairness considerations. Their research backs the statement made above that employees typically consider nominal wage cuts to be unfair.

In this context one should also consider that labor relations – like many other social interactions – rely on reciprocity. Reciprocal behavior patterns conflict with the standard economic model – individuals should base their behavior solely on their self-interest. However, a lot of experimental studies show that individuals respond to behavior of other individuals that is sensed to be unfair with "punishment actions". Individuals even show reciprocal behavior if this behavior only produces costs and not any (material) earnings (Fehr and Gächter,

6 The existence and importance of money illusion is also documented by experimental studies (cf. Fehr and Tyran, 2001, 2007).

2000). Hence, an employer will refrain from actions that employees could perceive as unfair – e.g., nominal wage cuts – if he or she expects employers to show reciprocal behavior patterns.

A further psychological explanation for DNWR builds on Keynes' (1936) hypothesis that employees' utility is affected by relative wages. Because wage changes are not implemented simultaneously in all firms a coordinating problem exists: employers refuse to cut the nominal wage, since this would result in a lower relative wage for their employees. However, would the same real wage decrease be achieved by an increase in the common price level, the employees' resistance would be much weaker because it would not result in a decrease of their relative wages. Bhaskar (1990) develops a detailed micro model based on this hypothesis, assuming that the utility loss caused by a decrease of the relative wage is larger than the utility gain caused by a wage increase.

1.2.2 Institutional Causes

The institutional approach for the explanation of DNWR builds upon the fact that individual or collective labor contracts generally fix a nominal wage for the total contract period. Moreover, even after the expiration of the validity of the contract, in many countries the fixed nominal wage is paid until a new (collective) agreement is negotiated (MacLeod and Malcomson, 1993; Holden, 1994).

To simplify the description, in the following I suppose that the wage bargaining takes place between a labor union and an employer organization of a certain business sector. As Holden (1994, 2004) shows, in the aftermath of negotiations this leads to a strategic disadvantage for the bargaining side that would like to change the negotiated wage. With positive inflation the labor union has a strong interest to increase wages, since the positive inflation leads to a real wage decrease if the nominal wage fixed by the old collective agreement is not changed. If the economic situation necessitates that the business sector decreases the real wages, the employer organization might try to achieve this through delaying the wage bargaining process. With zero or even negative inflation rates, however, a real wage decrease can only be implemented by a decrease of the nominal wage. In this case the employer organization has a strong interest in a new collective agreement. The labor union on the other hand is able to avoid a real wage decrease through delaying the wage bargaining process.

Both parties involved in the collective agreement can fall back on collective action – strike and lock-out, respectively – to achieve their aims. However, these measures have costs that have to be fractured in realistically by the respective party. The possible use of collective action does not change the basic argument:

the wage pressure is lower in times of high inflation – because of the weaker bargaining position of the labor union – and higher in times of low inflation – because of the weaker bargaining position of the employer organization.

Labor unions have more options to oppose wage cuts than individual employees; therefore the institutional approach for the explanation of DNWR should be more distinctive in countries with a high degree of tariff liaisons and a high rate of unionization. In the case of non-unionized workers (free labor) the degree of DNWR should depend in particular on the magnitude of job protection, since job protection enables (single) employees to oppose nominal wage cuts (cf. Holden, 2004).

1.2.3 Conclusion

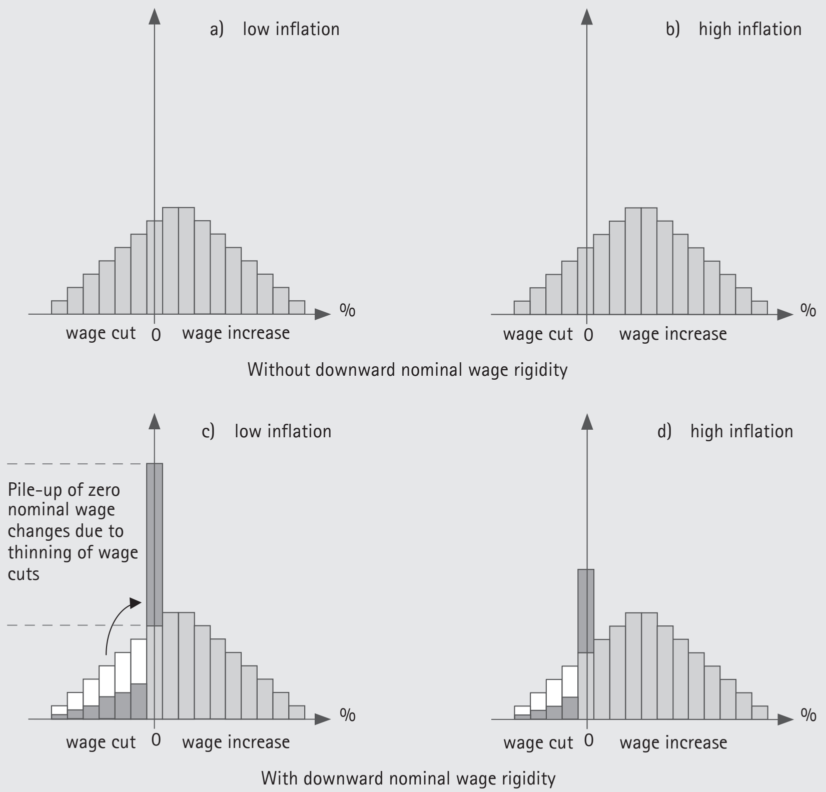
In the end, it remains an open question whether DNWR can be better explained by psychology or by institutions. Correlations between institutional settings and country-specific degrees of DNWR rarely lead to significant results – moreover, these are often inconsistent (Knoppik and Beissinger, 2009). Thus, the effect of institutional variables on the degree of DNWR seems to be rather weak. The psychological approach – based upon fairness considerations – seems to play a more important role for the explanation of different degrees of country-specific DNWR. Also, research based on firm interviews largely comes to the conclusion that firms consider fairness considerations in their wage setting and hence try to avoid nominal wage cuts. This discussion will be picked up again when I look at the existence and the extent of DNWR in Section 1.4.

1.3 Identification of Downward Nominal Wage Rigidity in Microdata

1.3.1 Basic Considerations

To answer the question whether and, if yes, to what extent firms in economic difficulties are unable to decrease real wages as desired because of DNWR one generally looks at wage changes (see Beissinger and Knoppik, 2005). Of vital importance is to keep in mind that firms as well as employees are heterogeneous. Hence, at any time – independently from the general economic situation – firms exist that do well while other firms do poorly. Even in the unrealistic case of total wage flexibility a wage change distribution would exist.

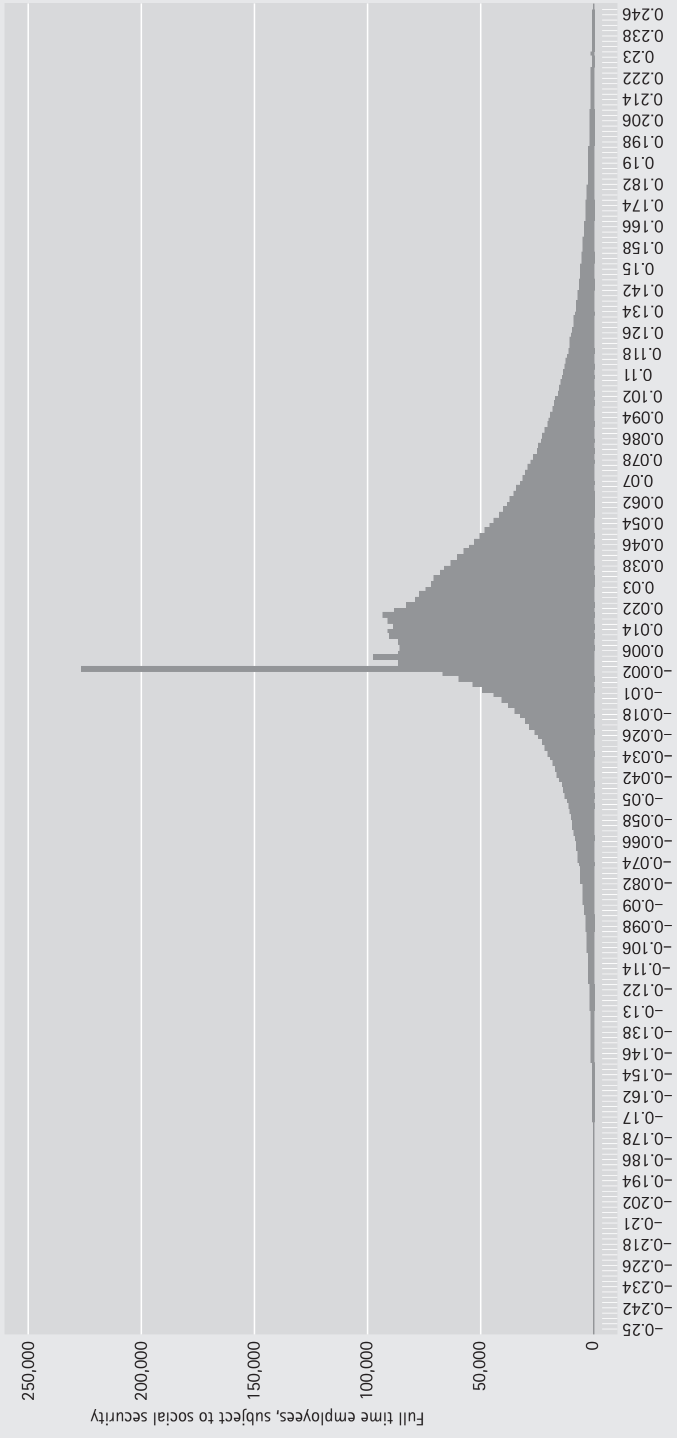
Figure 1.2: Distribution of wage changes with and without downward nominal wage rigidity according to hitherto existing studies



The distribution present under total wage flexibility is called the counterfactual distribution or notional distribution. With a given inflation rate it would typically cover wage increases as well as wage cuts (see Figures 1.2a and 1.2b).

If DNWR binds, firms are not or less often able to cut nominal wages. Thus, nominal wage cuts are replaced by zero nominal wage changes. This leads to a characteristic reshaping of the wage change distribution: a thinning of the distribution in the area of nominal wage cuts and a pile-up at zero (see Figures 1.2c and 1.2d). This effect can be observed in reality. Figure 1.3 exemplarily shows the distribution of observed wage change for Germany from 2006 to 2007. The distribution exhibits the characteristically thinning of the wage cuts and the pile-up at zero.

Figure 1.3: Empirical distribution of wage changes in Germany



Note: Figure shows the wage change of all full time working employees (subject to social security) which did not change the employer from 2006 to 2007. Wage change is defined as: ln(annual wage 2007) – ln(annual wage 2006). For more information on the data selection process see Appendix B.1.

A comparison between Figures 1.2a and 1.2b shows that an increase in the inflation rate leads to a rightward shift of the notional distribution. Because of the general price increase there is more room for nominal wage increases and the number of desired wage cuts decreases. Also, because of the rightward shift of the distribution, a smaller fraction of it is affected by DNWR. Hence the pile-up at zero decreases (see Figure 1.2d).

To evaluate the degree of DNWR empirical studies concentrate on the reshaping of the wage change distribution described above. The problem is, that the notional distribution is unknown. It could, e.g., be possible that the notional distribution is also characterized by a thinning in the area of wage cuts and a pile-up at zero. Hence, one cannot really be certain whether one observes a distribution that is unaffected by DNWR, or a distribution that is influenced by the existence of DNWR. In order to distinguish the two possibilities one needs identification strategies that assure that certain characteristics of the distribution – e.g., the thinning of the lower part and the pile-up at zero – can be traced back to the existence of DNWR. Those identification strategies are outlined in the next section.

1.3.2 Identification Strategies and Analytical Approaches

The most basic strategy to identify DNWR relies on the assumption of a specific form of the notional wage change distribution. Variations from the notional distribution in the areas of nominal wage cut and at zero nominal wage change are interpreted as evidence in favor of the existence of DNWR. The literature uses several assumptions for the notional wage change distribution: Card and Hyslop (1997) assume that the distribution is symmetric (symmetry approach). They do not assume an influence of DNWR on wage increases; hence they use this part of the distribution to create the counterfactual distribution for the area of nominal wage cuts. Dickens et al. (2007a) argue that the counterfactual wage change distribution is described by a (symmetric) Weibull-distribution while Behr and Pötter (2010) argue that the counterfactual distribution is described by an (asymmetric) generalized hyperbolic distribution. In the so-called earnings-function approach by Altonji and Devereux (2000), the counterfactual wage changes are described by a Mincer-type regression. For the error term of this regression typically a normal distribution is assumed. Hence the "conditional" counterfactual distribution, i.e. the wage change distribution conditional on given characteristics of human capital variables and further regressors, is assumed to be normally distributed. The necessity to make assumptions about the counterfactual distribution is the weak point of this identification strategy. If the respective assumption about the notional distribution is incorrect, then the conclusion about the degree of DNWR derived based on it is problematic as well.

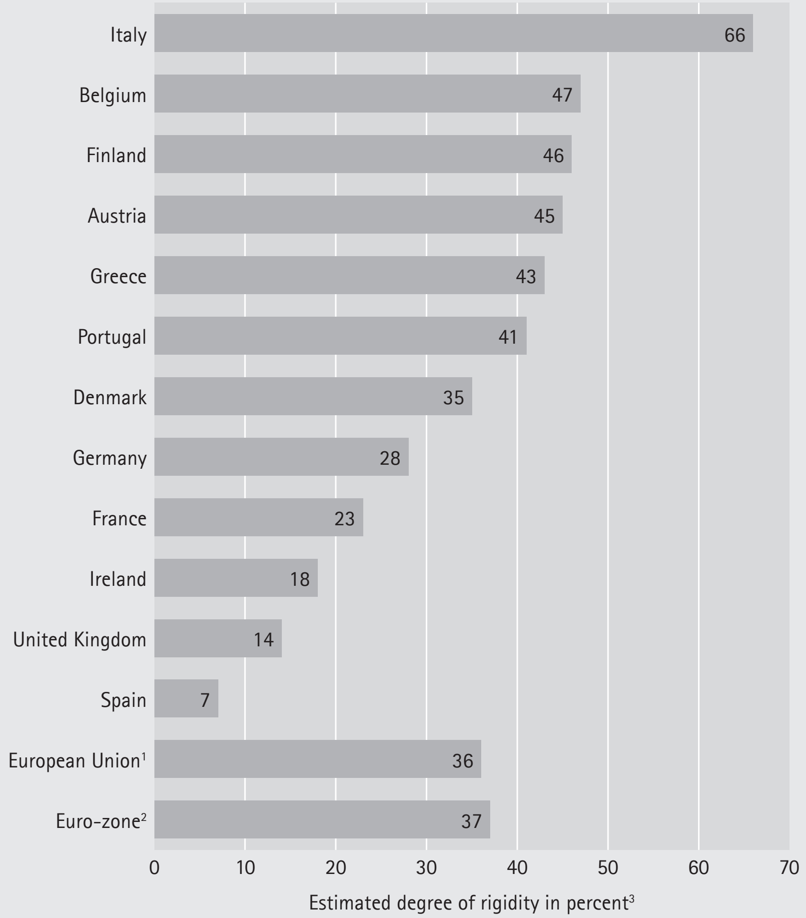
A more sophisticated identification strategy relies on the fact that shifts of the wage change distribution along the x-axis can lead to systematic shape changes if DNWR exists. Such shifts take place when the average nominal wage growth alters. Together with changes in the rate of productivity growth the inflation rate is an important cause for changes of the position of the nominal wage change distribution. For an illustration I refer to Figure 1.2. As the comparison of Figures 1.2a and 1.2b shows, an increase in the inflation rate leads to a rightward shift of the notional wage change distribution. Because of the rightward shift of the wage distribution a smaller fraction of it is influenced by DNWR. This affects the shape of the actual wage change distribution – the pile up at zero decreases (see Figures 1.2c and 1.2d). One might conclude that DNWR exists if such a systematic shift of the shape of the distribution of actual wage changes can be observed. The advantage of this second identification strategy is that no assumption on the shape of the counterfactual distribution of wage changes is necessary. However, one has to assume that the shape of the counterfactual wage change distribution is stable over time. Several studies use this identification strategy. McLaughlin's (1994) so-called skewness-location approach analyzes whether the skewness of the wage change distribution varies systematically with changes of the position of the wage change distribution. This approach makes it possible to decide about the existence of DNWR, but not about the degree of rigidity. A quantification of the degree of rigidity is possible with the histogram-location approach (Kahn, 2007) or the kernel-location approach (Knoppik, 2007). The first approach uses histograms and the second kernel density estimations to econometrically determine whether the shape of the wage change distribution varies systematically with changes of the position of the wage change distribution.

1.4 Existence and Extent of Downward Nominal Wage Rigidity

1.4.1 Microdata Studies

The existence of DNWR on the individual (micro) level has empirically been demonstrated for many countries. For Germany, e.g., Knoppik and Beissinger (2009) estimate a degree of (downward nominal) wage rigidity of 28 percent. The degree of downward nominal wage rigidity measures the share of desired wage reductions that are prevented by DNWR. Hence, in Germany 28 percent of firms' desired wage cuts are avoided because of DNWR. Other European countries have quite high degrees of wage rigidity as well (see Figure 1.4).

Figure 1.4: Downward nominal wage rigidity of selected EU countries



[1] The estimation for the European Union is based upon 12 of the 15 old member states (without Luxembourg, the Netherlands, and Sweden).
[2] The estimation for the Euro-zone is based upon 10 of the 12 original member states (without Luxembourg and the Netherlands).
[3] The degree of (downward nominal) wage rigidity measures the share of desired wage reductions that are prevented by downward nominal wage rigidity.

Note: Results are taken from Knoppik and Beissinger (2009, Tab. 1, p. 330).

In this summary I will concentrate on five multicountry studies (see Table 1.1) where all economies are analyzed using the same approach. Therefore, the results are better comparable than results from single-country studies.[7]

7 The results of some selected single-country studies are presented in Table A.1 of Appendix A.

Table 1.1: Multicountry studies and their results

Study	Countries[a]	Data	Approach
Behr and Pötter (2010)	10 EU-countries: B, D, DK, E, F, GB, GR, I, IRL, P	European Community Household Panel (ECHP, 1994–2001)	hyperbolic-notional approach, histogram-location approach
Highest degree of rigidity: GR (42 %), P (40 %) and I (36 %)			
Lowest degrees of rigidity: E (3 %), IRL (5 %) and GB (9 %)			
Knoppik and Beissinger (2009)	12 EU-countries: A, B, D, DK, E, F, FIN, GB, GR, I, IRL, P	ECHP (1994–2001)	histogram-location approach
Average degree of rigidity over all 12 countries: 36 %			
Highest degrees of rigidity: I (66 %), B (47 %) and FIN (46 %)			
Lowest degrees of rigidity: E (7 %), GB (14 %) and IRL (18 %)			
Holden and Wulfsberg (2008)	19 OECD-countries: A, B, CDN, D, DK, E, F, FIN, GB, GR, I, IRL, L, N, NL, NZ, P, S, USA	Sector data of OECD (1973–99)	empirical-notional approach
Degree of rigidity for north-European countries (DK, FIN, N, S): 50 %			
Degree of rigidity for south-European countries (E, GR, I, P): 41 %			
Degree of rigidity for central-European countries (A, B, D, F, L, NL): 23 %			
Degree of rigidity for English speaking countries (CDN, GB, IRL, NZ, USA): 20 %			
On the level of individual countries the simulation finds some positive degree of rigidity for all countries except for F, GR and E. However, the results for CDN, F, D, GR, E, N, GB and the USA are not significant different from zero.			
Highest significant degrees of rigidity: I (100 %), P (86 %) and A (71 %)			
Lowest significant degrees of rigidity: NZ (21 %), GB (21 %) and B (23 %)			
Dickens et al. (2007b)	16 countries: A, B, CH, D, DK, F, FIN, GB, GR, I, IRL, N, NL, P, S, USA	ECHP and 19 further datasets[b]	Weibull-notional approach
Rigidity degrees between 9 % and 66 %			
Dickens et al. (2007a)	16 countries: A, B, CH, D, DK, F, FIN, GB, GR, I, IRL, N, NL, P, S, USA	ECHP and 19 further datasets[b]	Variant of the symmetry approach
Average degree of rigidity over all 16 countries: 28 %			
Rigidity degrees between 4 % (IRL) and 58 % (P) with a standard deviation of 13 percentage points			
Lowest degrees of rigidity: IRL, DK and F			
Highest degrees of rigidity S, USA and P			

Notes: Degree of (downward nominal) wage rigidity measures the share of desired wage reductions that are prevented by DNWR.

[a] A = Austria, B = Belgium, CDN = Canada, CH = Switzerland, D = Germany, DK = Denmark, E = Spain, F = France, FIN = Finland, GB = Great Britain, GR = Greece, I = Italy, IRL = Ireland, L = Luxembourg, N = Norway, NL = the Netherlands, NZ = New Zealand, P = Portugal, S = Sweden, USA = United States of America.

[b] Dickens et al. (2007b,a) use 20 datasets. The time period (beginning of the 1970s to the beginning of the 2000s) covered varies from dataset to dataset; on average 12 years are covered. For a data overview see Dickens et al. (2007b, Tab. 2, p. 47f.) or Dickens et al. (2007a, Tab. 1, p. 198).

Dickens et al. (2007b) use different national and international data and two different methods to estimate the degree of DNWR. What they call their "simple method" is a quantitative version of the symmetry approach; their second approach assumes a (symmetric) Weibull-distribution for the counterfactual wage change distribution.

In Dickens et al. (2007a) it is implicitly assumed that the counterfactual distribution has no pile-up at zero. The degree of rigidity is determined by the number of zero nominal wage changes in relation to the sum of the number of nominal wage cuts and the number of zero nominal wage changes.

Knoppik and Beissinger (2009) and Behr and Pötter (2010) use data of the European Community Household Panel (ECHP). The advantage of the ECHP is that the survey – conducted in the 15 "old" member states of the EU – is based on a uniform questionnaire. Hence, the data are comparable across countries. Knoppik and Beissinger (2009) use a multicountry version of the histogram-location approach, while Behr and Pötter (2010) assume – in their favored approach – that the counterfactual distribution can be represented by a generalized hyperbolical distribution. To facilitate comparisons with other studies, they also apply the histogram-location approach for individual countries.

Holden and Wulfsberg (2008) develop a completely non-parametrical approach – they run simulations based on empirically observed distributions. This empirical-notional approach is based – as the histogram-location approach – on the assumption that a change of the position of the wage change distribution leads to a characteristic shape change of the distribution if DNWR binds.

All the above mentioned studies use individual wage changes, apart from the study of Holden and Wulfsberg (2008) that uses sector data. All five studies find significant DNWR for all observed countries (see Table 1.1). The estimated degrees of rigidity are quantitatively pretty similar, too. Behr and Pötter (2010) and Knoppik and Beissinger (2009), e.g., find that inside the EU-10 the south European countries Greece, Italy and Portugal exhibit the highest degrees of rigidity, Ireland, Great Britain, and Spain the lowest.

Knoppik and Beissinger (2009) use Bravais-Pearson and Spearman correlation coefficients to compare their results to results of other studies. The corresponding correlation coefficients are displayed in Table 1.2. The coefficients show that the results of the different studies are all quite similar.

Table 1.2: Correlations between estimates of the degree of rigidity of Knoppik and Beissinger (2009) with other studies

	Pearson correlation coefficients		Spearman correlation coefficients	
	Pairwise[a]	Casewise[b]	Pairwise[a]	Casewise[b]
Behr and Pötter (2005): HNA[c]	0.80 (10)	0.75 (9)	0.77 (19)	0.80 (9)
Behr and Pötter (2005): HLA[c]	0.77 (10)	0.73 (9)	0.75 (10)	0.73 (9)
Dickens et al. (2006)[d]	0.56 (11)	0.66 (9)	0.31 (11)	0.54 (9)
Holden and Wulfsberg (2006)[e]	0.65 (12)	0.56 (9)	0.58 (12)	0.40 (9)

Notes: Results are taken from Knoppik and Beissinger (2009, Tab. 2, p. 333). Number of considered countries in brackets.

[a] Results based only on countries that are observed in both studies.

[b] Results based only on countries that are observed in all 5 studies.

[c] Compared results are based on the results of the hyperbolic-notional approach (HNA) and the histogram-location approach (HLA), respectively. Results of the cited discussion paper (Behr and Pötter, 2005) are identical to the published paper (Behr and Pötter, 2010).

[d] A revised version is available: Dickens et al. (2007b).

[e] Results of the cited discussion paper (Holden and Wulfsberg, 2006) are identical to the published paper Holden and Wulfsberg (2008).

Other studies also calculated correlation coefficients for the degree of rigidity. Dickens et al. (2007b), e.g., calculate for their results and the results from Holden and Wulfsberg (2008) a correlation coefficient of $r = 0.66$ (excl. USA). If they take the rigidity degree of the USA into account the correlation coefficient drops to $r = 0.43$. In the revised paper version (Dickens et al. 2007b) they calculate for their results and the results from Holden and Wulfsberg (2008) a correlation coefficient of $r = 0.45$ (excl. USA) and for their results and the results from Knoppik and Beissinger (2005)[8] a correlation coefficient of $r = 0.74$ (excl. USA). Dickens et al. (2007a) calculate for 15 countries, which are also in the sample of Holden and Wulfsberg (2006), a correlation coefficient for the estimated degree of rigidity of $r = 0.46$. For the rigidity degrees of 11 countries of the study from Knoppik and Beissinger (2005) they calculate a correlation coefficient of $r = 0.45$. Dickens et al. (2007a) conclude that these correlation coefficients are a strong indicator that their results are correct, since the studies use different analytical approaches as well as different datasets for different time periods.[9]

Knoppik and Beissinger (2009) also compare the different results for individual countries – see Table 1.3 – using outputs of the studies by Dickens et al. (2006),

8 Discussion paper version of Knoppik and Beissinger (2009).

9 Results of the cited discussion papers Holden and Wulfsberg (2006) and Knoppik and Beissinger (2005) are identical to the published papers Holden and Wulfsberg (2008) and Knoppik and Beissinger (2009), respectively.

Behr and Pötter (2005)[10], and Holden and Wulfsberg (2006)[11]. The results show relatively high rigidity degrees for Italy and Portugal and relatively low rigidity degrees for Great Britain and Spain. For the other countries, e.g. Greek, France, and Ireland, the rigidity degrees (of the different studies) vary stronger.

Table 1.3: Estimated degree of rigidity and ranks across studies

Country[a]	Degree of rigidity					Rank[d]				
	Knoppik and Beissinger (2009)	Other set of results[b]				Knoppik and Beissinger (2009)	Other set of results[b, e]			
		min	mean	max	N[c]		highest rank	mean	lowest rank	N[c]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
I	0.66	0.29	0.58	1.00	4	1	1	2.25	3	4
B	0.47	0.17	0.20	0.23	4	2	4	5	6	4
FIN	0.46	0.20	0.43	0.66	2					
A	0.45	0.37	0.54	0.71	2					
GR	0.43	–0.13	0.27	0.63	4	3	1	4.25	8	4
P	0.41	0.35	0.57	0.86	4	4	1	1.5	2	4
DK	0.35	0.27	0.35	0.46	4	5	2	3.5	5	4
D	0.28	0.06	0.11	0.16	4	6	6	7	9	4
F	0.23	–0.20	0.09	0.40	4	7	4	7	9	4
IRL	0.18	0.05	0.14	0.32	4	8	4	7	9	4
GB	0.14	0.06	0.14	0.22	4	9	6	7.25	9	4
E	0.07	–0.05	0.00	0.04	3					

Notes: Results are taken from Knoppik and Beissinger (2009, Tab. 3, p. 334).

[a] Countries: A = Austria, B = Belgium, D = Germany, DK = Denmark, E = Spain, F = France, FIN = Finland, GB = Great Britain, GR = Greece, I = Italy, IRL = Ireland, P = Portugal.

[b] Ranges based on the sets of estimated national degrees of downward nominal wage rigidity from Behr and Pötter (2005) – the discussion paper version of Behr and Pötter (2010) – (based on the generalized-hyperbolic-notional approach and the histogram-location approach), Dickens et al. (2006), and Holden and Wulfsberg (2006) – the discussion paper version of Holden and Wulfsberg (2008).

[c] N = Number of studies in that the country is observed.

[d] Rank: 1 = highest degree of rigidity, 9 = lowest degree of rigidity.

[e] Results based only on countries that are observed in all 4 studies.

So far the question, why different countries exhibit different degrees of wage rigidity, has not been discussed. According to Section 1.2, DNWR can be explained by different notions of fairness or by institutional conditions. To analyze whether institutional conditions can explain different degrees of DNWR between countries Knoppik and Beissinger (2009) regress their estimated rigidity degrees on

10 Discussion paper version of Behr and Pötter (2010).

11 Discussion paper version of Holden and Wulfsberg (2008).

different institutional variables, e.g. indicators for the strictness of employment protection legislation, union density and bargaining coverage. Their results show a significantly positive correlation between the degree of DNWR and the degree of coordination within an economy. For the remaining institutional variable they do not find significant correlations. Dickens et al. (2007b) also apply regressions on different institutional variables. They only find a significant negative correlation between the rate of unionization and the degree of DNWR. A contradictory result is found by Holden and Wulfsberg (2008): a positive correlation between the rate of unionization and the degree of DNWR.

The three studies show that the correlation between the degree of DNWR and institutional variables is astonishingly weak. It looks as if the variation in the degree of DNWR between different countries can hardly be explained by institutional variables. Therefore one might argue that psychological approaches – based on fairness considerations – maybe are more important in explaining the country-specific degrees of DNWR.

1.4.2 Survey Studies

A further possibility to obtain an impression on the extent of and the reasons for DNWR is the conduction and analysis of firm surveys in regard to wage setting policies.

A disadvantage of surveys is that they are relatively expensive if one wants to obtain a large sample that is "representative". Therefore, most surveys do not constitute a representative firm sample – often only a few firms or certain sectors of the economy are surveyed. A further disadvantage with voluntary surveys is the self-selection of firms that are willing to provide answers. It is possible that firms, that voluntary provide answers, systematically differ from firms that are not willing to take part in such surveys.

Table 1.4 summarizes some studies based on surveys and their results. The fourth column shows that – except for one survey – the documented DNWR is high in all of the studies. The existence of DNWR can usually be confirmed for most firms. However, attention should be paid to the fact that the importance of nominal wage cuts depends on an economy's inflation and the growth rates. If a survey is conducted, e.g., in times of high inflation it is not surprising that hardly any firm would want to cut nominal wages. But the surveys of Agell and Lundborg (2003) and Agell and Bennmarker (2007) for Sweden, e.g., were conducted during a period of low inflation. Those studies show that even after a longer time of low inflation the degree of DNWR does not decrease.

Table 1.4: Survey studies and their results

Country[a]	Study	Data	Degree of rigidity[b]	Cause	
				fairness	institutional regulations
S	Agell and Bennmarker (2007)	885 firms (among 300 small firms) of the manufacturing industry, the services sector, and the public sector of the year 1999.	very high	yes	yes
J	Kawaguchi and Ohtake (2007)	90 firms and 1557 male worker from the region Chubu of the year 2000.	n.s.	yes	n.s.
S	Agell and Lundborg (2003)[†]	Panel data of 157 predominantly large firms of the manufacturing firms for the years 1991 and 1998.	very high	yes	yes
D	Franz and Pfeiffer (2003)[†]	801 businesses with more than 10 workers from 6 economic sectors of the manufacturing industry and services sector of the year 2000.	n.s.	yes	yes
USA	Bewley (1999)[†]	335 manager, labor leader, placement officers, etc. of the years 1992 to 1994.	very high	yes	no
USA	Campbell III and Kamlani (1997)[†]	111 large firms and 73 smaller firms of the manufacturing industry and services sector of the years 1993/94.	n.s.	yes	no
USA	Blinder and Choi (1990)[†]	13 large firms of the manufacturing industry and 6 large firms of the services sector of the year 1988.	medium	yes	no
GB	Kaufman (1984)[†]	20 small non-unionized firms and 6 large unionized firms of the manufacturing industry and services sector of the year 1982.	very high	yes	no

Notes: Results of studies marked with an [†] are taken from (Beissinger and Knoppik, 2005, Tab. 1, p. 177).

[a] D = Germany, GB = Great Britain, J = Japan, S = Sweden, USA = United States of America.

[b] Degree of rigidity: The denoted degree of (downward nominal) wage rigidity refers to the share of firms that in the past did not cut nominal wages. This share depends on the level of the inflation rate and the level of productivity growth; hence this quantitative measure for wage rigidity has to be interpreted with caution.

An advantage of surveys is the fact that one can ask questions concerning the reasons for DNWR. Agell and Bennmarker (2007), e.g., come to the conclusion that managers think that employees underlie money illusion. Similar results are found by Blinder and Choi (1990) and Bewley (1999) for the USA. According to those studies, DNWR is caused by fairness considerations – where the employees' concept of "fairness" is centered on nominal variables. The two studies for Sweden (Agell and Lundborg, 2003; Agell and Bennmarker, 2007) and the study of Franz and Pfeiffer (2003) for Germany on the other hand, come to the conclusion that

DNWR is also caused (or reinforced, respectively) by institutional settings. This result, however, is only found for countries where labor unions and collective labor agreements etc. play an important role.

1.4.3 Macroeconomic Consequences of Downward Nominal Wage Rigidity

For the assessment of the macroeconomic impact of DNWR it is not sufficient to know which fraction of desired nominal wage cuts are avoided because of DNWR. Further information is necessary. One needs, e.g., to know how many wage cuts were desired by the employers – this however depends on the inflation rate. The higher the inflation rate, the easier it is to decrease real wages without falling back on nominal wage cuts.

There are considerably fewer studies on the macroeconomic implications of DNWR than on the microeconomic existence and extent of DNWR. Knoppik and Beissinger (2003) calculate for Germany, based on their estimates, the impact of DNWR in a hypothetical situation of zero inflation. They come to the conclusion that given constant prices the equilibrium unemployment rate would increase because of DNWR by approximately one percentage point. The macroeconomic implications for the USA, estimated by Akerlof et al. (1996), are a bit higher. According to their simulation, a drop of the inflation rate from 3 to 0 percent would increase the unemployment rate by between 1 to 2.6 percentage points.

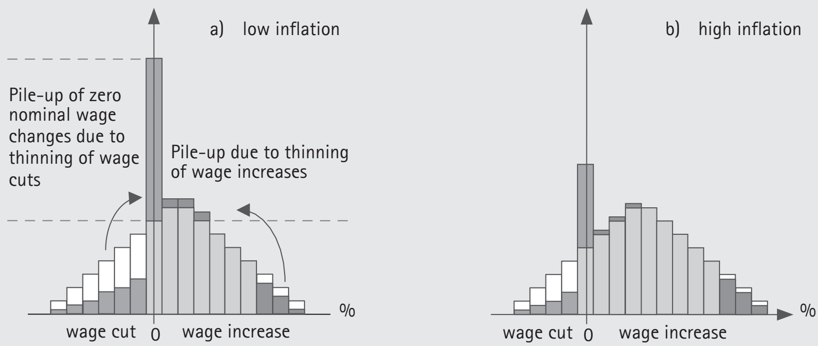
In the context of macroeconomic implications of DNWR one criticism – based on the so-called Lucas-critique – is expressed quite regularly (see, e.g., Ball and Mankiw, 1994; Gordon, 1996). Gordon (1996, p. 62), e.g., argues: "If the macroeconomic environment were different, microeconomic behavior would be different. Nominal wage reductions would no longer be seen as unusual if the average nominal wage was not growing." If individuals adapt to an environment with low inflation in the long-run, than the phenomenon of DNWR should vanish. Hence there should not be any negative effect on the wage level and the unemployment rate.

This criticism is attenuated by the fact that some of the microeconomic studies are based on data of periods of low inflation. Also the two above mentioned survey studies from Sweden (Agell and Bennmarker, 2007; Agell and Lundborg, 2003) were conducted in a period of low inflation – and found a high degree of DNWR. The psychological approach even suggests that in times of low inflation individuals more often use the nominal instead of the real frame. Hence the phenomenon of DNWR does not necessarily have to vanish in periods of consistently low inflation.

There also exist discrepancies between microeconomic and macroeconomic results. On the level of individual (microeconomic) wage changes nearly all studies find strong DNWR. The macroeconomic implications of DNWR on the average real wage increase and the unemployment rate seem to be considerably too weak based on what one would except because of the microeconomic evidence. Lebow et al. (1999) call this contradiction the "micro-macro-puzzle".

However, recent research seems to be able to explain this puzzle. According to Elsby (2009), DNWR not only causes wage cuts to be compressed (i.e. that wage cuts are avoided or that wages decline less) but DNWR also compresses wage increases (see Figure 1.5). This is explained by forward-looking behavior of firms. Firms do not increase nominal wages as much as possible if they anticipate that they cannot achieve necessary real wage cuts in the future. The compression of wage increases counteracts the increase of the average real wage caused by avoided nominal wage cuts. Therefore, evidence for strong DNWR on the individual (microeconomic) level can be consistent with weak or even non-existing macroeconomic effects. The next chapter will discuss this finding in detail and I show that for Germany a decrease in the inflation rate of one percentage point only causes an average increase of the average real wage growth of between 0.013 and 0.060 percent. Elsby (2009) finds similar results for Great Britain and the USA. The results indicate that DNWR does not provide a strong argument against low inflation targets of central banks.

Figure 1.5: Distribution of wage changes with downward nominal wage rigidity (DNWR) according to recent studies



With downward nominal wage rigidity and compression of wage increases

1.5 Conclusions

This chapter summarized and evaluated the fundamental results of the literature on causes and extent of DNWR.

Microeconomic studies usually detect a significant degree of DNWR. In Germany, approximately 28 percent of wage cuts desired by employers are avoided because of DNWR (Knoppik and Beissinger, 2009). For other countries the existence of DNWR is also documented, and sometimes even higher degrees of DNWR are found (see, e.g., Figure 1.4).

Based on the fact that DNWR exists, it is often concluded that low inflation leads to wage pressure on the macro level. Hence money policy aiming at low inflation is blamed for causing excess unemployment.

However, recent studies – that will be presented in the next chapter – indicate that because of DNWR not only wage cuts are dampened but also wage increases. Taking the effect of DNWR on wage increases into account, it seems that DNWR hardly affects the average wage level. Hence there do not seem to be any negative consequences on aggregate employment and DNWR cannot be used to make a case for higher inflation targets of central banks.

2 Does Downward Nominal Wage Rigidity Dampen Wage Increases?

Concerns about potentially adverse employment effects of low inflation have given rise to a plethora of studies on the extent of downward nominal wage rigidity (DNWR), such as the microeconometric multicountry studies of Behr and Pötter (2010), Knoppik and Beissinger (2009), and Dickens et al. (2007a)[1] or the survey evidence provided by Bewley (1999). These concerns are based on Tobin's (1972) hypothesis that if nominal wages are downwardly rigid, a certain amount of positive inflation may be necessary to ease firms' real wage adjustments in response to idiosyncratic shocks ("inflation may grease the wheels of the labor market"). Focusing on the compression of wage cuts, microeconometric studies usually find a high degree of DNWR. However, the resulting macroeconomic effects on aggregate real wages and employment seem to be surprisingly weak, leading Lebow et al. (1999) to speak of a "micro-macro puzzle".

A possible solution to that puzzle has been offered by Elsby (2009), who develops an intertemporal model in which downward wage rigidity arises because nominal wage cuts are followed by sharp decreases in productivity. Wage increases therefore become irreversible to some degree. Firms that increase wages during upswings may find it difficult to reverse their decisions later when the economic environment will possibly deteriorate. Forward-looking firms take the path dependence of wage changes into account when determining the optimal wage policy; they refrain from large wage increases to reduce the probability of costly future nominal wage cuts. Moreover, because DNWR raises the wage level inherited from the past, firms do not have to raise wages as much or as often as in a situation without wage rigidity to obtain the profit-maximizing wage level. As a consequence, firms will compress wage increases as well as wage cuts in the presence of DNWR. This leads to the surprising prediction that average real wage growth, and hence aggregate real wages, should not be affected by DNWR and that the aggregate employment effects should be weak or nonexistent.

In this chapter I first extend the empirical approach of Elsby (2009) by applying unconditional quantile regressions (UQR) to the data in addition to variants of Elsby's (2009) OLS model specification. The application of UQR enables me to take into account the variance and the cross variable covariance in the microdata. Second, I provide an empirical analysis of the effects of inflation on the shape of the real wage change distribution for Germany, a country with stronger labor unions

1 Dickens et al. (2007a) also deal with the extent of real wage rigidities. Holden and Wulfsberg (2008) have carried out a multi-country study on DNWR using industry data. See also Section 1.4 for further examples of studies on the extent of DNWR.

and a higher labor union density than in the United States and the United Kingdom – for which Elsby (2009) provides empirical evidence. My analysis provides some insights into whether Elsby's (2009) predictions can be observed in a country that may already be affected by wage compression due to its labor market institutions.

In line with the empirical literature on DNWR, the analysis focuses on the wage change distribution of "job stayers", i.e. employees who have a "stable employment relationship" with an employer, whereas Elsby's (2009) analysis also includes "job movers". This inclusion may lead to a systematic relationship between inflation and the compression of the real wage change distribution that has nothing to do with DNWR. The reason is that during economic upswings, inflation often rises, and at the same time, more voluntary job changes occur that go hand in hand with real wage increases (see, e.g., Cornelißen et al., 2007). As a robustness check and a further innovation relative to Elsby's (2009) analysis I also analyze whether the results are changed if inflation forecasts are used instead of actual inflation, because for the distribution of wage changes expected future inflation could be more relevant.

The empirical analysis is undertaken for West Germany for the 1975–2007 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 dataset comprises the total population gainfully employed and covered by the social security system. After the data selection, the remaining employment spells enable me to analyze over 169 million earnings changes, amounting to more than 5.2 million earnings changes per year on average. Among the main advantages of this dataset are the sheer wealth of information and the high reliability of the earnings data.

The remainder of the chapter is structured as follows. The next section summarizes the key findings of Elsby's (2009) model. Section 2.2 contains the data description. Section 2.3 presents the empirical implementation and results as well as a comparison with Elsby's (2009) results. Section 2.4 deals with the macroeconomic implications, and Section 2.5 concludes.

2.1 The Model

In this section, I explain the main idea of the underlying model and present the key findings needed for the empirical testing.

The main feature of Elsby's (2009) intertemporal model of worker resistance to wage cuts is that wage increases become irreversible to some degree because nominal wage cuts lead to a sharp decrease of work effort. This assumption is based on Bewley's (1999) findings that a key reason for the reluctance to cut nominal wages is the belief that nominal wage reductions can damage worker morale and that morale is a key determinant of worker productivity. A wage increase will raise work effort. However, a wage cut of the same amount will reduce effort by a greater amount. This implies that wage increases can be reversed in the future only at an extra cost. As a consequence, forward-looking firms will not only reduce the incidence of wage cuts, but will also moderate wage increases.[2] Formally, this is captured by an effort function in the spirit of the fair-wage effort hypothesis of Solow (1979) and Akerlof and Yellen (1986), with an additional term reflecting the impact of nominal wage cuts on effort.

$$e = \ln\left(\frac{W}{B}\right) + c\ln\left(\frac{W}{W_{-1}}\right)\mathbf{I}(W < W_{-1}), \tag{2.1}$$

where W is the nominal wage, W_{-1} is the lagged nominal wage, $c > 0$ is a parameter varying the productivity costs of a nominal wage cut to the firm, and $\mathbf{I}()$ is the indicator function for a nominal wage cut. Real unemployment benefits $b = B/P$ are assumed to be constant over time, where B denotes nominal unemployment benefits, and P is the price level. The price level evolves according to $P_t = e^{\pi} P_{t-1}$, where π reflects the inflation rate.

Given the effort function (2.1), Elsby (2009) considers a discrete-time, infinite-horizon model. In the model, price-taking worker-firm pairs maximize the expected discounted value of profits by choosing the nominal wage W_t at each date t. The worker-firms' productivity function is given by $(A/P) \times e$, where A denotes a nominal technology shock. The shock is idiosyncratic to the worker-firm pair, is observed contemporaneously, and acts as the source of uncertainty in the model. The shocks evolve according to a geometric random walk. This has the implication that average nominal productivity rises in line with inflation π and productivity growth μ.

The value of a job in recursive form is given by:

$$J(W_{-1}, A) = \max_{W}\left\{A\left[\ln\left(\frac{W}{B}\right) + c\ln\left(\frac{W}{W_{-1}}\right)\mathbf{I}(W < W_{-1})\right] - W + \beta e^{-\pi}\int J(W, A')dF(A'|A)\right\}, \tag{2.2}$$

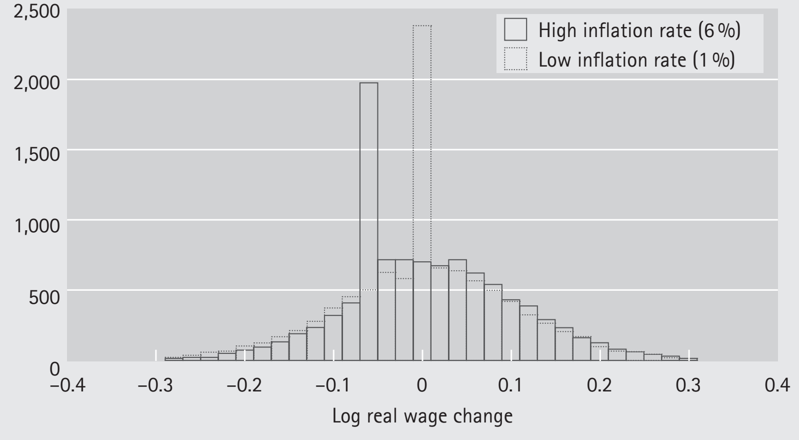
where $\beta \in [0,1)$ is the real discount factor of the firm. Lagged values are denoted by the subscript −1, and forward values are denoted by a prime. By setting $c = 0$ the model is reduced to a frictionless model. It can be shown that frictionless nominal wages are equal to the nominal shock A. Hence, wage changes fully reflect changes in productivity.

2 It must be stressed that the argument that DNWR leads to a compression of wage increases does not depend on this specific justification for DNWR. It would also apply under other reasons for DNWR, e.g. if it is caused by the fact that the wage of the old wage contract still determines pay while the bargaining parties bargain over a new contract (so-called holdout). For a detailed theoretical discussion see MacLeod and Malcomson (1993) and Holden (1994).

DNWR changes the shape of the frictionless wage change distribution in two characteristic ways. First, there is a range of values for the nominal shock *A*, for which the firm finds it optimal not to change the nominal wage. This leads to a spike at zero in the nominal wage change distribution and accordingly to a spike at minus the inflation rate in the real wage change distribution. Second, if the change in *A* is strong enough and the firm decides to change the nominal wage, the wage change will be compressed relative to the frictionless case. Not surprisingly, wage cuts are compressed because they imply a discontinuous fall in productivity at the margins. More interestingly, the model predicts that wage increases are compressed as well. One reason is that forward-looking firms take the path dependence of wage changes into account when determining the optimal wage policy; they refrain from large wage increases to reduce the probability of costly future nominal wage cuts. Moreover, the firms will generally inherit higher wages from the past. Consequently, firms do not have to increase nominal wages by as much or as often in order to achieve the desired wage level.

Figure 2.1 presents simulated real wage change distributions for high and low inflation based on the predictions of Elsby's (2009) theoretical model. One can see that real wage increases are compressed in the case of low inflation.[3]

Figure 2.1: Simulated log real wage change distributions



Notice that in the absence of DNWR, a change in the productivity growth rate should lead to a one-to-one shift of the real wage change distribution, whereas a change in the inflation rate should leave the distribution unaltered. In contrast, if

3 In the simulation, the rate of productivity growth has been kept constant. Similar effects on the real wage change distribution are obtained if the (average) rate of productivity growth is changed instead of a change in the inflation rate.

DNWR exists, one should observe a systematic relationship between changes in the inflation rate and/or productivity growth rate, on the one hand, and changes in the shape of the real wage change distribution, on the other hand. In the following, I will focus on the impact of the inflation rate on the shape of the real wage change distribution because the inflation rate can be controlled by monetary policy.

The compression of nominal wage changes will have effects on the percentiles of the real wage change distribution. If DNWR is present, the model generates the following predictions about the effect of the inflation rate on the percentiles of the real wage change distribution, depending on whether the percentiles

1. lie below the range of zero nominal wage changes;
2. lie in the range of zero nominal wage changes;
3. lie above the range of zero nominal wage changes.

(1) Nominal wage cuts will be compressed relative to the frictionless case because of the implied fall in productivity. The probability that a firm is willing to increase nominal wages will increase as the inflation rate and/or productivity growth rise. With higher inflation and/or higher productivity growth, a firm is more likely to reverse nominal wage cuts in the future. As a result, a firm is less inclined to incur the costs of wage cuts. With higher inflation, one should therefore observe fewer and less pronounced nominal wage cuts. This implies that low percentiles of the real wage change distribution, lying below the range of zero nominal wage change, will rise with the inflation rate and productivity growth.

(2) Because of DNWR, a nonnegligible range of the percentiles of the real wage change distribution will correspond exactly to zero nominal wage changes and therefore be equal to minus the inflation rate. Those percentiles fall one-to-one with the inflation rate. With higher inflation, firms affected by DNWR are able to achieve reductions in real labor costs without falling back on costly nominal wage cuts. It is in this sense that inflation greases the wheels of the labor market in the presence of DNWR.

(3) In an uncertain world, a firm affected by DNWR will also compress nominal wage increases because raising wages increases the risk of costly future nominal wage cuts. If inflation is low, upper percentiles of the wage change distribution will therefore be reduced relative to the frictionless case. The probability that a firm wishes to reduce nominal wages will decline when the inflation rate and/or productivity growth rise. In this case, firms are less likely to cut wages in the future and no longer need to restrain increases as much as in times with low inflation. On average, this should lead to more than a one-to-one increase of the upper percentiles of real wage change distribution with productivity growth as well as to an increase with inflation.

Because of these theoretical predictions, one expects the following coefficients in a regression of the percentiles of the log real wage change distribution on the inflation rate and the productivity growth rate (see Table 2.1).

Table 2.1: Predicted effects of the rate of inflation and of productivity growth on the unconditional percentiles of the log real wage change distribution according to Elsby's (2009) model

τth percentile of the log real wage change distribution (P_τ)	Coefficient on	
	inflation rate	productivity growth
$P_\tau <$ minus inflation rate	> 0	> 1
$P_\tau \approx$ minus inflation rate	< 0	attenuates towards zero (< 1)
$P_\tau >$ minus inflation rate	> 0	> 1

2.2 Data

The empirical analysis is undertaken for West Germany for the 1975–2007 period using the BeH, the Employee History File of the IAB. 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. From 1975 to 2007, the BeH contains information about 72,695,902 people as well as 1,171,326,023 employment spells (IAB, 2009). Important advantages of this dataset 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 studies based on compensation data from household surveys, measurement error due to erroneous reporting should be less of a problem in my analysis.

The earnings data are right censored at the contribution assessment ceiling (Beitragsbemessungsgrenze). For employees whose earnings are censored, earnings changes cannot be computed. For the analysis, I use noncensored earnings spells[4] of full-time working male employees from West Germany aged 16 to 65. In line with the literature, the analysis is confined to job stayers. Usually, job stayers are defined as full-time working employees who do not change the employer between two consecutive time periods. I apply a narrower and better suited concept and require that the employees continually exercise the same job at the same employer for at

4 After dropping the censored earnings spells the sample shows an underrepresentation of highly qualified (white collar) workers (see Tables B.3 and B.4 of Appendix B.1). Hence, the results are somewhat less generalizable.

least two consecutive years.[5] In contrast to my data selection, Elsby (2009) includes job movers in his analysis. This inclusion might lead to a systematic relationship between inflation and the compression of the wage change distribution that has nothing to do with DNWR. The reason is that during economic upswings inflation usually rises, and at the same time, more voluntary job changes occur that go hand in hand with real wage increases (Cornelißen et al., 2007).

After the selection, more than 169 million earnings changes remain in my sample. I am therefore able to analyze an average of more than 5.2 million earnings changes per year. The sample size is a large advantage in comparison to the data applied in Elsby (2009). His largest dataset, the New Earnings Survey (NES) for the UK, allows him to analyze on average less than 74,000 observations per year. For the USA, it is less than 24,000 and 1,800 observations using the Current Population Survey (CPS) and the Panel Study of Income Dynamics (PSID), respectively. A further advantage of the German data is the longer time period of 32 years compared to 21–24 years in Elsby's (2009) analysis. A disadvantage of the German data is the fact that I am not able to observe hourly wages, only daily wages. There is also the problem that shifts from part-time to full-time work, and vice versa, that occur during the course of the year do not lead to a new report of the employer.[6] Because such shifts are much more common for female employees (see, e.g., Schäfer and Vogel, 2005), I exclude women from the analysis. This is in contrast to Elsby's (2009) analysis in which male and female employees are included.

As the inflation rate, I use the log change in the consumer price index (CPI).[7] Following Elsby (2009), I measure productivity growth using the observed average regional real wage change. The reason for not directly using a productivity measure is that real wages adjust to changes in productivity with a time lag. I would have to model some kind of error-correction mechanism for the discrepancy between real wage changes and productivity growth. I can avoid these complications by using the average regional real wage change as a proxy variable reflecting the impact of (regional) productivity growth on wages. It is a suitable proxy because, according to the theoretical predictions, DNWR should have no effect on average wage changes.[8]

5 The breakdown of occupations is very detailed, but still not every job change leads to a change in the occupation classification. Therefore, some spells of persons who changed jobs within a firm may not be excluded. The narrower "same position" restriction has also been applied by Christofides and Stengos (2001).

6 A new status is conveyed with the annual report at the end of a year. This status applies for the whole year.

7 Stüber and Beissinger (2010) show that using the producer price index (PPI) instead of the CPI does not significantly affect the results. The CPI is more relevant for employees, whereas the PPI is more relevant for firms' wage setting.

8 As one of the referees of Stüber and Beissinger (2012) correctly pointed out, there are other arguments for including the average regional wage growth in the regression apart from it being a proxy for regional productivity growth – e.g. because it is an important alternative wage for the workers.

Among the other control variables, the absolute change in the rate of inflation is included. This is motivated by the hypothesis of Groshen and Schweitzer (1999) that higher inflation volatility yields greater dispersion in relative wages, regardless of the existence of DNWR. The current and lagged regional unemployment rates are included because DNWR may affect unemployment. The unemployment rates are used to control for changes in the wage change distribution due to workers "leaving" the distribution. Further control variables for the applied regression methods are shown in Table 2.2. For more details concerning the data and the data selection, see Appendix B.1.

Table 2.2: Variables for the applied regression methods

Variables	Seemingly Unrelated Regression/ OLS Regression	Unconditional Quantile Regression
	Dependent Variable	
Real wage change	τth percentile from re-weighted regional log real wage change distribution	Recentered influence function (RIF) of the individual log real wage change
	Explanatory Variables	
Inflation rate	Log change in the consumer price index (CPI) for Germany	
Productivity growth	Average regional real wage change	
	Microvariables	
Age	Mean age of employees in region	Age and age squared
Education	Percentages of employees in region within seven educational classes	Education class of employee
Foreign nationality	Percentage of employees in region with foreign nationality	Dummy for employee with foreign nationality
Occupation	Percentages of employees in region within six occupational fields	Occupation field of the employee
Worker	Percentage of white-collar workers in the region	Dummy for white-collar worker
	Regional Variables	
Absolute change in the rate of inflation	Absolute change in the rate of inflation (CPI)	
Unemployment rate	Current and lagged regional unemployment rate	
	Dummy Variables	
Year 1984	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. This leads to a level effect on the 1983–1984 earning changes. For more details, see Appendix B.1.	
Regions	Dummies for the 10 former West German states (excluding Berlin)	

2.3 Empirical Implementation and Results

Elsby (2009) uses an OLS regression to estimate the effect of the inflation rate and the average regional real wage change (as a proxy for productivity growth) on the percentiles of the real wage change distribution and finds evidence for wage compression for the upper percentiles. A disadvantage of this OLS regression is that only aggregate data at the regional level can be used, thereby neglecting the variance and the cross-variable covariance in the microdata. First, an identical mean does not imply that the distributions are also identical. Second, for example, it is possible to observe two regions with the same mean age of employees and the same composition of the educational classes. Using OLS regression, these two regions are identical in terms of age and education. However, a closer look might reveal that in one region mainly young employees are highly educated, while in the other region mainly older employees are highly educated. Elsby (2009) accounts for changes in sample composition by applying the method of DiNardo, Fortin, and Lemieux (1996), henceforth "DFL", to the wage data. The DFL method enables the estimation of counterfactual (re-weighted) real wage change distributions that would prevail if the distribution of worker characteristics did not change. However, this method is not able to take the general equilibrium effects of changes in sample composition on wages into account, because actual wages are used to calculate the counterfactual wage change distribution. Therefore, an approach in which the information contained in the microdata can be directly used is preferable.

Because of the above-mentioned critique, I apply two regression methods. To enable a comparison with Elsby's (2009) results, I first apply variants of his OLS approach to the data and estimate the impact of inflation and other variables on the percentiles of the real wage change distribution. Second, I apply a new regression method proposed by Firpo et al. (2009): unconditional quantile regression (UQR). It allows us to directly use the information contained in the microdata and to estimate the impact of explanatory variables, such as inflation, on the unconditional percentiles of the real wage change distribution. The advantage of UQR over OLS is that it takes the whole distribution of explanatory variables into account.[9] Finally, I briefly compare my results with the results Elsby (2009) obtained for the USA and the UK.

9 I also applied quantile regression to the data to look at the effects of the inflation rate or of productivity growth on the real wage change distribution conditional on the attributes of the employee and conditional on the region where the employee works. Results are shown in Table B.5 in Appendix B.2.

2.3.1 Impact of Inflation on the Unconditional Percentiles using Seemingly Unrelated Regression

To take the effects of changes in sample composition on the shape of the real wage change distribution – albeit imperfectly – into account, I first apply the DFL method. The worker characteristics for the re-weighted density are age, age squared, class of worker, a dummy for foreign nationality, qualification level and occupational field. The DFL method requires no parametric assumptions on the effect of these controls on wage changes.[10]

I use the re-weighted real wage change distributions to calculate the τth percentile of the distribution for region r at time t ($P_{\tau, rt}$), with $\tau = 10, 20, \ldots, 90$. As a first approach, I estimate the effect of the inflation rate, π, on $P_{\tau, rt}$ using regressions of the following form:

$$P_{\tau, rt} = \alpha_\tau + \eta_\tau \pi_t + \lambda_\tau \mu_{rt} + \mathbf{z}'_{rt} \varphi_\tau + \varepsilon_{\tau, rt} = \alpha_\tau + \mathbf{x}'_{rt} \beta_\tau + \varepsilon_{\tau, rt} \tag{2.3}$$

In Equation 2.3 I take into account that the location of the real wage change distribution for region r at time t depends on productivity growth μ_{rt}, measured as average regional real wage growth. The vector $\mathbf{z}_{rt}$ contains further control variables, shown in Table 2.2.

Elsby (2009) uses OLS regressions with region-specific dummies – least squares dummy variable (LSDV) regressions. However, because I regress the different percentiles of one single distribution, the residuals are very likely simultaneously correlated across equations. Therefore, I use an LSDV approach within a seemingly unrelated regression (SUR) with small-sample adjustment and weighting by region size:[11]

$$P = \begin{bmatrix} P_{10,rt} \\ P_{20,rt} \\ \vdots \\ P_{90,rt} \end{bmatrix} = \mathbf{X}\beta + \varepsilon = \begin{bmatrix} \mathbf{x}'_{10,rt} & 0 & \ldots & 0 \\ 0 & \mathbf{x}'_{20,rt} & \ldots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & \ldots & \mathbf{x}'_{90,rt} \end{bmatrix} \begin{bmatrix} \beta_{10} \\ \beta_{20} \\ \vdots \\ \beta_{90} \end{bmatrix} + \begin{bmatrix} \varepsilon_{10,rt} \\ \varepsilon_{20,rt} \\ \vdots \\ \varepsilon_{90,rt} \end{bmatrix} \tag{2.4}$$

with $\mathbf{x}'_{\tau, rt} = (\mu_{rt} \;\; \pi_t \;\; \mathbf{z}'_{rt})$.

10 See DiNardo et al. (1996) or Fortin et al. (2011) for a description of the procedure. I apply the DFL method to each region, and I choose the final sample year (2007) as the "base year". The weights are estimated using a probit model according to the Stata ado file provided by Fortin: http://faculty.arts.ubc.ca/nfortin/datahead.html.

11 I performed "within" fixed effects and random effects regressions for all percentiles. For each percentile I tested whether or not there are significant differences in the coefficients of the two regressions using Hausman-Tests. The Hausman-Test was rejected for every percentile. Therefore, I use a fixed effects model.

The results of the SUR estimates can be found in Table 2.3.[12] The results show that the upper tail of the wage change distribution is compressed as a result of DNWR, as predicted by the model (see Table 2.1). The estimated impact of the inflation rate is significantly positive for the 80*th*–90*th* percentiles; the coefficients on the average regional real wage change (significantly) exceed unity for these percentiles. These results are consistent with lower inflation leading to a compression of wage increases.

Table 2.3: Effects of inflation and productivity growth on the unconditional percentiles of the real wage change distribution using seemingly unrelated regression

	Consumer price index			Average regional real wage growth (as a proxy for productivity growth)		
	Coef.	**Std.Err.**	$P>\|t\|$	**Coef.**	**Std.Err.**	$P>\|t\|$
p10	–0.063	0.027	0.021	0.912^{+}	0.024	0.000
p20	–0.114	0.015	0.000	0.858^{+}	0.013	0.000
p30	–0.082	0.016	0.000	0.927^{+}	0.015	0.000
p40	–0.101	0.021	0.000	0.959^{+}	0.019	0.000
p50	–0.088	0.017	0.000	0.958^{+}	0.016	0.000
p60	–0.043	0.015	0.004	0.987	0.013	0.000
p70	0.005	0.013	0.723	1.004	0.012	0.000
p80	0.047	0.016	0.003	1.024	0.014	0.000
p90	0.091	0.033	0.005	1.057	0.029	0.000

Notes: Seemingly unrelated regression with small-sample adjustment weighted by region size. Controls: regions, mean age, absolute change in inflation, current and lagged unemployment rate, dummy for the year 1984, percentage of the educational classes, percentage of workers with foreign nationality, percentage of white-collar worker, percentage of the occupational fields. $^{+}$: coef. for productivity growth significantly different from 1 at the 5 % level.

For reference, Table 2.3 also reports estimates on the effects of inflation and the average regional real wage change on lower percentiles. Note that the predictions of the model on the coefficients for lower percentiles depend on the position of the zero nominal wage change in the distribution of the real wage change distribution (see Table 2.1).

The results for percentiles in the range of zero nominal wage changes are consistent with the predictions of the model. In my data, the spike at zero nominal

12 For comparison, the results of an LSDV regression ignoring the contemporaneous correlation of the residuals are documented in Table B.6 of Appendix B.3.

wage change predominantly appears above the 10*th* and below the 30*th* percentile.[13] In this percentile range – in Table 2.3 represented by the 20*th* percentile – the coefficient on the inflation rate is significantly negative, and the coefficient on the average regional real wage change is significantly below one and attenuates towards zero compared to the coefficients of the 10*th* and the 30*th* percentiles.

For percentiles that predominantly lie below the range of zero nominal wage changes – the 10*th* percentile – the model predicts a coefficient of the inflation rate larger than zero. Here the prediction of the model fails because the coefficient on the inflation rate is significantly negative. This may be due to the fact that for 13 years, the spike at zero nominal changes lies near the 10*th* percentile (between the 6*th* and the 14*th* percentile). The coefficient on the average regional real wage change is higher than the one of the 20*th* percentile, but it does not rise above unity as predicted by the model.

2.3.2 Impact of Inflation on the Unconditional Percentiles using Unconditional Quantile Regression

In the following, I apply the UQR approach proposed by Firpo et al. (2009) to estimate the impact of explanatory variables, such as inflation, on the *unconditional* percentiles of the real wage change distribution, taking into account the variance and cross-variable covariance in the microdata.[14] A standard quantile regression (Koenker and Bassett, 1978; Koenker, 2005) is only able to observe the effects of inflation on the *conditional* percentiles of the real wage change distribution. Wage changes that correspond to a certain conditional percentile can, however, be distributed over the entire observed wage change distribution. The UQR allows us to estimate the impact of changes in the distribution of explanatory variables, $\mathbf{X}$, on the marginal percentiles of the dependent variable, Y. A further advantage of applying UQR to the data is that I do not need to apply the DFL method in the first step to estimate counterfactual wage change distributions.

To estimate the average marginal effect $E[d\Pr[Y > P_\tau|\mathbf{X}]/d\mathbf{X}]$ Firpo et al. (2009) propose, inter alia, a recentered influence function OLS (RIF-OLS) regression.[15] This regression provides consistent estimates if $\Pr[Y > P_\tau|\,\mathbf{X} = x]$ is linear in x. In case

13 Zero nominal wage changes appear in the range between the 6*th* and the 37*th* percentile. For the early years of the dataset with higher inflation the spikes predominantly appear in very low percentiles, while for later years, with very low inflation, the spikes predominantly appear in higher percentiles of the range. For later years, the spike is often observed over more than one percentile. Generally, I observe the zero nominal wage change seven times for percentiles ≤ the 10*th* percentile, 12 times for the range above the 10*th* and ≤ the 20*th* percentile, 24 times in the range above the 20*th* and ≤ the 30*th* percentile and nine times for percentiles above the 30*th* percentile.

14 The "unconditional percentiles" are the percentiles of the marginal distribution of the outcome variable.

15 For a brief introduction see also Fortin et al. (2011).

of quantiles, the conditional expectation of the recentered influence function $E[RIF(Y; P_\tau, F_Y) \mid \mathbf{X}]$ can be viewed as an unconditional quantile regression.

The *RIF*-OLS consists of regressing the (recentered) influence function *RIF* of the outcome variable Y for the τth percentile P_τ on the explanatory variables $\mathbf{X}$ by OLS. The *RIF* is computed by estimating the sample percentile P_τ and the density of the outcome variable $\hat{f}_Y(\cdot)$, using kernel (or other) methods: $\widehat{RIF}(Y; \hat{P}_\tau) = \hat{c}_{1,\tau}\mathbf{I}(Y > \hat{P}_\tau) + \hat{c}_{2,\tau}$, where $\mathbf{I}(\cdot)$ is an indicator function, $\hat{c}_{1,\tau} = 1/f_{Y(P_\tau)}$, $f_{Y(P_\tau)}$ is the density of Y evaluated at P_τ, and $\hat{c}_{2,\tau} = P_\tau - c_{1,\tau}(1 - \tau)$. I follow Firpo et al. (2009) and use a kernel density estimator $\hat{f}_Y(\hat{P}_\tau) = \dfrac{1}{Nb}\sum_{i=1}^{N} K_Y((Y_i - \hat{P}_\tau)/b)$, where $K_Y(\cdot)$ is a kernel function, and $b > 0$ denotes the scalar bandwidth.[16] I make use of the *RIF*-OLS and regress the percentile-transformed individual log real wage change on $\mathbf{X} = (\mu_{rt} \;\; \pi_t \;\; \mathbf{z}'_{irt})$. The vector $\mathbf{z}$ contains the control variables on the individual level wherever possible (see Table 2.2). To estimate the density of the individual log real wage change, I use a Gaussian kernel.[17] The bandwidth b is set to the "optimal" width.[18] For the regression, I use a 10 %-stratified sample of the data.[19] The results for the UQR can be found in Table 2.4.[20]

The UQR shows a significantly positive coefficient for the inflation rate for the 90*th* percentile. This result is consistent with lower inflation leading to a compression of wage increases – the upper tail of the wage change distribution is compressed as a result of DNWR. However, only very high wage increases are compressed. In contrast, as has been shown above, for the SUR, the coefficients for the 80*th* and the 90*th* percentile of the inflation rate are significantly positive. This points to an overestimation of the compression of wage increases using SUR.

For reference, Table 2.4 also reports estimates on the effects of inflation and the average regional real wage change (as a proxy for productivity growth) on lower percentiles.

The results for the percentiles in the range of zero nominal wage changes are consistent with the predictions of the model summarized in Table 2.1.[21] In the

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16 The influence function $IF(Y; \nu, F_Y)$ of a distributional statistic $\nu(F_Y)$ represents the influence of an individual observation on that distributional statistic. Adding back the statistic $\nu(F_Y)$ to the *IF* yields what Firpo et al. (2009) call the recentered influence function (*RIF*). Therefore, for the τth percentile, the $RIF(Y; P_\tau, F_Y) = P_\tau + IF(Y; P_\tau, F_Y) = P_\tau + (\tau - \mathbf{I}(Y > P_\tau))/f(P_\tau)$.

17 For the *RIF*-OLS, I used the Stata ado file provided by Fortin: http://faculty.arts.ubc.ca/nfortin/datahead.html.

18 The "optimal" width is the width that would minimize the mean integrated squared error if the data were Gaussian and a Gaussian kernel were used. Thus, it is not optimal in a global sense.

19 The sample has been stratified by region, age, foreign nationality, worker class, occupational field, and year.

20 To identify the impact of the individual-level control variables, I run regressions omitting individual characteristics (see Table B.7 of Appendix B.4). This leads to an overestimation of the wage compression for very high percentiles of the wage change distribution.

21 In the data, the spike at the zero nominal wage change predominantly appears between the 10*th* and the 30*th* percentiles. For an overview of the distribution of the position of the zero nominal wage change, see Footnote 13.

percentile range above the 10*th* and below the 30*th* percentiles – in Table 2.4 represented by the 20*th* percentile – the coefficient on the inflation rate is significantly negative, and the coefficient on the average regional real wage change is significantly below one and attenuates towards zero compared to the coefficients of the 10*th* and the 30*th* percentiles. For percentiles below the range of zero nominal wage changes, the model predicts coefficients of the inflation rate larger than zero. Here the prediction of the model fails because the coefficient for inflation for the 10*th* percentile is significantly negative but smaller in absolute value than for the SUR.

Table 2.4: Effects of inflation and productivity growth on the unconditional percentiles of the real wage change distribution using unconditional quantile regression

	Consumer price index			Average regional real wage growth (as a proxy for productivity growth)		
	Coef.	Std.Err.	$P>\|t\|$	Coef.	Std.Err.	$P>\|t\|$
p10	–0.043	0.004	0.000	0.862†	0.003	0.000
p20	–0.148	0.002	0.000	0.716†	0.002	0.000
p30	–0.152	0.002	0.000	0.813†	0.002	0.000
p40	–0.136	0.002	0.000	0.862†	0.002	0.000
p50	–0.142	0.001	0.000	0.949†	0.001	0.000
p60	–0.165	0.002	0.000	0.993†	0.002	0.000
p70	–0.125	0.002	0.000	0.952†	0.002	0.000
p80	–0.037	0.003	0.000	0.950†	0.003	0.000
p90	0.068	0.005	0.000	0.979†	0.005	0.000

Notes: Unconditional quantile regression. Controls: region dummies, age, age squared, absolute change in inflation, current and lagged unemployment rate, dummy for the year 1984, educational class, dummy for worker with foreign nationality, occupational fields, dummy for white-collar worker. Bootstrapped standard errors, 50 replications. †: coef. for productivity growth significantly different from 1 at the 5 % level.

As for the average regional real wage change I find coefficients that are very similar to those obtained using SUR. The coefficients are highest for very high percentiles, and the coefficient for the 10*th* percentile is higher than for the 20*th* percentile. However, the coefficients for very high percentiles do not rise above unity, which may be due to the fact that the average regional wage growth not only reflects productivity growth but also acts as an outside wage in the wage-setting process.

As a robustness check I also analyzed whether similar results are obtained if inflation forecasts from auto-regressive integrated moving average (ARIMA) models are used instead of actual inflation, because it can be argued that wage compression should depend on expected future inflation, and not current inflation. A detailed description of how I constructed the series of expected inflation is contained in Appendix B.5. In this Appendix I also report the estimation results using expected inflation in the UQR. It turns out that the compression of wage increases becomes more pronounced than in the UQR estimates with actual inflation. Hence, the basic conclusion that wage increases are compressed in times of low inflation holds for both current inflation and expected inflation.

2.3.3 Comparison with Results for the USA and the UK

Elsby (2009) analyzes whether a compression of wage increases can be found for the USA and the UK. He uses OLS regressions, but otherwise his approach is similar to the Seemingly Unrelated Regression introduced in Section 2.3.1.

For the empirical analysis, Elsby (2009) uses data taken from the NES (1975–1999) for the UK and data taken from the PSID (1971–1992) and the CPS (1979–2002) for the USA. The results are similar to my results for Germany. Elsby (2009) provides evidence that as a result of DNWR, the upper tail of the real wage change distribution is compressed. For all three datasets, the estimated impact of inflation is positive for the 70*th*–90*th* percentiles and often significant. The coefficients on the average regional real wage change exceed unity for these upper percentiles of the real wage change distribution and are strongly significant. For the range of the zero nominal wage change, the coefficients on inflation are negative, and the coefficients on the average regional real wage change attenuate towards zero for all of these percentiles. For percentiles below the range at zero nominal wage changes, the respective coefficients on inflation are either significantly negative or they are insignificantly positive. Here, the prediction of Elsby's (2009) model fails as it did for Germany. The coefficients on the average regional wage change rise above unity using CPS and NES data.

2.4 Macroeconomic Implications

In this section, I look at the effect of DNWR on average real wage growth and compare the estimated effects using the predictions from the SUR and UQR. According to the underlying theoretical model, DNWR should have no effect on average real wage growth and hence on the average real wage level. Previous empirical studies, however, which neglected the compression of wage increases, report positive estimates on the effects of DNWR on average real wage growth (Card and Hyslop, 1997) or the average real wage level (Knoppik and Beissinger, 2003).

In the previous section, I showed that wage increases in Germany are compressed when inflation is low. This compression should dampen the so-called "wage sweep-up" effect of DNWR and may even completely annihilate any effect of DNWR on average real wage growth. To quantify the impact of DNWR on real wage growth, I estimate the average log real wage change when inflation is low π_L and average log real wage change when inflation is high π_H and calculate $\hat{\lambda}$, the difference of the estimates. If DNWR has no effect on average real wage growth, $\hat{\lambda}$ should be zero:

$$\hat{\lambda} = \hat{E}(\Delta \ln w \,|\, \pi_L, \mu, z) - \hat{E}(\Delta \ln w \,|\, \pi_H, \mu, z) \tag{2.5}$$

I estimate the expected average log real wage change using the predictions from the SUR and UQR from Section 2.3. For the calculations, I use the fact that the mean of a random variable may be expressed as a simple average of its percentiles.

As for the SUR, I conduct the regression for 99 percentiles. I then use the results to simulate 99 percentiles of the real wage change distribution for a given inflation rate π for each region. Finally, I calculate means (weighted by region size) for the 99 percentiles $P_\tau \,|\, \pi$.

As for the UQR, I estimate the effect of inflation for the $\tau = 1, 2, \ldots, 99$ percentiles of the real wage change distribution. I then use the results to simulate 99 real wage change distributions for a given inflation rate π. Finally, I use the τth simulated distribution to calculate the τth percentile $P_\tau \,|\, \pi$.

I apply these procedures for the SUR and the UQR for low inflation π_L as well as for high inflation π_H, and then calculate $\hat{\lambda}$ using the predicted percentiles P_τ, for $\tau = 1, 2, \ldots, 99$. Hence, $\hat{\lambda} \approx \left(\sum_{\tau=1}^{99} P_\tau \,|\, \pi_L - \sum_{\tau=1}^{99} P_\tau \,|\, \pi_H \right) / 99$.

I use a value for π_L equal to 1 percent and a value for π_H equal to 6 percent.[22] Because I estimate $\hat{\lambda}$ using a difference in inflation of five percentage points, I can interpret $\hat{\lambda}/5$ as the average change in average real wage growth caused by a decrease in inflation by one percentage point. According to the results shown in Table 2.5, a decrease in inflation by one percentage point causes an average increase of real wage growth between 0.013 and 0.060 percent. The results show stronger effects on average real wage growth than Elsby's (2009) results: for the USA, a decrease in inflation by one percentage point causes an average increase of real wage growth in the range of 0.002 to 0.008 percent and for the UK of 0.001 percent. Still, my results indicate that the effects of DNWR in combination with low inflation on average real wage growth, and hence on aggregate real wages, are quite small.

22 These inflation rates lie in the range of observed inflation rates during the sample period, see Table B.2 of Appendix B.1.

Table 2.5: Increase of the average real wage growth due to a decrease in inflation

Regression method	Average log real wage change caused by a decrease in inflation by 1 percentage point ($\hat{\lambda}/5$)
SUR	0.013 %
UQR	0.060 %

Unfortunately, a comparison with results from previous studies (e.g. Card and Hyslop, 1997; Knoppik and Beissinger, 2003) is not possible. Those studies use a counterfactual wage change distribution – a distribution that would prevail if DNWR would not bind – to calculate the wage sweep-up.[23] According to my results, the identification of a counterfactual wage change distribution is not possible because the whole distribution is affected by DNWR. Hence, I cannot ascertain by how much previous studies overestimate the effect of DNWR on average real wage change. However, I certainly know that they do overestimate it.

To get an insight into the effects of inflation on the amount of real wage cuts and increases, I estimate $E(\Delta \ln w \mid \pi)$ for negative and positive real wage changes (see Table 2.6).[24] The results confirm that with low inflation a compression of wage increases takes place – the expected real wage increases during low inflation are smaller than the expected real wage increases during high inflation. With rising inflation the expected real wage increases get larger, but less people experience a real wage increase. For example, the results for the UQR show that for low inflation 59 percent of the workers experience a real wage increase, while for high inflation only 55 percent experience a real wage increase. However, in the latter case, the wage increase is more pronounced. In contrast, for low inflation only 40 percent of the workers experience real wage cuts, while for high inflation 44 percent experience real wage cuts. It is in this sense that inflation greases the wheels of the labor market in the presence of DNWR.

23 Knoppik and Beissinger (2003) using the IABS from the Institute for Employment Research, a 1 percent random sample drawn from the German social-security accounts for the years 1975–1995, estimate at zero inflation a sweep-up range from 0.3 to 0.4 additional percentage points of individual expected real wage growth due to wage rigidity. Corneließen and Hübler (2008), using the German Socio-Economic Panel (GSOEP) for the years 1984–2004, estimate that downward wage rigidity increases real wage growth by 3.4 to 4.9 percentage points.

24 Specifically, I estimate

$$E(\Delta \ln w \mid \pi_L, \Delta \ln w < 0) = \left(\sum_{\tau=1}^{n} P_\tau \mid \pi_L\right)/n,$$

$$E(\Delta \ln w \mid \pi_L, \Delta \ln w \geq 0) = \left(\sum_{\tau=n+1}^{99} P_\tau \mid \pi_L\right)/(99-n) \text{ and}$$

$$E(\Delta \ln w \mid \pi_H, \Delta \ln w < 0) = \left(\sum_{\tau=1}^{m} P_\tau \mid \pi_H\right)/m,$$

$$E(\Delta \ln w \mid \pi_H, \Delta \ln w \geq 0) = \left(\sum_{\tau=m+1}^{99} P_\tau \mid \pi_H\right)/(99-m), \text{ respectively.}$$

Table 2.6: Conditional expected real wage change for negative and positive real wage changes

Regression method	$\pi = \pi_L$		$\pi = \pi_H$	
	$E(\Delta \ln w \mid \Delta \ln w < 0)$	$E(\Delta \ln w \mid \Delta \ln w \geq 0)$	$E(\Delta \ln w \mid \Delta \ln w < 0)$	$E(\Delta \ln w \mid \Delta \ln w \geq 0)$
SUR	−3.440 % (33)	4.224 % (66)	−3.208 % (38)	4.599 % (61)
UQR	−5.120 % (40)	8.651 % (59)	−5.036 % (44)	9.048 % (55)

Notes: The numbers in brackets show how many percentiles are considered calculating the expected value.

I also estimate $E(\Delta \ln w \mid \pi)$ for negative and positive nominal wage changes (see Table 2.7).[25] The results show that, as expected, with high inflation one observes less nominal wage cuts. For example, the results for the UQR show that of the workers experience a nominal wage cut when inflation is low, while with high inflation only experience a nominal wage cut.

Table 2.7: Conditional expected real wage change for negative and positive nominal wage changes

Regression method	$\pi = \pi_L$		$\pi = \pi_H$	
	$E(\Delta \ln w \mid \Delta \ln w < -\pi)$	$E(\Delta \ln w \mid \Delta \ln w \geq -\pi)$	$E(\Delta \ln w \mid \Delta \ln w < -\pi)$	$E(\Delta \ln w \mid \Delta \ln w \geq -\pi)$
SUR	−4.723 % (23)	3.604 % (76)	−9.154 % (6)	2.296 % (93)
UQR	−6.111 % (33)	7.686 % (66)	−11.105 % (13)	4.889 % (86)

Notes: The numbers in brackets show how many percentiles are considered calculating the expected value.

2.5 Conclusions

The evidence presented in this chapter indicates that in times of low inflation, DNWR not only hinders wage cuts but also leads to a compression of wage increases. If the latter effect is taken into account, DNWR has a negligible effect on average real wage growth and hence on aggregate real wages.

The empirical analysis has been undertaken for West Germany for the 1975–2007 period using the BeH, the Employee History File of the IAB. In line with the literature, my analysis has been confined to "job stayers", i.e., full-time employees who continually exercise the same job at the same employer for at least two consecutive years. After the data selection, I was still able to analyze about 169 million earnings changes, i.e., an average of more than 5.2 million earnings changes per year. The huge

25 Specifically, I estimate
$E(\Delta \ln w \mid \pi_L, \Delta \ln w < -\pi_L) = \left(\sum_{\tau=1}^{n} P_\tau \mid \pi_L \right) / n,$
$E(\Delta \ln w \mid \pi_L, \Delta \ln w \geq -\pi_L) = \left(\sum_{\tau=n+1}^{99} P_\tau \mid \pi_L \right) / (99 - n)$ and
$E(\Delta \ln w \mid \pi_H, \Delta \ln w < -\pi_H) = \left(\sum_{\tau=1}^{m} P_\tau \mid \pi_H \right) / m,$
$E(\Delta \ln w \mid \pi_H, \Delta \ln w \geq -\pi_H) = \left(\sum_{\tau=m+1}^{99} P_\tau \mid \pi_H \right) / (99 - m)$, respectively.

sample size and the reliable earnings data are great advantages for the analysis of the impact of DNWR on the shape of the real wage change distribution.

Applying SUR to the percentiles of the log real wage change distribution at the regional level, I have shown that in Germany a compression of wage increases takes place due to DNWR – wage increases are compressed when inflation is low. Because the SUR approach does not consider the variance and cross-variable covariance of the microdata, I have also applied UQR. This allows me to estimate the impact of changing the distribution of explanatory variables on the marginal percentiles of the dependent variable. Using UQR, I estimated the impact of inflation on the unconditional percentiles of the real wage change distribution. The results confirm a compression of wage increases due to DNWR. However, compared to the SUR estimates, less percentiles of the wage change distribution are affected. I also checked whether similar results are obtained if inflation forecasts from ARIMA models are used instead of actual inflation, because it could be argued that wage compression should depend on expected future inflation, and not current inflation. Using expected inflation in the UQR it turns out that the compression of wage increases becomes more pronounced than in the UQR estimates with actual inflation. To summarize, my finding of a compression of wage increases in times of low inflation is quite robust to the estimation method and the inflation variable used.

As for the macroeconomic implications of DNWR, I find that a decrease in inflation of one percentage point only causes an average increase of real wage growth between 0.013 and 0.060 percent. These results indicate that DNWR does not provide a strong argument against low inflation targets. However, it must be stressed that this conclusion is based on evaluating different steady state rates of inflation, where the inflation rate is correctly foreseen. It is not argued that this analysis should be used e.g. to calculate the possible costs of downward nominal wage rigidity in southern European countries with high nominal cost level in the current crisis of the Eurozone. These countries have ended up in a situation which they did not foresee. It does not seem to be the case that firms have anticipated binding downward nominal wage rigidity and that this has had a mitigating effect on the nominal wage increases in these countries. Thus it cannot be concluded from my analysis that a higher temporary inflation target in the European Monetary Union would not be helpful for countries with a relatively high cost level in the current situation.

3 Downward Nominal Wage Rigidity in a Cross Section: An Analysis of Linked Employer-Employee Data for the Years 1995 to 2007

As outlined in the last two chapters, concerns about negative employment effects of low inflation have given rise to many studies on the extent of downward nominal wage rigidity (DNWR). These concerns are based on Tobin's (1972) hypothesis that if nominal wages are downwardly rigid, then a certain amount of positive inflation could be necessary to ease the firms' real wage adjustments in response to idiosyncratic shocks. Looking at microeconometric evidence, Tobin's (1972) concern appears to be justified: the empirical evidence overwhelmingly points to a high degree of DNWR (see, e.g., the multicountry studies from Dickens et al., 2007a; Knoppik and Beissinger, 2009; Behr and Pötter, 2010). However, the resulting macroeconomic effects on aggregate real wages and employment appear to be surprisingly weak. This contradiction in the empirical evidence leads Lebow et al. (1999) to speak of a "micro-macro puzzle." However, recent studies – presented in the last chapter – show that pronounced wage rigidity on the individual level can be consistent with weak macroeconomic effects. These studies show that in the presence of DNWR and low inflation, not only are wage cuts compressed but – due to the forward-looking behavior of firms – also wage increases. Because of the compression of the wage increases, the average real wage growth is hardly affected by DNWR. The results indicate that DNWR does not provide a strong argument against the low inflation targets of central banks.

However, even if the macroeconomic effects of DNWR are negligible, one should look closely at the workers who are affected by DNWR. If wage changes are unevenly distributed across workers, a microeconomic analysis could reveal effects of nominal wage rigidity where a macroeconomic analysis cannot. So far, several studies show that certain types of workers experience nominal wage freezes more often, while other types of workers experience nominal wage cuts (see, e.g., Kahn, 1997; Beissinger and Knoppik, 2001; Anspal and Järve, 2011) and that the firm characteristics play a crucial role in DNWR (see, e.g., Babecký et al., 2010). However, there is no empirical evidence showing whether workers in the upper part of the wage change distribution are affected differently by DNWR and whether this effect is conditional on the worker's characteristics, the firm characteristics and/or the position of the worker in the wage change distribution. It could be, for example, that certain types of workers are "discriminated" against due to DNWR – they could not only be more affected by nominal wage cuts, but they could also experience stronger compressions of wage increases. If DNWR affects workers differently over the wage change distribution, conditional on their individual characteristics and/or on the characteristics of their workplace, this result should be considered in any forthcoming theoretical and empirical research on the microeconomic consequences of DNWR.

For the empirical analysis, I apply unconditional quantile regressions on a linked employer-employee dataset to provide an in-depth empirical analysis on how DNWR affects different worker types conditional on their position in the wage change distribution. However, analyzing the extent of DNWR or the macroeconomic effects of DNWR is beyond the scope of this chapter.

The remainder of the chapter is structured as follows. In the next section, I describe the research design and the data. In Section 3.2 I present and discuss the results, while Section 3.3 summarizes and concludes the chapter.

3.1 Methodology, Data, and Data Selection

To analyze whether the wage changes over the wage change distribution are affected if DNWR binds, I follow the approach of Elsby (2009). He considers the percentiles of the real wage change distribution. In the absence of DNWR, a change in the inflation rate should leave the real wage change distribution unaltered. In contrast, if DNWR exists, a systematic relationship between the changes in the inflation rate and the changes in the shape of the real wage change distribution should be observed. For a detailed introduction of Elsby's (2009) model see Section 2.1.

For the empirical testing I apply the unconditional quantile regression (UQR, or RIF-OLS) introduced by Firpo et al. (2009). I estimate the effect of inflation and of further controls on the percentiles of the recentered influence function of the individual log real wage change. Applying this regression, I estimate the impact of the inflation rate on the unconditional percentiles of the real wage change distribution.[1] A standard quantile regression (see, e.g., Koenker and Bassett, 1978; Koenker, 2005) would only observe the effects of inflation on the conditional percentiles of the real wage change distribution. However, wage changes that correspond to a particular conditional percentile can be distributed over the entire observed (unconditional) wage change distribution. A brief introduction of the UQR is provided in Section 2.3.2.

The empirical analysis is undertaken for Germany over the 1995 to 2007 period[2] using the linked employer-employee dataset (LIAB) from the Institute for Employment Research (IAB). The LIAB is created by matching the data from the

1 In the framework of the UQR, the "unconditional percentiles" are the percentiles of the marginal distribution of the outcome variable.

2 East Germany is included from 1996/97 onwards.

IAB Establishment Panel and the data from the Employee History File (BeH), and it includes all workers who were employed in one of the firms included in the Establishment Panel as of July 1 for the data year. The Establishment Panel is an annual survey of establishments in Germany that represents all industries and establishment sizes nationwide. The BeH comprises the total population that is gainfully employed and covered by the social security system. Those not covered are self-employed persons, family workers assisting in the operation of a family business, civil servants (Beamte) and regular students.[3]

Advantages of the LIAB are its huge sample size and its reliable earnings data. One disadvantage of the data is that it does not allow fringe benefits to be separated from "regular" earnings. In addition, the BeH contains no data on the hours worked except for information about part-time or full-time employment. Therefore, I calculate gross average daily earnings. To avoid any contamination with effects from working time, I only observe full-time blue-collar and white-collar workers, aged 16 to 65 years (subject to social security without particular tokens).[4] Unfortunately, the wage data are right-censored at the contribution assessment ceiling (Beitragsbemessungsgrenze). For workers whose wages are censored, the wage change cannot be computed. Therefore, I analyze only the non-censored wage spells.[5]

Consistent with the literature, the analysis is confined to "job stayers." I define job stayers as workers who continually execute the same job at the same employer for at least two consecutive years.[6] Including job movers in the analysis could lead to a systematic relationship between inflation and the compression of the wage change distribution that is unconnected with DNWR. The reason for this relationship is that inflation often rises during economic upswings and, simultaneously, more voluntary job changes occur that go hand in hand with real wage increases (see, e.g., Cornelißen et al., 2007).

After the selection, the dataset contains more than 10.7 million wage changes from nearly 3.1 million workers who work in a total of 20,596 firms. The control variables that are used in the regressions are displayed in Tables 3.1 and 3.2.

As an individual control variables, I use, inter alia, the gender and the wage level of the worker. Controlling for sex is important for two reasons: first, it has

3 A general introduction to the IAB Establishment Panel is provided by Kölling (2000); more detailed information is provided by Fischer et al. (2009). A general introduction to the LIAB is provided by Alda et al. (2005).

4 The BeH contains eight classes of workers. I drop all classes except "white-collar workers," "unskilled workers" and "skilled workers." The two latter classes are combined to form the class "blue-collar workers."

5 This leads to an underrepresentation of highly qualified (white collar) workers, making the results somewhat less generalizable. See Appendix C.1 for more information on the contribution assessment ceiling and data selection.

6 The breakdown of occupations is very detailed, but still, not every job change leads to a change in the occupation classification. Therefore, some spells for persons who changed their job within a firm may not be excluded. The "same position" restriction has also been applied by Christofides and Stengos (2001) and in Chapter 2.

been shown that the average nominal wage increase for women in Germany is higher and that female workers are less frequently affected by nominal wage cuts (see, e.g., Pfeiffer, 2003, Table 2, p. 624). Second, controlling for the sex allows me to control for the fact that shifts from part-time to full-time employment (and vice versa) are more common for female workers (see, e.g., Schäfer and Vogel, 2005).[7] Kahn (1997) shows that minimum wage workers in the USA are more often affected by zero nominal wage changes and less often affected by negative nominal pay changes than other workers. Therefore, I control for the wage level of the workers using ten dummy variables.

Table 3.1: Summary statistics for worker spells

	Mean	Std. Err.	Min.	Max.
Individual (micro) data				
Change in log real wage	0.01	0.05	–0.19	0.22
Change in log nominal wage	0.03	0.05	–0.18	0.24
Age	41.99	9.65	17	65
Female (yes = 1)	0.32	0.47	0	1
Non-German (yes = 1)	0.08	0.27	0	1
White-collar workers (yes = 1)	0.45	0.50	0	1
Tenure (days worked in firm)	4,275	2,610	730	11,869
Education:				
Lower secondary school and intermediate (secondary) school without vocational qualification	0.15	0.36	0	1
Lower secondary school and intermediate (secondary) school with vocational qualification	0.71	0.46	0	1
Upper secondary school examination without vocational qualification	0.01	0.08	0	1
Upper secondary school examination with vocational qualification	0.04	0.20	0	1
Post-secondary technical college degree	0.03	0.17	0	1
University degree	0.03	0.17	0	1
No formal education and no classification applicable	0.03	0.18	0	1

7 Shifts from part-time to full-time work and vice versa that occur during the course of the year do not lead to a new report for the employer. A new status is conveyed with the annual report at the end of a year – this status applies for the whole year. However, because I only observe wage changes for full-time workers, the observed wage change can only be overestimated – due to changes from part-time to full-time employment – but the wage change cannot be underestimated.

	Mean	Std. Err.	Min.	Max.
Establishment data				
Work council (yes = 1)	0.95	0.21	0	1
Wages paid above standard rate (yes=1)	0.48	0.50	0	1
Establishment size	4,239	8,321	1	51,155
Union variable:				
Collective agreement (agreements at industry level)	0.85	0.36	0	1
In-house rate (agreements at the firm level)	0.13	0.34	0	1
No collective agreement	0.02	0.15	0	1

Note: Pooled data from 13 years (1995/1996 to 2006/2007). Number of observations = 10,733,205. The dataset also contains dummies for 6 occupation fields and 10 dummies for wage levels.

Table 3.2: Summary statistics for regional (macro) variables

	Obs.	Mean	Std. Err.	Min.	Max.
Inflation (log change in consumer price index)	13	0.015	0.004	0.006	0.022
Absolute change in the rate of inflation	13	0.005	0.003	0.001	0.010
Regional productivity growth (average regional real wage growth)	197	0.012	0.012	–0.027	0.046
Regional unemployment rate	197	0.133	0.048	0.055	0.221
Regional lagged unemployment rate	197	0.133	0.047	0.055	0.221

Note: Pooled data from 13 years (1995/1996 to 2006/2007). The dataset also contains dummies for the 16 German federal states.

The use of the LIAB also allows me to control for institutional characteristics. Because the labor unions and other forms of worker participation still have a large influence on wage setting in Germany, they could influence the wage changes of workers. Therefore, I control for whether a work council is present in a firm, whether the firm pays wages according to an agreement at the industry or the firm level, and whether a firm pays wages above the standard rate.[8]

For the inflation rate, I use the log change in the consumer price index (CPI, see Table B.2 in Appendix B.1). Following Elsby (2009), productivity growth is measured by the observed average regional real wage change rate. Productivity is not directly measured because the real wages adjust to changes in productivity with a time lag.[9] The absolute change in the rate of inflation is included because Groshen and Schweitzer (1999) hypothesized that higher inflation volatility leads

8 In 1999, the question of the LIAB questionnaire on union agreements was changed slightly. The category "firm-level collective agreement" was replaced by "firm-level collective agreement underwritten by a union." I did ignore this modification because Dustmann et al. (2007, p. 45) found that "[…] its impact is almost invisible on time series plots of the evolution of union recognition."

9 Alternatively, one could model some type of error-correction mechanism for the discrepancy between real wage change and productivity growth. I avoid these complications by using the average regional real wage change rate as a proxy variable reflecting the impact of (regional) productivity growth on wages. It is a suitable proxy because, according to the theoretical predictions of Elsby (2009), DNWR should have no effect on the average wage change.

to greater dispersion in relative wages regardless of the existence of DNWR. The current and lagged regional unemployment rates are included because DNWR can affect unemployment. The unemployment rates are used to control for changes in the wage change distribution due to workers "leaving" the distribution.

3.2 Empirical Implementation, Results, and Discussion

To analyze whether workers' wage changes are affected by changes in the inflation rate, conditional on their position in the wage change distribution and their individual and/or firm characteristics, I estimate a UQR that has several variables interacted with inflation. I regress the percentile-transformed individual log real wage change – the recentered influence function $\left(\widehat{RIF}\right)$ – of the individual log real wage change (Δw) against $\mathbf{X} = (\pi_t \;\; \mathbf{a}'_{irt} \;\; \mathbf{b}'_{irt} \;\; \mathbf{c}'_{irt})$.

π_t is the inflation rate of year t, and $\mathbf{a}$ and $\mathbf{b}$ are vectors containing further control variables on the individual level i or on the regional level r (see Tables 3.1 and 3.2). Vector $\mathbf{c}$ contains the same six control variables as vector $\mathbf{b}$, but they are interacted with the inflation rate. The six variables contained in vectors $\mathbf{b}$ and $\mathbf{c}$ are dummies for white-collar worker, female, work council, wages paid above standard rate, and the two union variables (collective agreement and in-house rate).[10]

Because I want to focus on whether the effect of inflation on the real wage change varies for workers and whether the effect depends on the position of the worker in the wage change distribution, I only display the coefficients for the inflation rate, the coefficients of the variables contained in vector $\mathbf{b}$ and the corresponding coefficients of the interaction terms contained in vector $\mathbf{c}$ (see Table 3.3).[11]

The coefficients for the inflation rate (see Table 3.3) can be interpreted as the marginal effect of inflation on the real wage change for the reference worker: a male blue-collar worker who is employed by a firm without a work council that is not paying according to a collective agreement and that is not paying wages above a collective agreement.[12]

10 Appendix C.2 shows that the LIAB appears to be suitable for the analysis. Furthermore, it shows that a decrease in the inflation rate leads to a compression of real wage increases – confirming the findings of Elsby (2009) and Chapter 2.

11 Looking at the coefficients of the variables that are not interacted with the inflation rate – presented in Table 3.3 – one can see that not only individual characteristics but also institutional characteristics have an influence on the real wage change of workers: nearly all coefficients for the variables are highly significantly different from zero but small in magnitude.

12 For this reference worker, the effect of the inflation rate perfectly fits the predictions of Elsby's (2009) model (see Appendix C.2).

Table 3.3: The marginal effects of the inflation rate and selected variables on the percentiles of the real wage change

Percentiles	10	20	30	40	50	60	70	80	90
Inflation (π)	0.3905***	−0.1798***	−0.3972***	0.1539***	0.3995***	0.4927***	0.7725***	1.2755***	2.1112***
	(0.0651)	(0.0449)	(0.0273)	(0.0251)	(0.0278)	(0.0219)	(0.0297)	(0.0452)	(0.0566)
Productivity growth	1.1421***	0.7887***	0.6132***	0.6453***	0.8225***	0.8145***	0.8741***	1.0779***	1.4255***
	(0.0054)	(0.0029)	(0.0016)	(0.0016)	(0.0017)	(0.0023)	(0.0020)	(0.0034)	(0.0051)
White-collar worker	−0.0057***	0.0021***	0.0053***	0.0058***	0.0052***	0.0048***	0.0029***	0.0014***	0.0009**
	(0.0003)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0004)
White-collar worker * π	0.5216***	0.0503***	0.0053***	−0.4549***	−0.5058***	−0.5837***	−0.5549***	−0.6460***	−0.9131***
	(0.0159)	(0.0117)	(0.0076)	(0.0058)	(0.0061)	(0.0084)	(0.0113)	(0.0152)	(0.0230)
Female	0.0103***	0.0070***	0.0061***	0.0070***	0.0083***	0.0089***	0.0087***	0.0069***	0.0076***
	(0.0003)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0003)
Female * π	0.1231***	0.1256***	0.0062	−0.1370***	−0.2275***	−0.2925***	−0.2646***	−0.0512***	0.0399**
	(0.0212)	(0.0106)	(0.0071)	(0.0058)	(0.0069)	(0.0077)	(0.0094)	(0.0131)	(0.0193)
Work council	0.0151***	0.0108***	0.0062***	0.0141***	0.0154***	0.0153***	0.0133***	0.0090***	0.0038***
	(0.0007)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0005)
Work council * π	−0.6039***	−0.2593***	−0.2275***	−0.5370***	−0.6857***	−0.7854***	−0.7462***	−0.6039***	−0.4759***
	(0.0420)	(0.0279)	(0.0209)	(0.0170)	(0.0164)	(0.0187)	(0.0219)	(0.0262)	(0.0342)
Collective agreement[1]	0.0062***	0.0046***	0.0045***	0.0069***	0.0056***	0.0036***	0.0013***	−0.0036***	−0.0084***
	(0.0008)	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0005)	(0.0006)	(0.0010)
Collective agreement * π	−0.0377	−0.0127	−0.0530**	−0.2737***	−0.2391***	−0.1950***	−0.1202***	0.0624	0.2442***
	(0.0496)	(0.0342)	(0.0244)	(0.0210)	(0.0222)	(0.0207)	(0.0300)	(0.0399)	(0.0566)
In-house agreement[1]	0.0169***	0.0058***	0.0027***	0.0031***	0.0012**	−0.0011***	−0.0004	0.0016**	0.0016
	(0.0009)	(0.0006)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.0005)	(0.0006)	(0.0011)
In-house agreement * π	−1.3372***	−0.4295***	−0.1287***	−0.1229***	0.0190	0.1467***	0.0799***	−0.0574	0.0031
	(0.0580)	(0.0410)	(0.0263)	(0.0213)	(0.0231)	(0.0224)	(0.0311)	(0.0422)	(0.0660)
Wages above std. rates	−0.0125***	−0.0110***	−0.0110***	−0.0098***	−0.0095***	−0.0091***	−0.0039***	0.0042***	0.0117***
	(0.0003)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0003)
Wages above std. rates * π	0.5127***	0.5369***	0.6760***	0.7203***	0.7464***	0.7927***	0.5236***	0.0835***	−0.4413***
	(0.0170)	(0.0101)	(0.0075)	(0.0058)	(0.0075)	(0.0076)	(0.0099)	(0.0131)	(0.0198)

[1] Reference category: no collective agreement.

Notes: Unconditional quantile regression. Bootstrapped standard errors (50 replications) are in brackets. The further controls used are as follows: 16 regions, age, age^2, absolute change in inflation, current and lagged regional unemployment rate, 8 educational classes, worker with foreign nationality, 6 occupational fields, firm size, (firm size)^2, (firm size)^3, (firm size)^4, West Germany, tenure, tenure^2, 10 wage levels. Gray colored columns indicate the range of percentiles where zero nominal wage changes are observed in the data. *** $p<0.001$; ** $p<0.05$.

If a coefficient for the inflation rate is positive, a decrease in the inflation rate is associated with a decrease of the real wage change; a negative coefficient is associated with an increase of the real wage change. Depending on the position in the wage change distribution, this association has different effects. The distribution can be divided into three segments: the lower part of the distribution where the nominal wage cuts are observed (the 10*th* percentile), the range of the distribution where zero nominal wage changes are observed (the 20*th* and 30*th* percentile), and the upper part of the distribution where nominal wage increases are observed (≥ 40*th* percentile).[13]

For the reference worker in the lower part of the distribution, a decrease in the inflation rate leads to more pronounced real wage cuts brought about by nominal wage cuts. This leads to a decompression of the distribution on the left hand side of the distribution. The compression on the left hand side of the wage change distribution occurs in the range where zero nominal wage changes are observed because DNWR leads to an increase of zero nominal wage changes. For the reference workers in the range of the distribution where zero nominal wage changes are observed, a decrease in the inflation rate is associated with an increase of the real wage change. It cannot be determined whether they experience a more pronounced real wage increase or a less pronounced real wage cut. For the reference workers in the upper part of the distribution, a decrease in the inflation rate will lead to a less pronounced real wage increase and hence to a compression of the distribution on the right hand side. This method of interpreting the coefficients for the inflation rate, and hence for the reference worker, also holds for the interpretation of the coefficient of the variables that are interacted with the inflation rate.

To observe how strongly the inflation rate affects the real wage change of workers, and to observe how this effect varies between workers, I calculate the marginal effect of inflation on the real wage change. The marginal effect is the sum of the coefficient of the inflation rate (η) and the 6 coefficients (λ_i) of the variables interacted with the inflation rate multiplied with the corresponding variable (b_i): $\eta + \Sigma_i \lambda_i b_i$.

Because two of the dummy variables interacted with the inflation rate are exclusive (in the sense that workers cannot get paid according to a collective agreement and an in-house agreement simultaneously), I can calculate the marginal effect of the inflation rate on real wage change for 48 worker types. To get a sense for how significantly the effects over the wage change distribution vary between worker types, I show summary statistics in Table 3.4.

13 See Appendix C.2.

Table 3.4: Summary statistics for the marginal effects of the inflation rate on the percentiles of the real wage change for 48 different settings of individual and firm characteristics

Percentiles	10	20	30	40	50	60	70	80	90
Min.	−1.551	−0.869	−0.753	−1.249	−1.259	−1.364	−0.913	−0.083	0.281
Max.	1.548	0.533	0.290	0.874	1.165	1.432	1.376	1.421	2.395
Mean	0.209	−0.100	−0.228	−0.183	−0.010	0.042	0.238	0.668	1.298
Range	3.098	1.402	1.044	2.123	2.424	2.796	2.289	1.504	2.114

Notes: The marginal effect of the inflation rate on the real wage change calculated for all 48 possible worker types. Gray colored columns indicate the range of percentiles where zero nominal wage changes are observed in the data. Unweighted mean.

The summary statistics show (see Table 3.4) that the real wage changes of workers are not equally affected by inflation. The marginal effect varies within and between the different percentiles of the real wage change distribution. Over the entire real wage change distribution, there are some workers whose real wage decreases with a decrease in the inflation rate, and vice versa. Other workers experience an increase of the real wage change with a decrease in the inflation rate, and vice versa. The range shows that the marginal effect of inflation on the wage change between the workers differs significantly for the 10*th* percentile of the real wage change distribution in particular – where the nominal wage cuts are observed. The ranges for the 20*th* and 30*th* percentiles – where zero nominal wage changes are observed – are small when compared to the ranges of the other percentiles.[14]

Table 3.5: Summary statistics for the coefficient of the variables interacted with inflation

	Min.	Max.	Mean
Individual characteristics			
White-collar worker	−0.913	0.522	−0.342
Female	−0.293	0.126	−0.075
Firm characteristics			
Work council	−0.785	−0.228	−0.547
Wages above std. rates	−0.441	0.793	0.461
Union variable (reference category: no collective agreement)			
Collective agreement (at the industry level)	−0.274	0.244	−0.069
In-house rate (collective agreement at the firm level)	−1.337	0.147	−0.203

Notes: Calculations based on the calculated coefficients for the 10th, 20th, ..., 90th percentile. Unweighted mean.

14 While interpreting the summary statistics, one should keep in mind that the presented mean values are unweighted – every worker type has the same weight regardless of how many workers it actually represents. Table 3.1, however, shows that, e.g., nearly 85 percent of the workers are employed by firms that pay according to a collective agreement.

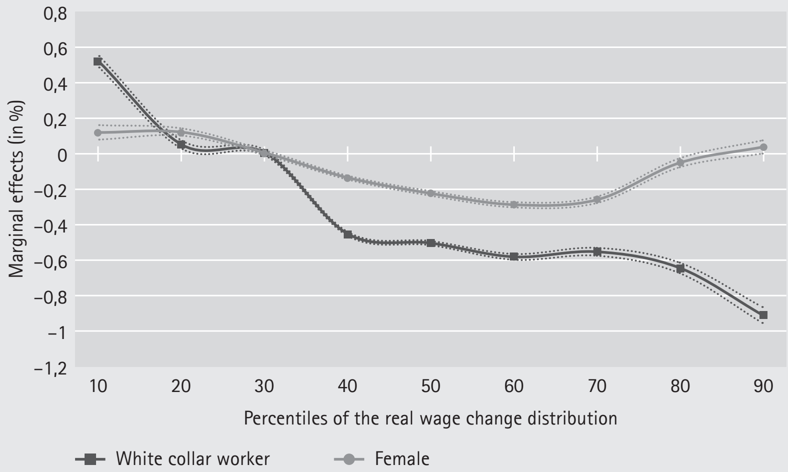
To observe which characteristics really influence the real wage change of workers when the inflation rate changes, I focus on the coefficients of the variables of vector **c**, which are interacted with the inflation rate. A look at Table 3.3 shows that the coefficients for these variables vary within and between the different percentiles of the real wage change distribution. To provide an overview, Table 3.5 shows some summary statistics for the coefficients of the variables. While interpreting the coefficients, one should always keep the general effect of the inflation rate on the real wage change – the marginal effect of inflation on the real wage change for the reference worker – in mind (see Table 3.3 and Figure 3.1). Because the marginal effects are linear, they add up. Hence, the marginal effect of inflation on the real wage change for a worker who is identical to the reference worker except that he is a white-collar worker is the sum of the marginal effect of the inflation rate and the marginal effect of the white-collar dummy interacted with inflation.

Figure 3.1: The marginal effects of inflation on the real wage change for the reference worker

Note: Reference worker: a male blue-collar worker who is employed by a firm without a work council that is not paying according to a collective agreement and that is not paying wages above a collective agreement. The dotted lines represent the 95 % confidence interval.

First, I take a closer look at the coefficients of the individual characteristics interacted with the inflation rate. The effect of a change in the inflation rate on the real wage change conditional on the class of the worker – in terms of white-collar worker or blue-collar worker – is especially strong for the very low and the very high percentiles of the wage change distribution (see Table 3.3 and Figure 3.2). For the 10th percentile – where workers experience nominal wage cuts – the coefficient is strongly positive: if the inflation rate decreases, the white-collar workers experience higher real wage cuts than the blue-collar workers. For the 20*th* and 30*th* percentiles – the range where the zero nominal wage changes are observed – the effect of a change in inflation barely differs between the white-collar worker and the blue-collar worker. Above the 30*th* percentile of the real wage change distribution, the coefficients are negative: a decrease in the inflation rate is associated with higher wage increases for white-collar workers.

Figure 3.2: The marginal effect of inflation on the real wage change for white-collar workers and female workers

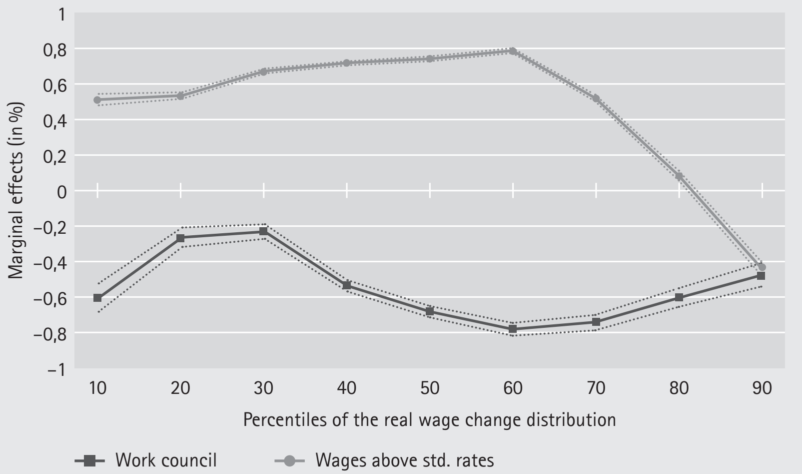


Note: Dotted lines represent the 95 % confidence interval.

The effects of a change in the inflation rate on the real wage change conditional on the sex is tiny when compared to the effects of a change in the inflation rate on the real wage change conditional on the class of the worker. For the 10*th*, 20*th* and 90*th* percentiles, the coefficients for the interaction term are positive: a decrease in the inflation rate is associated with lower real wage changes. For the women in the 10*th* percentile and in the 20*th* percentile of the real wage change distribution, this real wage cut goes hand in hand with a nominal wage cut. Hence, women experience higher wage cuts. This finding is consistent with Anspal and Järve's (2011) findings for Estonia: using Kahn's (1997) histogram-location approach, they find that women resist pay cuts less than men. For the 40*th* to the 80*th* percentile, the coefficients are negative: a decrease in the inflation rate is associated with higher real wage changes.

Institutional characteristics also have an influence on the real wage change of workers when the inflation rate changes (see Table 3.3 and Figures 3.3 and 3.4). This influence is particularly strong for the existence a work council and whether a firm pays wages above standard rates. For the workers of firms that pay wages above the standard rates, all of the coefficients of the interaction term are positive except for the 90*th* percentile. However, for the workers of firms with a work council, all of the coefficients of the interaction term are negative. Therefore, a decrease in the inflation rate is associated with a lower wage change for the workers who are paid above the standard rate, while the workers at firms with a work council experience an inverse effect: a decrease in the inflation rate is associated with higher wage changes.

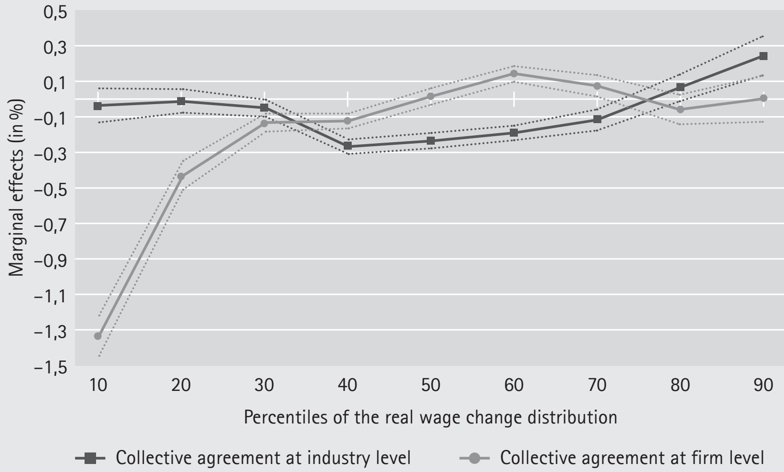
Figure 3.3: The marginal effect of inflation on the real wage change for workers at firms with a work council and workers at firms that pay wages above the standard rates



Note: Dotted lines represent the 95 % confidence interval.

Surprisingly, labor unions do not appear to have a strong influence on how a change in the inflation rate influences the real wage change – the base category for both variables is "no collective agreement." For workers who are paid according to a collective agreement at the industry level, the coefficients for the 30*th* to the 70*th* percentile of the interaction term are significantly negative, while only the coefficient for the 90*th* percentile is significantly positive. For the workers who are paid according to an in-house rate (collective agreement at the firm level), the coefficients of the interaction term are significantly negative for the 10*th* to the 40*th* percentile and significantly positive for the 60*th* to the 70*th* percentile. Aside from the fact that the coefficients for the labor unions are not statistically significant for quite a few of the percentiles, the coefficients that are significantly different from zero are fairly small when compared to the coefficients of the other institutional characteristics. The only exception is the coefficient for the 10*th* percentile for workers of firms that pay according to an in-house rate.

Figure 3.4: The marginal effect of inflation on the real wage change for workers of firms that pay according to a collective agreement (at the industry level) or that pay according to an in-house rate (collective agreement at the firm level)



Note: Dotted lines represent the 95 % confidence interval.

3.3 Conclusions

Applying UQR, I estimated the impact of inflation on the unconditional percentiles of the real wage change distribution. The empirical analysis has been undertaken for Germany over the 1995 to 2007 period using the LIAB of the IAB. The analysis has been confined to "job stayers," i.e., full-time workers who continually exercise the same job at the same employer for at least two consecutive years. After data selection, nearly 11 million earnings changes are analyzed.

Using interacting dummies for the individual and the firm characteristics with the inflation rate, I show that the effect of the inflation rate on the workers' real wage changes differs not only *between* but also *within* the percentiles of the wage change distribution. The effect is conditional on the workers' position in the wage change distribution, and it is conditional on the workers' individual characteristics and on the firm characteristics; the conditional effects also differ over the wage change distribution. In particular, the class of the workers (in terms of white- and blue-collar workers), whether an employee pays wages above the standard rates, and/or whether a work council exists in the firm, have a strong influence on how a change in the inflation rate affects the real wage change of the worker.

The results show that some workers are "discriminated" against by DNWR: previous results are confirmed, e.g., that women resist pay cuts less than men (see, e.g., Anspal and Järve, 2011), and new insights are gained, e.g., that blue-collar workers in particular are affected by the compression of wage increases.

Given the results of this section, future research on the microeconomic consequences of DNWR should consider that DNWR affects not only the lower tail of the wage change distribution but also the upper part of the wage change distribution. Furthermore, research should consider that the effect of inflation on the workers' real wage change is conditional on the individual characteristics and the firm characteristics and that these effects differ over the wage change distribution. Considering these insights in further research will provide a better picture of the microeconomic effects of DNWR.

Part II
Real Wage Rigidity

4 Real Wage Rigidity over the Business Cycle – Previous Research and Recent Developments

While the first part of the book deals with downward nominal wage rigidity, the second part concentrates on real wage rigidity. More precisely, it deals with real wage rigidity of newly hired workers over the business cycle. This chapter provides a brief overview on previous research on this topic, while the next chapter analyzes the cyclical behavior of real wages of newly hired workers in Germany.

4.1 Introduction

The recent interest in real wage rigidity has been driven by the ongoing debate on the ability of the canonical Mortensen-Pissarides search and matching model (Mortensen and Pissarides, 1994) to generate realistically large cyclical fluctuations in unemployment (see, e.g., Shimer, 2005; Hall, 2005; Veracierto, 2008).

Shimer (2005, p. 45) 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."[1] This puzzle is usually referred to as the "Shimer-Puzzle". That this puzzle also exists for Germany is shown, e.g., by Gartner et al. (2012). They 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." (Gartner et al., 2012, p. 106)

One way of solving the "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).

4.2 Early Research on Real Wage Rigidity

To estimate whether real wages are rigid over the business cycle a common practice has been to regresses real wages on a business cycle variable, e.g. the

1 The Beveridge curve is a graphical representation of the relationship between unemployment and the job vacancy rate.

unemployment rate. The first approaches used aggregated wage data – the results suggested that wages move procyclically (see, e.g., Dunlop, 1938; Tarshis, 1939).[2] However, this early literature did not reach a definite conclusion on whether wages move procyclical or countercyclical.[3] One reason for the inconclusive results could be the use of aggregated data. Solon et al. (1994) show that using aggregated time series data instead of longitudinal microdata leads to an underestimation of wage cyclicality due to a composition bias in aggregated statistics. Bils (1985, p. 667) argues in more general terms that an aggregation "[...] involves a loss of information and therefore of estimating efficiency." This criticism led to the use of microdata in the analysis of real wage cyclicality.

First microdata studies often analyzed the real wage cyclicality for job movers and job stayers (e.g., Shin, 1994; Devereux and Hart, 2006; Shin and Solon, 2007) and showed that using microdata in the analysis has pitfalls as well (see, e.g., Bils, 1985; Mitchell et al., 1985). Keane et al. (1988) show, e.g., that failure to control for unobserved heterogeneity leads to a countercyclical bias. Three possible kinds of heterogeneities should be taken into account analyzing real wage cyclicality: worker, firm, and job heterogeneity.

Worker heterogeneity

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 to low-skilled workers during expansions than during recessions." (Solon et al., 1994, p. 1)

Firm heterogeneity

Industry compositions change over the business cycle due to the exit and entry of firms.

Job heterogeneity

Job compositions within firms might change over the business cycle due to firing, hiring, and the promotion of incumbent workers. For example, a firm might lower its hiring standards in a boom to increase employment, while holding entry-wages rigid. "Such cyclicality in job assignments could cause the real wages of

2 An early exception is Mehra (1982), who examined the dynamic relationship of real wages and employment at the industry level.

3 Evidence of countercyclical real wages is, e.g., provided by: Canzoneri (1978); Chirinko (1980). Evidence of procyclical real wages is, e.g., provided by: Kuh (1966); Bodkin (1969).

the firm's worker to be procyclical even if wages by job are sticky." (Solon et al., 1997, p. 403).

Papers analyzing the wage cyclicality of job movers and job stayers show that wages of job movers are more procyclical than wages of job stayers (e.g., Bils, 1985; Shin, 1994; Devereux and Hart, 2006; Shin and Solon, 2007). An explanation for this finding is given by Hagedorn and Manowskii (forthcoming, p. 5): "[…] workers can sample from a larger pool of job offers in a boom than in a recession, and workers with lower quality of the current match benefit more from the expansion of the pool of offers in a boom."[4]

4.3 Recent Developments

Pissarides (2009) shows that the decision of opening a vacancy or not is mainly influenced by the real wage of newly hired workers (entrants). Pissarides (2009, p. 1340 f.) 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 […]." This point of view is also shared by Haefke et al. (2012). They show that wages of entrants 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. These new insights caused the recent focus on the real wage rigidity of newly hired workers.

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).

4 Hagedorn and Manowskii (forthcoming, p. 52) include the match quality in regressions (using labor market tightness – measured as the ratio of aggregate stock of vacancies to the unemployment rate – as a proxy) to control for unobserved idiosyncratic productivity. Due to the control for the match quality, "[…] the differences in the volatility of wages between job switchers and job stayers disappears."

However, 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. Therefore, one has to control for so-called "cyclical upgrading" in booms and "cyclical downgrading" in recessions:[5] "Suppose, for example, that a highly skilled machinist takes a job as a low-paid cabdriver in a recession ["cyclical downgrading"] and then is reemployed as a high-paid machinist in a boom ["cyclical upgrading"]." (Gertler and Trigari, 2009, p. 73) 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.

Whether or not introducing wage rigidity into the canonical search and matching model is justified should be subject to an empirical investigation: how rigid are real wages – especially the real wages of newly hired workers – over the business cycle? 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 (cf. Pissarides, 2009). Hence, the empirical assessment of recent theories of the rigidity of entry-wages requires an approach that identifies cyclical variation in hiring wages within employer/employee matches.

So far, to the best of my knowledge, only two studies for Portugal exist that control for "cyclical up- and downgrading" in employer/employee matches: Carneiro et al. (2012) and Martins et al. (2012b). 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 these recent developments the next chapter provides empirical evidence on the real wage cyclicality of newly hired workers in Germany. I analyze how changes in the unemployment rate affect the "typical" real wage of entry jobs and the individual real wages of newly hired workers, respectively. In my regressions I control for "cyclical up- and downgrading" in employer/employee matches through the use of firm-job fixed effects. Furthermore, I argue

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

that the methodologies to estimate the real wage cyclicality of real entry-wages, as applied by Carneiro et al. (2012) and Martins et al. (2012b), probably produce biased estimates. I apply both methodologies to 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.

5 Real Wage Cyclicality of Newly Hired Workers in Germany

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). However, by introducing rigid wages into the model, it is better in fitting fluctuations in unemployment (see, e.g., Shimer, 2005). Especially the real wage rigidity of newly hired workers (entrants) seems to play a crucial role in generating realistically cyclical fluctuations in the unemployment rate, since the decision of opening a vacancy or not is mainly influenced by the real wage of newly hired workers (cf. Pissarides, 2009).

However, there is little empirical evidence on how real wages of newly hired workers react to business cycle conditions. Also, 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 (cf. Gertler and Trigari, 2009).

This chapter provides empirical evidence on the wage cyclicality of newly hired workers in Germany. I use two stage regressions to estimate how changes in the unemployment rate affect the wages of newly hired workers. In the regressions I control for "cyclical up- and downgrading" in employer/employee matches through the use of firm-job fixed effects. For the empirical analysis I 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 – 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 chapter'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.[1] 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

1 So far comparable empirical evidence exists, to the best of my knowledge, only for Portugal (see Martins et al., 2012b; Carneiro et al., 2012).

wages and individuals' real wages, respectively, as the endogenous variable might be biased in different directions. By running separate regressions for both endogenous variables I obtain a range of point estimates that should include the true parameter. 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 chapter 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 5.2, while Section 5.3 presents the statistical models and the empirical results. In Section 5.4 I discuss the results and their implications, while Section 5.5 concludes.

5.1 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 up- and downgrading" in employer/employee matches: Martins et al. (2012b) 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. (2012b) 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. (2012b) 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. (2012b) 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, job title, 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]

5.2 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 be much weaker. Also, the BeH allows a matching of workers with firms, which is crucial to control for "cyclical up- and downgrading" in employer/employee matches, i.e. by controlling for firm-job fixed effects, as outlined in Chapter 4.

5.2.1 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.

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 5.2.1 and a number of data inconsistencies in the first years of the BeH (see Appendix D.1) the analysis can only be run for the 1977 to 2009 period. Data from earlier years is used for identifying newly hired workers.

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 D.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., 2012b). 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 newly hired workers either.

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]

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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.

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. Also, I 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 be 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.

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]
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. (2012b). However, applying these further selection criteria (FSC) hardly affects the regression results. The FSC are outlined in Appendix D.2 and regression results using this dataset are displayed in Appendix D.4.

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

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

11 The contribution assessment ceiling is annually adjusted to the changes in earnings (see Table D.5 in Appendix D.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., Tables B.3 and B.4 of Appendix B.1.

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]

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 5.1.

Table 5.1: 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 newly hired into job *j*. Summary statistics are provided in Table 5.2. 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 D.3 (see Appendix D.3) shows the sample sizes by year for this sub-sample.

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.

Table 5.2: 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 5.3. In Appendix D.3 I provide some further information on the data. Table D.4 provides statistics for the different years, 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 D.5 provides the unemployment and inflation rates.

Table 5.3: 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 D.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.

5.3 Empirical Analysis

5.3.1 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 5.2.1). 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. (2012b) and Carneiro et al. (2012), using both the "typical" real wages of entry jobs and the job entrants individual real wages.

I apply three models to estimate the cyclicality of entry-wages. Table 5.4 provides an overview of these models. They only differ with respect to the first stage regressions, while the second stage regressions are identical.

Table 5.4: 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

Model 1

In model 1 I analyze how "typical" real wages are related to changes in the unemployment rate. I follow Martins et al. (2012b) 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:

$$\ln(w_{jt}) = \alpha_j + \beta_t + \varepsilon_{jt}, \tag{5.1}$$

where w_{jt} denotes the "typical" real wage paid in period t to workers newly hired into job j, e.g. the modal real wage. α_j is a job fixed effect and ε_{jt} is the error term with mean zero representing temporary job-specific departures from the general period effect. To quantify the cyclicality of entry-wages I regress in the second stage the estimated time series of β_t ($\hat{\beta}_t$) on the unemployment rate u_t, controls for secular time trends, and a dummy that is one for 1984 and every following year ($D_{\geq 1984}$):

$$\hat{\beta}_t = \delta u_t + \lambda_0 t + \lambda_1 t^2 + D_{\geq 1984} + \varepsilon_t \tag{5.2}$$

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).

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]

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 5.2.2, 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 5.3) allows, inter alia, to control for this data issue by controlling for the length of the employment spell:

$$\ln(w_{ijt}) = \alpha_j + \beta_t + \gamma'\mathbf{x}_{it} + \varepsilon_{ijt}, \tag{5.3}$$

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 5.3). 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 5.2).

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 5.4) 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 5.2.2). 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

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 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.

time period. Furthermore, I require that these jobs are observed for at least 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]

$$\ln(w_{ijt}) = \alpha_i + \alpha_j + \beta_t + \gamma' \mathrm{x}_{it} + \varepsilon_{ijt}, \tag{5.4}$$

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 5.2).

5.3.2 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 5.5).[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. (2012b): 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. (2012b) 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 Table D.5 of Appendix D.3).[22] Hence a weighting in the second stage regressions

18 Further details on the sub-sample are provided in Section 5.3.2.

19 I use the Stata ado file "a2reg" by Ouazad (2007).

20 Martins et al. (2012b, Figure 3, p. 44) 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 D.1 in Appendix D.5. The differences between "typical" wages and individual workers' wages seems to be stronger for the dataset with FSC (right panel of Figure D.1). This first visual impression is also supported by simple summary statistics (see Table D.11 in Appendix D.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 D.4). Hence, the selection criteria from Martins et al. (2012b) do not seem to influence the outcome of the regressions.

22 In Martins et al. (2012b) 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.

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.

Table 5.5: Model 1 – estimated coefficients of the unemployment rate ($\hat{\delta}$) using "typical" real entry-wages

	Modal wage	Mean wage
(1.0) 1st and 2nd stage unweighted OLS	–1.03*** (0.35)	–0.94*** (0.34)
(1.1) 1st stage unweighted OLS, 2nd stage OLS weighted by number of entry jobs per year	–1.00*** (0.34)	–0.92*** (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 ≥ 1984.

The results of model 2 (see Table 5.6) – using individual real 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]

Table 5.6: Model 2 – estimated coefficients of the unemployment rate ($\hat{\delta}$) using individual real wages

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

Note: *** Significant at 1 % level.

Robust standard errors in brackets. Jobs are observed at least 3 years of the 1977 to 2009 period. Further controls used: secular time trend controls (t and t^2) and a dummy for years ≥ 1984. Individual controls used in the 1st stage regression: education, sex, nationality, 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. Thus, I draw a sub-sample of employment spells of workers which enter at least 5 jobs during the observed time period. Furthermore, these 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

23 Some robustness checks for the regressions of Tables 5.5 and 5.6 are provided in Tables D.8, D.9, and D.10, respectively, of Appendix D.4.

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 5.6 (model 2) using the sub-sample as a robustness check (see Table 5.7). The estimated coefficients of the control regressions (3.0 and 3.1) have about the same magnitudes as in the original sample (see Table 5.6). Hence, it seems that using the sub-sample for the regressions hardly affects the results.

Controlling 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.

Table 5.7: Model 3 – estimated coefficients of the unemployment rate ($\hat{\delta}$) using individual real wages

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

Note: *** Significant at 1 % level.

Robust standard errors in brackets. Further controls used: secular time trend controls (t and t^2) and a dummy for years ≥ 1984. Individual controls used in the 1st stage regression: (a) education, sex, nationality, and (b) age, age^2, and length of the employment spell.
I only use wage spells of job entrants which I observe at least 5 times and the jobs must be observed in at least 5 years in the 1977 to 2009 period. Due to the sampling the dataset is reduced from 62,317,577 to 10,335,054 employment spells of job entrants.

In the next Section I discuss the regression results 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.

5.4 Discussion of the Results

The estimated coefficients of the unemployment rate displayed in Tables 5.5 and 5.6 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 5.7).

5.4.1 Evaluation of the Regression Models

Using "typical" real 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 wages 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. 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)

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.

point out, the "[...] composition of the labor force may change considerably over the course of the business cycle."[27] Solon et al. (1997) further state, that firms might lower hiring standards in a boom to increase employment, while holding entry-wages stable. 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." Lastly, using the "typical" wage 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.

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. 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 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 (education) of the employee.

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 in the unemployment rate by one percentage

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.

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

point leads to a decrease of real wages by between 0.92 and 1.27 percent. The true parameter should lie within this range. Also, all estimates are not statistically different from each other at the 5 % level.

5.4.2 Implications of the Results

The estimated coefficients of the unemployment rate – using individual wages, controlling for job and worker fixed effects – are in the general vicinity of −1.27 (see Table 5.7). 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., 2012b, 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. (2012b) 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.

$$\Delta u = \alpha_1 + \beta_1 \log(\Delta GDP_{real}) + t + D_{\geq 1991} \tag{5.5}$$

$$\Delta \log\left(\frac{employment}{population}\right) = \alpha_2 + \beta_2 (GDP_{real}) + t + D_{\geq 1991} \tag{5.6}$$

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 5.7). 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 5.5).

Finally, I address the question of whether the Mortensen-Pissarides model can account for the cyclical variability of unemployment in light of the magnitude of the entry-wage cyclicality found for Germany. 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., 2012b). 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 5.8).

Table 5.8: Wage volatility in Kennan's (2010) informational rent model

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

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

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

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

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.

5.5 Conclusions

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 in 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 in 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 all estimates are not statistically different from each other at the 5 percent level.

The results of this chapter 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 up- and 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., 2012b) and 2.67 percent (Carneiro et al., 2012), respectively. Studies for the USA – that do not control for "cyclical up- and 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.

Part III
Conclusion

6 Concluding Remarks and Outlook

The book contributes to the current discussion on wage rigidity: downward nominal wage rigidity (DNWR) and real wage rigidity of job entrants over the business cycle.

6.1 Downward Nominal Wage Rigidity

In Part I of the book I show that microeconomic studies usually detect a significant degree of DNWR. In Germany, approximately 28 percent of wage cuts desired by employers are avoided because of DNWR (Knoppik and Beissinger, 2009). Based on the fact that DNWR exists, it is often concluded that low inflation leads to wage pressure on the macro level. Hence monetary policy aiming at low inflation is sometimes blamed for causing excess unemployment. However, the observed macroeconomic effects on aggregate real wages and employment seem to be surprisingly weak, leading Lebow et al. (1999) to speak of a "micro-macro puzzle".

Recent studies indicate that because of DNWR not only wage cuts are conducted to a lower degree as is economically advisable, but also wage increases (see, e.g., Elsby, 2009). If wage increases are compressed, this could explain the "micro-macro puzzle". Therefore, I analyze for Germany whether a compression of wage increases occurs when DNWR is binding by applying Unconditional Quantile Regressions (UQR) and Seemingly Unrelated Regressions to a dataset comprising more than 169 million wage changes of "job stayers," i.e., full-time workers who continually exercise the same job at the same employer for at least two consecutive years. I find evidence of a compression of wage increases and only very small effects of DNWR on average real wage growth. The compression of wage increase in times of low inflation is robust to the estimation method and the inflation variable used (actual inflation or expected inflation). Hence there do not seem to be negative consequences on aggregate employment, and DNWR cannot be used to justify higher inflation targets of central banks.[1]

Even if the macroeconomic effects of DNWR are negligible, one should, however, take a close look at workers affected by DNWR. If wage changes are unevenly distributed across workers, a microeconomic analysis can reveal effects of nominal wage rigidity that a macroeconomic analysis overlooks. Applying UQR to nearly 11 million earnings changes of job stayers from a linked employer-employee dataset,

1 It must be stressed that this conclusion is based on evaluating different steady state rates of inflation, where the inflation rate is correctly foreseen. It is not argued that this analysis should be used e.g. to calculate the possible costs of downward nominal wage rigidity in southern European countries with high nominal cost levels in the current crisis of the Eurozone. It cannot be concluded from my analysis that a higher temporary inflation target in the European Monetary Union would not be helpful for countries with a relatively high cost level in the current situation.

I show that the effect of the inflation rate on workers' real wage changes differs not only *between* but also *within* the percentiles of the wage change distribution. The effect of the inflation rate on workers' real wage changes is conditional on the workers' position in the wage change distribution, the workers' individual characteristics, and on firm characteristics. In particular, the class of the worker (in terms of white- and blue-collar workers), whether an employer pays wages above the standard rates, and/or whether a work council exists in a firm have a strong influence on how a change in the inflation rate affects a worker's real wage change. The results show that some workers are "discriminated" against by DNWR. Previous results are confirmed, e.g., that women more often experience nominal wage cuts (see, e.g., Anspal and Järve, 2011), and new insights are gained, e.g., that blue-collar workers in particular are affected by the compression of wage increases.

Given these results, future research on the (microeconomic) consequences of DNWR should consider that DNWR affects not only the lower tail of the wage change distribution but also its upper part. Furthermore, future research should consider that the effect of inflation on workers' real wage change is conditional on individual and firm characteristics, and that these effects differ over the wage change distribution. Considering these insights in future research will provide a better picture of the microeconomic effects of DNWR.

The results can also be used to evaluate different approaches to analyze DNWR in microdata. My empirical results show that low inflation in combination with DNWR also affects the upper tail of the wage change distribution. As a consequence, empirical results based on the normality approach by Borghijs (2001) and the symmetry approach by Card and Hyslop (1997), which assume a symmetric counterfactual wage change distribution and infer the shape of the lower tail of the counterfactual using the upper part of the wage change distribution, must be re-interpreted. Other approaches are also challenged, such as the earnings-function approach by Altonji and Devereux (2000), the histogram-location approach by Kahn (1997) or the approach based on the generalized hyperbolic distribution by Behr and Pötter (2010). They do not assume symmetry of the counterfactual wage change distribution but that DNWR does not affect higher percentiles of the real wage change distribution – an assumption that is challenged by my results.

6.2 Real Wage Rigidity

In Part II of the book I focus on real wage rigidity of newly hired workers over the business cycle. So far, little empirical evidence exists on how real wages of newly hired workers react to the business cycle. I fill 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. The analysis is undertaken for the 1977 to 2009 period using German 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 entrants within given firm-jobs by between 0.92 and 1.27 percent. In light of the magnitude of the entry-wage cyclicality it seems that the idea of introducing wage rigidity in the Mortensen-Pissarides model – in order to generate realistic volatility of unemployment – is not supported by the empirical evidence. Researchers are now challenged to develop search and matching models that are able to generate realistic amounts of volatility of, e.g., unemployment when considering the empirically documented wage cyclicality. Furthermore I show that the procyclicality of the employment/population ratio is (nearly) identical to the procyclicality of real entry-wages.

As outlined in Section 5.3.2, 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 (cf. 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 5.4.1, the estimate is probably procyclically biased.

Future research should also consider the following two ideas:

1) The effect of a change in the unemployment rate on real wages might not be symmetric. Hence results of regressions not allowing for asymmetric reactions might be biased. First evidence for Germany from work in progress (Snell and Stüber, 2012) shows that in general a change of the unemployment rate of one percentage point leads to about 0.9 percent lower/higher real wages.[2] Allowing for asymmetric reactions, an increase in the unemployment rate of one percentage point leads to about 0.7 percent lower wages. A decrease in the unemployment rate of one percentage point, however, leads to about 1.3 percent higher real wages. As expected due to the strong evidence in favor of downward (nominal) wage rigidity (see Chapter 1) wages seem to be more rigid in downswings than in upswings. Asymmetric effects of unemployment on real wages are also found for the USA. Shin and Shin (2008, p. 13), show that for male job stayers "[...] wage growth in expansions [...] is much greater than wage reduction in recessions [...]."

2) Martins et al. (2011, p. 1) "[...] show that panel estimates of tenure specific sensitivity to the business cycle of wages is subject to serious pitfalls."

2 We analyze the wage cyclicality for job entrants and incumbent workers. In the regressions we control, inter alia, for worker fixed effects and firm fixed effects; however we do not control for firm-job fixed effects.

Three canonical variates are used in the literature: (1) the minimum unemployment rate during a worker's time at the firm, (2) the unemployment rate at the start of his/her tenure, and (3) the current unemployment rate interacted with a new hire dummy. The paper shows that all three variates can be significant and "correctly" signed even when each worker in the firm receives the same wage, regardless of tenure (equal treatment). In matched data this problem can be resolved by including firm-year interaction fixed effects into the regression to annihilate the common wage components within each firm. In the revised version of the paper (Martins et al., 2012a) we show in an empirical exercise – using an administrative German dataset – that large changes in the coefficient estimates occur when applying the proposed method.

Hence, future research on the cyclicality of real wages could combine the ideas of Carneiro et al. (2012), Martins et al. (2012b, 2011, 2012a), Snell and Stüber (2012): analyzing the real wage cyclicality of newly hired workers and incumbents workers (cf. Carneiro et al., 2012), controlling for job-firm fixed effects (cf. Carneiro et al., 2012), controlling for job-firm fixed effects (cf. Martins et al., 2012b) and firm-year fixed effects (cf. Martins et al., 2011, 2012a), while allowing asymmetric reactions (cf. Snell and Stüber, 2012). This would consider all recent suggestions regarding the reduction of estimation biases in the analysis of real wage cyclicality and should hence provide a better picture of the real wage cyclicality.

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A Appendix of Chapter 1

The constraint on studies that use the earnings-function approach (EFA) or the histogram-location approach (HLA) is due to the introduced analytical methods in Section 1.3.2. Extensive overviews of studies are given by Kuroda and Yamamoto (2007, p. 52–57) and Bläs (2008, p. 63–70).

Table A.1: Selected microdata-studies and their results

Study	Country[a]	Data[b]	Approach[c]	Degree of rigidity[d]
Devicienti et al. (2007)	I	WHIP (1985–99)	EFA	low
Fehr and Goette (2005)	CH	SLFS (1991–98), SSIF (1990–97)	EFA	very high
Kuroda and Yamamoto (2005)	J	BSWS (1985–2001)	HLA	medium (till 1997), very low (since 1998)
Knoppik and Beissinger (2003)	D	IABS (1975–95)	EFA	high/very high[e]
Lebow et al. (2003)	USA	ECI (1981–99)	HLA	medium
Beissinger and Knoppik (2001)	D	IABS (1975–95)	HLA	low/low[e]
Castellanos et al. (2004)	MEX	IMSS (1986–2001)	HLA	high
Altonji and Devereux (2000)	USA	PSID (1971–92)	EFA	very high
Kahn (1997)	USA	PSID (1970–88)	HLA	Medium/low[e]

[a] CH = Switzerland, D = Germany, I = Italy, J = Japan, MEX = Mexico, USA = United States of America.
[b] BSWS = Basic Survey on Wage Structure, ECI = Employment Cost Index, IABS = IAB Beschäftigten-Stichprobe, IMSS = Administrative records of the Instituto Mexicano del Seguro Social, PSID = Panel Study of Income Dynamics, SLFS = Swiss Labor Force Survey, SSIF = Sample of the Social Insurance Files, WHIP = Worker History Italian Panel.
[c] EFA = earnings-function approach, HLA = histogram-location approach.
[d] Degree of rigidity: degree of (downward nominal) wage rigidity measures the share of desired wage reductions that are prevented by DNWR. Classification of the degree of rigidity: 0 % ≥ very low < 20 %, 20 % ≥ low < 40 %, 40 % ≥ medium < 60 %, 60 % ≥ high < 80 %, 80 % ≥ very high.
[e] Blue-collar worker/white-collar worker.

B Appendix of Chapter 2

B.1 Data Selection and Description

For the analysis, I only use the earnings spells of male employees from West Germany[1] aged 16 to 65. I distinguish between white-collar workers and blue-collar workers. The workers must be subject to social security taxes and must be gainfully employed in the same occupation by the same employer throughout the year for at least two consecutive years.[2] The earnings are right-censored at the contribution assessment ceiling (Beitragsbemessungsgrenze). For employees whose earnings are censored, the earnings changes cannot be computed correctly. Because the monthly income is censored as well, it is possible that yearly earnings are below the contribution assessment ceiling, even if several monthly earnings are censored. This causes some noise for earnings slightly below the contribution assessment ceiling. Therefore, earnings spells above 0.96 times the contribution assessment ceiling of the compulsory pension insurance scheme are dropped. The lower limit of earnings is given by the earnings limit for "marginal" part-time workers/fringe workers (Geringfügigkeitsgrenze; see Table B.2). Workers with earnings below the lower earnings limit are not included in the BeH.

I also control for further employment spells. If a person has more than one employment spell liable to social security taxes, regardless of full- or part-time, I drop the employment spell(s) of that person for that particular year. Still, there are some implausibly high growth rates of (annual) earnings – up to 260 percent. Until 1999, these are concentrated mainly in the group of employees younger than 25 years. This is because not every change in an employment relationship leads to a new spell. For example, until 1999, the BeH-item "class of worker" contains only the last status of the particular year. If a person ends an apprenticeship in the middle of a year and then is gainfully employed by the same employer for the rest of the year as well as the next year, I will observe the person as being gainfully employed two years in a row. Given that after the apprenticeship the respective person is typically earning more than double the previous income, an implausibly high growth rate of annual earnings is observed. To make sure that this and other effects are not at work in the data, I only analyze (annual) wage changes that are higher than the one percent percentile and lower than the 99 percent percentile.

After the selection, the remaining spells comprise 50,575,416 salary changes of white-collar workers as well as 118,593,371 wage changes of blue-collar workers (see Table B.1).

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1 Except (West) Berlin.

2 The BeH contains 32 classifications for employment relationships. I only consider employees in regular employment. Therefore, I drop, e.g., trainees, insured artists and publicists, and employees in partial retirement.

Table B.1: Earnings spells and observable earnings changes for the BeH and the used datasets

Year	Employee History File (BeH)[a]							Dataset for white-collar workers (job stayers)		Dataset for blue-collar workers (job stayers)	
	total BeH			white-collar workers		blue-collar workers		Observable salary changes to the previous year	% of all white-collar workers spells	Observable wage changes to the previous year	% of all blue-collar workers spells
	Number of spells	Number of persons	Number of new persons	Number of spells	% of all BeH spells	Number of spells	% of all BeH spells				
1975	25,477,714	22,229,687	22,229,687	8,017,135	31.47	13,115,611	51.48	–	–	–	–
1976	26,312,435	22,027,301	1,821,120	8,162,966	31.02	13,588,660	51.64	1,223,461	14.99	4,001,617	29.45
1977	26,536,964	22,268,246	1,524,711	8,326,823	31.38	13,423,461	50.58	1,339,689	16.09	4,008,105	29.86
1978	26,582,142	22,280,456	1,422,128	8,504,452	31.99	13,125,102	49.38	1,455,036	17.11	3,987,076	30.38
1979	27,735,013	23,050,680	1,519,340	8,741,313	31.52	13,666,833	49.28	1,549,174	17.72	4,026,094	29.46
1980	27,915,481	23,368,670	1,447,888	8,958,331	32.09	13,641,632	48.87	1,550,299	17.31	4,074,915	29.87
1981	27,446,754	23,465,968	1,234,982	9,062,261	33.02	13,059,120	47.58	1,613,492	17.80	4,229,974	32.39
1982	26,601,318	23,174,161	1,115,916	8,912,796	33.51	12,293,271	46.21	1,724,945	19.35	4,267,181	34.71
1983	25,999,555	22,761,297	1,084,306	8,785,081	33.79	11,786,115	45.33	1,823,678	20.76	4,260,338	36.15
1984	26,649,448	22,892,553	1,145,787	8,811,489	33.06	12,226,538	45.88	1,652,739	18.76	4,057,593	33.19
1985	26,704,365	22,781,837	1,091,527	8,759,642	32.80	12,232,551	45.81	1,541,769	17.60	3,966,506	32.43
1986	27,541,879	23,436,642	1,119,212	9,256,438	33.61	12,326,805	44.76	1,539,611	16.63	4,014,362	32.57
1987	28,116,787	23,677,568	1,074,500	9,554,798	33.98	12,439,379	44.24	1,555,887	16.28	4,018,113	32.30
1988	28,698,344	23,786,816	1,033,231	9,882,373	34.44	12,667,343	44.14	1,587,020	16.06	3,988,695	31.49
1989	29,822,255	24,267,501	1,199,883	10,322,363	34.61	13,178,397	44.19	1,587,684	15.38	3,961,452	30.06

1990	31,784,818	25,217,847	1,645,845	10,910,750	34.33	14,143,744	44.50	1,517,988	13.91	3,815,151	26.97
1991	37,527,796	30,390,685	6,141,237	11,316,503	30.15	14,230,801	37.92	1,485,069	13.12	3,912,383	27.49
1992	39,806,357	32,367,400	4,452,503	14,526,173	36.49	17,151,872	43.09	1,518,998	10.46	4,086,827	23.83
1993	38,726,145	31,468,111	1,258,045	14,288,414	36.90	16,307,187	42.11	1,559,944	10.92	4,040,428	24.78
1994	37,109,938	30,765,834	1,150,297	13,658,194	36.80	15,413,334	41.53	1,678,306	12.29	3,993,359	25.91
1995	37,428,190	30,718,658	1,158,163	13,808,067	36.89	15,391,415	41.12	1,704,648	12.35	3,872,681	25.16
1996	36,116,981	30,284,347	1,096,866	13,279,672	36.77	14,530,125	40.23	1,695,637	12.77	3,759,385	25.87
1997	36,708,737	30,034,750	1,224,453	13,221,938	36.02	14,277,362	38.89	1,755,706	13.28	3,765,959	26.38
1998	37,126,961	30,696,402	1,485,883	13,559,574	36.52	13,878,339	37.38	1,716,299	12.66	3,620,074	26.08
1999	45,866,082	35,023,973	2,979,728	14,432,989	31.47	14,799,245	32.27	1,582,533	10.96	3,311,996	22.38
2000	48,046,644	35,989,747	1,700,270	14,635,674	30.46	15,064,596	31.35	1,538,716	10.51	3,222,105	21.39
2001	48,957,095	36,063,811	1,421,173	15,132,476	30.91	15,157,937	30.96	1,479,744	9.78	3,057,588	20.17
2002	47,356,880	35,459,833	1,185,798	14,927,452	31.52	14,240,501	30.07	1,422,480	9.53	2,906,075	20.41
2003	50,878,383	35,163,454	1,142,687	14,850,117	29.19	14,130,373	27.77	1,450,294	9.77	2,901,505	20.53
2004	47,152,731	35,076,422	1,105,978	14,314,460	30.36	13,370,424	28.36	1,650,762	11.53	2,889,431	21.61
2005	46,250,593	34,574,481	1,092,777	13,542,098	29.28	12,957,363	28.02	1,710,424	12.63	2,920,200	22.54
2006	47,148,366	34,856,424	1,154,210	13,559,054	28.76	13,237,171	28.08	1,759,427	12.98	2,907,775	21.97
2007	49,182,872	35,427,149	1,235,771	14,326,287	29.13	13,710,683	27.88	1,603,957	11.20	2,748,428	20.05
Sum	1,171,326,023	–	72,695,902	382,348,153	32.64	454,763,290	38.82	50,575,416	13.23	118,593,371	26.08

[a] Source: IAB (2009, Tab. 3.4, p. 13 f.)

For each employee I have the following information.

Gross annual earnings:

- salary: gross annual salary of a full-time, white-collar worker
- wage: gross annual wage of a full-time worker

The BeH does not allow separating fringe benefits from "regular" earnings. This is important because 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. This leads to a level effect on the 1983 1984 (log) earnings changes. However, observations before and after 1984 should be valid. 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.

Gross average daily earnings:

- gross average daily salary of a full-time, white-collar worker
- gross average daily wage of a full-time, blue-collar worker

The BeH contains no data on hours worked except for information about part-time or full-time employment. Therefore, it is not possible to compute hourly earnings. Because I cannot observe changes in the working time – as long as the threshold for part-time employment is not crossed – I sometimes observe implausibly high growth rates of (annual) earnings. Using gross annual earnings and the duration of the employment spell, I calculate gross average daily earnings. Because white-collar workers are being paid the same salary every month, irrespective of the number of working days, I calculate the gross average daily salary for a 365-day year. For workers, I use the exact duration of the employment spell to calculate the gross average daily wages. To avoid any contamination with working-time effects, only full-time employment spells are included.

Table B.2: Contribution assessment ceiling for Western Germany, lower earnings limit, and inflation

Year	Contribution assessment ceiling for Western Germany (€ per year)[a]		Lower earnings limit (§ 8, Social Code IV)	Change of the German consumer price index[b] to the previous year in %
	Compulsory pension insurance scheme	'Knappschaftliche' pension insurance		
1975	17,179.41	20,860.71	2,147.40	6.03
1976	19,020.06	23,314.91	2,377.56	4.22
1977	20,860.71	25,769.11	2,607.60[c]	3.70
1978	22,701.36	28,223.31	2,392.80	2.72
1979	24,542.01	29,450.41	2,392.80	4.13
1980	25,769.11	31,291.06	2,392.80	5.40
1981	26,996.21	33,131.71	2,392.80	6.33
1982	28,836.86	35,585.91	2,392.80	5.24
1983	30,677.51	37,426.57	2,392.80	3.23
1984	31,904.61	39,267.22	2,392.80	2.48
1985	33,131.71	41,107.87	2,454.24	2.04
1986	34,358.81	42,334.97	2,515.56	–0.12
1987	34,972.36	43,562.07	2,638.32	0.25
1988	36,813.02	44,789.17	2,699.64	1.25
1989	37,426.57	46,016.27	2,760.96	2.83
1990	38,653.67	47,856.92	2,883.72	2.63
1991	39,880.77	49,084.02	2,945.04	3.73
1992	41,721.42	51,538.22	3,067.80	3.93
1993	44,175.62	54,605.97	3,251.76	3.57
1994	46,629.82	57,673.72	3,435.84	2.71
1995	47,856.92	58,900.82	3,558.60	1.63
1996	49,084.02	60,127.93	3,619.92	1.38
1997	50,311.12	61,968.58	3,742.68	1.93
1998	51,538.22	63,195.68	3,804.00	1.00
1999	52,151.77	63,809.23	3,865.32	0.55
2000	52,765.32	65,036.33	3,865.32	1.42
2001	53,378.87	65,649.88	3,865.32	1.94
2002	54,000.00	66,600.00	3,900.00	1.48
2003	61,200.00	75,000.00	3,900.00	1.04
2004	61,800.00	76,200.00	4,800.00	1.65
2005	62,400.00	76,800.00	4,800.00	1.52
2006	63,000.00	77,400.00	4,800.00	1.60
2007	63,000.00	77,400.00	4,800.00	2.26

[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 (1995–2007) interlinked with the cost-of-living index of all private households for West Germany (1974–1994). Source: Statistisches Bundesamt.
[c] After July 1st, 1977: € 2,270.16.

Duration of employment:
The duration of employment is not consistent with the actual days worked but represents the duration of the employment contract. To make sure that a person is employed all year, I drop all spells with durations of employment of less than 365 days.

Class of worker:
The BeH contains eight classes of workers: (1) trainees, (2) workers, (3) skilled workers[3], (4) master craftsmen and foremen[4], (5) white-collar workers, (6) home workers, (7) people with less than 18 weekly hours of work, and (8) people with 18 and more weekly hours of work but not fully employed. I drop all classes except "white-collar workers", "workers", and "skilled workers". The two latter classes are combined to the class "blue-collar workers".

Occupational classification:
This variable describes the field of an employee's occupational specialization. The BeH covers 86 occupation groups containing 328 occupations. These groups are used to control for job stayers. They are subsumed to six occupational fields, which are used in the regressions.

Qualification level of an employee:
This variable includes eight categories: (1) no formal education, (2) lower secondary school and intermediate (secondary) school without vocational qualification, (3) lower secondary school and intermediate (secondary) school with vocational qualification, (4) upper secondary school examination without vocational qualification, (5) upper secondary school examination with vocational qualification, (6) postsecondary technical college degree, (7) university degree, and (8) no classification applicable. The qualification level 'no classification applicable' is subsumed to 'no formal education'.

Age of a person:
The age a person is turning in the particular year-only spells from persons aged 16 to 65 are kept.

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

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

Further, I have the following information:

Inflation:
Change of the consumer price index (CPI) for Germany from the previous year (see Table B.3). I interlinked the CPI (available for 1995–2007) with the cost-of-living index of all private households for West Germany (available for 1962–1999).

Contribution assessment ceiling (Beitragsbemessungsgrenze):
The earnings covered by the BeH are right censored at the contribution assessment ceiling. The contribution assessment ceiling is annually adjusted to the changes of earnings. Some employees-miners, mine employees, sailors and railroad employees-are insured in a special pension insurance, called "knappschaftliche" pension insurance. The contribution assessment ceiling of this pension insurance is always higher than for the compulsory pension insurance scheme (see Table B.3). Since 1999, the BeH no longer indicates the pension insurance through which a person is insured. For this reason, I only use the contribution assessment ceiling of the compulsory pension insurance scheme. As shown in Tables B.4 and B.5, the elimination of the censored earnings spells leads to an underrepresentation of highly qualified (white collar) workers.

Table B.3: Mean age and percentage of white collar workers before and after dropping top-coded earnings spells

Year	Mean age		Share of white collar worker in %	
	with	w/o	with	w/o
	top-coded spells			
1975	40.31	39.83	34	23
1976	39.66	39.19	35	25
1977	39.76	39.29	35	26
1978	39.90	39.43	36	28
1979	40.15	39.68	36	28
1980	40.22	39.71	36	28
1981	40.27	39.71	37	28
1982	40.27	39.68	38	29
1983	40.32	39.71	38	30
1984	40.36	39.62	39	29
1985	40.37	39.58	38	28
1986	40.36	39.52	39	28
1987	40.32	39.42	39	28
1988	40.35	39.45	40	29
1989	40.52	39.60	40	29
1990	40.60	39.60	40	29
1991	40.54	39.48	40	27
1992	40.52	39.43	41	28
1993	40.48	39.42	41	29
1994	40.51	39.52	42	31
1995	40.61	39.62	42	31
1996	40.63	39.69	42	32
1997	40.83	39.93	43	33
1998	40.99	40.13	43	33
1999	41.12	40.26	44	33
2000	41.28	40.44	44	33
2001	41.39	40.59	45	33
2002	41.60	40.81	46	33
2003	41.76	41.10	46	36
2004	41.88	41.27	47	37
2005	42.09	41.47	48	38
2006	42.46	41.85	48	38
2007	42.48	41.86	48	37
Mean	40.75	40.00	41	31

Table B.4: Qualification level of the employees – before and after dropping top-coded earnings spells

Year	No formal education/no classification applicable		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	
	Share with top-coded spells in %	**Share w/o top-coded spells in %**	with	w/o	with	w/o	with	w/o	with	w/o	with	w/o	with	w/o
1975	7.85	8.52	22.85	26.87	63.03	63.30	0.38	0.21	0.94	0.34	2.69	0.57	2.25	0.19
1976	5.61	5.85	23.52	27.09	64.50	65.42	0.39	0.23	0.95	0.39	2.68	0.73	2.35	0.29
1977	5.23	5.44	22.72	25.89	65.47	66.70	0.40	0.25	0.97	0.45	2.77	0.89	2.45	0.39
1978	5.06	5.23	22.42	25.35	65.52	66.98	0.41	0.28	1.02	0.52	2.94	1.11	2.62	0.53
1979	5.01	5.19	22.07	24.79	65.78	67.30	0.42	0.29	1.04	0.57	2.99	1.22	2.70	0.63
1980	4.94	5.13	21.84	24.70	65.78	67.29	0.43	0.31	1.09	0.61	3.12	1.27	2.80	0.68
1981	4.87	5.08	21.52	24.43	65.76	67.41	0.44	0.32	1.10	0.63	3.29	1.36	3.01	0.76
1982	4.79	5.01	21.24	23.97	65.74	67.54	0.44	0.33	1.17	0.72	3.43	1.54	3.18	0.89
1983	4.71	4.94	20.41	23.08	66.36	68.27	0.44	0.34	1.24	0.80	3.50	1.62	3.34	0.96
1984	4.54	4.85	19.50	22.64	67.14	69.07	0.45	0.33	1.31	0.82	3.58	1.43	3.49	0.87
1985	4.47	4.80	19.41	22.65	67.07	69.05	0.45	0.33	1.36	0.87	3.62	1.40	3.61	0.90
1986	4.37	4.69	18.83	22.08	67.33	69.44	0.47	0.35	1.47	0.98	3.73	1.48	3.80	0.99
1987	4.31	4.66	18.23	21.51	67.50	69.78	0.49	0.36	1.59	1.09	3.88	1.54	4.01	1.07
1988	4.31	4.65	17.63	20.73	67.70	70.16	0.50	0.37	1.70	1.22	3.99	1.68	4.18	1.19
1989	4.35	4.70	17.24	20.39	67.80	70.35	0.50	0.38	1.81	1.31	4.03	1.67	4.27	1.20
1990	4.48	4.86	16.75	20.11	67.71	70.39	0.52	0.39	1.94	1.40	4.15	1.64	4.44	1.20
1991	4.59	5.00	16.41	19.93	67.59	70.47	0.53	0.40	2.06	1.48	4.21	1.52	4.60	1.20
1992	4.82	5.27	16.08	19.59	67.25	70.26	0.55	0.41	2.19	1.58	4.31	1.57	4.79	1.32
1993	5.03	5.47	15.59	18.76	67.00	70.26	0.56	0.43	2.37	1.76	4.42	1.80	5.03	1.52
1994	5.21	5.63	14.95	17.78	67.04	70.46	0.57	0.45	2.54	1.95	4.48	2.00	5.21	1.72
1995	5.43	5.86	14.52	17.28	66.97	70.47	0.58	0.46	2.70	2.10	4.55	2.03	5.25	1.79
1996	5.70	6.12	14.24	16.80	67.18	70.37	0.59	0.48	2.88	2.27	4.57	2.11	4.84	1.86
1997	5.86	6.26	13.84	16.28	66.96	70.32	0.60	0.49	3.03	2.42	4.65	2.24	5.06	2.00
1998	6.12	6.53	13.53	15.93	66.36	70.12	0.60	0.50	3.15	2.52	4.62	2.28	5.60	2.13
1999	6.48	6.94	12.90	15.33	65.89	70.02	0.61	0.51	3.33	2.64	4.67	2.33	6.11	2.24
2000	6.91	7.46	12.69	15.27	64.92	69.49	0.65	0.53	3.55	2.76	4.83	2.28	6.45	2.22
2001	7.56	8.16	12.43	15.07	64.00	68.86	0.68	0.56	3.71	2.86	4.92	2.27	6.70	2.23
2002	7.98	8.64	12.30	15.00	63.03	68.13	0.70	0.58	3.88	2.96	5.10	2.33	7.01	2.36
2003	8.39	8.93	11.84	14.00	62.23	67.20	0.78	0.64	4.01	3.26	5.29	2.91	7.45	3.07
2004	8.85	9.37	11.44	13.39	61.78	66.71	0.81	0.68	4.20	3.48	5.30	3.08	7.60	3.30
2005	9.54	10.15	10.86	12.78	61.01	66.17	0.82	0.70	4.45	3.71	5.48	3.13	7.83	3.36
2006	10.1	10.8	10.23	12.11	60.75	66.04	0.81	0.69	4.58	3.80	4.58	3.13	8.01	3.42
2007	9.98	10.76	10.27	12.36	60.86	66.49	0.78	0.66	4.54	3.67	5.59	2.92	7.97	3.14
Mean	5.98	6.39	16.68	19.51	65.49	68.49	0.56	0.43	2.36	1.76	4.12	1.85	4.79	1.56

B.2 Impact of Inflation on the Conditional Percentiles using Quantile Regression

To observe the effect of inflation on the *conditional* percentiles of the real wage change distribution, I regress the real wage change Δw on the inflation rate π, the average regional real wage growth rate μ (as a proxy for productivity growth), and further control variables.

I make use of the quantile regression (Koenker and Bassett, 1978; Koenker, 2005) and model conditional percentiles of the real wage change distribution as functions of predictors:

$$Q_{\Delta w_i} = (\tau | \mathbf{x}_{irt}) \mathbf{x}'_{rt} \beta(\tau) \tag{B.1}$$

with $\mathbf{x}'_{irt} = (\mu_{rt} \;\; \pi_t \;\; \mathbf{z}'_{irt})$.

The vector $\mathbf{z}$ contains, as for the UQR, the control variables (see Table 2.2). For the quantile regressions, I use a 1-percent stratified sample of my data.[5]

The results (see Table B.5) show that at some degree not only the highest wage increases are compressed if inflation is low (see the results in Section 2.3), but also the highest wage increases conditional on the attributes of the employee and conditional on the region where the employee works are compressed, if inflation is low and DNWR binds.

Table B.5: Effects of inflation and productivity growth on the conditional percentiles of the real wage change distribution using quantile regression

	Consumer price index			Average regional real wage growth (as a proxy for productivity growth)		
	Coef.	Std.Err.	$P>\|t\|$	Coef.	Std.Err.	$P>\|t\|$
p10	−0.075	0.012	0.000	0.904†	0.007	0.000
p20	−0.138	0.007	0.000	0.847†	0.005	0.000
p30	−0.148	0.005	0.000	0.854†	0.004	0.000
p40	−0.152	0.004	0.000	0.865†	0.003	0.000
p50	−0.142	0.004	0.000	0.871†	0.003	0.000
p60	−0.114	0.005	0.000	0.876†	0.004	0.000
p70	−0.072	0.006	0.000	0.893†	0.005	0.000
p80	−0.019	0.009	0.036	0.906†	0.006	0.000
p90	0.002	0.017	0.909	0.919†	0.012	0.000

Notes: Quantile regression. Controls: region dummies, age, age squared, absolute change in inflation, current and lagged unemployment rate, dummy for the year 1984, educational class, dummy for worker with foreign nationality, occupational fields, dummy for white-collar worker. Bootstrapped standard errors, 50 replications. †: coef. for productivity growth significantly different from 1 at the 5 % level.

5 The sample has been stratified by region, age, foreign nationality, worker class, occupational field, and year.

B.3 Impact of Inflation on the Unconditional Percentiles using Least Squares Dummy Variable Regression

I estimate regressions with region-specific dummies of the following form by OLS: $P_{\tau_{rt}} = \alpha_\tau + \eta_\tau \pi_\tau + \beta_t \mu_{rt} + z'_{rt}\rho_\tau + \varepsilon_{\tau_{rt}}$, where $P_{\tau_{rt}}$ is the τth percentile of the DFL reweighted real wage growth distribution in region r at time t, μ_{rt} is the frictionless average real wage growth (measured using the observed regional average real wage growth rate), and π_τ is the inflation rate. The vector $\mathbf{z}_{rt}$ contains further control variables, shown in Table B.5.

Table B.6: Effects of inflation and productivity growth on the unconditional percentiles of the real wage change distribution using least squares dummy variable regression

	Consumer price index			Average regional real wage growth (as a proxy for productivity growth)		
	Coef.	Std.Err.	$P>\|t\|$	Coef.	Std.Err.	$P>\|t\|$
p10	–0.063	0.062	0.317	0.912	0.052	0.000
p20	–0.114	0.025	0.000	0.858[†]	0.026	0.000
p30	–0.082	0.031	0.013	0.927	0.036	0.000
p40	–0.101	0.048	0.041	0.959	0.048	0.000
p50	–0.088	0.040	0.035	0.958	0.039	0.000
p60	–0.043	0.035	0.233	0.987	0.032	0.000
p70	0.005	0.030	0.878	1.004	0.025	0.000
p80	0.047	0.032	0.144	1.024	0.030	0.000
p90	0.091	0.059	0.129	1.057	0.058	0.000

Notes: Least squares dummy variable regression. I estimate the regressions weighed by region size and relax the assumption of independence within years. Controls: regions, mean age, absolute change in inflation, current and lagged unemployment rate, dummy for the year 1984, percentage of the educational classes, percentage of workers with foreign nationality, percentage of white-collar worker, percentage of the occupational fields. [†]: coef. for productivity growth significantly different from 1 at the 5 % level.

The estimated coefficients of this least square dummy variable (LSDV) regression are identical to those of the SUR described in Section 2.3.1. However, the residuals differ because the LSDV regression ignores the contemporaneous correlation of the residuals.

A comparison with the results of the SUR shows that all estimated coefficients of the SUR are at least as significant as the results of the LSDV regression, and most coefficients of the SUR, especially those for inflation, are even more highly significant as the coefficients of the LSDV regression.

B.4 Impact of Inflation on the Unconditional Percentiles using Unconditional Quantile Regression without Controlling for Individual Characteristics

Table B.7: Effects of inflation and productivity growth on the unconditional percentiles of the real wage change distribution using unconditional quantile regression without individual control variables

	Consumer price index			Average regional real wage growth (as a proxy for productivity growth)		
	Coef.	Std.Err.	$P>\|t\|$	Coef.	Std.Err.	$P>\|t\|$
p10	−0.050	0.004	0.000	0.843†	0.003	0.000
p20	−0.150	0.002	0.000	0.708†	0.002	0.000
p30	−0.150	0.002	0.000	0.815†	0.002	0.000
p40	−0.130	0.001	0.000	0.870†	0.001	0.000
p50	−0.132	0.002	0.000	0.964†	0.002	0.000
p60	−0.148	0.002	0.000	1.017†	0.002	0.000
p70	−0.100	0.002	0.000	0.990†	0.002	0.000
p80	−0.002	0.003	0.427	1.009†	0.003	0.000
p90	0.140	0.005	0.000	1.084†	0.005	0.000

Notes: Unconditional quantile regression. Controls: region dummies, absolute change in inflation, current and lagged unemployment rate, dummy for the year 1984. Bootstrapped standard errors, 50 replications.
†: coef. for productivity growth significantly different from 1 at the 5 % level.

B.5 Impact of Expected Inflation on the Unconditional Percentiles using Unconditional Quantile Regression

Expected future inflation is based on inflation forecasts using ARIMA modeling for quarterly inflation data. The seasonally adjusted monthly CPI series USFB99 of the Deutsche Bundesbank has first been converted to quarterly data. Up to 1994 the series USFB99 refers to West Germany and after 1995 to Unified Germany. It is already linked over January 1995. The quarterly inflation series has been computed as log difference to the previous year. For the estimations described below the maximum sample starts in 1968.q4, hence the unit root tests refer to the sample 1968.q4–2007.q4. Applying the augmented Dickey-Fuller test and the Phillips-Peron test, I find that inflation is generated by an I(1) process. After the inspection of the correlogram of the first difference of inflation, I start with the estimation of an ARIMA(8,1,4) model for inflation for the sample 1968.q4–2007.q4. The size of the model is then reduced using information criteria. The highest values of the Akaike information criterion and the Schwarz information criterion are obtained for an ARIMA(0,1,2) model. Such a model has also been used by

Deutsche Bundesbank (2001) to estimate expected inflation when constructing a real interest rate series for Germany. However, the Breusch-Godfrey LM test on autocorrelation points to autocorrelation in this and all other model versions in which only MA terms are included. As an alternative to the ARIMA(0,1,2) model I therefore also consider the ARIMA(1,1,1) model for which the hypothesis of no autocorrelation cannot be rejected. Using the White heteroskedasticity test, in both ARIMA models no heteroskedasticity is found. Based on the Jarque-Bera test the hypothesis of normally distributed errors cannot be rejected for both models.

Table B.8: Effects of forecasted inflation and productivity growth on the unconditional percentiles of the real wage change distribution using unconditional quantile regression

	Consumer price index ARIMA(1,1,1)			Average regional real wage growth (as a proxy for productivity growth)		
	Coef.	Std.Err.	$P>\|t\|$	Coef.	Std.Err.	$P>\|t\|$
p10	–0.040	0.003	0.000	0.903†	0.003	0.000
p20	–0.115	0.002	0.000	0.771†	0.001	0.000
p30	–0.084	0.001	0.000	0.864†	0.002	0.000
p40	–0.068	0.001	0.000	0.895†	0.002	0.000
p50	–0.050	0.001	0.000	0.972†	0.001	0.000
p60	–0.024	0.002	0.000	1.013†	0.002	0.000
p70	0.025	0.002	0.000	0.977†	0.002	0.000
p80	0.044	0.002	0.000	0.948†	0.003	0.000
p90	0.053	0.003	0.000	0.935†	0.004	0.000
	Consumer price index ARIMA(0,1,2)			**Average regional real wage growth (as a proxy for productivity growth)**		
	Coef.	**Std.Err.**	$P>\|t\|$	**Coef.**	**Std.Err.**	$P>\|t\|$
p10	–0.042	0.003	0.000	0.896†	0.003	0.000
p20	–0.113	0.002	0.000	0.765†	0.002	0.000
p30	–0.085	0.001	0.000	0.866†	0.001	0.000
p40	–0.067	0.001	0.000	0.896†	0.001	0.000
p50	–0.042	0.001	0.000	0.967†	0.001	0.000
p60	–0.008	0.001	0.000	1.005†	0.002	0.000
p70	0.036	0.002	0.000	0.974†	0.002	0.000
p80	0.045	0.002	0.000	0.952†	0.002	0.000
p90	0.049	0.004	0.000	0.946†	0.003	0.000

Notes: Unconditional quantile regression. Controls: region dummies, age, age squared, forecasted absolute change in inflation (forecasted inflation rate of year t minus actual inflation rate of year t–1), current and lagged unemployment rate, dummy for the year 1984, educational class, dummy for worker with foreign nationality, occupational fields, dummy for white-collar worker. Bootstrapped standard errors, 50 replications. †: coef. for productivity growth significantly different from 1 at the 5 % level.

As a next step I estimate ARIMA(1,1,1) models and ARIMA(0,1,2) models using rolling samples with a five year moving window and then forecast inflation for the subsequent year. For example, the first estimation is done using the sample from 1969.q1 to 1973.q4 and the inflation forecasts then relate to 1974.q1 to 1974.q4. Averaging over these four values leads to the value for expected inflation for the year 1974. The second estimation is then done using the sample from 1970.q1 to 1974.q4, while the forecasts relate to 1975.q1 to 1975.q4, yielding expected inflation for 1975 and so on. As Junttila (2001) points out, these rolling regressions take parameter instability in the inflation process into account, which is important when dealing with inflation expectations.[6]

My series of expected inflation, based on the ARIMA(1,1,1) model and the ARIMA(0,1,2) model, are used instead of actual inflation in the UQR. The results are reported in Table B.8.

6 Deutsche Bundesbank (2001, p. 36) also uses a five-year rolling window approach to estimate expected inflation.

C Appendix of Chapter 3

C.1 Data Description and Data Selection

The contribution assessment ceiling is annually adjusted to the changes in earnings. Some employees – miners, mine employees, sailors and railroad employees – are insured in the 'knappschaftliche' pension insurance. The contribution assessment ceiling of this pension insurance is always higher than that for the compulsory pension insurance scheme. Since 1999, the BeH no longer indicates through which pension insurance a person is insured. For this reason, I use only the contribution assessment ceiling of the compulsory pension insurance scheme (see Table B.2).

Because the monthly wage is also censored, it is possible that the yearly wages are below the contribution assessment ceiling even if the wages for several months are censored. This causes some noise for the wages that are just below the contribution assessment ceiling. Therefore, the wage spells that are above 0.96 times the contribution assessment ceiling of the compulsory pension insurance scheme are dropped. The lower limit of earnings is given by the earnings limit for the "marginal" part-time workers/fringe workers (Geringfügigkeitsgrenze; see Table B.2). These workers are not included in the BeH.

C.2 Model Predictions and the Suitability of the Dataset

The LIAB covers a much shorter time period than the BeH dataset used in Chapter 2 and the inflation rate in this shorter time period is less volatile.

To ensure that the LIAB is suitable for the analysis, I run a UQR that is comparable to the UQR of Chapter 2: I run the UQR from Section 3.2 without the variables interacted with the inflation rate.

If the wage cuts and the wage increases are compressed due to DNWR, one should observe – according to Elsby's (2009) model – positive coefficients for the inflation rate for the percentiles of the real wage change distribution below and above minus the inflation rate. For the percentiles of the real wage change distribution that correspond to minus the inflation rate, one should observe negative coefficients for the inflation rate (see Table 2.1).

In the LIAB, the zero nominal wage changes ($P_\tau \approx$ minus inflation rate) appear in the range equal to and above the 13*th* percentile and equal to and below the 31*th* percentile of the wage change distribution. The mean of these observed percentiles is the 24*th* percentile. With this information, I am able to check whether the coefficients for the inflation rate and the productivity growth that were obtained applying the UQR (see Table C.1) fit the predictions of Elsby's (2009) model (see Table 2.1).

Table C.1: The marginal effects of the inflation rate and productivity growth on the percentiles of the real wage change distribution without interaction terms

	Consumer price index		Average regional real wage growth (as a proxy for productivity growth)	
	Coef.	Std.Err.	Coef.	Std.Err.
p10	0.159***	(0.009)	1.158***†	(0.005)
p20	−0.160***	(0.006)	0.799***†	(0.003)
p30	−0.473***	(0.004)	0.623***†	(0.002)
p40	−0.511***	(0.004)	0.654***†	(0.002)
p50	−0.402***	(0.004)	0.831***†	(0.002)
p60	−0.386***	(0.005)	0.823***†	(0.002)
p70	−0.119***	(0.005)	0.881***†	(0.002)
p80	0.480***	(0.008)	1.082***†	(0.004)
p90	1.259***	(0.012)	1.425***†	(0.007)

Notes: Unconditional quantile regression. Bootstrapped standard errors (50 replications). Further controls are used as follows: 16 regions, age, age^2, absolute change in inflation, current and lagged regional unemployment rate, 8 educational classes, workers with foreign nationality, 6 occupational fields, establishment size, (establishment size)^2, (establishment size)^3, (establishment size)^4, West Germany, tenure, tenure^2, 10 wage levels, white-collar worker, female, work council, collective agreement, in-house rate, wages paid above standard rate. *** $p < 0.001$. †: the coefficient for productivity growth is significantly different from unity at the 5 % level. The gray colored rows indicate the range of percentiles where zero nominal wage changes are observed in the data.

As predicted for the percentiles below minus the inflation rate – the 10*th* percentile – I find a coefficient for the inflation rate that is positive and a coefficient for productivity growth that is larger than one. For the percentiles equal to minus the inflation rate – the 20*th* and 30*th* percentiles – I find coefficients for the inflation rate that are below zero and coefficients for productivity growth that are below one. Similar to the results of Chapter 2, I find further positive coefficients only for the very high percentiles – the 80*th* and 90*th* – of the real wage change distribution. As predicted for those percentiles, the coefficients for the productivity growth are larger than unity. The results clearly show that in the presence of DNWR, wage increases are compressed-confirming the finding of Chapter 2 that in Germany, a decrease in the inflation rate leads to a compression of wage increases. Because I find that the inflation rate has similar effects on the real wage change using the LIAB, as I did in Chapter 2 for Germany using the BeH data for the years 1975 to 2007, I am confident that the LIAB is suitable for the analysis despite the shorter time period covered by the dataset.

D Appendix of Chapter 5

D.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 $A = 0 \rightarrow$ 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

D.2 Data Selection Using the Selection Criteria of Martins et al. (2012b)

In addition to the sample selection criteria described in Section 5.2.1 – 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. (2012b). 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[1]. Martins et al. (2012b) 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. (2012b, p. 41) "[…] do not mean, however, to subscribe to [… the] stark description in which firms hire into only a limited number of such jobs, with other jobs filled almost exclusively by internal promotions and reassignments. [.. The] focus on jobs that recurrently show new hires […] is driven mainly by a pragmatic concern – to identify cyclical variation in hiring wages by job, we need those wages to be observed in multiple years spanning different business cycle conditions."

Due to the very restrictive FSC not only a lot of jobs but also a lot of firms are dropped from the original dataset. Table D.1 provides summary statistics and shows the effects of the FSC on sample sizes.

1 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.

Table D.1: **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 D.2 again provides summary statistics and shows the effects of the FSC on sample sizes.

Table D.2: **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

D.3 Data Description and Data Selection – Further Tables

Table D.3: Number of entry jobs and job entrants by year for the dataset with individual real wages without FSC and the drawn sub-sample of this dataset

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

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

Table D.4: Number of entry jobs and job entrants by year for different samples

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

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

Table D.5: 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] (in %)
	Compulsory pension insurance scheme		Lower earnings limit (§ 8, Social Code IV)		Index	Change to previous year (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: Deutsche 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.

D.4 Robustness Checks

To assure the robustness of the results from Section 5.3, I run several additional regressions. Tables D.6 and D.7 show estimated coefficients of the unemployment rate using the FSC dataset (see Appendix D.2). Tables D.8 and D.9 show estimated coefficients of the unemployment rate in slightly altered versions of the baseline models (presented in Tables 5.5 and 5.6), and Table D.8 shows estimated coefficients of the lagged unemployment rate.

Table D.6: Model 1 – estimated coefficients of the unemployment rate ($\hat{\delta}$) 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** (0.38)	−1.03*** (0.35)	−0.88** (0.33)	−0.94*** (0.34)
(1.1) 1st stage unweighted OLS, 2nd stage OLS weighted by number of entry jobs	−0.84*** (0.37)	−1.00*** (0.34)	−0.88** (0.32)	−0.92*** (0.33)

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

Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix D.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.

Table D.7: Model 2 – estimated coefficients of the unemployment rate ($\hat{\delta}$) using individual real wages

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

Note: *** Significant at 1 % level.

Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix D.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. Individual controls used in the 1st stage regression: education, sex, nationality, age, age^2, 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 5.1 and 5.3) change to:

$$\ln(w_{jt}) = \alpha_j + \beta_t + East_{jt} + \varepsilon_{jt} \text{ and} \quad (D.1)$$

$$\ln(w_{ijt}) = \alpha_j + \beta_t + \gamma'\mathbf{x}_{it} + East_{jt} + \varepsilon_{jt}, \text{ respectively.} \quad (D.2)$$

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.

Table D.8: Robustness checks for model 1 – estimated coefficients of the unemployment rate ($\hat{\delta}$) 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 and with a dummy for East Germany in the 1st reg.	−0.72**	−0.93***	−0.78**	−0.85**
	(0.35)	(0.33)	(0.30)	(0.32)
Like (1.1) but 2nd reg. unweighted and with a dummy for East Germany in the 1st reg.	−0.84**	−1.03***	−0.88**	−0.94**
	(0.38)	(0.35)	(0.33)	(0.34)

Notes: OLS regression. Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix D.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 5.5.

Table D.9: Robustness checks for model 1 – estimated coefficients of the lagged unemployment rate ($\hat{\delta}$) 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 with a dummy for East Germany in the 1st reg.	−0.80**	−0.82**	−0.77**	−0.75**
	(0.32)	(0.31)	(0.28)	(0.30)
Like (1.1) but 2nd reg. unweighted and with a dummy for East Germany in the 1st reg.	−0.89**	−0.89**	−0.84**	−0.82**
	(0.34)	(0.34)	(0.30)	(0.32)

Notes: OLS regression. Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix D.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 5.5.

Table D.10: Robustness checks for model 2 – estimated coefficients of the unemployment rate ($\hat{\delta}$) using individual real wages

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

Notes: OLS regression. Robust standard errors in brackets. FSC: "further selection criteria" (see Appendix D.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 5.6.

D.5 Evaluation of the Regression Models – Further Tables

Table D.11: Summary statistics for the differences between individual worker's log real wage and "typical" real wage in job/year

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

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

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

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

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

Abstract

This book contributes to two current issues of discussion on wage rigidity: downward nominal wage rigidity (DNWR) and real wage rigidity of newly hired workers over the business cycle.

The first and major part of the book focuses on DNWR. Chapter 1 provides an overview on causes, extent, and implications of DNWR with a focus on Germany. The second chapter deals with the macroeconomic consequences of DNWR. I show that wage cuts as well as wage increases are compressed in the presence of DNWR. Because of the compression of wage increases, the macroeconomic effects on aggregate real wages are weak. I find that a decrease in inflation of one percentage point only causes an average increase of real wage growth between 0.013 and 0.060 percent. The results indicate that DNWR does not provide a strong argument against low inflation targets. The third chapter analyzes whether or not DNWR affects workers differently, conditional on their characteristics, their position in the wage change distribution, and their employers' characteristics. The results confirm previous results, e.g., that women resist nominal wage cuts less than men (see, e.g., Anspal and Järve, 2011), and new insights are gained, e.g., that blue-collar workers in particular are affected by the compression of wage increases.

The second part of the book focuses on real wage rigidity of newly hired workers over the business cycle. One way of generating realistically cyclical fluctuations in the unemployment rate in the canonical Mortensen-Pissarides model is the introduction of rigid wages into the model. This part of the book contributes to the current discussion on whether or not this assumption can be confirmed empirically. The fourth chapter provides a brief overview on previous research and recent developments concerning the real wage rigidity of newly hired workers over the business cycle. The fifth chapter presents the first empirical evidence for a large economy, namely for Germany, on the cyclicality of real entry-wages while controlling for "cyclical upgrading" and "cyclical downgrading" in employee/employer matches, by introducing firm-job fixed effects in the regressions. The results show that entry-wages in Germany are not rigid, but considerably respond to business cycle conditions. An increase in the unemployment rate of one percentage point leads to about 0.92 to 1.27 percent lower real entry-wages. The results strengthen Pissarides' (2009) dismissal of theories based on cyclically rigid hiring wages.

The third part of the book summarizes the results and offers an outlook for future research.

Kurzfassung

Dieses Buch leistet einen Beitrag zu zwei Themenbereichen der Lohnrigidität, die aktuell in der Wissenschaft diskutiert werden: Abwärtsnominallohnrigidität und Reallohnrigidität neu eingestellter Arbeitnehmer über den Konjunkturzyklus.

Der erste Teil des Buches beschäftigt sich mit Abwärtsnominallohnrigidität. Kapitel 1 bietet einen Überblick über Ursachen, Ausmaß und Implikationen von Abwärtsnominallohnstarrheit. Im zweiten Kapitel werden die makroökonomischen Konsequenzen von Abwärtsnominallohnstarrheit betrachtet. Ich zeige, dass Abwärtsnominallohnrigidität kein starkes Argument gegen die Niedriginflationspolitik der Zentralbanken bietet. Ein Rückgang der Inflation um einen Prozentpunkt bewirkt lediglich eine durchschnittliche Zunahme der Reallöhne zwischen 0,013 und 0,060 Prozent. Das dritte Kapitel untersucht, ob die Wirkung von Abwärtsnominallohnstarrheit auf die Löhne der Arbeitnehmer von Charakteristiken des Arbeitnehmers, ihrer Position in der Lohnänderungsverteilung, und/oder von Charakteristiken des Arbeitgebers abhängt. Die Ergebnisse bestätigen frühere Erkenntnisse, z. B. dass Frauen stärkere nominale Lohnkürzungen hinnehmen (müssen) als Männer (vgl. Anspal and Järve, 2011), und neue Erkenntnis können gewonnen werden, z. B. dass Arbeiter stärker als Angestellte von der Kompression von Lohnerhöhungen betroffen sind.

Der zweite Teil des Buches beschäftigt sich mit Reallohnrigidität neu eingestellter Arbeitnehmer über den Konjunkturzyklus. Eine Möglichkeit, im kanonischen Mortensen-Pissarides Search and Matching Modell realistisch starke zyklische Schwankungen in der Arbeitslosigkeit zu generieren, ist die Implementierung von rigiden Löhnen. Dieser Buchteil ist ein Beitrag zur aktuellen Diskussion, ob die Implementierung rigider Löhne empirisch gestützt werden kann. Das vierte Kapitel bietet einen Überblick bisheriger empirischer Untersuchungen und neuester Entwicklungen. Das fünfte Kapitel stellt die erste empirische Evidenz zur Zyklizität von Einstiegslöhnen, unter Kontrolle der Arbeitnehmer-Arbeitgeber-Partie, für Deutschland bereit. Die Ergebnisse zeigen, dass reale Einstiegslöhne in Deutschland nicht starr sind, sondern wesentlichen konjunkturellen Schwankungen unterliegen. Ein Anstieg der Arbeitslosenquote um einen Prozentpunkt führt zu 0,92 bis 1,27 Prozent niedrigeren realen Einstiegslöhnen. Die Ergebnisse stärken Pissarides' (2009) Ablehnung von Theorien, die auf zyklisch rigiden Löhnen aufbauen.

Abschließend fasst der dritte Teil des Buches die Ergebnisse der ersten beiden Teile zusammen und bietet einen Ausblick für zukünftige Forschung.