Unconditional and Conditional Institutional Effects on Unemployment: A Bayesian Model Averaging Approach

Der Fakultät für Mathematik und Wirtschaftswissenschaften der Universität Ulm
zur Erlangung des akademischen Grades eines

Dr. rer. pol.

eingereichte Dissertation

von
Andreas Sachs
aus
Schorndorf

Datum der Einreichung: 02.05.2012
Tag der mündlichen Prüfung: 19.07.2012

Amtierender Dekan: Professor Dr. Paul Wentges
Referent: Professor Dr. Werner Smolny
Koreferent: Professor Dr. Dr. h.c. mult. Wolfgang Franz

Acknowledgements

First of all I would like to thank Werner Smolny for being an excellent advisor. He always encouraged me to follow my own ideas, but gave me guidance when it was necessary. Several of his remarks and comments improved this thesis substantially.

Second, but not less important, I am grateful to Wolfgang Franz for at least the following two reasons. He gave me the opportunity to work at the *Centre for European Economic Research (ZEW)* where he conceded me enough time to prepare this thesis in addition to the routine work. He also supported me to start working on the relationship between labor market institutions and unemployment, and shared his profound knowledge on labor markets with me.

There are a lot of people who contributed either way to this thesis. My colleagues from the research group *Growth and Business Cycles* Claudia Busl, Jan Hogrefe, Marcus Kappler, Martin Scheffel, Frauke Schleer, and Atılım Seymen helped me a lot with constructive recommendations how to improve this work. Other colleagues, especially Sarah Borgloh and Diana Heger, mainly supported me by providing sympathy and distraction. I appreciate the time we spent in wine villages in Rhineland-Palatinate, at Fontanellas in Mannheim, and on the track. Furthermore, Daniel Siepe and Phillip Buss from Ulm University delivered me a lot of detail information for preparing both thesis text and defense.

My biggest thank goes to Britta. Without her patience and enduring will for unconditional support I would not have finished this work. Thank you for tolerating even the biggest mood swings. I am also grateful to my parents who had always been willing to listen to all the problems related to the preparation of this work, and to my friends for showing me that there is life outside academia.

Abstract

Labor and product market institutions are regarded as central factors for explaining differences in the evolution of unemployment across countries. It is generally assumed that reforms which serve to reduce institutional rigidities are a valuable approach to reduce unemployment.

However, both the theoretical as well as the empirical literature provide inconclusive results to the question of which institutions matter for unemployment, and which reforms help to lower the unemployment rate. A large number of potentially relevant institutional factors makes it difficult to specify the econometric model correctly. Additionally, possible interdependencies between institutions enlarge the set of explanatory factors considerably, which further complicates the derivation of valid estimates.

This thesis clearly identifies six institutional factors which contribute significantly to the explanation of unemployment rates in 17 OECD countries between 1982 and 2005. The main innovation is the application of a Bayesian model averaging approach which allows the clear-cut identification of significant institutions when the number of explanatory factors is high and observations are limited.

Furthermore, this approach also proves to be well-suited for the incorporation of institutional interdependencies which can be estimated systematically for the first time. 22 bivariate interactions are identified as significantly related to the unemployment rate. The outcome of this analysis allows the determination of potential reform effects for various institutions for all 17 OECD countries separately, what is a substantial improvement over the existing empirical as well as theoretical literature.

Contents

1 Introduction **1**

2 Literature review **9**

2.1 Theory 9

2.1.1 Equilibrium unemployment 10

2.1.2 Institutional effects 12

2.2 Empirical evidence 31

2.2.1 Unconditional institutional effects 32

2.2.2 Conditional institutional effects 48

2.3 Summary and formulation of hypotheses 54

3 Methodology and Data issues **63**

3.1 General empirical strategy 64

3.2 Achieving robustness 68

3.2.1 Extreme bounds tests and model averaging 69

3.2.2 Bayesian model averaging 71

3.2.3 Sampling and convergence criteria 80

3.3 Data 84

3.3.1 Unemployment 84
3.3.2 Determinants of Unemployment 87

4 Unconditional institutional effects **105**

4.1 Benefits from the *BACE* approach 105
4.2 Baseline estimation 108
4.3 Alternative prior model sizes 112
4.4 Further sensitivity checks 115
4.4.1 Additional control variables 115
4.4.2 Single model estimation 119
4.5 Discussion and classification 126

5 Conditional institutional effects **139**

5.1 Robust and significant interactions 139
5.1.1 Specifying interaction models 140
5.1.2 Model averaging results 142
5.1.3 Alternative interaction selection strategies 145
5.2 Explanatory power of interactions 147
5.3 Marginal institutional effects 150
5.3.1 Calculating marginal effects 151
5.3.2 Marginal effects for selected countries 157
5.3.3 Economic significance and country heterogeneity 162
5.4 Discussion and classification 163
5.4.1 Testing four hypotheses 164

6 Policy implications **173**

7 Conclusions **187**

A Data sources and construction **195**

A.1 Unemployment 195

A.2 Determinants of Unemployment 196

B Additional results **207**

B.1 Results for alternative selection strategies 207

List of Figures

2.1 The interaction relationship in a wage-setting/price-setting model taken from Bassanini and Duval (2006) 20

4.1 Relationship between active labor market policy spending and consumption tax in 2005 128

List of Tables

3.1 Harmonized unemployment rate 86

3.2 Labor tax system 88

3.3 Bargaining coordination and centralization, and minimum wages 93

3.4 Union density and coverage 94

3.5 Unemployment benefit system 97

3.6 Employment protection legislation 100

3.7 Product market regulation 102

4.1 Correlations between selected institutional indicators 106

4.2 Unconditional effects in the baseline specification 110

4.3 Results for different prior model sizes 114

4.4 Results for different prior model sizes with contemporaneous and lagged shocks 116

4.5 Results for different prior model sizes and inflation rate change . 118

4.6 Robustness checks in a single model 120

4.7 Further robustness checks in a single model 122

4.8 Test statistics for the instrumental variable estimation 124

5.1 Conditional effects for different prior model sizes 143

5.2 Comparing the unconditional with the conditional model 149

5.3 Descriptive Statistics for selected institutions 153

5.4 Calculation of threshold levels in the interaction model 156

5.5 Descriptive statistics of institutions in Germany 158

5.6 Marginal institutional effects for Germany 159

5.7 Marginal institutional effects for France and the United States . 160

5.8 Marginal institutional effects for Japan and Sweden 161

6.1 Interaction effects for simulating the Hartz reforms 175

6.2 Interaction effects for simulating the $EPLt$ reform 183

A.1 Correlation coefficients between all institutions 206

B.1 Conditional effects for interaction term selection with $\rho = 0.9$ . . 208

B.2 Conditional effects for interaction term selection with $\rho = 0.8$ . . 209

Chapter 1

Introduction

The Great Recession in the late 2000's resulted in a sharp unemployment rise in many, but not in all industrialized countries. For example, the unemployment rate in Spain doubled from 10% in 2007 to 20% in 2010. In contrast, Germany achieved to lower its unemployment rate during this period from 10% to 7%. Such substantially different developments of unemployment rates across countries raises the question of the causes of unemployment.

Since cyclical conditions deteriorated to a comparable extent in all industrialized countries, there must be other explanations for differences in the evolution of unemployment. One of the most prominent approaches for explaining unemployment has been to focus on the role of institutions like employment protection or unemployment benefits. It is generally assumed that countries with poorly working labor markets can lower their unemployment rates by conducting reforms to reduce institutional rigidities. Before a successful reform can be carried out it is necessary to know which institutions actually influence unemployment, and which reforms can lead to a declining unemployment rate. In this context, it is crucial to consider that the impact of a reform can also be dependent or, in other words, conditional on country-specific institutional

settings. However, the discussion on which institutions are detrimental for the labor market and which reforms can reduce unemployment is still ongoing. This is due to the fact that numerous institutional factors are suspicious to impact on unemployment. The inclusion of such a large number of factors causes model uncertainty in empirical specifications. This hinders the clear identification of fundamental institutions related to unemployment. The consideration of interdependencies between institutions is impeded for the same reason.

This thesis aims at empirically identifying institutional factors which contribute significantly to the explanation of unemployment rates in 17 industrialized countries between 1982 and 2005. The main innovation is the application of a Bayesian model averaging approach which is expected to allow the clear-cut identification of significant institutions when the number of explanatory factors is high and observations are limited. Within this framework, the explanatory power of institutions is evaluated in different model specifications. For each institutional factor, weighted averages of its respective explanatory power can be constructed. Findings produced with this method are robust to variations in the model specification.

A plethora of macroeconomic contributions have sought to identify unconditional institutional effects on unemployment. However, the results of these empirical studies are conflicting since no agreement addressing the question of which institutions are significantly related to unemployment has been discovered. One central problem involves the large number of institutional indicators which have to be considered. Degrees of freedom are substantially reduced in macroeconomic studies which include all available indicators since observations are limited. Subjectively selecting a limited set of institutional indicators raises the potential risk of omitting relevant explanatory factors.

Both can lead to biased results which renders estimates to be invalid. The findings generated with the Bayesian model averaging approach are expected to be superior to the results of the previous literature through the explicit consideration of model uncertainty. Institutions which are significantly related to unemployment should therefore be unambiguously identified with this method.

A second focus of this thesis is on the role of interdependencies between institutions. Theory predicts that such linkages between institutions exist, but the theoretical literature provides clear-cut predictions for a very limited set of interactions only. Reliable empirical results on conditional institutional effects on unemployment are not available. Such interdependencies between institutions, usually modeled as multiplicative interaction terms, boost the number of potentially explanatory factors considerably. It is hardly feasible to systematically estimate conditional institutional effects in a macroeconomic model. The importance of institutional interdependencies for the labor market is therefore unclear from an empirical perspective. The Bayesian model averaging approach is expected to be particulary well-suited for the empirical analysis of such institutional interdependencies, as it allows the incorporation of much more available interactions. Model misspecification can be avoided by taking model uncertainty into account. The investigation of interdependent institutions can contribute to a better understanding of institutional effects. It is assumed to clarify which combinations of institutions are responsible for high unemployment. Consequently, the findings of this thesis enable to design institutional reforms in a way that allows for success in specific institutional environments. In the following, the thesis structure is briefly outlined.

As dealing with theory is essential to understand how institutions are linked to labor market performance, a simple theoretical model explaining unemployment is introduced at first in chapter 2. Based on this theoretical model, key

mechanisms explaining how institutions affect unemployment are given and the institutional categories relevant for this work are discussed in a consecutive order. The main focus of this section is on the unconditional impact of institutions, but since institutions do not work in isolation, theoretical mechanisms which are able to explain institutional interactions are considered accordingly.

Empirical research can add substantial information about which of these theoretical mechanisms prove to be important. Thus, the section on theory is complemented by a comprehensive review of the empirical literature on institutions and unemployment. Here, predominantly macroeconomic contributions are considered, but microeconomic studies are also taken into account when beneficial. The main findings on both unconditional as well as conditional institutional effects are discussed and limitations of the literature are highlighted. In this context, it is particularly important whether results are conclusive across studies. The chapter concludes with a summary of the main findings of both theoretical as well as empirical studies, and with the formulation of testable hypotheses for the empirical investigation.

The *Bayesian Averaging of Classical Estimates (BACE)* approach, which combines a pure Bayesian setting with classical estimation techniques, is explained in detail in the first part of chapter 3. The general empirical strategy necessary for the application of the *BACE* approach to a panel data set is outlined, and shortcomings are mentioned. After a brief description of the history of model averaging techniques, the basic Bayesian econometric theory is provided in order to understand the mechanism of the *BACE* approach. A description of details concerning the application of the method and specification in the context of the empirical investigation concludes the econometric discussion.

An overview on data selection, construction, and meaning for all variables is given in the second part of chapter 3. Descriptive statistics are presented for the unemployment rate as the factor of interest and its appropriateness as a measure of labor market performance across countries is discussed. Fundamental evolutions over time are described for all 17 countries, and striking developments both across countries as well as over time are highlighted. The indicators for each institutional category are displayed consecutively, and problems concerning indicator quality are pointed out. The chapter concludes with displaying macroeconomic variables which are necessary control factors for the empirical analysis.

After having described hypotheses, method, and data, the empirical analysis can be conducted. The focus of chapter 4 is on the clear identification of unconditional institutional effects on the unemployment rate. A baseline strategy for the application of the *BACE* approach is specified and the corresponding estimations are carried out. Both significance as well as direction of impact of all institutional indicators can be identified. While the method is assumed to be particularly suited to provide robust results independent of which institutional indicators are included, it may not be immune to other changes. The baseline findings are therefore further assessed in terms of robustness. Alterations of the control variables, the econometric specification, and the sample size are employed in order to evaluate whether the results are driven by specification error, outliers, or omitted control factors. The unconditional results produced with the *BACE* approach are then assessed in light of the particular hypotheses derived from the literature review in chapter 2. Improvements over previous results as well as limitations of the findings are emphasized. Based on this outcome, the economic significance and the resulting relevance for policy-makers is delineated.

The unconditional findings of chapter 4 provide average institutional effects across countries, but the relevance for concrete policy recommendations derived from these findings is limited since interdependencies between institutions render possible country-specific institutional effects. Hence, chapter 5 aims at extending the unconditional model such that interdependencies between institutions can be taken into account. The econometric model explaining unemployment is enhanced to allow for bivariate institutional interactions. Once the basic interaction model is set up, robust and significant institutional interactions are identified with aid of the *BACE* approach. The sensitivity of the findings is evaluated by altering the previously defined set of interactions. In doing so, risks related to changes in the interaction selection can be identified and considered accordingly. The so produced results on conditional effects can be used to determine institutional influences on unemployment for each of the 17 countries separately.

Marginal effects are presented for selected countries. The country-specific impact of institutions and potential reform options are discussed in the light on these results. At the same time, it is examined whether institutions exhibit a complementary or a substitutive relationship. This relates to the question of potential benefits and drawbacks of joint reforms in a particular country, and is a first step towards designing country-specific reform packages. A comparison of the findings with the hypotheses of chapter 2 and the evaluation of their economic significance completes the chapter.

The empirical findings of chapter 5 set up the framework necessary to evaluate the impact of institutional reforms on the unemployment rate. However, the reliability of the interaction model could be limited due to data measurement error or unobserved heterogeneity. Chapter 6 therefore conducts a simple empirical test of the validity of the interaction model. The impact of a Hartz

IV reform component recently introduced in Germany is assessed with help of the marginal effects calculated in chapter 5. The Hartz IV reforms have mainly changed the German unemployment benefit system. By means of the findings of the interaction model it is simulated what effect the corresponding change in the indicators of this institutional category has had on the unemployment rate in Germany. This simulated impact is then compared to existing evidence on the influence of the Hartz IV reform component. Such a descriptive analysis can give important insights on the reliability of the results produced with the *BACE* approach. Finally, chapter 7 concludes.

Chapter 2

Literature review

This chapter deals with the discussion of the literature on institutional effects on unemployment. The first part is dedicated to key theoretical mechanisms of institutional influence on unemployment. The second part is a review of the corresponding empirical literature. The chapter concludes with a summary and illustration of main hypotheses as the basis for the empirical analysis.

2.1 Theory

A simple theoretical macroeconomic framework is described, which serves for the analysis of the relationship between unemployment and institutions. After introducing the concept of equilibrium unemployment, theoretical mechanisms of institutional influence on unemployment are discussed separately for each institutional category. The focus is on unconditional institutional effects. However, it is implausible to assume that institutional factors merely act in isolation. Conditional effects of institutions on unemployment are likely. An institutional effect on unemployment then depends on one or more institutional

factors. Theoretical mechanisms able to describe such conditional effects are added to the respective institutional category which appears first.

2.1.1 Equilibrium unemployment

There is no general framework available that delivers a fully specified economic model capturing the several institutional effects on the labor market. The simple model presented here can help explain the main features of institutional characteristics and how changes therein may impact the labor market.

The basic model setup is taken from Layard et al. (2003) where an extensive examination of several implications and alterations of the basic model is provided, and Franz (2005). Unemployment is affected by the wage- and price-setting of market actors; that is firms and employees. Firms make decisions in regards to their product prices by setting a mark-up on expected wages. Workers demand nominal wages as a mark-up above expected prices. The corresponding price- and wage-setting equations are[1]

$$\Delta(p - w^*) = \beta_0 - \beta_1 u \qquad (\beta_0, \beta_1 \geq 0) \tag{2.1}$$

and

$$\Delta(w - p^*) = \gamma_0 - \gamma_1 u \qquad (\gamma_0, \gamma_1 > 0). \tag{2.2}$$

Δp^* and Δw^* are the change of the log of expected price and wage levels, which are consistent with a constant inflation rate. Δp is the log of actual price changes set by the firm, Δw is the log of the actual wage change, and u

[1]It is assumed that the wage-setting and price-setting curves are adequate representations of labor supply and labor demand, respectively.

is the unemployment rate. By using equations (2.1) and (2.2) it can be derived that

$$u = \frac{\beta_0 + \gamma_0 - \Delta(p - p^*) - \Delta(w - w^*)}{\beta_1 + \gamma_1}. \tag{2.3}$$

Equilibrium is defined by assuming that the expected price change Δp^* is equal to the actual price change Δp, and that the same holds true for wages, i.e. $\Delta w^* = \Delta w$. Equilibrium unemployment u^* at a constant inflation rate can be described as

$$u^* = \frac{\beta_0 + \gamma_0}{\beta_1 + \gamma_1}. \tag{2.4}$$

Although ramifications of the model considering different situations with $\Delta w^* \neq \Delta w$ or $\Delta p^* \neq \Delta p$ are conceivable, the focus here is on the determination of the equilibrium unemployment rate displayed in equation (2.4). Factors that affect β_0, β_1, γ_0 or γ_1 influence equilibrium unemployment. Increases in β_0 and γ_0 mean rising demand concerning the respective mark-up of the firm or the worker. Such adjustments do result in higher equilibrium unemployment if the inflation rate remains constant and if the other party is not willing to reduce its mark-up correspondingly. Increasing wage or price flexibility, expressed by rising values of β_1 or γ_1, and called real wage flexibility by Layard et al. (2003), push equilibrium unemployment down. Determinants of the real wage flexibility term measure how real wages respond to a change in unemployment. Examples for such determinants are employment protection or bargaining coordination, which directly impact the wage flexibility by affecting wage adjustments.

From equation (2.2) it is clear that if unemployment rises the mark-up of wages over prices goes down. Similarly, the mark-up of prices over wages drops if unemployment increases. Labor and product market institutions only indirectly appear in the model as determinants of the four parameters of equation (2.4). For example, the wage effect of unions endowed with a strong bargaining position appear in the wage-setting equation (2.2) in γ_0. In other words, unions push real wages up. When firms are not willing or not able to react through a mark-up reduction in prices, equilibrium unemployment will increase. Similarly, a lower degree of bargaining coordination decreases γ_1 and, thus, the flexibility to adjust wages. In the following, the theoretical mechanisms belonging to each institutional factor, which appears in the empirical estimations of chapters 4 and 5, will be discussed and set in relation to the basic model displayed here. When necessary it will be abstracted from the basic model and the respective mechanism is explained more generally. Additional unconditional mechanisms as well as theories explaining interdependencies between institutions are also discussed.

2.1.2 Institutional effects

Labor tax system

Formally, a tax increase can affect both the price-setting as well as the wage-setting equation (2.1) and (2.2) via higher values of β_0 and γ_0, respectively. When an income tax increase results in higher wage demand of workers, firms would have to reduce their mark-up in order to keep unemployment unaffected. If firms are not willing to accept such a mark-up reduction, unemployment will rise. Alternatively, a payroll tax increase directly affects labor making it more costly from the firm perspective. Again, the two possible consequences are a

mark-up reduction (since β_0 rises) consistent with stable unemployment, or a price increase and higher unemployment.[2]

The extent of influence of both price- and wage-setting is up to their particular elasticities. Wage-setting is affected when the real wage income relative to the outside option changes. This occurs when the gap between the real wage and unemployment benefits diminishes. Let us assume that a worker with a wage marginally above its reservation wage suffers from an income tax increase. If its real wage is now below its reservation wage, working is no longer the best option. Especially when high unemployment benefits drive up the reservation wage, labor supply can be substantially affected. The price-setting depends on the labor costs a firm has to face. If these costs grow due to a tax increase, the hiring and firing decision of the firm is affected, especially when the additional costs cannot be carried over to a price increase due to strong competition or real wage inflexibility.

Daveri and Tabellini (2000) state that the effect of a labor tax increase is linked to the degree of the workers' bargaining power. Whether labor supply or demand are mainly affected depends on who bears the majority of the costs. Higher labor taxes influence labor demand if the workers' bargaining power is high, thus influencing labor supply if the workers' bargaining power is low. It could also depend on the degree of bargaining coordination. Strong unions or workers could accept a reduction in real wages when the coordinative dimension highlights the risk of an increase in unemployment. Labor taxes should therefore theoretically interact with indicators of the bargaining system. In the literature, it is sometimes argued that a cost shift to workers is preferable

[2]Layard et al. (2003) state that taxes cannot have a long-run impact on unemployment since in the long-run the tax burden is borne by the workers and labor demand is unaffected. This might be a too simplified assumption if, for example, labor supply is reduced in the long-run due to real wage decreases caused by high taxes.

to avoid an unemployment increase. Hence, labor supply is assumed to be less elastic compared to labor demand. An empirical validation of this assumption is still missing and might even be impossible for macroeconomists given that the elasticities can change in relation to each country and over a period of time.

Additionally, shifting a tax increase completely to the workers might leave unemployment unaffected in the short-run, but the decline in real wages could result in a reduction of total demand and, finally, labor demand. If the tax income partially serves as funding for, say, qualification measures for unemployed to reduce the duration of unemployment or to facilitate labor market integration of the long-term unemployed, taxes can indirectly help to reduce unemployment. However, this aspect is generally not considered strong enough to compensate the negative tax effects on labor supply and demand.

Bargaining system

Wage bargaining, which directly affects the wage-setting equation through affecting both γ_0 and the wage flexibility γ_1, is determined by the behavior of the negotiants, i.e. the employers and the employees. The latter party is usually organized in trade unions which bargain over the wage for their members. In the following, the main aspects of the bargaining process will be briefly characterized. According to Layard et al. (2003), the bargained wage, which is a solution to the Nash bargaining problem $\Phi = ((W_i - A)S_i)^{\beta}\Pi_i$ with respect to W_i, can be characterized by[3]

$$\frac{\beta}{W_i - A} + \frac{\beta}{S_i}\frac{\partial S_i}{\partial W_i} - \frac{N_i}{\Pi_i} = 0. \tag{2.5}$$

[3]See Layard et al. (2003), page 102, for a derivation and discussion of the following.

W_i is the real wage set in firm i, A is the workers wage alternative if he loses his job, N_i is the level of employment in firm i, S_i is the survival probability of a worker in firm i, Π_i are the profits of firm i, and β is a parameter measuring the union bargaining power. Equation (2.5) can be transformed to

$$\frac{W_i - A}{W_i} = \frac{1 - \alpha\kappa}{\epsilon_{SN} + \frac{\alpha\kappa}{\beta}}. \tag{2.6}$$

κ gives the degree of product market competition, ϵ_{SN} is the elasticity of the survival probability S with respect to employment N both in firm i, and α is the output elasticity with respect to labor stemming from a Cobb-Douglas production function for firm i.[4] Equation (2.6) shows that the mark-up of wages over the wage alternative A is an increasing function of the union power β and a decreasing function of the degree of product market competition κ and the output elasticity α. The higher union power β in firm i the higher the bargained wage over the alternative wage. The theoretical impact of β on unemployment can be analyzed by further defining the alternative wage as

$$A = (1 - \varphi u)W^a + \varphi u B. \tag{2.7}$$

$(1 - \varphi u)$ is the re-employment probability with the alternative wage W^a after losing a job, and B represents absolute unemployment benefits. Now it is assumed that the alternative wage is equal to the actual wage and $W^a = W_i$. Furthermore, the firm-specific wage is assumed to be equal to the aggregate

[4]The production function as well as the construction of the product market competition measure will be explained in greater detail when dealing with the impact of product market competition on unemployment.

wage, hence, $W_i = W$.[5] Solving (2.7) for $\frac{W-A}{W}$ and inserting in (2.6) delivers

$$u^* = \frac{1 - \alpha\kappa}{(\epsilon_{SN} + \frac{\alpha\kappa}{\beta})(1-b)\varphi}. \tag{2.8}$$

Equation (2.8) shows that equilibrium unemployment u^* increases with union power β. Stronger unions demand higher wages, inducing firms to react via an employment adjustment to the determined wage.[6]

A minimum wage also influences the mark up $\frac{W_i-A}{W_i}$ in equation (2.6) by setting a lower ceiling for W_i. Given that the unemployment benefits b are independently set of the minimum wage, the mark-up is an increasing function of the minimum wage. Stigler (1946) argues that minimum wages reduce labor demand and finally increase unemployment by setting wages above a level which is justified by the workers' productivity. Thus, especially low-skilled workers with wage payments below a (hypothetical) minimum wage are blocked out from the labor market. If the minimum wage is set too low such that the actual wage payments are already above the minimum wage, both wages and unemployment remain unaffected.

However, Manning (1995) shows in a model of shirking that minimum wages can decrease unemployment by lifting wages to a level at which shirking is less likely due to the increased incentive to work. Similarly, a minimum wage also increases the gap between the achievable wage for work and the transfer payment for being unemployed. As a result, people move from being out of the labor force into the labor force. Labor supply goes up since working becomes financially more attractive.

[5] It is assumed that the firms are identical and wages are equal over firms.

[6] This holds true if the replacement rate $b = \frac{B}{W}$ is exogenously set by the government.

The level at which bargaining takes place is of importance, as well. One of the most prominent studies, published by Calmfors and Driffill (1988), establishes the hypothesis of a hump-shaped relationship between the bargaining centralization and the level of unemployment. The authors argue that in highly centralized countries, unions with a high bargaining power take the negative consequences of real wage increases into account.[7] This corresponds to an increase in γ_1 in equation (2.4). Hence, unions prefer to reduce overall unemployment at the expense of lower real wages. With decentralized bargaining, unions do not have enough market power to enforce considerable wage increases. Decentralized bargaining also renders it possible for workers to be paid according to their individual productivity since no aggregate or centralized wage is set.

Both situations bring about reasonable wage claims. The less beneficial situation is medium centralized bargaining, where unions have sufficient bargaining power for wage increases without considering the negative macroeconomic consequences of their actions. However, Flanagan (1999) mentions that the positive effects of decentralized bargaining only apply in a closed economy. The ability of firms to pass on wage increases to the consumers is higher than in an open economy where competition from abroad additionally accrues. In this case, centralized bargaining is the most beneficial situation.

Concerning interdependencies, the bargaining system is one of the theoretically most important factors. The main mechanisms for interactions including this institution are described in the following. While a potential interaction with the labor tax system has already been highlighted, a connection to product market regulation is also possible. Griffith et al. (2007) claim that de-

[7]Some studies use bargaining coordination for describing the level at which bargaining takes place. This issue is further discussed in section 3.3.

creasing competition lowers unemployment more in countries with high union bargaining power. Both factors are thus expected to be substitutive. The stricter one factor is regulated, the more beneficial a deregulation of the other factor becomes.[8] The argument for this is that strong unions prescind from excessive wage claims for the benefit of higher employment. In the case of product market reform, moderate wages lead to greater price decreases as well as output increases compared to the situation with low union bargaining power. Fundamental for this argument is the ideology that unions care both about wages and employment. This is opposed to the aforementioned unemployment-increasing union power mechanism.

Baccaro and Rei (2007) apply the same line of reasoning to interactions including bargaining coordination. They claim that coordination interacts with all other institutions which have a detrimental impact on unemployment. The larger coordination within an economy, the more likely it is than an internalization of negative externalities of both employers and unions will occur. Without coordination, the externalities would push unemployment upwards.[9]

A different line of reasoning with the same outcome as Griffith et al. (2007) and Baccaro and Rei (2007) is given by Fiori et al. (2012). They point out that with strong union power, wages are too high to guarantee full employment. In such a situation, reforms fostering competition and reducing firm profits, and the corresponding union share can generate large employment gains. Nominal wages are expected to fall when competition increases. This leads to a new equilibrium with higher employment. In contrast, when wages are set such that

[8]More beneficial could also mean less detrimental. Let assume that a deregulating reform leads to a rise in unemployment. Then, for substitutive institutions, unemployment would increase less when the conditioning factor is highly regulated. In contrast, complementary institutions are most beneficial when the level of regulation of both interacting factors converge.

[9]Whether union power as in Griffith et al. (2007) or coordination as in Baccaro and Rei (2007) is the crucial factor for moderate wage demands needs to be answered empirically.

employment is close to the market clearing level, an increase in competition will dissipate, and at most small employment gains are created. This argument could be extended to further institutional interactions. Generally, reforms that increase labor supply or labor demand level are especially successful if labor supply or demand and, thus, the level of employment, are at low levels due to the strict institutional regulation.

Bassanini and Duval (2006) argue that decentralized bargaining is consistent with an elastic or flat wage-setting curve (concerning the high employment adjustment elasticity with respect to real wage changes). A reform that leads to a rise in the level of the price-setting curve (a reduction in product market regulation, for instance) is then more beneficial with decentralized bargaining and a flat wage-setting curve than with the alternative, centralized bargaining. The reason for this is that with decentralized bargaining employment gains through a reform shifting the price-setting curve upwards do not transform into substantial or excessive wage increases from the worker side.

A contradictory argument also given by Bassanini and Duval (2006) states that insiders set the highest possible wages at a medium centralized bargaining level. The level of unemployment only marginally affects their wage-setting decision. The resulting flat wage-setting curve makes deregulating reforms (linked to an upward shift in the level of the price-setting curve) more beneficial in terms of employment gains. While both theoretical arguments emphasize that the level of centralization is an important conditioning factor, it is ambiguous whether decentralized or medium centralized bargaining leads to a flat wage-setting curve.

The interaction argument above can then be extended to other institutions. Bassanini and Duval (2006) claim that not only the level of centralization but all factors shaping the wage-setting curve interact with determinants of

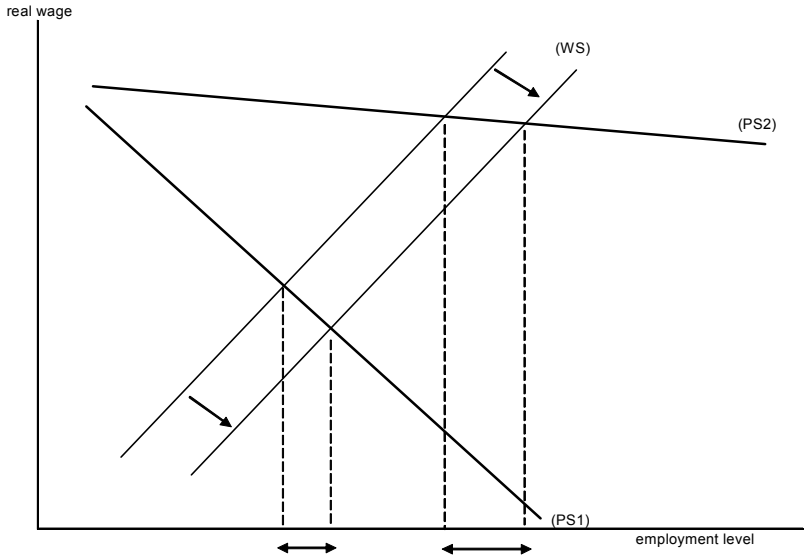


Figure 2.1: The interaction relationship in a wage-setting/price-setting model taken from Bassanini and Duval (2006)

the price-setting curve. A flat wage-setting curve makes reforms resulting in a lifted price-setting curve and increased labor demand more beneficial. The crucial assumption for this is that higher labor demand does not lead to a sharp real wage rise such that the additional employment potential is exploited. This argument can also be reversed. An downward shift of the wage-setting curve is also assumed to be more beneficial if the price-setting curve is flat. In such a case, the increased labor supply expressed by such a downward shift of the wage-setting curve causes real wages to fall. This results in higher employment gains if the price-setting curve is flat such that labor demand reacts sharply to the real wage drop. Such a relationship between the price-setting and the wage-setting curve is demonstrated in figure 2.1. This illustration is taken from Bassanini and Duval (2006). It is shown that a downward shift of the wage-

setting curve (denoted WS) has larger employment effects if the price-setting curve is flat (denoted $PS2$).[10]

Given that deregulation leads to either a flattening of or a fall in the wage-setting (a rise or flattening of the price-setting curve), determinants of the wage-setting curve are expected to interact with determinants of the price-setting curve in a complementary manner.Note that a flat wage-setting or price-setting curve is also beneficial for deregulating reforms within the same category. In other words, a reform which lifts the price-setting curve is more beneficial if the price-setting curve is flat. In the same way, a reform resulting in a fall of the wage-setting curve is more beneficial if the wage-setting curve is flat.[11] Which institutions are related to wage- and price-setting depends on the exact model specification. Belot and Van Ours (2004) describe such a model in the following way. Indicators for unemployment benefits, employment protection, bargaining power, and product market regulation affect the wage-setting curve. Indicators for employment protection, labor taxes, and product market regulation are determinants of the price-setting curve. This classification is adopted for the construction of hypotheses in regards to institutional interdependencies in section 2.3 and chapter 5.

[10]Note that the mechanism of Fiori et al. (2012) predicting substitutive institutions fits well in the wage-setting/price-setting model once a convex wage-setting curve is assumed. According to Layard et al. (2003), page 381, such a convex style very likely. The argument is that short-term unemployed have a higher search intensity than long-term unemployed. If the number of short-term unemployed is large, this makes real wages fall since competition for a job is high. If it is now assumed that this situation mainly occurs when overall unemployment is high, a convex wage-setting curve can be derived.

[11]This argumentation fits well to the model of Coe and Snower (1997) who provide a simple framework for analyzing several interactions between distinct regulatory characteristics. The authors argue that reforms of the unemployment benefit system, the employment protection legislation, the union bargaining power, the cost of job search, and the barriers to entry in the product market are complementary in the sense that jointly deregulating them would yield the highest employment gains. This occurs due to the ability of joint reforms to gather political support, and due to the reinforcing character of joint reforms.

Unemployment benefit system

The unemployment benefit system affects unemployment via its effect on the search behavior of firms and workers. Layard et al. (2003) defines H as the number of unemployed that find a job in a given period. c is a factor that characterizes job search effectiveness. The probability of finding a job in a given period is $\frac{H}{cU}$, with U being the number of unemployed. N is the number of employed people and s is the share of people that lose their job in this period. Since the number of hired (H) and fired (sN) people must be equal in equilibrium, it holds that

$$\frac{H}{cU} = \frac{s}{c\frac{U}{N}}. \tag{2.9}$$

u can be used as an approximation to $\frac{U}{N}$ and equation (2.2) can then be extended to[12]

$$\Delta(w - p^*) = \gamma_0 - \gamma_1(\frac{cu}{s}). \tag{2.10}$$

Equation (2.10) shows that a more efficient job search lowers the wage mark-up since competition for vacancies is stronger. This permits the reduction of unemployment given that the price mark-up remains unaffected.

The search effectiveness in turn is determined by several factors; one of them being the unemployment benefits. Factor c increases with the search intensity of the unemployed. The search intensity is, amongst other things, affected by the difference between the wage of a job i w_i and a worker's reservation wage w_R. The latter is the wage at which the unemployed is indifferent between working and being unemployed. More formally, if $w_i > w_R$ the unemployed will

[12]This is taken from Layard et al. (2003), page 35.

accept a job opportunity. The reservation wage is determined by the expected future income and the cost of searching. These search costs are in turn affected by the unemployment benefits, as they improve income during unemployment. It holds that the reservation wage increases with the unemployment benefits. Hence, higher benefits increase the reservation wage, lower the search intensity, and eventually reduce the search effectiveness c.

The unemployment benefit scheme may have a positive impact on the labor market via other channels as well. One prominent unemployment benefit effect is the increase of labor supply due to a benefit rise as shown by Mortensen (1977). The unemployed not eligible for unemployment benefits want to cash in on an increase in unemployment benefits. They intensify their search activity or lower their reservation wage in order to receive at least the governmental transfer payments. Given that the benefit payments depend on having been employed for some time, labor supply is indeed lifted. Whether this results in lower or higher unemployment depends on the exact design of the unemployment benefit system. In case of a generous system, the unemployed not eligible for benefits have a high incentive to search for a job in order to qualify for benefit payments in case of future unemployment. Labor supply can also be increased when persons from out of the labor force want to qualify for benefit payments and move into the labor force. If this switching from out of the labor force into the labor force allows for eligibility of benefits without the pressure to search for a job, it is more probable that unemployment, expressed as the percentage of the unemployed to the total labor force, increases.

Three additional theoretical mechanisms militate against the orthodox view of a detrimentally generous unemployment benefit system. First, according to Holmlund (1998), increased benefits can push the productivity of employees upwards since unemployment benefits are usually a percentage of the last wage.

Second, Acemoglu and Shimer (1999, 2000) highlight the beneficial influence of unemployment insurance if workers are risk averse. Benefits, which are the outside option for the employees, serve as an insurance against unemployment. Risk averse workers prefer to search jobs associated with a low risk of getting offset, but also with lower wages and productivity. Firms adjust to these preferences and provide less risky, but also less productive jobs. If unemployment benefits are high enough to provide an acceptable substitute for the previous wage, workers are willing to take jobs associated with higher unemployment risk but also higher wages, higher job quality and, eventually, increased output and lower unemployment.

The third aspect, discussed in Arulampalam (2001), pertains to the permanent negative effects of periods of unemployment. It is argued that periods of unemployment can reduce the job finding probability. A worker who becomes unemployed loses job-specific human capital. This decreases his opportunities of finding a job. The fear of losing the job can convince workers to disregard the insurance dimension of high unemployment benefits and to accept job offers for which the wage is below the reservation wage. This increases the probability to remain employed and prevents them from experiencing a period of unemployment. Consequently, the benefit level effect on the job search and wage bargaining behavior should vanish.

In terms of interdependencies, the link between unemployment benefits and employment protection is discussed by Blanchard and Tirole (2008). In this contribution, it is claimed that financing unemployment benefits should be carried out through layoff taxes instead of payroll taxes, as it is practiced in several European countries in order to be beneficial. Then, the question of whether a layoff is economically reasonable is answered by the firm which decides upon the trade-off between paying the wage or the layoff tax. If un-

employment benefits are financed through payroll taxes and if there are no layoff taxes, the firm does not face any direct costs for a dismissal. Hence, unemployment benefit costs caused by a dismissal are externalized. In order to internalize such costs, high unemployment benefits should be accompanied by high layoff taxes, which are used to finance unemployment benefits.

Employment protection

Employment protection can be described as norms and regulations that avoid or hinder the dismissal of a worker, by imposing severance payments or firing taxes on the firm to the dismissed worker or to a third party.[13] The implicit or explicit costs of a dismissal have two main effects on the employers' labor demand (for a more extensive discussion see Ljungqvist (2002)). First, the flow out of employment into unemployment will be reduced due to the potential costs associated with a dismissal. Second, the flow from unemployment into employment will be lowered as well since employers incorporate the potential firing costs when deciding upon opening a vacancy. Theoretically, it is not clear which effect prevails.

Employment protection can affect the wage-setting equation (2.2) by giving employees the opportunity to demand higher wages through a stronger bargaining position. However, if firms consider the potential costs of a dismissal or the potential wage increase, prices will be set such that the price mark-up remains stable. The impact of employment protection also depends on whether potential firing costs are taken into account by the firms when bargaining the wage. Cahuc and Zylberberg (2004) show that if firms involve the price for a dismissal in the hiring costs, the negative effect on the creation

[13] Boeri and Van Ours (2008) mention a lawyer as an example for such a third party.

of jobs is reduced. Employees then indirectly pay for the possible costs of their dismissal.

Some further aspects surrounding the impact of employment protection should be mentioned. Under the assumption that newly filled jobs have a maximum value of productivity, Cahuc and Zylberberg (2004) argue that a high degree of employment protection reduces the overall productivity level since necessary or beneficial labor force adjustments are not carried out. This rests on the assumption that newly created jobs are accompanied by a technology level that is above the level of existing jobs, due to the exploitation of the latest innovations, for instance.

Franz (2005) mentions the beneficial effects of employment protection for the human capital investment decision. Workers who know about the employment stability of their job are probably more willing to accept training courses closely related to their job. Additionally, a stable employment relationship creates incentives for firms to invest in the qualification of their employees.

An appealing mechanism to describe the influence of employment protection on unemployment is characterized by Ljungqvist and Sargent (1998, 2007, 2008) in a series of papers. They provide a theoretical explanation as to why European unemployment has been persistently above unemployment in the United States during the 80's and 90's. According to these studies, strict employment protection (expressed as high firing costs in the model) is especially harmful if economic turbulence is high. The concept of turbulence corresponds to the loss of skills and earning potential after a job dismissal. Turbulence is high if a worker's earning potential drops considerably after involuntarily losing his job.

The observation that most of European unemployment is long-term unemployment is combined with the claim that turbulence and, thus, considerable skill losses subsequent to involuntary dismissals, has increased from the 60's to the 80's and 90's. The authors argue that low employment transition rates associated with high employment protection in Europe have made the problem of long-term unemployment persistent. In contrast, a less restrictive employment protection legislation in the United States has been less detrimental seeing as the probability of getting easily re-employed is higher. This in turn avoids considerable skill losses. A high level of employment protection then corresponds to high unemployment.[14] However, the turbulence concept is not fully accepted in the literature. Nickell et al. (2005) highlight some points of critique, one of them being large differences and distinct evolutions of unemployment inside Europe, despite similar levels of employment protection and unemployment benefits.

Finally, Boeri and Garibaldi (2007) claim that a two tier reform of employment protection can have transitional beneficial effects on the labor market by fostering job creation and increasing employment. Such a two-tier reform is characterized by a reduction in the job protection of temporary workers while leaving the job protection of regular workers untouched. According to this, employment protection for regular and for temporary jobs are assumed to show a substitutive relationship. It is, however, important to note that the study does not address any medium- or long-run effects of such a two-tier reform scheme.

[14]Actually, the authors construct the model including both employment protection and unemployment benefits in their framework. Europe can be characterized by high unemployment benefits and strict employment protection. Hence, the story can be applied to the impact of unemployment benefits, as well.

Product market regulation

Product market competition is supposed to affect the wage bargaining process and, consequently, unemployment via the firms' profit share. Following Layard et al. (2003), suppose that the production technology is a Cobb-Douglas production function with[15]

$$Y_i = N_i^{\alpha} K_i^{1-\alpha} \tag{2.11}$$

and product demand follows

$$Y_i = (P_i)^{-\eta} Y_{di}. \tag{2.12}$$

Y_i corresponds to product demand, N_i is employment, K_i is the capital stock, Y_{di} represents a demand index, all variables with respect to firm i. α is the output elasticity with respect to labor, and η is the elasticity of product demand with respect to the price. The latter elasticity is used later to calculate a measure for product market competitiveness. It holds that the relative profit share is given by $\gamma = \frac{\Pi_i^e}{W_i N_i^e}$ with Π_i^e being expected profits and N_i^e expected employment of firm i, and that $\Pi_i = \frac{1-\alpha\kappa}{\alpha\kappa} W_i N_i$. The profit share can be set in relation to competitiveness as

$$\gamma = \frac{1-\alpha\kappa}{\alpha\kappa} \tag{2.13}$$

where $\kappa = 1 - \frac{1}{\eta}$ is the measure for product market competitiveness.[16][17] What can be learned from equation (2.13) is that the relative share of profits is a

[15] The following exposition is taken from Layard et al. (2003), pages 99-107.
[16] This of course only holds if expectations are fulfilled.
[17] Hence, the competitiveness κ is an increasing function of the output elasticity η.

decreasing function of product market competitiveness. As already displayed in equation (2.6) the wage mark-up over the alternative wage can be described as

$$\frac{W_i - A}{W_i} = \frac{1 - \alpha\kappa}{\epsilon_{SN} + \frac{\alpha\kappa}{\beta}}.$$

From this equation it can be seen that the wage mark-up over the alternative wage is a decreasing function of competitiveness. The lower the competition, the higher the firms' profit share and the higher the workers' wage mark-up. The negative consequence of low product market competition can be displayed with help of the following equilibrium unemployment equation

$$u^* = \frac{1 - \alpha\kappa}{(\epsilon_{SN} + \frac{\alpha\kappa}{\beta})(1 - b)\varphi}. \tag{2.14}$$

The lower the competition κ, the higher equilibrium unemployment.[18] ϵ_{SN} is the elasticity of the survival probability S with respect to employment N, and β is the workers' bargaining power.

In other words, less competition gives firms in a market the opportunity to gain a higher mark-up in comparison with a more competitive market. Workers receive a larger portion of this mark-up and a higher wage. The increased wage reduces labor demand and, consequently, drives unemployment up. Competition can be increased by, for instance, reduced market entry barriers or the privatization of public enterprises. The incumbent firms suffering from a lower profit share as pointed out in equation (2.13) are then forced to reduce costs in order to remain competitive.

[18] Layard et al. (2003) point out that this holds if unemployment benefits b are set exogenously by the government.

While the theoretical literature is fairly consistent in regards to this competition effect, Ebell and Haefke (2009) introduce a countervailing overhiring effect as a consequence of high product market regulation. Overhiring refers to the assumption that firms employ workers even when marginal costs exceed marginal productivity. In this model, all workers are as equal as the marginal worker due to individual bargaining between firm and workers. This is due to the fact that firms have the opportunity to permanently bargain over the wage with any worker. Increasing employment reduces the marginal product of labor. The reason for this is that returns to scale decrease with rising employment. Since firms pay the marginal worker according to the marginal costs of hiring, wages of all workers can be then reduced in the individual bargains. In other words, increasing employment reduces the individual bargaining power due to the diminished (hypothetical) marginal productivity of the marginal worker. With rising competition, overhiring diminishes because of the additional job alternatives for all workers. Ebell and Haefke (2009) conclude that product market regulation increases unemployment in their model, but the effects are dampened by the overhiring effect.

Much less theoretical research has dealt with the potentially distinct impact of several dimensions of product market regulation. Blanchard and Giavazzi (2003) point out that lowering the barriers to entry and increasing competitiveness in a market by, for instance, privatization can have distinct effects on unemployment. Increasing competition without lowering entry barriers leads to a situation where in the long-run firms that leave the market are not replaced due to high entry costs and lower profitability. As a result, unemployment is not reduced. In contrast, a deregulation in conjunction with or caused by lowered entry barriers has beneficial long-run effects on the labor market. This underlines the interdependency between entry barriers and other factors of product

market regulation. Furthermore, Schiantarelli (2008) mentions that increased competition can simplify monitoring of managers through better comparison opportunities with other firms. It can also increase management incentives and effort through a higher probability of bankruptcy for private firms. Similarly, privatization can also lead to more efficient management structures, especially if privatization does not result in monopolies. Distinguishing between entry barriers and other types of competition within a market seems favorable in order to capture the distinct dimensions of product market regulation.

Felbermayr and Prat (2011) draw a similar conclusion. In a search model framework, the authors model the impact of regulation on the labor market via the productivity channel. Regulation can appear in the form of entry costs or fixed costs. It is shown that high entry barriers reduce the replacement of unproductive firms by more productive ones. Fewer new firms can compensate the costs of entry while incumbent firms are not facing these types of costs. It is predicted that unemployment increases with the level of entry barriers. In contrast, costs stemming from increased regulation (administrative costs that affect all firms in a market) can cause less productive firms to leave the market since the relative cost increase hurts them most. Then, overall productivity rises and the so called selection effect leads to higher employment. Whether the competition effect compensates the selection effect of Felbermayr and Prat (2011) is theoretically unclear.

2.2 Empirical evidence

This section complements the first part of chapter 2 by reviewing the empirical literature on unconditional and conditional institutional labor market effects. The overview focusses mainly on macroeconomic examinations. When benefi-

cial, microeconomic perspectives are presented, as well. The section is divided in two parts. First, the literature on unconditional effects of institutions on unemployment is given. This corresponds to the empirical analysis conducted in chapter 4. Second, the literature on conditional institutional effects is presented. The analysis performed in chapter 5 is based on this research.

2.2.1 Unconditional institutional effects

Labor tax system

Consistent with the main theoretical predictions of section 2.1.2, the early empirical macroeconomic evidence points to a positive effect of an increase of labor taxes on unemployment. Nickell (1997) finds a positive impact of a rise in the tax wedge measured as the sum of payroll, income and consumption taxes on unemployment when controlling for several other institutional factors, a dummy for the first period, and the change in the inflation rate. This estimation is based on 20 OECD countries and two periods from 1983 to 1988 and 1989 to 1993. Baker et al. (2005) repeat the aforementioned study using an updated data set for the same countries and slightly changed time periods. However, the authors haven't been able to confirm the findings. In regards to the tax wedge, the coefficient is insignificant for the same specification as in Nickell (1997).

Belot and Van Ours (2004) exploit longer time series using 5-year averages for all institutional variables ranging from 1960 to 1999 for 17 OECD countries. Compared to both aforementioned studies, the set of institutions is considerably smaller. Measures for the benefit duration, the active labor market policy, and the union coverage are missing. The tax rate is the sum of payroll and income taxes while consumption taxes are not considered. The results imply

that the tax rate does not have a significant impact on the unemployment rate once it is controlled for time- and country-specific effects. This is similar to what has been reported by Baccaro and Rei (2007). They fail to find a significant impact of the tax wedge, measured as the sum of the payroll, income and consumption tax, on unemployment using 5-year averaged data from 1960 to 1998. In that study, several additional specifications with both annual and averaged data including the tax wedge have been estimated. However, these additional results are not comparable since an interaction between the tax wedge and the bargaining coordination is also taken into consideration. This renders the comparative interpretation of the unconditional effect impossible.

The use of averages over some years is always accompanied by problems like the starting point of the averaged periods or the length of the period. Annual data, if available, is clearly superior. Bassanini and Duval (2006) have been among the first to regress annual unemployment on institutional factors. In contrast to previous studies, the authors use a shorter estimation period ranging from 1982 to 2003. Five macroeconomic shocks have been included to capture short-run fluctuations of the unemployment rate.[19] The tax wedge is measured as the sum of the personal income tax and all social security contributions. Its impact on unemployment is either insignificant or positive, depending on the particular model specification.

One of the main points of criticism of the cross-country literature stated by Daveri (2003) is the imprecise estimation of tax effects. This is assumed to be a result of omitted variables such as the reservation wage or the wage mark-up over the reservation wage, which are unobservable. Significance lev-

[19]This follows Nickell et al. (2005) when using annual data and the shock variables. Nickell et al. (2005) consider a model explaining unemployment with interactions including the tax rate. Hence, the results concerning this variable are not unconditional and are therefore discussed later.

els could then be determined with bias. Planas et al. (2007) elude the necessity of estimating a fully specified model by using an unobserved components model. In this framework, actual unemployment is made up of the equilibrium unemployment rate and short-term fluctuations around the equilibrium. Equilibrium unemployment is determined by labor taxes measured as the sum of payroll and income taxes, and a term containing the unobserved components. These unobserved components are explicitly modeled through a second order random walk process. By further specifying the short-term fluctuations around equilibrium unemployment, the coefficient of the labor tax measure can be consistently derived. Planas et al. (2007) conclude that there is a positive tax increase effect on unemployment.

Microeconomic evidence (Bennmarker et al. (2009), for instance) similarly points to positive employment effects through a reduction in the labor tax rate, thus supporting the outcome of the unobserved component model. However, the findings of Planas et al. (2007) are produced on the basis of aggregated data over a number of countries. Only variation concerning the time dimension has been exploited. In this context, the authors admit that the labor tax effect is measured with considerable uncertainty.

Berger and Everaert (2010) extend the unobserved components approach by including the cross-section dimension into the model. The countries are grouped according to their wage-setting regime. This accounts for possible heterogeneity of labor tax effects on equilibrium unemployment. Indeed, the positive findings of Planas et al. (2007) are only valid for the group of central-European countries with strong unions and decentralized bargaining. For

Nordic and Anglo-Saxon countries, no significant impact of a change in the labor tax rate can be reported.[20]

The composition of taxes deserves further attention. Blanchard (2006) states that taxes affecting unemployed and employed equally don't have an effect on labor demand and, consequently, on unemployment. Taxes that are only levied on labor increase the wage demand of the worker since the outside option (unemployment which is not affected by the labor tax) becomes more attractive. The higher wage demands are expected to reduce labor demand. This increased wage demand effect does not show up when the outside option also changes due to the tax increase. In this case, labor demand remains unaffected. Distinguishing between the former and the latter type of labor taxes is necessary to disentangle the macroeconomic consequences of tax changes. Income and payroll taxes have different effects on employed and unemployed while consumption taxes affect both groups equally. The latter tax type is thus unlikely to affect unemployment. It is possible that studies aggregating taxes of both types mix up different tax effects.

Bargaining system

In empirical macroeconomic studies, bargaining power is usually characterized by indicators like union density or union coverage (also referred to as bargaining coverage). Further indicators of the bargaining system include bargaining coordination or centralization, and the existence of a minimum wage.

First of all, the relationship between bargaining centralization and coordination has to be clarified. This difference is again taken up in section 3 when the data for the empirical analysis of sections 4 and 5 are introduced

[20]This points to potential interaction effects. The section on conditional institutional effects takes up that aspect.

and discussed. Nevertheless, it is also relevant to make this distinction when comparing the results of empirical studies that use different measures for the wage bargaining system, i.e. centralization or coordination. According to Soskice (1990), bargaining centralization delivers information at which level, from firm-level to country-level, wage-setting takes place. Alternatively, bargaining coordination also provides information on the informal coordination within the wage bargaining process.

A comprehensive overview on coordination is provided by Aidt and Tzannatos (2008) for empirical studies up to 2006. By sub-dividing the distinct estimation approaches of 28 studies, the authors find that 70% out of 41 considered estimations find a negative relationship between bargaining coordination and the unemployment rate. However, it is emphasized that the different estimation results can be assessed further according to the specific estimation procedure. Interestingly, pooled cross-country studies that take account of unobservable country-specific differences via fixed effects show less clear-cut results. The exploited time-period also seems to be of importance. Studies using longer periods have a higher tendency to find a negative significant relationship.

Note that a number of studies analyzing the relation between coordination and unemployment have not been considered by Aidt and Tzannatos (2008), who mainly include studies with an exclusive focus on coordination. Howell et al. (2007) also provide a summary of studies up to 2006 analyzing coordination effects on unemployment. They rely on cross-country studies that take a set of institutional factors into account; one of them being coordination. Similar results to Aidt and Tzannatos (2008) are provided. Seven out of ten studies show a negative and significant relationship between coordination and unemployment. The remaining studies report an insignificant coefficient.

In a nutshell, the empirical evidence points to a negative correlation between bargaining coordination and the unemployment rate for different measures of coordination and for distinct periods and specifications.

More recent studies are not directly comparable for two reasons. Nickell et al. (2005) and Baccaro and Rei (2007) include interactions with bargaining coordination variables. This makes the comparative interpretation of the unconditional effect impossible. Bassanini and Duval (2009) include a corporatism measure which is significantly negative, but they do not mention whether this measure takes up the coordination or the centralization perspective.

In two cross-country studies, an indicator for bargaining centralization has been used instead of a coordination measure. In the study of Elmeskov et al. (1998), bargaining centralization is marginally significant, but only if no measures for informal coordination or cooperation are considered. Belot and Van Ours (2004) use the same data on centralization as the aforementioned study. They report an insignificant centralization coefficient once it is controlled for unobserved heterogeneity by including fixed effects.

The empirical literature also draws attention to non-linear effects of bargaining coordination or centralization on unemployment. The empirical evidence does not support the non-linear hypothesis, neither for coordination nor for centralization measures. According to Aidt and Tzannatos (2008), 25 out of 99 estimations in empirical studies show such a non-linear relationship, whereas most of the significant ones are just simple correlation-based examinations. The more sophisticated the estimation framework, the less likely it is that a significant relationship is present.

While the level at which bargaining takes place delivers valuable information on the bargaining system, it does not address the distribution of power in the

bargaining process between employee and employer. The union power can differ in industries or economies which exhibit the same level of centralization or coordination. Analyzing the bargaining system requires the consideration of both the level of centralization or coordination as well as the distribution of the bargaining power between the negotiating partners. Union density and coverage are assumed to be appropriate indicators for capturing bargaining power. The first represents the share of workers organized in trade unions. The second displays the share of workers affected by union bargaining agreements.

Since union density is much more comprehensively available, it is often preferred over the union coverage indicator in cross-country or panel studies. Empirical evidence is rather mixed and therefore only confirms the predictions of section 2.1 to some extent. Scarpetta (1996), controlling for some other institutional factors including a bargaining coordination measure, reports a significant positive effect of union density on the unemployment rate. He performs a cross-country study comprising of 17 countries from 1983 to 1993. For a very similar time-frame, a slightly larger set of countries and the same set control variables, Elmeskov et al. (1998) find no significant relationship between union density and unemployment. Results of these early studies may offer limited reliability due to a low number of observations, crude institutional measures, or econometric methods of limited power. However, more recent studies show similarly ambiguous findings.

Baker et al. (2005) estimate an insignificant effect of union density on unemployment. The OECD (2006) highlights a negative, insignificant, and a positive union density impact on unemployment within the same study resulting from a change in the control factors. Similarly, Bassanini and Duval (2009) estimate an insignificant and a negative union density influence on unemployment. One specification includes gross and the other specification considers

net replacement rates. This ambiguity is confirmed by the study of Aidt and Tzannatos (2008). They summarize empirical studies between 1986 an 1999 dealing with the bargaining power influence on the labor market. The impact of an increase in the union membership rate is prevailingly insignificant for unemployment in nearly all investigations, as long as measures for formal or informal coordination are included, as well.

In regards to the union or bargaining coverage, much less evidence is available. The difficulty is constructing an internationally comparable, annually available measure. Few cross-country studies have taken averages of the coverage indicator. Similar to union density, the findings concerning the coverage are rather inconclusive, as well. Nickell (1997) reports a significantly positive effect of union coverage on unemployment, while Blanchard and Wolfers (2000) do not find any significant effect. Both studies use an indicator ranging from 1 to 3 for the bargaining coverage with very few changes over time where the highest value is assigned to most countries (see Nickell (1997), Table 5). Baker et al. (2005) are one of the first to introduce a union coverage measure which is based on the percentage of workers affected by bargaining agreements instead of a simple index. More time- and cross-country variation delivers a detailed description of the range of collective bargaining and, thus, of the actual bargaining power. A significant positive coverage effect on unemployment is estimated by Baker et al. (2005). While this can serve as a hint about the link between union coverage and unemployment, a confirmation by other analyses is missing.

Similar to the ambiguous theoretical impact of minimum wage effects, the empirical evidence is far from consistent. Microeconomic studies which usually consider employment as the target variable, generally focus on a specific sector like the restaurant or the retail sector to investigate the effects of a change in

the minimum wage. Sabia (2009) reports slightly negative employment effects for retail employment. In contrast, Addison et al. (2009) do not find support for adverse employment effects following a minimum wage increase in the US retail-trade sector. Dube et al. (2010) emphasize the importance of controlling for local economic conditions to disentangle employment trends caused by a minimum wage increase or by local heterogeneity. For a longer time period and comparably detailed data on national minimum wages for the United Kingdom, Dolton et al. (2011) reach a similar conclusion. Both studies largely confirm Addison et al. (2009) by finding no adverse employment effects of a minimum wage increase.[21]

The cross-country literature suffers from imprecise measures of minimum wages. Indicators only measure whether a minimum wage exists or not, but not its particular level. No information is delivered on the relation to the actual wage paid. This limits analyses to the question of whether the existence of a minimum wage affects unemployment or not. Macroeconomic studies seldom consider a measure for minimum wages, and the few findings are predominantly insignificant (see Elmeskov et al. (1998), for example). One exception in terms of indicator quality is the study of Bassanini and Duval (2006). They use the statutory minimum wage level as a percentage of the median wage. Although preferable to a simple index, a considerable reduction of the sample was necessary since information on statutory minimum and median wage levels are available only for some countries. Using this measure, the authors report no significant impact of minimum wages on unemployment.

[21]These findings of microeconomic studies should be interpreted with care due to three aspects. Wage effects do not necessarily transform into measurable employment effects. Whether employment effects translate into changes into unemployment is also not clear. Finally, microeconomic studies cannot display general equilibrium effects affecting the whole economy. For instance, a minimum wage change in the restaurant sector could have employment effects in other sectors, as well. Nevertheless, the microeconomic evidence can serve as an indication for insignificant effects of minimum wage changes on unemployment.

Unemployment benefit system

Early studies like Scarpetta (1996), Nickell (1997) or Elmeskov et al. (1998) all estimate a positive effect of an unemployment benefit increase on unemployment. The selected indicator is the wage replacement rate for an average production worker averaged over five years of unemployment, with distinct family situations. The application of longer time periods, annual data and sophisticated estimation methods[22] have risen some doubts concerning such a clear-cut positive effect. According to this, Belot and Van Ours (2004), Baker et al. (2005), and Baccaro and Rei (2007) report insignificant coefficients of unemployment benefits.

An illustrative overview on the relation between unemployment benefits and unemployment is given by Kim (2011) by means of a meta study. It is claimed that the probability of estimating a significant positive effect of level and duration of unemployment benefits depends on the data choice[23], the estimation method[24], as well as the set of control variables. While such a meta study can only deliver limited information on the comparability of econometric settings or data, the results highlight the difficulty of establishing a clear-cut effect. Depending on the particular specification, the impact of unemployment benefits on unemployment is either insignificant or significantly positive.

One major point of critique of such macroeconomic studies is that gross replacement rates are an inappropriate measure for the unemployment benefits since they are calculated before the deduction of taxes. Differences in the tax base can skew the comparison of unemployment benefits across countries.

[22]The inclusion of country- and time-specific effects to take account of unobserved heterogeneity is an example for such an improvement.

[23]Whether the benefits are calculated for the first year or as an average value over 5 years or whether annual or averaged data is used, for instance.

[24]The usage of random effects decreases the probability of finding a significant and positive coefficient, compared to a fixed effects model.

Similarly, reforms of the tax base within a country can change the gross replacement rates without an actual change in the benefits. Howell and Rehm (2009) compare the superior net replacement rates recently published by the OECD with the traditional gross replacement rates used in nearly all macroeconomic empirical contributions. Findings based on correlation evidence suggests that the link between benefits and unemployment is insignificant. However, the study suffers from the short time span for which net replacement rates are available. A more reliable econometric evaluation is therefore currently infeasible.

It seems that more recent contributions with access to more sophisticated econometric methods and better data availability and quality are less likely to find a significant relationship between unemployment benefits and unemployment. This is a preliminary conclusion, and the adequacy of using gross replacement rates as indicators for the unemployment benefits is at least questionable. Thus, results from microeconomic studies can help to clarify the link between unemployment benefits and unemployment. Most of these country-specific examinations focus on the relationship between unemployment benefit duration and the duration of unemployment or of the post-unemployment job.

Van Ours and Vodopivec (2008) find for Slovenia that a considerable reduction in the benefit duration (between three and six months for five to 15 years pre-unemployment job duration) does not negatively influence post-unemployment job duration. For Norway, Roed and Zhang (2003) report that rising unemployment benefits have a marginal, but negative impact on the escape rate out of unemployment. Similarly, Lalive (2007) finds for Austria that a large extension of the eligibility period (170 weeks) increases the duration of the unemployment spell. All three papers highlight the negative consequences

or at least do not exemplify positive labor market effects of a generous unemployment insurance system.

While the effect measured by Lalive (2007) holds for large extensions, such a link cannot be observed for small extensions (13 weeks) of the benefit eligibility period. Tatsiramos (2009) emphasizes the beneficial effects of unemployment benefits by focussing on the post-unemployment job duration. Using microeconomic data for eight European countries, this study finds that higher unemployment benefits can indeed stabilize job duration. This is especially true for countries with generous unemployment benefit systems.

In reference to Norwegian data, Gaure et al. (2008) find that increasing the generosity of the unemployment benefit system improves the post-unemployment job quality measured as earnings, and post-unemployment job duration. The beneficial effect is strongest during the first six months of unemployment, and decreasing afterwards.

Summarizing the microeconomic literature, there is evidence that high unemployment benefits have disincentive effects and reduce the job seekers' search intensity. However, this does not seem to be the unique mechanism at work. Improvements of the matching quality and the post-unemployment job productivity or the deterrent impact of periods of unemployment counteract, and can alleviate or even compensate the detrimental benefit effects.

Employment protection

The empirical cross-country literature provides mixed results in regards to the employment protection impact on unemployment. The majority of studies

like Nickell (1997) and Belot and Van Ours (2004)[25] or Nickell et al. (2005)[26] report insignificant results. Blanchard and Wolfers (2000) find a positive impact, and Bassanini and Duval (2006, 2009) report a negative impact for some specifications. One reason for the inconclusive results is the underlying data. Nearly all studies use a measure for employment protection that is at least partially based on an indicator constructed by the OECD. Notably earlier studies combine this indicator with information from other sources.[27]

Blanchard and Wolfers (2000) merged data from Lazear (1990) on employment protection up to 1980 with OECD data for two periods; the first for the late 80's and the second for the late 90's. For some years no information is available. The authors solved this by interpolating in order to boost the number of observations. The so constructed indicator has been further extended, annualized and used in a similar form by subsequent studies. Howell et al. (2007) criticizes this indicator for mixing up distinct dimensions of employment protection. While the measure of Lazear (1990) focusses solely on severance payments and notice periods, the OECD indicator adds two dimensions: procedural inconveniences and consequences on unfair dismissal.

The blending of distinct data series and the interpolation to receive annualized data makes this indicator inappropriate for capturing the effects of employment protection since comparability over time is not given. The most reliable studies are therefore the ones by Bassanini and Duval (2006, 2009) who exclusively rely on the comprehensively constructed OECD indicator. Hence, it is at least questionable whether a look at cross-country study results can

[25]Both use a static setting with 5-year averaged data, the former without and the latter with country-and time-specific effects.

[26]This study uses annual data and a dynamic framework.

[27]The OECD indicator of employment protection for overall employment considers information on employment protection for temporary and permanent employment. In nearly all studies, the overall measure is used.

shed light on the macroeconomic consequences of employment protection. For this institutional category it is reasonable to go beyond the findings of macroeconomic empirical studies, and to focus on microeconomic work instead.

Using Colombian data, Kugler (1999) reports that a decrease in firing costs raises the exit rates both into and out of unemployment for workers affected by the reform. Combining both effects suggests that the net impact on unemployment of the Colombian employment protection reform is negative. Unemployment is lower after reducing dismissal costs. Kugler and Pica (2008) exploit data on an Italian labor market reform of employment protection for small businesses. They point out that while the reform affected the job transition rates, it had no impact on employment. Blanchard and Portugal (2001) reach a similar conclusion when comparing the Portuguese and the US labor market; the first hallmarked by strict and the second by lax job protection standards. The authors find that employment protection legislation has considerably distinct effects on job creation and job destruction and on the employment flows in both countries. The impact on unemployment, however, does not differ much. Both systems show similar evolutions of unemployment. It is concluded that different levels of employment protection are not the cause for distinct unemployment rates.

Until now, no explicit distinction has been made between employment protection for regular and for temporary contracts. However, microeconomic evidence suggests that reforms of employment protection for temporary employment can have different effects on the labor market compared to reforms of job protection of regular contracts. Blanchard and Landier (2002) report for French data on young workers that job turnover (i.e. job creation and destruction) has increased substantially following a reform increasing flexibility of employment protection for temporary employment. As a consequence, unem-

ployment has risen due to frequent unemployment spells of temporary workers until a regular job is found. The authors claim that such a transition to more flexible employment protection is likely to have adverse labor market effects.[28]

In a similar vein, Cahuc and Postel-Vinay (2002) shows within a matching model framework that increasing labor market flexibility for temporary jobs can have detrimental effects on unemployment. Job creation increases after facilitating the implementation of temporary jobs. The positive effect is offset by increased turnover and more frequent unemployment spells of temporary workers. A different outcome in regards to the relation between employment protection for temporary jobs and unemployment is presented by Kahn (2010). It is shown for a European data set on individuals that increasing the flexibility to open temporary contracts shifts workers from permanent to temporary jobs. However, overall employment is unaffected by such a change. This argues against the detrimental labor market effect caused by increased job transition rates. Reforms reducing employment protection for permanent contracts raises employment as long as it is not controlled for country-specific trends. Once such trends are taken into account, the effect becomes insignificant and employment remains unaffected.

Product market regulation

The macroeconomic empirical literature only recently started to focus on the link between product market regulation and unemployment. This was not a sign of disregard but a consequence of the difficulty of measuring product market regulation. One exception is Scarpetta (1996) who uses a measure for trade openness to approximate the degree of product market regulation. The

[28]However, Kahn (2007) state that the findings of Blanchard and Landier (2002) might be driven by an overall deteriorating economy.

findings of this study indicate that lower unemployment follows higher openness. However, the results become insignificant if it is controlled for bargaining coordination in the estimation. One of the first studies to use a comprehensive and reliable indicator for product market regulation is Nicoletti and Scarpetta (2005). Three different indicators are included in the estimations: an overall indicator of product market regulation comprising several dimensions of regulation; a measure for state control; and an indicator for the barriers to entry.[29] For most econometric specifications, decreasing regulation raises employment significantly.

Using the same overall regulation indicator, Bassanini and Duval (2009) and Amable et al. (2007) confirm the significantly detrimental effect of product market regulation for the labor market. Controlling for possible endogeneity and based on firm profitability as a measure for product market competition, Griffith et al. (2007) also report a positive influence of higher regulation on unemployment.[30]

Theory predicts distinct labor market effects for different categories of product market regulation. For data concerning the French retail industry, Bertrand and Kramarz (2002) show that a more restrictive entry policy in this market was followed by increased concentration and a decline in employment. Berger and Danninger (2007) similarly find for a cross-section of countries that higher barriers to entrepreneurship lower the employment growth rate. An overall indicator of regulation, administrative regulation[31], and barriers to trade and investment also seem to have an adverse impact on the labor market.

[29]The indicators are largely the same which are used in the estimations of chapters 4 and 5. Further details on the indicators can be found in chapter 3.

[30]An alternative indicator, the Economic Freedom of the World Index provided by the Fraser Institute, has been used by Feldmann (2008). While the results point to adverse labor market effects through higher regulation, the index is often criticized for being based on surveys and for underestimating labor protection (see Freeman (2007) for a brief discussion).

[31]This could be taken as a measure for fixed costs following Felbermayr and Prat (2011).

In contrast, no significant effect has been detected for economic regulation and the extent of state control.[32] Estimates in this study suffer from at least two shortcomings. First, the indicators are only available for three points in time, and data points have been generated by interpolating. Second, in the estimations using the disaggregated regulation measures, no control variables appear. The indicators are not included jointly, but separately, and the reliability of the findings is questionable.

The ambiguity of several dimensions of product market regulation is emphasized by Fiori et al. (2012), as well. They exploit a similar data set that is used in chapters 4 and 5 which is available for annual data and for different dimensions of product market regulation. In a dynamic setting, the overall regulation indicator as well as the indicator measuring the level of state control do not influence the employment rate. The overall regulation indicator less public ownership as well as the indicator for barriers to entry and vertical integration indeed show a significantly negative effect on employment. The findings suggest a beneficial labor market effect for some indicators like entry barriers, while other factors like public ownership are less important. This underlines the importance of distinguishing between different dimensions of product market regulation.

2.2.2 Conditional institutional effects

Some of the most influential empirical studies on interdependent institutions have been conducted by Belot and Van Ours (2001, 2004).[33] The authors de-

[32] This indicator is comparable to a public ownership measure.

[33] The earlier study is based on a simplified theoretical model but uses almost the same empirical approach. Consequently, empirical findings are very similar.

rive three interaction hypotheses from a right-to-manage model.[34] The first interaction is between the unemployment benefit system and the labor tax system, while the second and the third are between the bargaining centralization on the one hand, and employment protection and union density on the other hand. Based on their theoretical model, the derivation of more interactions is generally possible. However, Belot and Van Ours (2004) do not comment further on the exclusion of other interactions. The reason is likely due to the difficulty of estimating a larger set of interdependencies with a limited number of observations.

The results imply that all three included interaction terms are significant. Both the interaction between labor taxes and unemployment benefits as well as the one between employment protection and bargaining centralization are positive. In contrast, the interaction between bargaining centralization and union density is negative. A negative coefficient sign means that the two institutional factors are complementary in a sense, that jointly (de)regulating them would yield the most beneficial effects.[35] A positive sign, indicating substitutive institutions, refers to a situation where deregulation (regulation) is only reasonable when the conditioning institution remains at a high (low) level.[36]

The following conclusions can be drawn from these findings. A reduction of the tax rate level (unemployment benefit level) is more beneficial if the unemployment benefit level (tax rate level) is high. Similarly, reducing employment

[34]The model shows that several institutional interactions are possible. No information can be derived on which interactions are important. The model has not been described in section 2.1 since it does not contribute to a better understanding of interaction mechanisms.

[35]Alternatively, deregulating one factor is most beneficial if the conditioning factor is already deregulated.

[36]The partial derivation of the unemployment equation with respect to an institutional factor X_1 dependent on another institutional factor X_2 yields: $\frac{\partial Y}{\partial X_1} = \beta_0 + \beta_1 X_2$. If Y is the unemployment rate, a negative sign for β_1 means that X_1 and X_2 are complementary, a positive sign corresponds to a substitutive relationship.

protection (bargaining centralization) has a more positive effect on the labor market if bargaining is centralized (employment protection is high). In contrast, reducing union density (bargaining centralization) is more beneficial if bargaining is decentralized (union density is low).[37] Further studies also deal with identifying the impact of some selected interactions on the labor market. These are described in the following.

Nickell et al. (2005) consider three interactions between institutional variables in their dynamic model specification explaining unemployment. No rationale for the selection of interactions is provided. The interaction between unemployment benefit duration and the level of unemployment benefits as well as the interactions of bargaining coordination with both union density and the tax wedge are taken into account. All three interactions appear to be significant in the estimation. The first interaction term exhibits a positive coefficient, while the second and third interaction show a negative one.

Three studies explicitly focus on the interaction between indicators of labor market and product market institutions. Griffith et al. (2007) estimate how an increase in product market competition influences the unemployment rate dependent on union coverage and union density. In this paper, competition is approximated by the profitability of firms and instrumented by four indicators of product market reforms. Both interactions (which are not jointly included) turn out to be significant, so indicating that competition lowers unemployment more for high union density and coverage rates. Similarly, Fiori et al. (2012) and also Nicoletti and Scarpetta (2005) find evidence for the hypothesis sug-

[37] Belot and Van Ours (2004) also estimate a different specification excluding insignificant individual institutions where the coefficient sign of two interactions changes. However, as is shown in chapter 5, excluding a constitutive part of an interaction renders impossible the correct estimation of the interaction coefficient.

gesting that an increase in competition is more beneficial if the labor market regulation, which is assumed to be linked to union bargaining power, is high.

The bargaining regime has been the main interaction partner in the study of Baccaro and Rei (2007). They interact a measure for bargaining coordination with indicators for employment protection, union density, level and duration of unemployment benefits, and the tax wedge. It is predicted that the interactions have a negative sign, i.e. the larger the coordination within an economy, the more likely is the internalization of negative externalities of both parties, employers and unions. Using annual data and distinct estimators, only the interaction between bargaining coordination and union density is significant. If the same model is specified based on 5-year averaged data, no interaction term appears to be significant and the prediction cannot be confirmed.

Furthermore, the IMF (2003) estimates a model including all bivariate combinations between employment protection, the tax wedge, and unemployment benefits, as well as the interaction between union density and bargaining coordination. Unfortunately, the grounds upon which the interaction terms have been selected are unclear. The estimation including all mentioned institutional interactions (which all turn out to be highly significant) does not include any macroeconomic controls. Further estimations taking such control variables into account only include the interactions of union density with both employment protection as well as the tax wedge. Both interactions are significant in this setup, but it is questionable whether this finding is robust to changes in the set of explanatory variables.

Daveri and Tabellini (2000) address the interaction between labor taxes and union bargaining power. Within a model of overlapping generations, it is predicted that a tax increase is more detrimental if the union bargaining power is high. The additional costs caused by the tax increase are then shifted

to the firms, which in turn reduce their labor demand. An empirical test with OECD data finds support for this hypothesis of taxes and bargaining power being substitutive. Similarly, Berger and Everaert (2010) estimate in an unobserved components model that tax effects on unemployment differ across OECD countries. They conclude that increasing labor taxes is only detrimental for the labor market in countries characterized by strong unions and decentralized wage bargaining which largely confirms the results provided by Daveri and Tabellini (2000).

Finally, Cahuc and Postel-Vinay (2002) analyze the relation between employment protection for temporary and for permanent employment. They claim that reducing protection for temporary employment particularly has adverse effects when employment protection (or firing costs in the model) for regular employment is high. The creation of regular jobs is hampered by high firing costs for permanent employment such that firms prefer substituting permanent workers with temporary ones. Complementary to this outcome is the finding of Kahn (2007). It is shown that increased employment protection for permanent employment inherently increases the probability that more vulnerable groups of workers, i.e. young, immigrants and women, lose their job or move from permanent to temporary contracts. According to this, decreasing job protection for temporary employment is detrimental, especially if regular employment is strongly protected.

Until now, none of the contributions studying interactions have made an attempt to estimate interaction effects between institutions on unemployment in a more structured manner. However, Bassanini and Duval (2006) state that concentrating on a small set of interactions raises the risk of omitted variable bias due to the exclusion of potentially relevant interactions. The findings of the studies dealing with a highly subjective set of interactions are therefore

dubious. In order to reduce this risk, Bassanini and Duval (2006) try a more comprehensive approach by considering all possible interactions between six institutional indicators. First, they estimate the interaction effect between both the tax wedge as well as employment protection, and the bargaining regime. A larger drop in unemployment through a tax cut is estimated for medium centralized bargaining. In contrast, the result of the employment protection interaction with the bargaining regime is insignificant. Second, interactions between all six institutions are estimated one at a time. Nine interactions, mainly including the tax wedge as well as the unemployment benefit variable, turn out to be significant in this framework. However, the authors point out that estimating interactions one at a time conveys a possible omitted variable bias by neglecting other (even insignificant) interactions.[38]

Bassanini and Duval (2006) offer two solutions to this problem. First, they use an instrumental variable estimator[39] to take the possible endogeneity due to omitted variables into account. Second, a model considering all available bivariate interactions in one equation is estimated. Only the interaction between unemployment benefits and union density is significant in both exercises. Nevertheless, the two approaches suffer from severe econometric problems; the first neglects implicit interactions and delivers estimates only for those interactions under which the instrument has proven to be valid, while the second can exploit only a considerably reduced number of degrees of freedom due to the large number of interactions.

[38]This omitted variable bias is explained more extensively in section 5.1.1.

[39]Deviations for each variable from the time-specific average are used as an instrument for the deviations from the time- and country-specific average.

2.3 Summary and formulation of hypotheses

In the following, main findings of both the theoretical and empirical literature are summarized. Testable hypotheses for both unconditional as well as conditional institutional effects are derived, which serve as the basis for the empirical analysis in chapter 4 and 5.

Labor tax system

Theory predicts that increased tax rates push unemployment up. Taxes can have a negative impact on both labor demand through an increase in labor costs, and on labor supply through a diminished gap between the real wage and the outside option. Empirical evidence largely confirms the prediction, and the majority of recent cross-country studies report a positive effect of tax increases on unemployment. Furthermore, the composition of taxes is important in the sense, that consumption taxes which affect employed and unemployed equally have no impact on labor demand. It is therefore expected that a payroll and an income tax increase leads to higher unemployment, while the consumption tax is more likely to leave unemployment unaffected.

Bargaining system

The unemployment situation in the whole economy becomes more important and excessive wage claims become less likely for unions and employers that exhibit a high degree of bargaining coordination or centralization. Empirical studies largely confirm this prediction for coordination, but not for centralization. It is possible that the coordination measure is more appropriate since it comprises of both formal and informal aspects of the bargaining process. Hence, it is expected that higher bargaining coordination decreases unemploy-

ment while the effect of higher bargaining centralization is more likely to be insignificant.

Union density and union coverage, which represent the union power in the bargaining process, are expected to be positively linked to unemployment. Strong unions claim higher wages resulting in firms to react with a fall in labor demand. The impact of an increase in union density on unemployment is rather mixed in the empirical literature. Most studies find no significant relationship. The findings concerning the union coverage are inconclusive, as well, but only few studies use an indicator for union coverage. Generally, union coverage is assumed to be superior over union density since it includes further information on workers which are not members of a union but nevertheless affected by the bargaining process. Given that the measure for union coverage used in chapter 4 and 5 is able to capture the union power, a positive effect on unemployment is expected. In contrast, union density is probably not able to represent union power and no significant relationship to unemployment is expected.

Minimum wages set a lower ceiling for the bargained wage and, thus, pushes labor costs up for jobs with wages below the minimum wage level. However, labor supply is positively affected by an increased incentive to work due to higher wage payments, and shirking is less likely. The empirical literature is inconclusive. Very few cross-country studies have considered a minimum wage variable due to the lack of appropriate indicators. Microeconomic studies can only report estimates for a specific sector.

Furthermore, the effect depends on the minimum wage level. Since the data used in the empirical analysis does not contain information on the level, predictions are hard to make. While from a theoretical point of view, a detrimental effect of a minimum wage on unemployment is likely, empirical research

does not provide much support for this prediction. Given that the indicator used here does not contain information on the minimum wage level, a positive effect on unemployment is expected if the majority of countries providing a minimum wage have levels such that labor demand and supply are affected. If not so, no significant effect is assumed.

Unemployment benefit system

High unemployment benefits adversely affect the labor market by increasing the reservation wage. This reduces the search intensity of the unemployed. Extending the eligibility period is expected to increase unemployment due to longer retention times in unemployment. People refrain from moving out of the labor force in order to remain eligible for a longer period. The negative incentive effect through benefit increases or extensions is faced by several beneficial effects of a certain degree of transfer payments. A benefit increase can lift labor supply by pushing the search intensity of those unemployed who are currently not eligible for transfer payments. It can improve the matching quality by giving the unemployed an opportunity to keep up their business network or by permitting the financial background for intensive job search. Risk averse workers are further attracted to accept risky jobs since their fall-back position improves.

The empirical cross-country evidence does not deliver a clear-cut impact of a change in the unemployment benefits. Earlier studies point to a significantly positive relationship between transfer payments and unemployment. More recent studies using more sophisticated estimation methods and data do not find significant results. Adding the findings of microeconomic studies leads to the conclusion that unemployment benefits in a certain range and for specific groups indeed increase unemployment.

Several mechanisms at work render it difficult to make clear predictions on the effect of an unemployment benefit increase. Since the insurance aspect is especially relevant for the short-term unemployed, it is expected that an increase of the first year benefits up to a particular level reduces unemployment. However, there is no information on the threshold level. As a result, insignificant or even positive effects on unemployment are also possible. For the medium- and long-term unemployment benefits, the negative incentive effect is assumed to prevail and lifting these benefits should result in an increase of the unemployment rate. Similarly, extending unemployment benefit coverage is expected to be a further disincentive for unemployed and is thus expected to foster unemployment. Finally, extending unemployment benefit duration is expected to increase unemployment. Retention times in unemployment are prolonged and moving out of the labor force is delayed.

Employment protection

While employment protection lowers the transition rates into and out of employment, the impact on the unemployment rate is ambiguous. Cross-country studies exploiting a single indicator measuring the degree of employment protection predominantly do not find a robust link to unemployment. This could be due to imprecise measurement of employment protection, missing or limited comparability across countries and over time, or mixing up distinct dimensions of protection. Microeconomic evidence which measures job protection more accurately is less ambiguous. Increasing employment protection for regular employment has negative labor market effects, while the opposite is more likely for employment protection for temporary employment.

Reducing employment protection for temporary employment is found to increase unemployment by pushing the frequency of job changes. Employment

protection for regular employment turn vulnerable groups out of the (regular) labor market. This increases unemployment if only temporary jobs are available with a high fluctuation into and out of unemployment. Distinguishing employment protection for regular and temporary employment is necessary to avoid capturing countervailing effects by one aggregate indicator. Increasing employment protection for temporary employment is expected to show beneficial effects on the unemployment rate, while the opposite is assumed for employment protection for regular employment.

Product market regulation

Stronger product market competition theoretically reduces firm mark-ups and labor costs. Real wages and output increase causing unemployment to drop. Competition can be affected by various factors. Empirical studies show that unemployment decreases when entry barriers are lowered. No significant effect is found for public ownership. With regards to administrative costs, the competition effect is faced by a selection effect. Lower administrative costs can prevent the least productive firms from leaving a market, which has negative labor market effects. However, no adequate indicator exist for the administrative burden. For the empirical estimations increasing barriers to entry is expected to push unemployment up, while the effect is less likely for public ownership. Nevertheless, given that less state control can foster efficiency, a significant positive impact of public ownership on unemployment is assumed.

Conditional effects

Theoretical studies highlight the importance of institutional interaction effects. Within a wage-setting/price-setting model, interactions between various insti-

tutional indicators can be predicted. The empirical literature generally focuses on a small set of institutional interdependencies due to the fact that including all theoretically relevant interactions is impossible in classical approaches due to the limited number of observations. However, macroeconomic setups require the comprehensive consideration of all relevant interactions. The problem is that excluding potentially relevant interactions can lead to the estimation of biased coefficients and standard errors. This causes misleading inference, and findings of studies that subjectively select a limited number of interactions are not robust across specifications.

Four central theoretical concepts of interactions can be identified which serve to derive testable hypotheses for the empirical analysis. Further mechanisms explaining individual interdependencies exist, but the consideration of a large number of interactions in the empirical analysis requires a comprehensive theoretical basis. Such a broad perspective is provided by the following four theoretical concepts, and it is abstracted from additional mechanisms in order to keep the analysis comprehensible.

First, bargaining coordination is assumed to cause unions and employers to internalize their market activities. The negative impact of reforms which are expected to raise unemployment by increasing regulation is likely to be compensated by a coordinated wage bargaining system. Interactions with bargaining coordination should therefore show a negative sign, indicating the complementarity between bargaining coordination and other institutional variables.

Second, reforms which increase labor costs push unemployment up, especially if union bargaining power is high. Additional costs are then shifted to the employers. Two indicators are assumed to be such cost-influencing institutions; employment protection and labor taxes. Here, a positive sign is

expected for such interactions between indicators of the labor tax system and employment protection on the one hand, and indicators of union bargaining power on the other hand.

Third, under the assumption that deregulating reforms either result in a flattening or a fall in the wage-setting curve, or a flattening or a rise in the price-setting curve, determinants of the wage-setting and the price-setting curve are expected to show a complementary relationship. The slope of both curves is also relevant for deregulating reforms within the same group. A reform which shifts the level of the wage-setting curve downwards (the level of the price-setting curve upwards) is more beneficial for the labor market if the wage-setting (price-setting) curve is already flat.

The fourth hypothesis is contradictory to this. Employment gains of deregulating reforms that lift labor demand or supply through corresponding shifts of the wage-setting or price-setting curve are largest when the employment level is far from full employment. In the wage-setting/price-setting model, this can be described by a convex wage-setting curve where employment reactions caused by institutional reforms are smaller when unemployment is already low since then the potential for additional employment is then limited. Based on this argumentation it can be expected that institutions which have an impact of wage-setting and price-setting curves exhibit a substitutive relationship which corresponds to a positive interaction term coefficient in the estimations.

According to the model of Belot and Van Ours (2004), indicators for unemployment benefits, employment protection, bargaining power, and product market regulation affect the wage-setting curve. Indicators for employment protection, labor taxes, and product market regulation are determinants of the price-setting curve. As an extension, it is assumed that minimum wages can also affect wage-setting and price-setting. Predictions for interactions in-

cluding minimum wages are made according to the four hypotheses. In chapter 5, it is tested which of the four theoretical arguments are supported by the empirical evidence.

Chapter 3

Methodology and Data issues

The first part of this chapter deals with the general empirical strategy that is followed in the estimations of chapters 4 and 5. In particular, the relevant model specifications and estimation techniques are introduced and explained. A brief review on model averaging from a classical econometrics perspective and a description of Bayesian concepts is contained in the second part. This is necessary to understand the *Bayesian Averaging of Classical Estimates (BACE)* as the key method for the empirical estimations. This method which has been developed by Sala-I-Martin et al. (2004) can be seen as a mixture of a pure Bayesian and a frequentist approach. The third part of the chapter is dedicated to the presentation of the data required for the empirical estimations. After introducing the unemployment rate as the dependent variable, the institutional variables are displayed and explained. Finally, the macroeconomic control variables are briefly delineated.

3.1 General empirical strategy

The empirical analysis is catered to analyze the unconditional as well as conditional impact of labor market institutions on unemployment. The annual harmonized unemployment rate is the dependent variable which is regressed on several institutional factors which explain the long-run evolution of the unemployment rate, and on macroeconomic control variables which are also expected to be related to unemployment. According to Nickell et al. (2005), the control factors are required to capture deviations of actual unemployment from equilibrium unemployment. This connects the empirical estimation strategy to the concept of equilibrium unemployment given in chapter 2.1.1. A more detailed description of the relevant data is provided in section 3.3.

In panel-data models which focus on the explanation of cross-country phenomena it is useful to consider the inclusion of fixed effects. These effects are constants that capture country- or time-specific factors which otherwise appear in the error term. One could think of, for instance, cultural differences between countries that significantly affect the country-specific unemployment rate, or of a macroeconomic shock that influences all countries equally but only in one period. Hence, if there are significant country- and time-specific effects, neglecting them would result in an incorrect econometric specification. In some comparable estimation settings, random effects have been used instead of country-specific fixed effects. However, random effects only deliver consistent estimates if there is no correlation of the random effects with all regressors. In contrast, as discussed by Baltagi (2003), the fixed effects model works even in the presence of such a correlation. Johnston and DiNardo (1997) note that except for experimental or quasi-experimental data, no correlation

between the individual effects and the regressors is at least very unlikely which is why the fixed effects model is preferred here.

Several approaches like the least squares dummy variable estimator or the *Within estimator* for a two-way error component regression model are available to set up a fixed effects estimator. The former approach requires the estimation of a dummy variable coefficient for each cross-section and time period. This is equivalent to throwing away degrees of freedom, which is particularly problematic if the number of observations is limited.

To avoid this, I use the *Within transformation* for a two-way error component regression model to get rid of country- and time-specific effects. This estimator has been developed by Wallace and Hussain (1969), and is extensively described in Baltagi (2003). The main aspects of this estimator are introduced in the following. The general fixed effects model that is used in the empirical estimations reads

$$u_{i,t} = x'_{i,t}\beta + \varepsilon_{i,t} \tag{3.1}$$

with $u_{i,t}$ being the actual unemployment rate of country i at time t. $x_{i,t}$ is a $Kx1$ vector where K is the number of explanatory factors for country i at time t. The error term consists of

$$\varepsilon_{i,t} = \alpha_i + \lambda_t + \nu_{i,t}. \tag{3.2}$$

Here, α_i is the country-specific and λ_t the time-specific effect, and $\nu_{i,t} \sim IID(0, \sigma_\nu^2)$. The *Within transformation* wipes out the fixed effects by aver-

aging over cross-sections and over time such that

$$(u_{i,t} - \overline{u}_{i.} - \overline{u}_{.t} + \overline{u}_{..}) = \\ (x_{i,t} - \overline{x}_{i.} - \overline{x}_{.t} + \overline{x}_{..})\beta + (\nu_{i,t} - \overline{\nu}_{i.} - \overline{\nu}_{.t} + \overline{\nu}_{..}) \quad (3.3)$$

where $\overline{u}_{.t}$ is the average over the cross-section at time t, $\overline{u}_{i.}$ over time for country i, and $\overline{u}_{..}$ over cross-section and time. Equation (3.3) can be rewritten using

$$\begin{aligned} \overline{u}_{i,t} &= (u_{i,t} - \overline{u}_{i.} - \overline{u}_{.t} + \overline{u}_{..}) \\ \overline{x}_{i,t} &= (x_{i,t} - \overline{x}_{i.} - \overline{x}_{.t} + \overline{x}_{..}) \\ \overline{\nu}_{i,t} &= (\nu_{i,t} - \overline{\nu}_{i.} - \overline{\nu}_{.t} + \overline{\nu}_{..}). \end{aligned} \quad (3.4)$$

to

$$\overline{u}_{i,t} = \overline{x}'_{i,t}\beta + \overline{\nu}_{i,t}. \quad (3.5)$$

In matrix notation the basic econometric specification can be expressed as follows:

$$\overline{U} = \overline{X}\beta + \overline{\nu}. \quad (3.6)$$

Now, $\overline{U}$ is $NTx1$, $\overline{X}$ is a $NTxK$ matrix including the explanatory factors which influence the unemployment rate, and $\overline{\nu}$ is $NTx1$. β is the corresponding coefficient vectors of size Kx1, while N is the number of countries and T the number of years. Equation (3.6) measures the impact of an institutional change from the time and country average on the country and time averaged unemployment rate.

In Nickell et al. (2005), it is argued that institutions do not have the same impact on unemployment in each country, that is the pooling assumption is invalid. The test result for poolability according to Baltagi (2003) mainly depends on whether an F- or a χ^2-distribution is assumed. Furthermore, the test results change when the set of explanatory variables is varied. Since model averaging requires the alteration of the set of explanatory variables, it is not reasonable to conduct a test for poolability. The gain of lower variance due to pooling comes at the price of a potentially incorrect poolability assumption. The same holds true for autocorrelation, endogenety and heteroskedasticity which are difficult to consider within the BACE framework.Recently, Doppelhofer and Weeks (2009) introduced heteroskedasticity and Eicher et al. (2009) for endogeneity within a Bayesian model averaging framework. However, these are pure Bayesian approach which require complex sampling procedures. In order to generate classical coefficients and standard deviations robustness checks are applied in chapters 4 and 5 within a classical estimation framework which is able to take autocorrelation, heteroskedasticity and endogeneity into account.

Nickell et al. (2005) additionally test for stationarity of the variables and find that the null hypothesis of non-stationarity cannot be rejected. Therefore, the authors perform the Maddala and Wu (1999) co-integration test and are able to reject the null hypothesis of no co-integration. However, Berger and Everaert (2009) point out that the co-integration test used by Nickell et al. (2005) is not valid since specific assumptions are not fulfilled.[40] The problem of such a test for co-integration applies here as well. The short time period and the limited number of cross-sections further lowers the reliability of such a test. It is also doubtful whether a stationarity test for a rather time-invariant

[40]Berger and Everaert (2009) mention that the number of countries needs to go to infinity and that the country-specific tests for co-integration should be independent. If this is not the case, the test procedure is invalid.

variable like the employment protection provides meaningful results. Instead, I rely on the economically plausible connection between unemployment and institutional variables and accept the risk of spurious regression.

3.2 Achieving robustness

The discussion on the fragility of econometric models is an old one. Micro-economists using data on households or firms can usually exploit large data sets. In contrast, macro-economists studying cross-country phenomena with annual or quarterly data usually suffer from a limited number of observations. The number of countries in the world is restricted to roughly 200, and the comprehensive collection of comparable data started only three to four decades ago, at least for a couple of the relevant economic information.

Furthermore, for some economic questions, theory might offer only limited guidance for empirical estimations since it develops economic concepts like human capital or the reservation wage. These concepts are difficult or even impossible to measure and can therefore only be approximated by indicators. The fixed effects panel data estimator described in the preceding section is therefore inappropriate since it requires a well-specified model to produce reliable results. The sensitivity of empirical results to variations of the underlying model, be it through a variation of the explanatory variables, the dynamics of the model or the estimator, is an important issue in macroeconomics.

One possibility of increasing the robustness of the results if only few observations are available is to test the sensitivity of the findings by applying systematic robustness checks. Model averaging techniques have performed especially well as a tool for increasing robustness. The following section gives a brief overview on the development of model averaging techniques. This serves

as the basis for the *BACE* approach for fixed effects panel data models, which is the key econometric method of chapters 4 and 5. For a more comprehensive literature survey, I refer to Moral-Benito (2010).

3.2.1 Extreme bounds tests and model averaging

Leamer (1983) documents the discussion on econometric robustness in the early 1980's and particularly points out the importance of systematically testing econometric models for fragility. Leamer and Leonard (1983) therefore introduce the concept of extreme bounds of the coefficient estimates. In this approach, the extreme values of the coefficient of a variable of interest are determined by estimating models that differ in a previously defined set of restrictions. These restrictions are imposed on the coefficients of some control factors. If the distance between the extreme values is large, Leamer and Leonard (1983) conclude that the estimation of the impact of the variable of interest is fragile and the reliability of the results is limited. If, however, the distance is small, this supports the conclusion that the influence of the variable of interest is not fragile, but robust. McAleer et al. (1985) heavily criticize this version of an extreme bounds test. He challenges the subjective decision to classify the explanatory factors in fix (variables which are always included) and doubtful (variables which work as control variables) ones. This classification can have a substantial impact on the results.

As a reply, Leamer (1985) emphasizes that extreme bounds analysis refers to a concept of global sensitivity analysis, comparing models that differ in several dimensions. When, for instance, different assumptions on the classification of explanatory variables exist, this points out the sensitivity of the model setup and is implicitly part of the global sensitivity analysis.

Despite this general definition, the literature has mainly focused on the question of whether the variation of the set of explanatory variables has a substantial impact on the reliability of one or more target variables. One of the most influential studies implementing that kind of extreme-bounds analysis has been conducted by Levine and Renelt (1992) for the determinants of economic growth. In their setup, the significant influence of a target variable on the dependent variable is insensitive to the model choice if the coefficient is significant and of the same sign for both extreme bounds. The conclusion that only very few variables remain significant once it is checked for sensitivity provoked others to criticize the restrictive determination of the extreme bounds. Granger and Uhlig (1990) emphasize the problem that all models, regardless of the model quality, receive an equal weight. Nonsensical models in a statistic or economic sense could be the cause of an extreme value that renders the variable of interest sensitive. They propose to include additional information about the model fit.

This critique is also taken up by Sala-I-Martin (1997). He changes the focus from only analyzing the extreme values to looking at the whole coefficient distribution. A weight based on the model likelihood is assigned to each model in order to put more weight on better models. In doing so, it can be avoided that a few low-quality models, in terms of explanatory power, dominate the extreme values or even the whole sensitivity analysis. An application to growth regressions shows that the negative picture conveyed by Levine and Renelt (1992) claiming that hardly any explanatory variable passing the sensitivity analysis can be corrected to more positive ones.

While for an extreme bounds analysis it is required to estimate several models where only the extreme values are used, the approach of Sala-I-Martin (1997) exploits all available information by constructing weighted averages over

the results of all models. However, the method is restricted to include only a previously defined number of variables in each regression. Hence, models consisting of a distinct number of explanatory variables cannot be compared. The reason for this is that the likelihood of each model which forms the basis for the construction of model weights increases with the number of included explanatory factors. Hence, larger models receive a higher weight per definition.

3.2.2 Bayesian model averaging

Frequentist approaches to model averaging like the ones implemented by Levine and Renelt (1992) or Sala-I-Martin (1997) are based purely on ad-hoc specifications concerning the model weights and size. In contrast, Bayesian methods treat the models and model weights as additional parameters which can be estimated. Hence, turning to Bayesian methods when model uncertainty is present is a small and natural step.

According to Hoeting et al. (1999), the first approaches for using a kind of Bayesian model averaging dates back to the 1960's. However, Moral-Benito (2010) mentions that it has been quite unpopular until the 1990's when sufficient computer resources started to pave the way for practical implementation. In the following, a description of the statistical representation of the Bayesian model averaging framework in accordance to Koop (2003) is given and the connection to the *BACE* method is provided. The method was first used in the context of growth regressions but has been applied to other economic problems, as well. Further applications of the approach can be found in, for instance, Lamla (2009) for the long-run determinants of pollution, Schrimpf

(2010) for international stock return predictability, or Bryant and Davis (2008) concerning the demand for meat in the US.

The calculation of posterior model probabilities

Let us assume that there are K explanatory variables available and that a possible regression model is

$$Y = X\beta + \epsilon \tag{3.7}$$

where Y and ε are vectors of size NTx1, X is a matrix of size NTxk with NT representing the total number of observations, and β is of size kx1. Indeed, this model is comparable to equation (3.6), but to facilitate the description of Bayesian model averaging techniques the notation will be kept as general as possible. A classical model would be deterministic about which kϵK variables to include in X. If the researcher is uncertain which variables should be included in X, and if this uncertainty comprises all K variables, 2^K possible models consisting of each combination of explanatory factors exist.

Now, the Bayes rule is introduced with which the model uncertainty can be explicitly stated. Following Koop (2003), the Bayes rule expresses in probabilities what we do not know (what is the true model) versus what we do know (the data). It holds that

$$p(M_i|\Delta) = \frac{p(\Delta|M_i)p(M_i)}{p(\Delta)} \tag{3.8}$$

where $p(M_i|\Delta)$ is the posterior probability of model M_i given the data $\Delta = \{Y, X\}$, and $p(M_i)$ is the prior probability of model M_i. $p(\Delta|M_i)$ is the marginal likelihood and expresses assumptions on the model characteristics,

and $p(\Delta)$ is the prior probability of the data.[41] The posterior model probability is necessary for model comparisons and is the target value. The following descriptions serve the determination of the remaining factors of equation (3.8). The most important part of equation (3.8) is the marginal likelihood. If one is interested in model parameters and applies the Bayes rule accordingly, the corresponding equation becomes

$$p(\theta^i|\Delta, M_i) = \frac{p(\Delta|\theta^i, M_i)p(\theta^i|M_i)}{p(\Delta|M_i)} \tag{3.9}$$

where θ^i is a set of parameters of, for instance, explanatory variables one is interested in. Equation (3.9) is helpful since the term $p(\Delta|M_i)$, which is the marginal likelihood of equation (3.8), appears in the denominator. According to Koop (2003), equation (3.9) can be rearranged. By integrating both sides and using the fact that $\int p(\theta^i|\Delta, M_i), d\theta^i = 1$,

$$p(\Delta|M_i) = \int p(\Delta|\theta^i, M_i)p(\theta^i|M_i)d\theta^i \tag{3.10}$$

is obtained. Consequently, in order to calculate one specific model probability given in equation (3.8), the marginal likelihood of equation (3.10), the prior model probability $p(M_i)$ and the prior density $p(\Delta)$ are required.

However, model probabilities only make sense when at least two models are compared. To do so, the relation of two models M_i and M_j using the respective

[41] Classical and the Bayesian econometrics use a different wording. Mixtures of both worlds like the *BACE* approach make it difficult to be explicit on the meaning of, for instance, the marginal likelihood or the prior density of the data. It is not intended to give an extensive introduction to Bayesian econometrics (Books like the one written by Koop (2003) are clearly more helpful) but to establish the originally Bayesian idea of model averaging which is necessary to understand the implementation of the *BACE* approach. This is not possible without introducing some explicitly Bayesian expressions.

model probabilities is expressed as

$$\frac{p(M_i|\Delta)}{p(M_j|\Delta)} = \frac{p(\Delta|M_i)p(M_i)}{p(\Delta|M_j)p(M_j)}. \tag{3.11}$$

The posterior probabilities correspond to (3.8). It is obvious that the term $p(\Delta)$ cancels out. $\frac{p(\Delta|M_i)}{p(\Delta|M_j)}$ which is the ratio of the marginal likelihoods of both models is called the Bayes factor and $\frac{p(M_i)}{p(M_j)}$ is the odds ratio. The problem with the marginal likelihood described in equation (3.10), which can be interpreted as a density function, is that one needs to specify a prior distribution. The researcher needs to express some knowledge on the distribution of the likelihood function. Fernandez et al. (2001) point out that the choice of the prior distribution can have a large impact on the calculation of the Bayes factor. Unfortunately, if the number of possible models is large the specification of a prior distribution for each model becomes infeasible. To overcome this problem, Fernandez et al. (2001) propose using a *natural conjugate g-prior specification* developed by Zellner (1986) which allows to describe the model with a minimum of subjective information on the prior. A benchmark prior for a linear regression model with model uncertainty concerning the choice of explanatory variables is also developed.[42] However, although the degree of subjective information the researcher needs to introduce on the g-prior is rather low, the method is difficult to implement and it is complicated to interpret the results, especially from the perspective of classical or frequentist econometrics.

[42]The explicit formulation of the marginal likelihood function given the g-prior specification and the corresponding descriptions of the general prior setup are not given here. For the interested reader, more detailed information is provided in Fernandez et al. (2001) and Ley and Steel (2009).

The *BACE* approach

Instead of specifying the g-prior, Raftery (1995) proposes using the *Bayesian Information Criterion* (BIC) which has been developed by Schwarz (1978) to approximate the marginal likelihood.[43] The BIC representation is

$$BIC_i = log(L_i) - \frac{1}{2}k_i log(NT) = log(p(\Delta|M_i)) \tag{3.12}$$

where L_i is the likelihood function of model M_i. NT is the number of observations, and k_i is the number of included explanatory factors in model M_i. If the estimation is performed by least squares, $-\frac{1}{2}NTlog(SSE_i)$, with SSE_i being the sum of squared residuals of the particular estimation of model M_i, is an appropriate specification of the likelihood function L_i (see Davidson and MacKinnon (2004), page 676). Similar to Raftery (1995), Sala-I-Martin et al. (2004) use the BIC approximation with the likelihood specification of Davidson and MacKinnon (2004) and receive:

$$p(\Delta|M_i) = p(M_i)exp(-0.5BIC_i). \tag{3.13}$$

Fernandez et al. (2001) provide an alternative derivation for the marginal likelihood approximation used in Sala-I-Martin et al. (2004). They show that the approximation is just a g-prior specification also used in equation (2.12) in Fernandez et al. (2001), with $g = \frac{1}{NT}$. Note that choosing a value for g means selecting the parameter for penalizing larger models. The smaller the g value, the higher the penalty term. With NT going to infinity, this asymptotically

[43] Raftery (1995) thus actually developed the main contributions for the *BACE* approach introduced by Sala-I-Martin et al. (2004), but he used it only to calculate Bayes factors to compare two specific models without applying it in a classical econometrics framework. This is mainly due to missing computational advances to face the estimation of the large number of possible models.

leads to the marginal likelihood approximation used in Sala-I-Martin et al. (2004). Ley and Steel (2009) remark that this asymptotic approximation may not be adequate in the growth context due to the low number of observations. It can indeed be critical for the 88 observations used in Sala-I-Martin et al. (2004). Since the data set I exploit here is considerably larger and consists of 408 observations, asymptotic consistency is more likely given.

Ley and Steel (2009) criticize the implicit assumption of Sala-I-Martin et al. (2004) on the g-prior since this might not be the best prior choice.[44] Admittedly, the prior specification of Sala-I-Martin et al. (2004) is, at least from a Bayesian perspective, just a special case of the g-prior specification and therefore inferior to the benchmark prior specification. However, as already mentioned, the connection to classical econometrics is only given for this specific prior specification through the BIC approximation.

Introducing equation (3.13) in (3.11) delivers a model comparison for two models M_i and M_j:[45]

$$\frac{p(M_i|\Delta)}{p(M_j|\Delta)} = \frac{p(M_i)}{p(M_j)} NT^{\frac{(k_j-k_i)}{2}} \left(\frac{SSE_i}{SSE_j}\right)^{-\frac{NT}{2}}. \tag{3.14}$$

The comparison of two models can be extended to incorporate a larger set of 2^K models with

$$\frac{p(M_i|\Delta)}{\sum_{j=1}^{2^K} p(M_j|\Delta)} = \frac{p(\Delta|M_i)p(M_i)}{\sum_{j=1}^{2^K} p(\Delta|M_j)p(M_j)} \tag{3.15}$$

[44] Indeed, using the criterion developed by Fernandez et al. (2001) to chose $g = \frac{1}{max\{NT, K^2\}}$, the estimations of chapter 4 with $K = 20$ already use the recommended prior. In contrast, for the estimations of chapter 5 the prior structure is not optimal since $K = 67$.

[45] Note that the term NT is equivalent to the total number of observations. This remark can be helpful in order to avoid confusion. The literature often considers the one-dimensional case, i.e. cross-section data like, for example, Sala-I-Martin et al. (2004) with N or T indicating the total number of observations.

where 2^K different models are considered. Consequently, from equation (3.12) it can be derived:

$$P(M_i|\Delta) = \frac{p(M_i)NT^{-k_i/2}SSE_i^{-NT/2}}{\sum_{j=1}^{2^K} p(M_j)NT^{-k_j/2}SSE_j^{-NT/2}}. \tag{3.16}$$

Equation (3.16) gives a measure for the quality of model M_i in comparison to all 2^K considered models and is the basis for the empirical applications.[46] In contrast to the purely Bayesian approach of model averaging, significance values for the explanatory variables as well as classical coefficients and standard errors can be calculated for which Raftery (1995) provides a rationale. Suppose that one is interested in a coefficient β_d. By summing up the β_d's over all models it is included in, and using the weighting given by the corresponding model probabilities taken from equation (3.16), one gets a measure for the posterior inclusion probability of β_d. This measures the importance of the coefficient β_d for explaining the dependent variable. If the corresponding variable is often included in models with higher quality, the posterior inclusion probability of β_d is comparably large. Raftery (1995) expresses this as

$$p(\beta_d \neq 0|\Delta) = \sum_{A_1} p(M_k|\Delta) \tag{3.17}$$

with $A_1 = \{M_k : k = 1, ..., K; \beta_d \neq 0\}$ reflecting the set of models that include β_d. As already mentioned, weighted coefficients and variances can be calculated, as well. The posterior mean and the posterior variance conditional

[46] Note the change in the notation from p to P concerning the model probabilities. P is assigned to the ratio of the probability of one specific model to the sum of the probabilities of all models. Up to now, the posterior inclusion probability referred to equation (3.8). In the following, the term posterior model probability is used for the probability ratio $P(M_i|\Delta)$ of equation (3.16).

on inclusion are

$$\mathbb{E}(\beta|\Delta) = \sum_{j=1}^{2^K} P(M_j|\Delta)\beta_j \tag{3.18}$$

$$VAR(\beta|\Delta) = \sum_{j=1}^{2^K} P(M_j|\Delta)Var(\beta|\Delta, M_j) + \sum_{j=1}^{2^K} P(M_j|\Delta)(\beta_j - E(\beta|\Delta))^2 \tag{3.19}$$

where β_j is the estimated coefficient of model j. Both moments incorporate information of all models which are considered. Note that the posterior variance captures two sources of uncertainty. The first term on the right hand side reflects the estimation uncertainty within a model. The second term considers the estimation uncertainty across different models.

The foregoing discussion mainly dealt with finding a Bayesian justification for a classical model averaging approach. This has been achieved by applying the BIC approximation to the marginal likelihood for the model probability given the data Δ. The specification of the prior model probability $p(M_i)$ is still pending. The most natural approach is to give equal prior weight to all models that are estimated. This gives $p(M_i) = \frac{1}{2^K}$ with K being the total number of explanatory variables.

However, for large numbers of K, very small and very large models receive the same probability as medium-sized models. If model averaging is carried out in order to decide which variables are important and which are not, this prior model probability specification is unreasonable. In this case it is helpful to abstract from giving equal weights to all models. This avoids the growth of the expected model size with the number of explanatory variables, as mentioned by Sala-I-Martin et al. (2004). Accordingly, Hoeting et al. (1999) mention

that taking information on the prior structure of the model probability into account can improve the estimation. They propose specifying the prior model probability as follows.

$$p(M_i) = \prod_{j=1}^{p} {\pi_j}^{\delta_{ij}} (1 - \pi_j)^{(1-\delta_{ij})} \tag{3.20}$$

where $\pi_j \epsilon [0, 1]$ is the prior inclusion probability of $\beta_j \neq 0$ and δ_{ij} is 1 if the variable j is included in model i, and 0 otherwise. In order to be able to calculate the prior inclusion probability of a variable, Sala-I-Martin et al. (2004) introduce the prior model size $\overline{k}$. This is the expected number of explanatory variables the model consists of before seeing the data. Note that the prior inclusion probability π is specified as $\frac{\overline{k}}{K}$ and equal for all k variables. The lower the value for $\overline{k}$, the more penalized the larger models become. Generally, if π_j in equation (3.20) is equal to 0.5, all models receive equal importance. If $\overline{k} = \frac{K}{2}$ then $\pi_j = \frac{\overline{k}}{K} = 0.5$ with the same prior model probability for each model. Ley and Steel (2009) state that if $\frac{\overline{k}}{K} < 0.5$, larger models are penalized, while for $\frac{\overline{k}}{K} > 0.5$, larger models are favored. The selection of the prior model size value is subjective, and often merely reflects the researcher's expectations. Ley and Steel (2009) recommend making the prior inclusion probability of a variable random by imposing a distribution on the prior inclusion probability. This makes sense when the researcher has no prior information on the mean model size and when only one prior model size specification is selected. Here, altering the choice of the prior model size serves as a sensitivity check.

The prior model probability of model M_i which involves k_i explanatory variables is

$$p(M_i) = \left(\frac{\overline{k}}{K}\right)^{k_i} \left(1 - \frac{\overline{k}}{K}\right)^{K-k_i}. \tag{3.21}$$

Note that the prior model probability is an increasing function of $\overline{k}$. Different values for the prior model size can now be assumed to calculate the corresponding prior model probabilities. Note that the same estimations produce higher prior model probabilities $p(M_i)$ for larger prior model sizes $\overline{k}$. The prior model size is a subjective choice but easy to check for sensitivity. With this information at hand, the posterior model probabilities can now be calculated by inserting (3.21) into equation (3.16).

3.2.3 Sampling and convergence criteria

The *BACE* approach requires the estimation of 2^K regressions. Obviously, when K is large this quickly becomes practically infeasible. For instance, Sala-I-Martin et al. (2004) include 67 different explanatory variables which requires the estimation of $1.48x10^{20}$ regressions. While in chapter 4 the BACE approach is applied to models consisting of a maximum of 20 regressors[47], the model averaging in chapter 5 is performed for models containing up to 91 variables.[48] Running all regressions is highly time-consuming or even infeasible without the appropriate technical equipment. Different approaches like the stochastic search variable selection (SVSS) or the Markov chain Monte Carlo Model composition (MC^3) have been discussed in the literature[49] to reduce the number of regressions to be estimated. When such methods are applied it is crucial to ensure that the results are not biased due to the restriction to a set of models.

[47] Hence, roughly a million models need to be estimated what is computationally feasible.

[48] The 91 variables refer to the model size without fixed control factors. The model taking interdependencies into account additionally considers 19 control variables which are included in all estimations.

[49] Further examples and the corresponding references are mentioned in Clyde et al. (1996).

Sala-I-Martin et al. (2004) apply a more simple approach which refers to as *stratified sampler*. Random sampling is used to select models from the whole model space. If the number of selected models is limited, randomly selecting models implies that some could appear more often than others. While asymptotic consistency is given, a large number of explanatory factors could be a problem. Model weights could then be wrongly calculated. This is relevant for the calculation of the posterior model probabilities $P(M_i|\Delta)$ in equation (3.16) since the randomly selected models appear in the denominator. If predominantly models with low weight are selected, the posterior model probabilities are overestimated. The model probabilities are therefore adjusted by the particular sampling probabilities to avoid the over-sampling of some models. Equation (3.16) can be rewritten as[50]

$$P(M_i|\Delta) = \frac{\frac{p(M_i)}{p_s(M_i)} NT^{-k_i/2} SSE_i^{-NT/2}}{\sum_{j=1}^{H} \frac{p(M_{m(j)})}{p_s(M_{m(j)})} NT^{-k_{m(j)}/2} SSE_{m(j)}^{-NT/2}}. \tag{3.22}$$

H gives the number of sampled models and $m(i)$ is an indicator for model i that is sampled. Hence, the weights are based on models that are actually sampled, and are adjusted for their sampling probabilities. For the random sampling procedure, one could chose the sampling probabilities $p_s(M_i)$ being equal to the prior model probabilities $p(M_i)$ such that both cancel out what facilitates the calculations. This is the case when the sampling probability of each variable is set such that it equals the variable's prior inclusion probability.

Random sampling suffers from the estimation of numerous low-quality regressions, which slows down the convergence process substantially. Instead, Sala-I-Martin et al. (2004) extend the random sampling by adjusting the sampling probabilities of the variables with their posterior inclusion probabilities

[50]This equation is given in the web appendix to Sala-I-Martin et al. (2004).

after a burn-in phase. More precisely, the sampling probability of each variable is updated using information from the current posterior inclusion probabilities. Since the model sampling probabilities $p_s(M_i)$ depend on the sampling probabilities of each variable, models with "better" variables, i.e. factors that have higher posterior inclusion probabilities, are favored. Sala-I-Martin et al. (2004) adjust the sampling probabilities after each $100,000$ draws and force the adjusted sampling probabilities to be in the interval [0.1, 0.85] in order to avoid the sampling of only a small set of variables. The sampling probabilities can be updated with distinct adjustment frequencies. Reasonable alterations which I carried out of both the adjustment frequency as well as of the interval bounds have no detectable effect on the results.

Furthermore, Sala-I-Martin et al. (2004) define two convergence criteria. The first focuses on changes of the standardized posterior mean of equation (3.18). The second looks at changes of the posterior inclusion probabilities of equation (3.17). The convergence criterion is satisfied when the change in the standardized posterior means of all coefficients is smaller than 10^{-6} or if the change in the posterior inclusion probabilities for all variables is smaller than 10^{-4}, both for ten consecutive regressions. The convergence check is executed each $10,000$ regressions. Note that variations of this value do not affect the speed of convergence.

The application of the stratified sampler algorithm in chapter 5 reveals that the first convergence criterion focussing on the change in the standardized posterior means works well. Robustness checks using the same setup for repeated runs indicate changes in the second decimal place for few coefficients. Since an increase in precision to 10^{-7} extends the speed of convergence considerably, I rely on the default criterion. In order to avoid that such changes affect the outcome, each run is carried out twice with the identical setup. If notable dif-

ferences occur, a change to the more precise convergence criterion is optional. The convergence criterion focussing on the change in the posterior inclusion probabilities is less reliable. Robustness checks disclosed changes in the first decimal place for a few coefficients.

In the literature on Bayesian model averaging, alternative sampling procedures like MC^3 sampling carried out by Moral-Benito (2011) or Gibbs sampling used by Doppelhofer and Weeks (2009) are discussed. In order to ensure that the results are not driven by the sampling procedure, Sala-I-Martin et al. (2004) test the quality of the stratified sampler with the help of a test data set. This test shows that the stratified sampler delivers adequately precise results compared to the MC^3 sampler.

Here, a test of the quality of the stratified sampler is performed as well. Chapter 4 basically applies the *BACE* approach without sampling. Only 20 explanatory variables are included which permits the estimation of all models. For testing purposes, the sampling procedure is applied to this data with restrictions to the number of regressions to $500,000$. The frequency of the sampling probability adjustment as well as the convergence check procedure are lowered, accordingly. The results produced with the stratified sampler and the findings of averaging over the whole model space do not show considerable differences. Only some coefficient values show changes in the third decimal place. Due to this evidence, the stratified sampler can be reliably used in the estimations of chapter 5.

To summarize, this section describes the evolution of methods for checking for robustness as the basis for the *BACE* approach, which is the key method of the empirical estimations of chapter 4 and 5. More specifically, the fundamental principles of Bayesian econometrics and the connection of a frequentist approach to model averaging have been explained in order to give insights on

the mechanism of the *BACE* method. This is necessary to properly evaluate and classify the empirical findings. The chapter has emphasized that this approach is particularly relevant for economic questions which empirically need to be answered with small data sets, plenty of potentially relevant explanatory factors, and limited guidance from theoretical models about which explanatory factors to consider. Finally, the technical fundamentals for the application of the *BACE* approach in chapters 4 and 5 are given to help with understanding and reproducing the empirical results.

3.3 Data

The presentation of the data used in the empirical estimations and the discussion of their main characteristics in detail is provided in this section. All variables are constructed such that the data set comprises of annual data without missing observations. The *BACE* approach only works for such a balanced data set which permits the availability of each data point.[51] Data for 17 OECD countries from 1982 to 2005 is considered. Note that detailed information on sources and construction methods for the relevant variables as well as a correlation matrix for all institutional factors can be found in Appendix A.

3.3.1 Unemployment

A country's labor market performance is usually expressed by the number of employed or unemployed people. For cross-country and panel studies, the number of unemployed is more convenient. Employment figures are more volatile

[51] An unbalanced data set causes the estimation of models with different numbers of observation. This impedes the comparison of models and the construction of proper model weights.

and, thus, more difficult to compare. In order to ensure comparability across countries, unemployment needs to be set in relation to the overall population, the total labor force, or other aggregate measures of potentially available workers. It is also necessary to use the same definition of an unemployed person in all countries.

The OECD takes this into account and delivers harmonized unemployment rates. These series are based on survey data and on the definition of the International Labour Organization (ILO) for unemployed persons and the labor force. While the harmonized unemployment rates might differ from national ones, comparability over countries is given. The unemployment rate is calculated as the number of unemployed persons as a fraction of the total labor force. Note that the ILO calculates unemployment rates from their labor force survey, which are not harmonized over countries. These series are partially based on different calculation methodologies compared to the OECD measure of unemployment. Hence, although both rates generally use labor force survey data to measure unemployment, the series are not identical. Generally, when not noted differently, the OECD measure is used as the dependent variable in the estimations.

Table 3.1 summarizes the evolution of the country-specific unemployment rates by means of taking 5-year averages over the sample period. The table displays a heterogeneous picture of labor market performances in OECD countries both across countries as well as over time. Countries like Austria or Japan started at a very low level and exhibited slightly increasing unemployment rates. The unemployment rate level in these countries is still comparably low in the last period. Spain, Belgium or Canada reported relatively high unemployment rates in the first half of the 1980's with a falling trend in the first half of the new century. In countries like Italy or France, the unemployment

Table 3.1: Harmonized unemployment rate

	82-86	87-91	92-96	97-01	02-05
Australia	8.48	7.38	9.40	7.20	5.68
Austria	3.38	3.19	3.88	4.01	4.62
Belgium	10.32	7.80	8.96	8.10	8.15
Canada	10.92	8.50	10.42	7.80	7.33
Denmark	7.14	6.51	7.79	4.81	5.09
Finland	6.16	4.42	14.92	10.62	8.85
France	8.84	9.06	10.98	10.02	9.05
Germany	5.24	4.22	7.76	8.38	9.53
Italy	7.76	9.28	10.32	10.56	8.20
Japan	2.62	2.37	2.81	4.39	4.94
Netherlands	8.30	6.55	6.17	3.41	3.93
Norway	2.75	4.49	5.88	3.34	4.18
Spain	15.86	14.50	17.76	13.14	10.50
Sweden	3.16	2.07	8.46	7.04	6.03
Switzerland	0.85	0.87	3.65	3.18	4.09
United Kingdom	10.86	8.28	9.14	5.84	4.90
United States	8.20	5.88	6.30	4.48	5.60

The unemployment rates are the country-specific averages over the corresponding time period.

rate in the last period returned to the level of the first period after an inverse u-shape development. The evolution of the unemployment rate in Switzerland appears to be exceptional, with values close to zero during the first two periods. The other extreme is Spain with unemployment rates of roughly 15% at the beginning of the sample period and almost 18% during the first half of the 1990's.

The institutional system in continental Europe which is regarded as being fairly generous has often been blamed for causing poor labor market performance. The table also shows why an anglo-saxon labor market is often distinguished from a continental European one. While the United States, Canada, Australia and the United Kingdom all exhibited considerably decreasing unemployment rates over time, the labor market situation deteriorated in Austria,

Germany, France, Italy or Switzerland. However, two additional continental European countries, Belgium and the Netherlands, successfully reduced their unemployment rates from the first to the last period. This indicates that the aggregation of continental European countries is oversimplified. There seems to be more heterogeneity within continental Europe than often stated. Later in this chapter it will become clear that continental Europe is largely heterogeneous, not only concerning the labor market performance, but also concerning the regulation of the institutional system.

3.3.2 Determinants of Unemployment

Labor tax system

The calculation of taxes on labor focus on the relation between the net wage received by the employee and the cost of labor levied on the employer. Taxes which further increase the wedge between the cost of labor paid by the employer and the employees' actual disposable income are added to measure the overall tax burden in a country. The former type of labor tax is called the direct and the latter the indirect labor tax burden. Several approaches, explained in greater detail in Sørensen (2004), are feasible to calculate both direct and indirect taxes. For this study, an approach that guarantees both the comparability across countries as well as the completeness of the series for all countries and years is needed. The approach provided by Nickell and Nunziata (2001) and updated by Nickell (2006) fulfills both requirements. Three indicators are constructed to represent the labor tax system, payroll, income, and consumption taxes.[52]

[52]Section 2.2.1 discusses the importance to distinguish between all three types of labor taxation.

Table 3.2: Labor tax system

	Payroll tax		Income tax		Consumption tax	
	82-93	94-05	82-93	94-05	82-93	94-05
Australia	0.00	0.00	16.17	16.72	18.38	19.16
Austria	13.82	16.00	17.03	18.31	21.87	21.68
Belgium	19.48	20.17	6.56	6.26	18.12	22.35
Canada	5.64	6.13	17.46	19.10	21.07	22.09
Denmark	0.49	0.03	29.73	32.25	27.10	29.49
Finland	16.97	23.36	19.86	21.90	22.77	24.32
France	28.05	27.25	12.66	13.69	22.74	24.92
Germany	13.08	15.55	17.64	16.72	15.70	17.89
Italy	23.49	27.64	12.71	14.99	13.16	21.09
Japan	7.84	8.31	13.36	12.53	12.43	13.46
Netherlands	13.08	8.06	22.47	19.96	16.38	21.75
Norway	15.96	14.17	18.50	19.46	24.12	26.04
Spain	20.86	21.22	20.39	21.24	15.06	16.45
Sweden	28.08	24.82	14.31	14.03	29.28	29.73
Switzerland	4.89	5.71	9.76	10.79	3.03	4.37
United Kingdom	6.67	6.82	13.51	13.23	20.65	20.24
United States	6.39	6.43	13.44	14.29	10.53	10.13

The values are country-specific averages from 1982 to 1993 and from 1994 to 2005.

The payroll taxes measure the employers' social security contributions as a share of the total compensation of employees. The income tax displays the direct taxes paid by the employee as a share of the household current receipt. Finally, the product taxes over the household's final consumption expenditure represent the consumption tax. All three indicators are expressed as percentages. The data is summarized in table 3.2 where the whole period is separated in two sub-periods, from 1982 to 1993 and from 1994 to 2005.

Most countries exhibited a considerable change in the tax rates from the first half to the second half of the sample. For instance in Italy and Finland, a substantial increase of the payroll tax can be observed while the same holds true for Belgium or the Netherlands in regards to the consumption tax. However, comparing the two periods before and after 1994 does not reveal any com-

mon tendency over all countries concerning reforms of the labor tax system. While there seems to be sufficient variation within each country, the variation between countries is also remarkable. Some countries like Australia for the payroll tax or Switzerland for the payroll and the consumption tax raise comparably low taxes. Others like Denmark or Sweden have a significantly higher tax burden.

Bargaining system

The literature generally focuses on a subset of the following bargaining system indicators: Union density, union coverage, minimum wages, bargaining coordination and bargaining centralization.[53] A close look at the definitions of these variables is necessary, in order to describe how the factors can be distinguished from each other, and why all of them are considered as being potential determinants of unemployment.

Union density, measuring the union members dependently employed as a share of all dependently employed, serves to capture the workers' bargaining power. It gives a formal description of the union power. The higher union density the better the unions' bargaining position. Union coverage provides the share of dependently employed affected by bargaining agreements. It goes a step further and incorporates the actual range of the union bargain. This can result in substantial differences between both indicators. According to Flanagan (1999), official regulations that bring about the automatic extension of wage agreements bargained by the union to non-union members could result in low union density rates due to free-rider behavior. Under these circum-

[53]Other measures of the bargaining power like strikes or lockouts cannot be considered since comparability over countries cannot be ensured due to distinct legal control, or since too much data is missing.

stances, a fall in the official union membership rate must not be necessarily associated with a reduction in union bargaining power. Union density would then wrongly measure the degree of bargaining power.

From this point of view, union coverage should be used to measure bargaining power. Taking both concepts for the measurement of the bargaining power into account is nevertheless important for the following reason. The literature often takes union density as the preferred measure for bargaining power. This indicator is available for a larger set of countries and for a longer time period. If both indicators are of equal importance for explaining unemployment in the analysis performed in chapter 4, union coverage does not contain additional relevant information. Union density is then a valid indicator for bargaining power and cross-country studies are less restricted in terms of country and time coverage. If, however, both factors contribute differently to the explanation of unemployment, union coverage is assumed to be more suitable for capturing bargaining power. Findings of studies using union density have at most limited explanatory power, and future studies should not focus on this indicator.

Similar to union density and coverage, bargaining centralization and coordination indicators are also closely related. Bargaining centralization is an indicator ranging from 1 to 5 and shows the formal level at which bargaining is conducted. According to Visser (2009), a value of 1 means that bargaining mainly takes place at the local or company level, indicating low centralization. Level 2 bargaining refers to the sectoral or industry level with additional local or company bargaining. A value of 3 represents bargaining at the sectoral or industry level, while level 4 means national or central level bargaining with additional sectoral, local, or company bargaining. A value of 5 refers to bargaining at the national or central level. Soskice (1990) argues that bargaining

centralization only measures the level at which bargaining is set. Bargaining coordination, in turn, is a more general concept which comprises, besides bargaining centralization, of other forms of centralization. Its indicator ranges from 1 to 5 with 1 indicating fragmented bargaining at the company level. A level of 5 shows economy-wide bargaining based on either enforceable agreements between different parties in the bargaining process, or governmental interventions such as wage schedules or ceilings. The description of the additional values of coordination is given in Appendix A.

Even if a country exhibits rather decentralized bargaining, coordination could be high with a considerable impact on the wage bargaining process. Soskice (1990) takes Switzerland and Japan, both with highly decentralized, company-level bargaining, as examples to illustrate such distinct centralization and coordination measures. Coordination comes in through higher-level employer organizations in Switzerland, and informal wage cartels in Japan.

While the coordination indicator seems to be superior to the centralization measure, it suffers from the problem that the assignment of scores to the informal part of the coordination indicator is a subjective choice. It depends on the researcher's opinion, as pointed out by Kenworthy (2001). Considering both indicators jointly and comparing their impact on unemployment can help decide whether the coordination measure is indeed preferable. If the bargaining coordination measure is not superior over the centralization measure, this should appear in comparably significant coefficients of both indicators. Then, the informal part of the coordination indicator turns out to be irrelevant for unemployment. Both indicators would be valid indicators for the wage bargain.

The last indicator for the bargaining system is a minimum wage indicator that ranges from 0 (no minimum wage) to 8 (national minimum wage set by the government). A description of all levels is given in Appendix A. Note,

this indicator only makes a statement about the existence and the degree of government intervention. It does not contain information about the actual minimum wage level. Such an indicator of the level provided, for instance, by the Fondazione Rodolfo de Benedetti (FRDB), would be superior, but is not comprehensively available.

It is often stated that indicators of the bargaining system are time-invariant for most countries. If a fixed effects panel data estimator is used to determine the impact of such time-invariant factors, they conflict with the country-specific fixed effects. This can cause very imprecise estimation results and lead to wrong inference. Table 3.3 shows the descriptive statistics for the three indicators of the bargaining system which are suspicious to be time-invariant for some countries: bargaining coordination (BCO), bargaining centralization (BCE), and the minimum wage (MW). The bargaining coordination indicator shows no variation over the whole time period for Germany, the United Kingdom and the United States. The remaining countries have changed its level of coordination at least once. Bargaining centralization is completely time-invariant for France, Japan, Switzerland and the United States. All other countries exhibited at least one change over time. It is assumed that there is enough within variation for both indicators to permit valid estimates. More critical is the situation for the minimum wage indicator. It shows changes in only six countries between 1982 and 2005, while the remaining eleven countries have the same minimum wage setting over the whole period. This little within variation of the minimum wage should be taken into account when interpreting the findings of fixed effects models.

The further indicators of the wage bargaining system, union density and union coverage, show much more variation in the data. Table 3.4 displays the averaged union density and coverage values for two periods. As expected,

Table 3.3: Bargaining coordination and centralization, and minimum wages

	BCO			BCE			MW		
	Max	Min	S.D.	Max	Min	S.D.	Max	Min	S.D.
Aus	5.00	2.00	1.06	4.00	2.00	1.02	7.00	1.00	2.23
Aut	5.00	4.00	0.20	4.00	2.00	0.41	1.00	1.00	0.00
Bel	5.00	4.00	0.51	4.00	3.00	0.48	5.00	3.00	1.01
Can	2.00	1.00	0.38	3.00	1.00	0.61	8.00	8.00	0.00
Den	5.00	3.00	0.83	3.00	2.00	0.38	2.00	1.00	0.28
Fin	4.00	3.00	0.46	5.00	3.00	0.78	2.00	1.00	0.44
Fra	5.00	2.00	0.61	2.00	2.00	0.00	6.00	6.00	0.00
Ger	4.00	4.00	0.00	3.00	2.00	0.41	1.00	1.00	0.00
Ita	5.00	2.00	1.06	4.00	2.00	0.70	1.00	1.00	0.00
Jap	5.00	3.00	0.96	1.00	1.00	0.00	8.00	8.00	0.00
Nld	5.00	4.00	0.20	4.00	3.00	0.46	6.00	4.00	1.02
Nor	5.00	3.00	0.74	5.00	3.00	0.87	1.00	1.00	0.00
Spa	4.00	3.00	0.48	5.00	3.00	0.68	6.00	6.00	0.00
Swe	5.00	3.00	0.72	5.00	3.00	0.72	1.00	1.00	0.00
Swi	4.00	3.00	0.50	2.00	2.00	0.00	1.00	1.00	0.00
UK	1.00	1.00	0.00	2.00	1.00	0.28	7.00	0.00	2.86
USA	1.00	1.00	0.00	1.00	1.00	0.00	8.00	8.00	0.00

BCO is the bargaining coordination, BCE the bargaining centralization, and MW the minimum wage. Max = Maximum value, S.D. = Standard deviation. All values are country-specific averages from 1982 to 2005. Aus = Australia, Aut = Austria, Bel = Belgium, Can = Canada, Den = Denmark, Fin = Finland, Fra = France, Ger = Germany, Ita = Italy, Jap = Japan, Nld = Netherlands, Nor = Norway, Spa = Spain, Swe = Sweden, Swi = Switzerland, UK = United Kingdom.

union coverage values are above union density values for both periods for most countries. Japan and Sweden are two exceptions where union density is higher than union coverage. According to Traxler (1994), the reason for this is that there are situations in some countries where specific labor market groups do not have the right to bargain. Union members need not necessarily be covered by collective agreements, and union coverage rates could be below union density rates. For some countries, the difference between union density and union coverage is remarkable. For instance, while only 12% dependently employed on average have been members of a union from 1982 to 1993 in France, the union coverage was at 90%. A similar pattern appears in the Netherlands, Germany and Spain. In contrast, union density and coverage values lie in the

Table 3.4: Union density and coverage

	Union density		Union Coverage	
	1982-1993	1994-2005	1982-1993	1994-2005
Australia	42.85	26.53	82.50	64.17
Austria	49.01	37.20	97.17	99.00
Belgium	52.75	53.00	96.10	96.00
Canada	34.97	31.63	37.92	34.32
Denmark	77.03	74.58	71.30	76.33
Finland	72.35	76.37	79.07	85.78
France	12.32	8.48	90.80	95.38
Germany	33.77	25.35	73.90	64.73
Italy	41.27	35.43	83.75	80.67
Japan	27.38	21.67	24.42	19.12
Netherlands	26.60	23.31	81.33	85.17
Norway	57.63	55.41	70.42	72.00
Spain	12.05	16.08	73.53	80.00
Sweden	82.70	80.98	79.60	90.83
Switzerland	25.91	21.13	49.63	45.33
United Kingdom	43.00	30.89	58.17	35.51
United States	16.87	13.19	19.58	15.18

The values are country-specific averages from 1982 to 1993 and from 1994 to 2005.

same range in Canada, Denmark, Finland and Sweden. The free-rider problem appears in the data, as well. In Austria, for example, union density declined from the first to the second period, while union coverage increased. Hence, the fall in union density seems to be overlaid by a de facto increase in the bargaining power measured by the union coverage.

Unemployment benefit system

Collecting aggregate data on unemployment benefits is difficult since there is no general level and duration for benefits that apply to all unemployed equally. Instead, the payments depend on factors like family status, level of the last wage received, duration of unemployment, or age. The OECD (1994) made an effort to define a concept for the calculation of country-wide indicators for un-

employment benefits which are comparable across countries. According to this OECD concept, indicators comprise information on family status, household income situation, and previous earnings level. Replacement rates for different combinations of the aforementioned characteristics are averaged to receive an aggregate indicator for unemployment benefits. Note that the values are gross replacement rates. Hence, the unemployment benefits are expressed as a percentage of previous gross earnings. Net replacement rates would clearly be preferable due to the existence of different tax systems across countries and over time. Unfortunately, such data is not comprehensively available for the required time period. Howell and Rehm (2009) studies the impact of net replacement rates which have been recently published by the OECD on unemployment, but the time series of this indicator starts in 2001. It is therefore relied on gross replacement rates in this thesis.

The OECD suggests to calculate replacement rates for three different states of unemployment; during the first, the second and third, and the fourth and fifth year of unemployment. As an example, the replacement rate for the first year of unemployment is the payment an average unemployed person receives during the first year of unemployment as a percentage of his previous earnings. For the latter two indicators, averages over the respective years are constructed. The distinction between the three states of unemployment is reasonable to capture differences in entitlement periods. Some countries provide very high benefits only for a short time period after a job loss. Others have more generous systems where long-term unemployment is less sanctioned through a cut in transfer payments. OECD data on gross replacement rates is

freely available up to 2003. Missing information for 2004 and 2005 is generated with a tax benefit model provided by the OECD.[54]

Furthermore, I follow Nickell (2006) in constructing indicators for aggregate unemployment benefits and for duration of benefit payments. The aggregate unemployment benefit indicator is the unweighted average of benefits during the first, second and third, and fourth and fifth year of unemployment. The benefit duration indicator is a weighted average of second and third year, and fourth and fifth year benefits, both as a share of first year benefits. It serves as a measure for the generosity of the benefit system for the long-term unemployed. Finally, the Fondazione Rodolfo de Benedetti (FRDB) delivers data on unemployment benefit coverage for the complete period as a fraction of job seekers entitled to benefits over the total number of job seekers.

The mean values over the complete period of first ($UB1$), second and third ($UB23$), fourth and fifth year benefits ($UB45$) as well as unemployment benefit duration (UBD), overall unemployment benefits (UBO), and unemployment benefit coverage (UBC) indicators are displayed in table 3.5. In the literature, it is often distinguished between different welfare-state models, i.e. delivering distinct strategies to successful labor market policies. Hall and Soskice (2001) provides a useful guide through the literature on varieties of capitalism. A generous welfare-state model with relatively strong governmental intervention in the labor market is often confronted with a liberal, market-driven model. While the classification of countries is not unambiguous, the generous model is often represented by northern European countries like Sweden, Finland, Denmark and Norway. The liberal model comprises of anglo-saxon countries like

[54]The OECD stopped providing detailed gross replacement rates when the publishing of net replacement rates started. Nevertheless, the original OECD gross replacement rates up to 2003 are still supplied by Nickell (2006) and the ifo DICE database.

Table 3.5: Unemployment benefit system

	UB1	UB23	UB45	UBD	UBO	UBC
Australia	24.46	24.85	24.85	1.01	24.72	0.66
Austria	36.90	26.71	26.71	0.72	30.10	0.86
Belgium	47.60	37.78	37.78	0.79	41.05	0.87
Canada	52.20	0.00	0.00	0.00	17.40	0.60
Denmark	71.11	64.26	35.71	0.75	57.03	0.85
Finland	53.28	35.21	20.00	0.55	36.16	0.57
France	59.58	35.60	15.96	0.47	37.05	0.72
Germany	38.09	22.01	22.01	0.58	27.37	0.47
Italy	23.71	13.48	0.00	0.14	12.39	0.36
Japan	29.21	0.00	0.00	0.00	9.74	0.38
Netherlands	70.16	57.80	26.05	0.64	51.34	0.56
Norway	60.46	44.92	6.67	0.49	37.35	0.65
Spain	68.81	34.15	2.36	0.32	35.11	0.45
Sweden	82.33	6.17	0.00	0.04	29.50	0.76
Switzerland	66.48	15.25	0.00	0.13	27.24	2.61
United Kingdom	21.90	10.43	16.37	0.72	16.23	0.38
United States	28.14	3.70	5.24	0.19	12.36	0.35

UB1 = First year unemployment benefits, UB23 = Second and third year unemployment benefits, UB45 = Fourth and fifth year unemployment benefits, UBD = Unemployment benefit duration, UBO = Overall unemployment benefits, UBC = Unemployment benefit coverage. The values are country-specific averages from 1982 to 2005.

the United States, Australia or the United Kingdom. The data on unemployment benefits support this distinction of welfare-state models.

While the replacement rates in Scandinavian countries, at least for the first year benefits with values between 53% and 82%, clearly exceed the sample average, the corresponding values for the anglo-saxon countries lie below 30%. Some countries like Australia do not discriminate between short-term and long-term unemployment. Both groups receive largely the same replacement rate. Others like Sweden or Switzerland build their labor market strategy on very high replacement rates during the first year of unemployment. Transfer payments converge to zero for long-term unemployed. Lowest replacement rates on average are offered in Japan with roughly 9% of the previous income. In

contrast, Denmark exhibits the most generous system with an average replacement rate of 57%.

The unemployment coverage indicator reveals a considerable heterogeneity in the entitlement to unemployment benefits. For instance, the lowest and the highest coverage rates in 2005 featured Japan and Austria with rates of 0.21 and 0.89, respectively. Similarly, the variation over time is also considerable. The Netherlands had a coverage rate of 0.19 in 1986 which increased to 0.81 in 1998. Considering the mean values, the country with the lowest coverage rate is the United States with 0.35, while the highest average coverage rates, with the exception of Switzerland, are observable in Belgium.[55]

Employment protection

Employment protection can be interpreted as costs which are imposed upon the employer in case of an employee dismissal. Unfortunately, these costs are difficult to measure. Lazear (1990) was the first to gather information on severance payments for dismissed workers and on the notice period, that is the notice period before an employment contract can be terminated. The OECD (OECD (1999), OECD (2004) and Venn (2009)) ties up on the work of Lazear by using information on severance payments and notice periods, but they construct an employment protection (EPL) indicator which comprises additional dimensions of worker protection. The OECD further makes a distinction between employment protection for regular and for temporary employment.[56]

[55] A special case is Switzerland with coverage rates larger than one. Here, partially unemployed are included in the number of unemployed entitled to benefits, but not in the number of job-seekers. Hence, the numbers for Switzerland are actually not directly comparable to those of the other countries. Unfortunately, the FRDB does not provide a disentangled characterization of job-seekers entitled to benefits which is why the data on Switzerland are used in the original form.

[56] Information on regulation of collective dismissals is also provided, but cannot be taken into account here due to missing observations for all countries from 1982 to 1985.

For regular employment, the classical information on severance payments, notice periods, and notification procedures are complemented by data like the length of the trial period or the compensation following an unfair dismissal. The employment protection indicator for temporary employment covers information like the maximum number of fixed-term contracts that can be concluded successively, or whether employees from temporary work agencies and regularly employed workers are treated equally by regulation. Both indicators range from 0 to 6 where the individual characteristics are weighted according to a rather complex system. The higher the employment protection value the higher the degree of protection of the respective type of contract. More details on the content of both indicators are delivered in Appendix A.

Similar to the wage bargaining system indicators, the employment protection indicators are suspected to show little within variation. This could make them inappropriate as explanatory variables in a panel data model with country-specific fixed effects. In order to shed some light on the characteristics of the employment protection indicators, the country-specific minimum and maximum values as well as the standard deviations from 1982 to 2005 are presented in table 3.6. As for the employment protection indicator for regular employment ($EPLr$), five countries exhibit no variation over time, while seven countries performed small-scale reforms only once. Merely in Australia, Austria, Finland, Germany and Spain do considerable changes in the degree of employment protection for regular employment occur. The employment protection indicators for temporary employment ($EPLt$) show more activity. Although again six countries have a time-invariant indicator, ten countries carried out substantial reforms. Hence, while empirical results of the employment protection indicator for regular employment definitely have to be taken with care, the indicator for temporary employment is more promising. Apart from

Table 3.6: Employment protection legislation

	EPLr			EPLt		
	Max	Min	S.D.	Max	Min	S.D.
Australia	1.50	1.00	0.25	0.88	0.88	0.00
Austria	2.92	2.37	0.19	1.50	1.50	0.00
Belgium	1.73	1.68	0.02	4.63	2.63	0.99
Canada	1.25	1.25	0.00	0.25	0.25	0.00
Denmark	1.68	1.63	0.03	3.13	1.38	0.89
Finland	2.79	2.17	0.25	1.88	1.88	0.00
France	2.51	2.34	0.07	3.63	3.06	0.27
Germany	3.00	2.58	0.12	3.75	1.25	1.00
Italy	1.77	1.77	0.00	5.38	1.88	1.40
Japan	1.87	1.82	0.02	3.00	1.00	0.62
Netherlands	3.08	3.05	0.01	2.38	1.19	0.55
Norway	2.25	2.25	0.00	3.54	2.88	0.29
Spain	3.88	2.46	0.65	3.75	3.25	0.22
Sweden	2.90	2.86	0.02	4.08	1.63	1.19
Switzerland	1.16	1.16	0.00	1.13	1.13	0.00
United Kingdom	1.12	0.95	0.08	0.38	0.25	0.05
United States	0.17	0.17	0.00	0.25	0.25	0.00

EPL reg. is the employment protection for regular employment and EPL temp. the employment protection for temporary employment. Max = Maximum value, S.D. = Standard deviation. All values are country-specific averages from 1982 to 2005.

the within variation, the 17 countries show a considerable degree of between variation. For example, the largest value of 3.88 appears for Spain during the period of 1982 to 1993. In contrast, the United States exhibits the lowest degree of employment protection for regular employment with a value of 0.17. Concerning employment protection for temporary employment, the extreme values range from 0.25 in Canada and the United States, to 5.38 in Italy from 1982 to 1996.

Product market regulation

One way of measuring the degree of product market regulation is to follow Griffith et al. (2007) and to look at data on firm profitability. Inherent to this

choice is the assumption that increased competition reduces the firms' mark-ups and their profitability. Yet, the construction of firm profitability requires capital stock data which is not comprehensively available for all 17 OECD countries from 1982 to 2005. As an alternative, the OECD delivers indicators for the degree of product market regulation. This indicator cover state controls of business enterprises, legal and administrative barriers to entrepreneurship, and barriers to international trade and investment. However, this indicator is only available for 1998, 2003, and 2008. The Fraser Institute publishes distinct indicators of business regulation. While current values are annually available, data before 2000 is only collected in five-year steps. Both types of indicator do not meet the requirement of comprehensive availability over time.

Less detailed, but fully available indicators, which are also provided by the OECD, concern the regulation in seven specific sectors. The sectors are telecom, electricity, gas, post, rail, air passenger transport, and road freight. Indicators for barriers to entry and for public ownership[57] for all seven sectors are available where increasing indicator values imply a rise in regulation. The indicators all range from 0 to 6. They are not survey-based but are based on the evaluation of objective regulatory constraints that are set by the government. For a detailed description about the construction of the product market regulation data, I refer to Conway and Nicoletti (2006).

Table 3.7 gives an overview on the development of the product market regulation for entry barriers (EB), public ownership (POS), and aggregate regulation (PMR) for two periods, 1982 to 1993 and 1994 to 2005. During the first period, the majority of countries had relatively high values showing the considerable degree of product market regulation. The United States and Canada are the only countries with a value below 3 for at least one of the

[57] This is sometimes called the level of state control in a market.

Table 3.7: Product market regulation

	EB		POS		PMR	
	82-93	94-05	82-93	94-05	82-93	94-05
Australia	4.24	1.60	4.16	3.20	3.93	2.19
Austria	4.69	2.68	4.86	4.27	4.66	3.21
Belgium	5.04	2.84	5.06	3.40	5.39	3.25
Canada	3.90	1.91	2.60	2.08	3.49	2.23
Denmark	5.45	1.67	5.44	3.75	5.21	2.52
Finland	5.12	1.74	4.76	4.13	4.85	2.69
France	5.60	3.40	5.88	5.17	5.57	3.91
Germany	5.25	2.30	4.42	2.62	5.03	2.52
Italy	5.93	3.39	5.57	4.08	5.75	3.89
Japan	5.09	2.86	2.88	1.99	4.44	2.90
Netherlands	5.24	2.22	5.40	3.57	5.21	2.63
Norway	5.17	2.50	4.99	4.21	4.72	2.95
Spain	5.27	3.09	3.68	2.75	4.87	3.05
Sweden	4.44	1.22	4.91	4.13	4.49	2.50
Switzerland	4.85	3.19	4.45	4.29	4.24	3.48
United Kingdom	3.64	1.11	3.61	0.91	3.65	1.32
United States	2.87	1.92	1.59	1.47	2.63	1.96

The values are country-specific averages from 1982 to 1993 and from 1994 to 2005. EB = Entry barriers, POS = Public ownership, PMR = Aggregate product market regulation.

indicators during the first period. During the last two decades, a lot of deregulating reforms have been performed. All displayed indicator values decreased from the first to the second period. The most profound reforms have been carried out in Denmark and Finland for barriers to entry, and in the United Kingdom for public ownership. With regard to barriers to entry during the second period, the less regulated markets are the United Kingdom, Finland, Australia, Denmark and Sweden. Entering a market is comparably costly in France, Italy, Switzerland and Spain. State control is lowest in the United Kingdom, the United States and Japan, and highest in France, Switzerland and Austria.

Control variables

The approach to construct macroeconomic shocks proposed by Nickell et al. (2005) is closely followed. All data that is necessary for the construction of the shock variables are provided by the OECD. Four shocks are considered; a real import price shock, a total factor productivity shock, a labor demand shock, and the real interest rate. The arguments as to why these shocks should be included are summarized briefly in the following.

Increasing real import prices deteriorate the terms of trade. According to Nickell et al. (2002), if real wages do not adjust to this due to, for instance, real wage resistance, the unemployment rate is expected to increase. The import price or terms of trade shock is measured as the change of the real import price times the import share in GDP. This variable is denoted as a shock since wages will adjust to the new terms of trade and the negative effect on unemployment will disappear. Similarly, a fall in total factor productivity may have temporary negative effects on unemployment if wages need some time to adjust to the productivity decline. The shock variable is constructed as the deviations from the TFP trend.

The third shock variable is a labor demand shock which should capture short-run fluctuations of aggregate labor demand that can affect unemployment. This variable is constructed as the change in the residuals of a labor demand equation. Finally, Blanchard and Wolfers (2000) point out that an increasing real interest rate raises the cost of capital and reduces capital accumulation and labor demand. It is not certain whether the real interest rate only affects unemployment in the short-run or in the long-run, as well. Nevertheless, it has been identified as a significant control variable (see, for instance,

Baccaro and Rei (2007) for the empirical relevance of the real interest rate) and is therefore included in unemployment equations.

Two additional macroeconomic control variables, the change in the inflation rate and labor productivity growth, appear in the estimations. Baccaro and Rei (2007) point out that both factors can significantly impact on unemployment. While the change in the inflation rate should reflect a short-run trade-off between inflation and unemployment, labor productivity growth is an alternative to the total factor productivity shock. Furthermore, according to Dromel et al. (2010), frictions on the financial markets which are captured by constraints to credit, are as well linked to unemployment. It is measured as the share of private credit by deposit money banks and other financial institutions over GDP. The higher the constraints to credit, the higher unemployment is due to a decline in labor demand. In chapter 4, three additional control factors appear. First, the output gap is used as an alternative measure to capture cyclical fluctuations of the unemployment rate. Second, public spending on active labor market policy as well as public spending in general are required for the interpretation of the results based on unconditional institutional effects. Both factors are provided by the OECD and measured as a percentage share of GDP.

Chapter 4

Unconditional institutional effects

Some of the findings of this chapter have been published as: Sachs (2012): What really drives unemployment? A bayesian approach to determine the impact of institutions on the unemployment rate, Economics Bulletin 32(1), 1008-1019.

The empirical evaluation of the unconditional impact of institutions on the unemployment rate using the *BACE* approach is explained in this chapter. In this context, advantages of the model averaging approach over classical estimation methods are briefly described. The main part of this chapter is dedicated to the clear identification of significant institutions. A discussion and classification of the findings concludes the chapter.

4.1 Benefits from the *BACE* approach

The estimation of unconditional institutional effects with classical approaches suffers from a low number of observations and a rather high number of available indicators. Two approaches are feasible to deal with that: including all available indicators, or selecting an indicator sub-set. Both approaches entail

severe econometric problems. These problems as well as the benefits of the *BACE* approach will be described in the following.

Estimating the impact of all indicators jointly reduces degrees of freedom substantially what lowers the reliability of the results. At the same time this procedure poses the problem of highly correlated institutions. Although resulting multicollinearity is not the worst problem in econometrics, it can nevertheless be a source of wrong inference since it causes large standard errors and low t-statistics. The data applied in this study comprises of some indicators which are highly correlated. Factors like bargaining centralization and coordination or union density and coverage measure very similar institutional concepts. They are therefore suspected to exhibit a high degree of correlation. The following table 4.1 gives the correlation coefficients for few selected institutions. The complete set of correlations is given in Appendix A in table A.1. As expected, bargaining coordination and centralization show a rather

Table 4.1: Correlations between selected institutional indicators

	BCE	UC	POS	UBO	EPLt
BCO	0.63				
UD		0.38			
EB			0.65		
UB23				0.93	
EPLr					0.51

Notes: BCO = Bargaining coordination, BCE = Bargaining centralization, UD = Union density, UC = Union coverage, EB = Entry barriers, POS = Public ownership, UB23 = Second/third year benefits, UBO = Aggregate benefits, EPLr = Employment protection for regular employment, EPLt = Employment protection for temporary employment

high degree of correlation with a value of 0.63. Similarly, entry barriers and public ownership are highly correlated. The highest correlation with a value of 0.93 is observed between second and third year benefits and aggregate benefits. This can be explained by the fact that the latter indicator to a large extent consists of the former. The same holds true for the substantial corre-

lation of the aggregate product market regulation indicator with both entry barriers and public ownership. In contrast, the union density and coverage are less correlated than assumed, as the correlation coefficient is 0.38. Generally, introducing such highly correlated institutional factors jointly in a classical model framework can lead to wrong inference as a result of multicollinearity.

Subjectively choosing an indicator sub-set to increase degrees of freedom and to avoid multicollinearity raises the question of whether the results hold when the set of explanatory factors is varied. For instance, given that employment protection is significant for unemployment in a model without unemployment benefits, it is unclear whether the same result occurs when unemployment benefits are taken into account. If unemployment benefits are significantly related to unemployment, the reduced model is misspecified and estimates can be severely biased. Estimating a model with a small set of institutions is thus prone to wrong inference due to the potential exclusion of significant variables. Both strategies are not expected to provide unambiguous evidence on which institutions are significantly related to unemployment.

The model averaging approach used in this analysis entails at least three advantages over classical methods. Firstly, the estimation of a large number of models permits the inclusion of all available indicators and the clear identification of significant variables. Secondly, these results are independent of the model specification and robustness of the findings is ensured by taking model uncertainty particularly into account. Thirdly, the significance of a variable does not depend on the inclusion of another variable, and multicollinearity between both factors does not pose a problem. According to the comment of David Draper in Hoeting et al. (1999), multicollinearity can also be a problem for model averaging methods if the number of explanatory variables is relatively large compared to the number of observations. Since the baseline model

consists of up to 20 explanatory factors plus some fixed control factors and 408 observations, it is irrelevant here.

4.2 Baseline estimation

The basic estimation procedure for the determination of unconditional institutional effects applies the *BACE* approach to a model explaining unemployment by various institutional factors. The pool of explanatory institutional factors comprises of 19 different institutional factors which can be grouped in five categories. More specifically, payroll, income and consumption taxes are included to capture the impact of the labor tax system. The bargaining system is represented by union density and coverage, bargaining coordination and centralization, and minimum wages. For the unemployment benefit system, first year, second and third year, and fourth and fifth year benefits, as well as an aggregate indicator of the benefit level are included. Additionally, indicators for unemployment benefit coverage and duration are taken into account. Finally, employment protection for temporary and regular employment as well as indicators for public ownership, barriers to entry and an overall indicator for product market regulation are considered.

The following control variables are included in all models which are estimated: a labor demand shock, an import price shock, a total factor productivity shock, as well as the real interest rate. These control factors are included to capture short-run fluctuations of the unemployment rate around its equilibrium. An indicator for the constraints to credit access is addition-

ally considered to belong to the set of institutional factors.[58] All factors are explained in detail in section 3.3. The central equation to be estimated is (3.6) which is shown here again for convenience. Details of the estimation strategy are extensively explained in chapter 3.

$$\overline{U} = \overline{X}\beta + \overline{\nu}.$$

$\overline{U}$ is the unemployment rate and $\overline{X}$ is a matrix comprising institutional variables as well as control variables which are both expected to be significantly related to the dependent variable. $\overline{\nu}$ is the error term, and all variables are deviations from country- and time-specific averages to wipe out country- and time-specific effects. The panel estimates are derived from annual data from 1982 to 2005 for 17 OECD countries.[59] The application of the *BACE* approach to this equation means that models differing in the set of institutional variables included in the matrix $\overline{X}$ are estimated. This gives the posterior inclusion probability and the (weighted) coefficient as well as the (weighted) standard deviation for each institutional factor. Variables with a higher inclusion probability are more likely to be significant explanatory factors of the dependent variable. In other words, the posterior inclusion probability gives a measure of the model fit containing a particular variable compared to models estimated without this variable. A posterior inclusion probability above the prior inclusion probability complies with a recommendation for inclusion while a value below the prior probability signifies omission.

[58] Generally, whether this financial market indicator is included as a macroeconomic control in each model, or as a further variable in the set of institutional indicators has no impact on the results. Note that the financial market indicator is highly significant with posterior inclusion probabilities close to 1 throughout the whole analysis.

[59] Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Italy, Japan, Netherlands, Norway, Spain, Sweden, Switzerland, United Kingdom, and United States.

Table 4.2: Unconditional effects in the baseline specification

Prior inclusion probability	0.3	(k = 6)		
Variable	Posterior inclusion prob. (1)	Posterior mean (2)	Posterior standard deviation (3)	Sign certainty prob. (4)
Payroll tax	0.999	0.00267	0.00040	0.999
EPL temporary	0.999	-0.01005	0.00160	0.999
Consumption tax	0.999	-0.00309	0.00058	0.999
Fourth/Fifth year benefits	0.974	-0.00068	0.00145	0.999
Union coverage	0.902	0.00066	0.00029	0.999
Bargaining coordination	0.894	-0.00376	0.00170	0.998
Income tax	0.699	0.00126	0.00096	0.994
EPL regular	0.633	0.00788	0.00686	0.993
First year benefits	0.407	0.00015	0.00145	0.987
Aggregate benefits	0.385	0.00015	0.00433	0.947
Public ownership	0.281	0.00201	0.00368	0.983
Entry barriers	0.252	-0.00112	0.00225	0.980
Second/Third year benefits	0.115	0.00004	0.00144	0.889
Unemployment benefits dur.	0.083	-0.00207	0.00865	0.910
Bargaining centralization	0.080	-0.00021	0.00085	0.920
Aggregate PMR	0.072	0.00009	0.00198	0.509
Unemployment benefits cov.	0.057	-0.00033	0.00169	0.900
Minimum wage	0.026	0.00000	0.00014	0.500
Union density	0.025	0.00000	0.00005	0.573

Notes: The dependent variable is the unemployment rate. Overall, 1048576 (2^{20}) estimations have been performed. The shock variables (labor demand shock, productivity shock, real import price shock and the interest rate) are included in each regression. Fixed country- and time-specific effects are swept by using the *Within transformation* (see Baltagi (2003)).

The prior model size is specified to be equal to six, i.e. the true model is expected to consist of six variables. Thus, the prior inclusion probability is $\frac{6}{20} = 0.3$. If the researcher has some knowledge on the true model size the information can be directly incorporated. For the model estimated here, no prior information on the true model size is available.

Nevertheless, given that 20 indicators are tested for significance the assumption that approximately $\frac{1}{3}$ are significant seems to be a reasonable choice. Additionally, variations of the prior model size are applied in the following section what rules out that a deficient prior assumption drives the findings. The corresponding estimation output can be found in table 4.2, where the variables are sorted in descending order according to their posterior inclusion probabilities in column (1). The weighted coefficients and standard deviations are displayed in columns (2) and (3).[60]

According to table 4.2, three indicators show a posterior inclusion probability close to one. Hence, it is virtually certain that these three indicators are essential determinants of unemployment. More precisely, the payroll tax, employment protection for temporary contracts, and the consumption tax are indubitably linked to the unemployment rate. Five additional variables, fourth and fifth year benefits, union coverage, bargaining coordination, the income tax, and employment protection for regular contracts show lower posterior inclusion probabilities which are nevertheless clearly above the prior. It is likely that these five variables are significant determinants of unemployment. Furthermore, first year benefits as well as aggregate benefits have posteriors marginally above the prior what indicates significance. Public ownership and entry barriers appear to be slightly below the prior and are insignificant given a prior model size of six. However, the sensitivity of the result concerning the four variables which are close to the prior requires further inspection. This is mainly done through variations of the prior model size in the next section. Finally, the nine variables with a posterior inclusion probability clearly below the prior are not related to unemployment.

[60]Note that for the model averaging the dependent variable has been divided by 100 for computational reasons. Hence, coefficients and standard deviations should be multiplied by 100 to be comparable to the results of the following single model estimation in section 4.4.2.

Some of the significant variables show rather high posterior standard deviations in relation to the corresponding posterior means. The fourth and fifth year benefits, for instance, have a posterior mean of 0.00068 and a posterior standard deviation of 0.00145. Denoting a variable significant is questionable if the direction of impact is uncertain. Sala-I-Martin et al. (2004) suggest to calculate sign certainty probabilities for the estimates of each variable in order to shed light on this sign uncertainty. The sign certainty probability measures the probability that a coefficient has the same sign as its mean. The closer the value to one, the higher the probability that the signs of the estimated coefficients do not change across models. The sign certainty probabilities for all variables in table 4.2 are reported in column (4). All significant variables show sign certainty probabilities above the threshold level of 0.975 suggested by Sala-I-Martin et al. (2004). The high standard deviations for a variable occur due to dispersed coefficient estimates which nevertheless lie on the same side of zero.

4.3 Alternative prior model sizes

The prior model size expresses the researcher's belief about the true model size, and varying the prior assumption on this parameter can be used as a sensitivity check. If the significance of a variable holds independent of the prior model size, robustness of the relationship is assumed to be given. In contrast, a drop of the posterior inclusion probability below the prior inclusion probability for one prior model size indicates that a significant relationship is not robust. Models with a size close to the prior model size is given a higher weight. Hence, the higher the prior model size, the more favored the larger models get.

The largest possible model in the setup given here consists of 20 variables.[61] It is assumed that the true model size does not exceed a value of ten, and that at least two institutional indicators are significantly related to unemployment. Prior model size values between 2 to 10 are therefore considered.[62]

In principle, the posterior model probability should increase with the prior model size. This becomes clear when looking at equation (3.21) in chapter 3, where it is shown that the prior model probability, which is positively related to the posterior inclusion probability, is an increasing function of the prior model size $\overline{k}$. If a variable appears to have a posterior inclusion probability above the prior for small models, and below the prior for larger models, this specific variable probably works as a "catch-all" for other effects. With a low prior model size in small models where only few other variables are included, a variable captures the impact of one or more other variables. This effect vanishes when models including these variables are favored more and weighted higher. Furthermore, a posterior below the prior for small models, and above the prior for large models indicates that a variable needs other conditioning variables to be significant. Generally, a variable is only called robust and significant if the posterior is above the prior, independently of the prior model size.

In the baseline specification, a prior model size of six has been assumed. Since the true model size is unknown in advance, the same estimations are performed for prior model sizes of two, four, eight and ten in order to evaluate whether this affects the outcomes. Table 4.3 reveals that the alteration of the prior model size only moderately influences the results of the baseline estimation. Eight variables are identified as significant independent of the prior

[61] Additional to the control factors which are fixed in each model.

[62] Higher prior model size values do not change the findings. The restriction to an upper level of ten is done to keep the tables readable.

Table 4.3: Results for different prior model sizes

Variable					
Model size	k = 2	k = 4	k = 6	k = 8	k = 10
Prior inclusion probability	(0.1)	(0.2)	(0.3)	(0.4)	(0.5)
Payroll tax	0.999	0.999	0.999	0.999	0.999
EPL temporary	0.999	0.999	0.999	0.999	0.999
Consumption tax	0.999	0.999	0.999	0.999	0.999
Fourth/fifth year benefits	0.985	0.979	0.974	0.969	0.961
Union coverage	0.490	0.792	0.902	0.948	0.970
Bargaining coordination	0.517	0.800	0.894	0.930	0.947
Income tax	0.307	0.571	0.699	0.765	0.806
EPL regular	0.238	0.498	0.633	0.705	0.749
First year benefits	*0.199*	*0.346*	*0.407*	*0.440*	0.468
Aggregate benefits	*0.164*	*0.313*	*0.385*	*0.425*	0.453
Public ownership	*0.593*	*0.346*	0.281	0.285	0.320
Entry barriers	*0.319*	*0.246*	0.252	0.304	0.385
Second/third year benefits	0.049	0.089	0.115	0.137	0.162
Unemployment benefits dur.	0.024	0.053	0.083	0.115	0.155
Bargaining centralization	0.091	0.084	0.080	0.087	0.103
Aggregate PMR	0.062	0.058	0.072	0.100	0.142
Unemployment benefits cov.	0.014	0.034	0.057	0.085	0.121
Minimum wage	0.007	0.016	0.026	0.038	0.056
Union density	0.007	0.015	0.025	0.037	0.053

Notes: The dependent variable is the unemployment rate. Overall, 1048576 (2^{20}) estimations have been performed. The shock variables (labor demand shock, productivity shock, real import price shock and the interest rate) are included in each regression. Fixed country- and time-specific effects are swept by using the *Within transformation* (see Baltagi (2003)).

model size. A model consisting of eight institutional factors therefore seems to represent the ideal model size with respect to the panel used here. Although public ownership, aggregate benefits, first year benefits, and entry barriers turn out to be significant for lower prior model sizes (written in *italics*), the effect disappears for larger prior model sizes. The significance of these four variables for smaller prior model sizes raises doubts about the robustness of the signif-

icant relationship. They are not called robust and significant determinants of unemployment.[63]

Remarkably, public ownership and fourth and fifth year benefits show a decreasing posterior inclusion probability when the prior model size is increased. As already explained, one would expect a larger prior model size to be linked to higher posterior inclusion probabilities for all institutional variables. The significance of fourth and fifth year benefits is unaffected with posteriors always clearly above the prior. Consequently, this indicator remains in the group of robust and significant variables. The public ownership indicator has a posterior inclusion probability above the prior for smaller models, and below the prior for larger models. It is a good example for a "catch-all" variable and is not considered as robust and significant.

4.4 Further sensitivity checks

The results in the preceding section have been produced based on a number of assumptions. Besides variations of the prior model size, further sensitivity checks should be carried out. In the following, it is tested whether the findings resist the variation and additional inclusion of control factors.

4.4.1 Additional control variables

One might argue that the shock variables included in all models have an impact lasting more than one year. The set of fixed regressors is therefore extended by taking into account the lagged values of the shock variables. This reduces

[63]Note that further estimations with increasing prior model sizes support the view expressed here. The four indicators slightly below the prior for a prior model size of ten remain there for higher prior model sizes. The eight indicators above the threshold keep on being significant for prior model sizes above ten.

the estimation period to 1983 to 2005. Indeed, some important changes occur which will be explained by means of the following table 4.4.

Table 4.4: Results for different prior model sizes with contemporaneous and lagged shocks

Variable					
Model size	k = 2	k = 4	k = 6	k = 8	k = 10
Prior inclusion probability	(0.1)	(0.2)	(0.3)	(0.4)	(0.5)
EPL temporary	0.999	0.999	0.999	0.999	0.999
Payroll tax	0.999	0.999	0.999	0.999	0.999
Consumption tax	0.999	0.999	0.999	0.999	0.999
Fourth/fifth year benefits	0.997	0.994	0.989	0.983	0.976
Public ownership	0.918	0.787	0.668	0.603	0.590
Bargaining coordination	0.199	0.436	0.633	0.764	0.843
Union coverage	0.142	0.370	0.601	0.773	0.880
Entry barriers	0.605	0.579	0.519	0.505	0.541
EPL regular	0.101	0.291	0.495	0.655	0.763
Income tax	0.097	*0.248*	*0.407*	*0.532*	*0.620*
Aggregate benefits	0.029	0.093	0.175	0.252	0.317
Bargaining centralization	0.094	0.137	0.149	0.150	0.156
First year benefits	0.023	0.070	0.129	0.189	0.245
Second/third year benefits	0.022	0.065	0.118	0.167	0.208
Aggregate PMR	0.091	0.100	0.108	0.129	0.164
Unemployment benefits dur.	0.009	0.023	0.046	0.077	0.115
Unemployment benefits cov.	0.008	0.018	0.031	0.048	0.070
Minimum wage	0.007	0.018	0.030	0.045	0.064
Union density	0.006	0.015	0.025	0.038	0.055

Notes: The dependent variable is the unemployment rate. Overall, 1048576 (2^{20}) estimations have been performed. The contemporaneous and one period lagged shock variables (labor demand shock, productivity shock, real import price shock and the interest rate) are included in each regression. Fixed country- and time-specific effects are swept by using the *Within transformation* (see Baltagi (2003)).

Again, the results are displayed in descending order according to the posterior inclusion probabilities of the model with a prior model size of $\overline{k} = 6$. The table shows qualitative changes for three indicators. Public ownership and entry barriers are now significant for all prior model sizes, while income taxes are insignificant for the model with a prior model size of two. This factor's

posterior inclusion probability of 0.097 is, however, only marginally below the prior of 0.100. For all other prior model sizes the posterior inclusion probability of the income tax indicator is clearly significant. Applying the rule for determining significance strictly means dropping income taxes from the list of robust and significant variables.

Some quantitative changes occur as well, which stem from two sources. First, neglecting observations from 1982 through the inclusion of the lagged shocks lowers the posterior inclusion probabilities for bargaining coordination and union coverage. Second, taking into account additional information through the lagged shocks pushes the posterior inclusion probability of entry barriers and public ownership substantially up. When observations for 1982 are dropped without adding lagged shocks, the posterior inclusion probabilities of entry barriers and public ownership remain unaffected.

Instead of changing the lag structure of the control variables, the set of control factors can also be varied. Baccaro and Rei (2007) suggest a slightly different set of control variables than the one used in Nickell et al. (2005). More specifically, the real interest rate, the change in inflation, a real import price shock and the change in labor productivity is considered by Baccaro and Rei (2007). The real import price shock as well as the real interest rate are already incorporated in the baseline model. Now, the lagged labor productivity growth is used as a substitute to the TFP shock, and the change in the inflation rate is added to the set of macroeconomic control factors.

Taking lagged productivity growth into account has no detectable impact on the findings. More severe is the additional inclusion of the inflation rate change as a further control factor in the baseline equation. The findings for this adjustment are given in the following table 4.5. In comparison to the baseline model of table 4.3, public ownership gains considerable importance.

This factor appears with a high posterior inclusion probability especially in models with low prior model sizes. In contrast, the income tax as well as employment protection for regular employment fall below the prior for a prior model size of two. In summary, the robust and significant impact of payroll

Table 4.5: Results for different prior model sizes and inflation rate change

Variable					
Model size	k = 2	k = 4	k = 6	k = 8	k = 10
Prior inclusion probability	(0.1)	(0.2)	(0.3)	(0.4)	(0.5)
Payroll tax	0.999	0.999	0.999	0.999	0.999
EPL temporary	0.999	0.999	0.999	0.999	0.999
Consumption tax	0.996	0.999	0.999	0.999	0.999
Fourth/fifth year benefits	0.965	0.946	0.935	0.930	0.928
Union coverage	0.175	0.445	0.677	0.824	0.905
Public ownership	0.904	0.736	0.595	0.524	0.508
Bargaining coordination	0.126	0.341	0.534	0.662	0.740
EPL regular	0.088	*0.274*	*0.458*	*0.587*	*0.670*
Income tax	0.076	*0.217*	*0.373*	*0.497*	*0.588*
Entry barriers	*0.340*	*0.374*	*0.354*	0.362	0.408
First year benefits	0.067	0.195	*0.313*	0.394	0.448
Aggregate benefits	0.052	0.159	0.266	0.346	0.402
Bargaining centralization	0.135	0.209	0.233	0.239	0.247
Unemployment benefits dur.	0.041	0.082	0.128	0.174	0.224
Second/third year benefits	0.040	0.084	0.120	0.148	0.175
Aggregate PMR	0.085	0.100	0.104	0.119	0.149
Unemployment benefits cov.	0.011	0.032	0.060	0.094	0.136
Union density	0.007	0.017	0.028	0.041	0.058
Minimum wage	0.007	0.016	0.026	0.040	0.057

Notes: The dependent variable is the unemployment rate. Overall, 1048576 (2^{20}) estimations have been performed. The contemporaneous shock variables (labor demand shock, productivity shock, real import price shock and the interest rate) and the inflation rate change are included in each regression. Fixed country- and time-specific effects are swept by using the *Within transformation* (see Baltagi (2003)).

taxes, employment protection for temporary employment, consumption taxes, fourth and fifth year unemployment benefits, union coverage, and bargaining

coordination on unemployment is not affected by changes in the set or the lag structure of control factors.

Four additional variables are likely important determinants, but some doubts surround this outcome. Income taxes are robust and significant in the baseline model. This factor becomes insignificant for a prior model size of two when lagged shocks or the inflation rate change are included as control factors. The posterior inclusion probability for a prior model size of two is only slightly below the prior and clearly above the prior for higher prior model sizes. Employment protection for regular employment is robust and significant in all specifications except for one. It exhibits a posterior inclusion probability of 0.088 given a prior of 0.100 once the inflation rate change is included. Public ownership is insignificant in the baseline model for prior model sizes above a value of four. It turns out to be significant for all considered prior model sizes when the set or the lag structure of control factors is varied. Entry barriers are not robust and significant in the baseline model, but become significant when lagged shock values are taken into account. The comparably low posterior inclusion probability especially for large models over all specifications raises some doubts about the importance of this variable.

4.4.2 Single model estimation

The model averaging approach serves to ensure that the findings are robust to variations in the set of explanatory institutional factors. It is, however, less appropriate to allow for variations of the econometric specification. While such adjustments are, in principle, feasible within a pure Bayesian setting, it is intended to keep the analysis comprehensible from a classical economics viewpoint.

Table 4.6: Robustness checks in a single model

Dependent variable:	Unemployment rate		
	(1)	(2)	(3)
Payroll tax	0.273**	0.284**	0.284**
	(0.034)	(0.034)	(0.033)
Consumption tax	-0.256**	-0.261**	-0.261**
	(0.043)	(0.043)	(0.043)
EPL temporary	-1.047**	-1.087**	-1.087**
	(0.128)	(0.139)	(0.143)
Bargaining coordination	-0.401**	-0.364**	-0.364**
	(0.111)	(0.109)	(0.108)
Union coverage	0.060**	0.055**	0.055**
	(0.012)	(0.016)	(0.014)
Fourth/fifth year benefits	-0.060**	-0.068**	-0.068**
	(0.012)	(0.012)	(0.011)
Credit constraints	-2.287**	-2.061**	-2.061**
	(0.365)	(0.362)	(0.324)
Income tax		0.156**	0.156**
		(0.057)	(0.057)
EPL regular		1.037*	1.037*
		(0.397)	(0.513)
Entry barriers		-0.343*	-0.343*
		(0.148)	(0.153)
Public ownership		0.602**	0.602**
		(0.201)	(0.197)
Shocks	yes	yes	yes
Heteroskedasticity	no	no	yes
Observations	408	408	408
R-squared	0.481	0.523	0.523

Notes: Institutional variables which have been identified as significant within the model averaging framework are included as explanatory factors. Fixed country- and time-specific effects are swept by using the *Within transformation* (see Baltagi (2003)). The shock variables are labor demand shock, real import price shock, TFP shock and the real interest rate. One asterisk marks significance at the 5% level, two asterisks at the 1% level, standard deviations appear in parenthesis. Specification (1) includes only the robust and significant institutional variables from the model averaging approach. Specification (2) extends (1) by institutional variables which are likely to be significant. Specification (3) additionally controls for heteroskedasticity.

Hence, alterations of the econometric specifications are conducted within a classical estimation framework. A model is set up consisting of the control variables from the baseline model plus the institutional variables which have been identified as significant in the model averaging framework. In this single

model, specification tests like controlling for cross-section heteroskedasticity or endogeneity can be adequately considered. All in all, six distinct specifications are estimated.

In the first specification, the six variables which have been identified as robust and significant in the *BACE* approach are included as explanatory variables for the unemployment rate. Column (1) of table 4.6 gives the result of this estimation. All six institutions turn out to be significant at the 1% level in this estimation. Column (2) extends the basic model by adding the four institutional variables that are likely important determinants of unemployment. The coefficients of all ten institutional variables appear to be significantly different from zero. While the model fit, measured by the adjusted R-squared statistic, increases slightly from 0.481 to 0.523, the effects of the employment protection for regular employment as well as the entry barriers are now only significant at the 5% level. Controlling for possible cross-section heteroskedasticity in column (3) has no qualitatively measurable impact on the outcome.

Table 4.7 takes three additional sensitivity checks into account. In some macroeconomic studies explaining unemployment, the output gap is used as an additional measure to control for cyclical fluctuations of the unemployment rate. Adding this factor as a control variable is expected to shed some light on the quality of the four shock variables. If the output gap appears significant and the findings for the institutional factors change, this indicates an omitted variable bias. As shown in column (4) of table 4.7, adding the output gap as a control variable indeed slightly changes the outcome. The most severe change is the fall of the entry barriers coefficient below the 10% significance level. In contrast to specifications (1) to (3), the public ownership indicator loses significance, and employment protection for regular employment is now significant at the 1% level.

Table 4.7: Further robustness checks in a single model

Dependent variable:	Unemployment rate		
	(4)	(5)	(6)
Payroll tax	0.171**	0.183**	0.277**
	(0.029)	(0.036)	(0.039)
Consumption tax	-0.151**	-0.219**	-0.215**
	(0.038)	(0.043)	(0.050)
EPL temporary	-0.832**	-0.649**	-0.875**
	(0.128)	(0.145)	(0.144)
Bargaining coordination	-0.278**	-0.417**	-0.477*
	(0.086)	(0.087)	(0.207)
Union coverage	0.051**	0.068**	0.071**
	(0.012)	(0.015)	(0.017)
Fourth/fifth year benefits	-0.062**	-0.034**	-0.067**
	(0.010)	(0.008)	(0.013)
Credit constraints	-2.427**	-2.400**	-2.168**
	(0.284)	(0.341)	(0.332)
Income tax	0.201**	0.252**	0.241**
	(0.048)	(0.053)	(0.069)
EPL regular	1.549**	2.182**	2.079**
	(0.428)	(0.668)	(0.406)
Entry barriers	-0.159	-0.152	-0.264
	(0.126)	(0.149)	(0.164)
Public ownership	0.365*	0.623**	0.498*
	(0.185)	(0.212)	(0.248)
Output gap	-0.396**	-0.407**	-0.356**
	(0.036)	(0.037)	(0.037)
Instruments	no	no	yes
Shocks	yes	yes	yes
Heteroskedasticity	yes	yes	no
Observations	408	408	391
R-squared	0.639	0.627	0.638

Notes: Institutional variables which have been identified as significant within the model averaging framework are included as explanatory factors. Fixed country- and time-specific effects are swept by using the *Within transformation* (see Baltagi (2003)). Institutions lagged by one year are used as instruments for the contemporaneous institutions. All institutional variables are assumed to be endogenous in specification (3). The shock variables are labor demand shock, real import price shock, TFP shock and the real interest rate. One asterisk marks significance at the 5% level, two asterisks at the 1% level, standard deviations appear in parenthesis. Specification (4) adds the output gap as a control factor to the model (3). Specification (5) substitutes the harmonized unemployment rate by the country-specific unemployment rates as the dependent variable. Finally, specification (6) applies an instrumental variable approach.

It can be further argued that the harmonized unemployment rate suffers from measurement error. This view finds some support from the very low

unemployment rate of below 1% in Switzerland during the period from 1982 to 1986 (see chapter 3 for details). The influence of the unemployment rate measurement is evaluated by considering the non-harmonized unemployment rates delivered by the ILO as an alternative measure of unemployment.

This non-harmonized measure also uses survey-based data, but sticks more closely to national concepts of unemployment. The risk of negative externalities caused by the harmonization process is reduced at the cost of limited comparability across countries. The results, given in column (5), are fairly similar to the findings given in column (4). Again, entry barriers are not significant while all other institutional variables show significance at least at the 5% level. The harmonization procedure of the unemployment rate therefore does not impact much.

Another problem surrounding the link between institutions and unemployment is endogeneity. Bassanini and Duval (2009) mention that especially the problem of endogenous institutions from reverse causality and from measurement error is a relevant topic in the literature and deserves attention. Reverse causality is plausible since high unemployment forces the government to react through adjustments in the institutional environment. The endogeneity problem is tackled here by applying an instrumental variable estimator where the lagged values of the endogenous variables are used as instruments. Generally, lagged values are appropriate instruments since they are not correlated with current unemployment, but with current institutional levels. Unfortunately, it is hard to claim that institutions are not a function of expected unemployment in the future. Furthermore, the approach might be misleading for hardly time-varying indicators.

It is therefore not assumed that the instrumental variable approach delivers unbiased results, but it is expected that it at least lowers the probability that

the results are interpreted incorrectly due to endogenous institutions.[64] The instrumental variable model is specified according to column (4) of table 4.7. It is assumed that all ten institutional variables are endogenous. The two-stage least squares approach is used for the instrumental variable estimation. Before the estimation can be carried out it first has to be checked whether the lagged values are valid instruments for the endogenous factors. This is done by calculating the F-statistic of the first stage regressions of the endogenous variables on the exogenous ones to test for joint significance of the instruments. The corresponding findings are displayed in table 4.8. A F-statistic value below

Table 4.8: Test statistics for the instrumental variable estimation

Endogenous Variables	F-statistic	R-squared
Payroll tax	116.303	0.812
Income tax	54.001	0.668
Consumption tax	96.072	0.782
Bargaining coordination	17.510	0.395
EPL regular	167.680	0.862
EPL temporary	155.096	0.852
Union coverage	699.861	0.963
Fourth/Fifth year benefits	77.874	0.744
Entry barriers	109.084	0.802
Public ownership	192.543	0.878

Notes Results of the first stage regressions according to the two-stage least squares procedure of table 4.7, columns (3) and (4).

ten is generally accepted as an indication for weak instruments (see Stock and Staiger (1997)). The F-statistics of the first stage regressions here are above a value of ten for all institutions. According to this, the endogeneity problem can be adequately addressed with the lagged institutional factors as

[64]Note that the empirical exercise performed here does not provide causal relationships. All findings have to be interpreted in the context, that given there is a theoretical link, institutions determine unemployment. While the issue of spurious regression cannot be fully dismissed, the empirical analysis performed in this study is based on the assumption that theoretically institutions are important determinants of unemployment.

instruments. Column (6) of table 4.7 presents the results of the instrumental variable estimation. Compared to the reference equation given in column (4), only the significance of the bargaining coordination falls below the 1% level, but is still significant at the 5% level. All other findings remain qualitatively the same as in column (4) of table 4.7.

Up to now, the findings hold for the panel of 17 countries over 24 years. However, it should be clarified whether the results are driven by outliers. In other words, do the findings hold only in the panel comprising of 17 countries from 1982 to 2005? Extending the panel or performing the estimations for a different country sample is impossible since data availability is restricted. A simple test for outliers is thus carried out by estimating data-reduced versions of the preferred specification given in column (4) of table 4.7. Five-year periods are successively excluded from the sample, starting with the period from 1982 to 1986, then 1987 to 1991, and so on. The results of all three tax measures, employment protection for temporary employment, bargaining coordination, fourth and fifth year benefits, and union coverage are not affected by the exclusion of 5-year periods from the sample. In contrast, entry barriers become insignificant in all sub-periods except for the one ranging from 1982 to 2001. Public ownership is insignificant if the period from 1997 to 2001 is neglected. Similarly, employment protection for regular employment loses importance if observations from the last four years ranging from 2002 to 2005 are not considered. Interestingly, all three critical institutions also appear fragile within the model averaging framework.

When each country is left out one at a time, some institutional effects appear to be driven by country outliers. First, entry barriers are insignificant for all country samples except for the case when Spain is excluded. Neglecting Denmark makes fourth and fifth year benefits insignificant. Similarly, when

Spain is left out employment protection for regular employment turns out to be irrelevant for unemployment. Less severe are the changes when Finland and the Netherlands are excluded. For the former, payroll taxes, and for the latter, income taxes are still significant, but only at the 10% level. Again, especially institutional variables which appear to show some fragility in the model averaging approach, that is income taxes, employment protection for regular employment, and entry barriers, are affected by sample size adjustments.

4.5 Discussion and classification

The following section serves to consecutively discuss and classify the findings on the unconditional labor market impact of institutions in the light of the hypotheses derived from the theoretical and empirical literature in chapter 2.

Labor tax system

Consistent with the predictions of chapter 2, the empirical findings provided in this chapter show that both payroll taxes as well as income taxes are positively linked to the unemployment rate. This result indicates that unemployment can be affected via both labor demand through an increase in labor costs for the employer (related to a payroll tax rise), and via labor supply through a diminished gap between the real wage and the outside option of the workers (related to an income tax increase). However, the income tax effect is not robust across specifications, while the link between payroll taxes and unemployment seems to be more stable. This lack of robustness of the income tax effect, which mainly has an impact on labor supply, indicates that labor demand, which is mainly affected by payroll taxes, is a more important channel for labor tax increases to unemployment than labor supply. This argumentation holds if

cost increases are not exposed to substantial redistributions between employers and employees. If an income tax rise is not shifted to the employer by increased wage demands, income taxes do not have a strong impact on labor demand. Based on this assumption, payroll taxes show a higher relevance for unemployment than income taxes.

In contrast to payroll and income taxes, increasing consumption taxes results in lower unemployment rates. The negative consumption tax coefficient is, at least at first glance, difficult to explain since an insignificant or slightly positive effect has been expected according to the hypotheses of chapter 2. It is hard to imagine why higher consumption taxes, which reduce the final consumption expenditure of households, should lead to less unemployment since the gap between the real wage and the outside option is reduced. An insignificant coefficient could have been explained with an argument provided by Blanchard (2006). He claims that taxes which affect employed and unemployed equally do not lead to higher wage claims of workers, and labor demand as well as unemployment remain constant. However, this argument does not provide an explanation why higher consumption taxes should lead to lower unemployment. Possibly, the consumption tax rate has not only a direct effect on the unemployment rate, but also works indirectly on the labor market through a variable which is omitted in the estimation. One candidate for such an omitted variable is active labor market policy for which spending on active labor market policies can be used as an indicator.[65] These policies can be financed by governmental tax revenues, and are intended to support the unemployed in finding a job, be it by training, further education or additional qualification. If mainly consumption taxes are used for financing these

[65]While it would be useful to include this indicator in the estimations to rule out the potential omitted variable bias, it cannot be included since it is not comprehensively available.

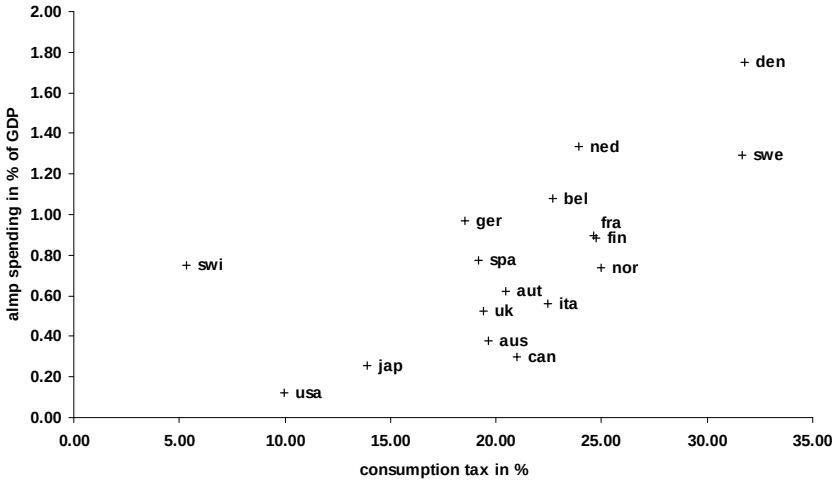


Figure 4.1: Relationship between active labor market policy spending and consumption tax in 2005

policies, and if it is further assumed that active labor market policies in fact help to reduce unemployment, the negative consumption tax coefficient can be explained.

The first part of this argumentation is tested by calculating the correlation between consumption taxes and active labor market policy (*almp*) spending in 2005. Figure 4.1 shows that both factors are highly positively correlated with most countries lying around an upward sloped line.[66] This means that countries with higher consumption taxes have a higher spending for active labor market policies what gives some support for the hypothesis that the consumption tax effect on unemployment is driven by active labor market policies. Interestingly, the *almp* measure is only correlated with consumption taxes; no such correlation is observed with payroll or income taxes. This further emphasizes the potential link between consumption taxes and active labor market policy spending.

[66] Similar findings appear for other periods as well.

The second part of the argumentation, that is active labor market policies help to reduce unemployment, can, in principle, be tested by considering the indicator as explanatory variable in the empirical analysis of this chapter. If consumption taxes turn out to be insignificant when the *almp* measure is also considered in the model explaining unemployment, it could be concluded that the significance of consumption taxes is driven by the omission of the active labor market policy measure. Unfortunately, as already mentioned the *almp* indicator cannot be considered in the estimations since it is not comprehensively available both across countries as well as over time. Alternatively, the overall governmental public spending could be used as an approximation to capture the impact of active labor market policies on unemployment. If such a measure, which is comprehensively available, is included in the model presented in column (4) of table 4.7, the results concerning the consumption tax remain significant. Additionally, the indicator of government spending is positive, indicating that unemployment rises when public spending is increased. The second argument therefore does not find support from this test, and the hypothesis cannot be confirmed. Either public spending does not reliably capture the spending for active labor market policy, or there is no bias of the consumption taxes due to the omission of an active labor market policy measure.[67]

In terms of economic significance, changes in all three tax variables appear to have an economically comprehensible impact on unemployment. It is difficult to say which particular specification delivers the most credible coefficient

[67] Another explanation for the consumption tax effect is that consumption taxes are assumed to be less distortive and, thus, performance-increasing, compared to income or payroll taxes. Tax systems which are mainly based on consumption taxes should therefore be less distortive, with positive effects on the macroeconomic performance. However, the data does not reveal any systematic relationship between the different tax measures and the unemployment rate. Countries with higher consumption taxes do not generally have lower income and payroll taxes.

estimates. Hence, the range over all specifications is given for the discussion of economic significance since the results are generally close together. The payroll tax coefficients lie in between the range from 0.17 and 0.28. This means that an increase in the payroll tax by 1 percentage point increases the unemployment rate by 0.17 to 0.28 percentage points. Similarly, increasing the income tax by 1 percentage point pushes the unemployment rate by 0.13 to 0.25 percentage points up. In contrast, raising consumption taxes by 1 percentage point results in an unemployment rate which is 0.15 to 0.31 percentage points lower. In comparison to the literature, the coefficient estimates are rather close to the findings of Berger and Everaert (2010) and Planas et al. (2007) who estimate elasticities between 0.15 and 0.3 percentage points for slightly different definitions of labor taxes.

Bargaining system

Bargaining coordination and centralization, union coverage and union density, as well as minimum wages have been considered as indicators for the bargaining system in the empirical analysis. It is expected from the hypotheses developed in chapter 2 that coordination, which captures both formal as well as informal aspects of the bargaining process, is beneficial for the labor market through the prevention of excessive wage claims. Bargaining centralization theoretically has a similar effect, but this indicator does only comprise of formal aspects of the bargaining process. A significantly beneficial labor market impact is therefore less likely for bargaining centralization.

Consistent with this, higher bargaining coordination is estimated to exhibit a significantly negative link to unemployment. The higher coordination, the lower is unemployment. Bargaining centralization turns out to be insignificant for unemployment, what highlights the importance of appropriately consider-

ing both formal as well informal aspects of the bargaining process. It can be concluded that the bargaining centralization indicator is an inadequate indicator for the bargaining process. Deriving quantitative effects of this finding is is hampered by the fact that bargaining coordination is measured by a crude index ranging from 1 to 6. The following implications should be interpreted carefully due to this imprecise measurement of coordination. According to the estimations, increasing the index by 1 point would lead to a fall in the unemployment rate by 0.28 to 0.48 percentage points. Given that most observations for bargaining coordination are within the range from 3 to 5, a change in this indicator of more than 2 points is unlikely. Hence, the labor market impact of a change in bargaining coordination given in this chapter is comparably small.

Furthermore, theory predicts that higher union bargaining power leads to a rise in unemployment. Two indicators are included two capture the union bargaining power effect; union coverage and union density. The first factor is estimated to be a robust and significant determinant of unemployment. In line with the hypotheses of chapter 2, its effect on unemployment is positive, and increasing union coverage is estimated to push unemployment up. This finding suggests that union coverage is a good approximation to the union bargaining power. In contrast, union density has no relevance for unemployment in the empirical analysis performed in this chapter. This finding of an insignificant union denisty effect is in line with the majority of the findings of the literature on union density and unemployment.

Summarizing the empirical results of both union coverage and density produced in this chapter, it can be concluded that the additional information which is contained in the union coverage indicator is of particular importance, which makes union density an insufficient indicator for union bargaining power. Future empirical studies should therefore confine attention to union coverage

instead of union density indicators in order to measure bargaining power. In terms of economic significance, increasing the union coverage rate by 10 percentage points raises the unemployment rate by 0.5 to 0.7 percentage points.

Finally, minimum wages do not show a significant impact on unemployment, and two explanations for this finding come into consideration. The first argument is that minimum wages indeed do not have a measurable impact on unemployment since beneficial and detrimental effects compensate each other. This would be in line with the latest microeconomic research on this topic carried out by Dube et al. (2010) and Dolton et al. (2011). The second argument is that the considered indicator is an inappropriate measure for actual minimum wage settings. As the macroeconomic indicator does not comprise of information about the level of the minimum wage, the latter explanation is more likely. However, this can only be clarified with more sophisticated data on minimum wages where the minimum wage level in a country should definitely be included.

Unemployment benefit system

The unemployment benefit indicators show the largest heterogeneity across specifications in terms of significance in the estimations carried out in this chapter. Only the unemployment benefits during the fourth and fifth year of unemployment turn out to be significant across all specifications, but the negative direction of influence is surprising. This indicates that increasing long-term unemployment benefits reduces unemployment. Theory predicts that unemployment benefits can have such a beneficial impact on the labor market through the insurance effect. This effect means that unemployed can keep up their job-specific networks or improve the quality of a job match with help of the transfer payments.

However, it is questionable why this effect arises for long-term but not for short-term unemployed. Thus, an alternative explanation which relates to eligibility criteria for unemployment benefits gains importance. It is possible that long-term unemployed which are not eligible for unemployment benefits have a higher incentive to search a job to become eligible for transfer payments when unemployment benefits are increased. This is less the case for short-term unemployed for whom eligibility is given without adjusting the job search intensity. This argumentation hinges on the country-specific eligibility criteria for short-term and long-term unemployed. In quantitative terms, the findings of this chapter indicate that raising unemployment benefits for the fourth and fifth year of unemployment by 10 percentage points results in a decreased unemployment rate by between 0.3 to 0.7 percentage points. The coefficients are close to the upper bound of 0.7 for nearly all specifications, while the lower bound comes from the model using the alternative definition of unemployment as the dependent variable.

Two additional indicators of the unemployment benefit system are estimated to be important for unemployment depending on the specification. First year unemployment benefits as well as aggregate unemployment benefits are significant in the baseline specification of the *BACE* approach for lower values of the prior model size. The direction of influence for both indicators is such that higher benefits increase unemployment. This gives support for the incentive effect which predicts that lower unemployment benefits entail a higher incentive for the unemployed to intensify job search activities. Nevertheless, this finding is doubtful since taking alternative control variables into account or increasing the prior model size in the *BACE* approach renders both indicators insignificant.

For both unemployment benefit duration as well as unemployment benefit coverage, a positive impact on unemployment has been expected since both factors determine the eligibility for unemployment benefits. Increasing duration or coverage makes unemployment benefits either achievable for a longer period, or for a larger group of people. Longer eligibility periods cause unemployed to prolong their retention time in unemployment to exploit the additional eligibility period. Higher unemployment benefit coverage rates reduce aggregate job search activities and prevent people who become eligible from moving into unemployment. However, these considerations are not supported by the findings of the *BACE* approach and both factors turn out to be insignificant within the model averaging framework. Similarly to this, the indicator for second and third year unemployment benefits neither shows significance for unemployment.

Employment protection

According to the results of this chapter, employment protection for temporary employment is identified as a robust and significant determinant of unemployment. The empirical outcome suggests that increasing employment protection for temporary employment reduces unemployment what is largely consistent with what has been found by the microeconomic literature. Flexible temporary job opportunities, which are expected to increase the transition rates out of and into unemployment, prevent the creation of regular employment, and push unemployment up.

Employment protection for regular employment also shows a significantly positive coefficient in nearly all specifications. The stronger the job protection for regular employment the higher the unemployment rate. However, some doubts exist with regard to the robustness of this finding as the indicator for

employment protection for regular employment becomes insignificant once the sample size is varied. Especially the exclusion of Spain from the sample is of importance. According to the discussion of the literature on employment protection in chapter 2, employment protection for temporary employment is particularly unfavorable for the labor market if job protection for regular employment is high. The creation of regular jobs is prevented and existing regular jobs are transferred into temporary ones. Exactly this situation exists in Spain, where the combination of high protection for permanent employment together with increased flexibility for temporary jobs has led to a segmented labor market as pointed out by Polavieja (2003). Given that the findings for job protection for regular employment are mainly driven by the specific situation in Spain, the positive impact of employment protection for regular employment on unemployment is doubtful.

The derivation of a reliable quantitative assessment of the findings for both job protection indicators is difficult because of the fact that the employment protection variables are discrete indexes which range from 1 to 6. Both are highly aggregated indicators, and thus are rather crude approximations for the true level of employment protection. Concerning the employment protection for regular employment, an increase in the index by 1 point lifts the unemployment rate by 0.79 to 2.18 percentage points. Raising the employment protection for temporary employment leads to a fall in the unemployment rate by 0.65 to 1.09 percentage points. It is obvious that the differences are substantial across specifications. Hence, these findings should be interpreted carefully and the room for policy recommendations with quantitative results is limited.

Product market regulation

The results for both entry barriers and public ownership are fairly sensitive to the selected specification, while the aggregate measure of product market regulation is insignificant in the *BACE* framework. In the baseline specification of the *BACE* approach both public ownership as well as entry barriers are not robust and significant. In contrast, the indicators turn out to be significant when lagged shocks variables are included as control factors in the model averaging approach. Additionally, public ownership is significant when controlling for the inflation rate change. In the single model estimation, public ownership appears significant in all specifications except for the one neglecting the period from 1997 to 2001. In contrast, the entry barriers have been insignificant in most of the classical models considered.

The hypotheses concerning the labor market impact of product market regulation predict that increasing both entry barriers as well as public ownership raise unemployment. Consistent with this prediction, public ownership shows a positive coefficient and it can be assumed that more state control in the private sector increases unemployment. This fits well with the argument of the theoretical literature that efficiency due to more competition, for instance, is easier to achieve in privately owned firms than in public companies. In contrast, the entry barriers appear with a negative coefficient in the estimations performed in this chapter. This finding is conflicting to both theoretical predictions and existing empirical evidence. Most empirical studies have found support for the hypothesis that lower entry barriers are linked to a drop in unemployment since competition is increased via the entry of new competitors. While the negative entry barriers effect estimated here is often not different from zero, the sometimes negative coefficient is difficult to interpret. One pos-

sible technical explanation is that the indicator for entry barriers covers only seven non-manufacturing sectors of an economy. Differences in the relative importance of these sectors or large differences in product market regulation between non-manufacturing and manufacturing sectors could thus be a source of bias and lead to the negative entry barriers coefficient.

According to the findings provided in this chapter, raising entry barriers by 1 point leads to a fall in the unemployment rate by 0.16 to 0.34 percentage points, while an increase in the public ownership indicator by 1 point is linked to a unemployment rate which is 0.20 to 0.62 higher. Similar to job protection or bargaining coordination, the findings should be transformed carefully in concrete policy recommendations due to the measurement of product market regulation as a crude index. While the outcome of both product market indicators seem to be economically reasonable, an assessment of the results requires that attention is paid to the imprecise measurement of both indicators and the possible shortcomings arising from limited sectoral coverage.

Chapter 5

Conditional institutional effects

The objective of chapter 5 is to analyze the impact of institutions on unemployment conditional on other institutional factors. The first part of this chapter deals with the identification of robust and significant interactions with help of the *BACE* approach. Based on these results, marginal effects are calculated for selected countries in order to discuss the relevance of interactions for the determination of reform effects on unemployment. A discussion and classification of the results concludes this chapter.

5.1 Robust and significant interactions

Model averaging enables the researcher to avoid a subjective decision on which variables to include in a model. Nevertheless, the number of explanatory variables is limited by data availability, and only such interactions can be considered for which data is comprehensively available. In the preceding chapter 4, 19 institutional variables have been taken into account. In principle, theoretical arguments generally serve to predict interdependencies between all 19 institutions. However, the analysis of unconditional effects has shown that

some indicators are highly correlated with others factors. No additional information is contained in these factors and they need not be considered in the analysis of interdependencies. In the following, it is discussed which indicators are redundant.

First, chapter 4 has shown that union coverage and bargaining coordination are superior indicators over union density and bargaining centralization, respectively. The content of the former factors goes beyond the information delivered by the latter factors which do not contain additional relevant information. Furthermore, aggregate unemployment benefits and aggregate product market regulation are likely to have only limited explanatory power. Their effect is assumed to be captured by the other more disaggregated indicators of the particular categories since both factors are almost linear combinations of these disaggregated indicators. Finally, the unemployment benefit duration indicator is highly correlated with fourth and fifth year unemployment benefits. Hence, union density, bargaining centralization, aggregate unemployment benefits, aggregate product market regulation, and unemployment benefit duration are not considered as interaction partners. The remaining 14 institutional variables result in up to 91 bivariate interaction terms that are tested for significance within the *BACE* approach.

5.1.1 Specifying interaction models

Estimating a conditional model is not as straightforward as for a linear unconditional model. An interdependency between two variables is usually taken into account via a multiplicative interaction term. According to Brambor et al. (2006), for any conditional model, it is required to include the constitutive terms of an interaction. In other words, if the interaction between the vari-

ables X_1 and X_2 is specified, X_1 and X_2 have to be included individually, as well. Excluding one or both of the constitutive terms of an interaction results in an omitted variable bias if X_1 and X_1X_2, but not X_2 are considered as explanatory factors. The same holds true if the analysis is limited to X_2 and X_1X_2. Excluding X_2 without justification thus results in biased coefficient estimates. Only if X_2 which measures the impact of X_2 on the dependent variable for $X_1 = 0$ is insignificant and not correlated with X_1 or X_1X_2, it could be excluded. Brambor et al. (2006) state that there are possible cases where a constitutive term could be neglected, but these cases are rare, and the according requirements are not fulfilled here.

The situation is even more problematic for conditional models with more than one interaction term. Braumoeller (2004) states that once a model with more than one interaction is specified, attention has to be paid to implicit interactions. This can be best explained with an example. Assume that a model with the interactions X_1X_3 and X_1X_2 and the corresponding constitutive terms X_1 and X_2 is set up. Then, neglecting the interactions X_2X_3 and $X_1X_2X_3$ is equivalent to forcing the coefficients of both excluded interactions to be equal to zero. If this is not the case, the remaining results are likely biased. More concretely, for the interaction models considered in this chapter it would be necessary to include a full set of $\sum_{k=1}^{n} \binom{n}{k}$ interactions where n is the number of institutional indicators. Given that interactions are constructed between 14 institutions, this would require the inclusion and interpretation of $16,383$ interactions. Obviously, this is infeasible, and the problem of implicit interactions cannot be adequately taken into account. The opportunity to gain information on bivariate interactions comes at the price of a possible bias due to the negligence of implicit interactions.

5.1.2 Model averaging results

In comparison to the application of the *BACE* approach in the preceding chapter, some changes occur. The 91 bivariate interaction terms build the sets of variables which vary across models. The five control variables, that is the real interest rate, the labor demand shock, the TFP-shock, the real import price shock and the credit constraints, as well as the fourteen individual institutional variables are now included in all models. Each model consists of 19 control variables and a varying set of interaction terms. Obviously, the models are much larger than in chapter 4. Consequently, not all models can be estimated for computational reasons. 91 varying factors sum up to $2^{91} = 2.48{*}10^{27}$ different models. Instead of estimating all models, the *stratified sampler* together with the convergence criteria explained in section 3.2.3 are applied to receive reliable results. Due to the large set of models and the ignorance regarding the true model size, rather dispersed prior model sizes are assumed. More concretely, the prior model size is set to values of 10, 20, 30, 40, and 50. This leads to prior inclusion probabilities of 0.11, 0.22, 0.33, 0.44, and 0.55.

Generally, an interaction term with a posterior inclusion probability above the corresponding prior inclusion probability is considered as significant dependent on the prior model size. An interaction term is only called robust and significant if its posterior inclusion probability is above the corresponding prior inclusion probability for all considered respective prior model sizes. The results are displayed in table 5.1 where the variables are sorted according to their posterior inclusion probability for a prior model size of 30 in a descending order.

Table 5.1: Conditional effects for different prior model sizes

Variable						
Model size	k = 10	k = 15	k = 20	k = 25	k = 30	k = 40
PIB	(0.11)	(0.16)	(0.22)	(0.27)	(0.33)	(0.44)
EPLt*UB23	0.999	0.999	0.999	0.999	0.999	0.999
PTX*MW	0.995	0.999	0.999	0.999	0.999	0.999
EB*POS	0.999	0.999	0.999	0.999	0.999	0.999
EPLt*UB45	0.999	0.999	0.999	0.999	0.999	0.999
PTX*ITX	0.992	0.997	0.998	0.999	0.999	0.999
UC*EB	0.884	0.983	0.996	0.999	0.999	0.999
ITX*MW	0.994	0.998	0.999	0.999	0.990	0.998
PTX*UB45	0.998	0.982	0.998	0.998	0.997	0.997
EPLr*UC	0.999	0.996	0.998	0.998	0.996	0.999
EPLr*UB1	0.887	0.918	0.969	0.982	0.985	0.997
BCO*UC	0.986	0.984	0.976	0.982	0.974	0.989
EPLt*MW	0.961	0.975	0.987	0.977	0.974	0.981
CTX*MW	0.608	0.882	0.928	0.970	0.955	0.987
PTX*UB23	0.829	0.818	0.926	0.963	0.965	0.970
MW*EB	0.696	0.900	0.892	0.904	0.945	0.950
CTX*EB	0.594	0.676	0.745	0.868	0.832	0.866
CTX*UB23	0.750	0.884	0.883	0.727	0.943	0.764
PTX*POS	0.460	0.595	0.646	0.576	0.838	0.822
POS*UB45	0.402	0.370	0.437	0.571	0.393	0.491
UC*POS	0.116	0.342	0.456	0.454	0.696	0.640
EPLr*EPLt	0.450	0.540	0.517	0.417	0.524	0.479
EPLr*EB	0.194	0.337	0.338	0.314	0.413	0.443
UC*UB1	0.109	0.107	*0.228*	*0.392*	0.301	*0.634*
CTX*UC	*0.217*	*0.455*	*0.467*	*0.379*	*0.541*	0.408
BCO*UB45	*0.186*	*0.272*	*0.334*	*0.363*	*0.379*	0.388
EPLr*UBC	0.105	*0.358*	*0.336*	*0.355*	*0.471*	0.400
EPLr*MW	*0.202*	*0.339*	*0.344*	*0.333*	*0.425*	0.404
UC*UB23	*0.283*	0.138	0.154	*0.306*	0.113	0.292
UB23*UB45	0.034	0.038	0.077	*0.297*	0.122	0.352
EPLr*UB45	*0.142*	0.135	0.172	0.183	0.264	0.356
BCO*UBC	*0.226*	*0.298*	*0.227*	0.164	0.181	0.212
EPLt*BCO	*0.189*	*0.166*	0.186	0.158	0.271	0.188
CTX*POS	*0.172*	*0.172*	0.185	0.124	0.180	0.175
EB*UB45	*0.145*	0.142	0.118	0.114	0.189	0.321
EPLt*EB	*0.124*	0.088	0.095	0.071	0.085	0.093

The dependent variable is the unemployment rate. The five control variables as well as the 14 institutional indicators have been included in all regressions. Fixed country- and time-specific effects are swept by using the Within transformation (see Baltagi (2003)). Only variables with a posterior inclusion probability (PIB) above the prior inclusion probability for at least one prior model size specification are displayed in the table. PTX = Payroll tax, ITX = Income tax, CTX = Consumption tax, EPLr = Employment protection reg., EPLt = Employment protection temp., BCO = Bargaining coordination, MW = Minimum wage, UC = Union coverage, EB = Entry barriers, POS = Public ownership, UB1 = First year benefits, UB23 = Second/third year benefits, UB45 = Fourth/fifth year benefits, UBC = Unemployment benefit coverage.

Only interaction terms which are significant for at least one prior model size specification are reported in order to avoid illegible tables. The findings attach a significant effect on the unemployment rate to 22 interaction terms independent of the prior model size.

Further, 13 interaction terms have posterior inclusion probabilities above the prior inclusion probabilities for some prior model sizes, but not for all. Hence, these 13 interactions are not called robust and significant. Similar to chapter 4, some variables have posterior inclusion probabilities close to one and have a very high probability of being important determinants for unemployment. Some have at least for some prior model sizes posterior inclusion probabilities which are only marginally above the corresponding prior inclusion probabilities. It is possible that performing the model averaging with further prior model sizes could result in slight changes of the results. However, since the prior model size principally can range from 1 to 91, running the approach with all prior model sizes does not seem very reasonable. The five selected prior model sizes present a sensible but still selective choice.

The technical procedure of estimating posterior inclusion probabilities with a large number of explanatory factors can lead to slightly changing results for two runs with the same specification.[68] While this is not relevant for most of the interactions, it can be of importance for variables with a posterior inclusion probability close to the prior inclusion probability. Hence, the results of table 5.1 have been generated twice for each prior model size to secure that the decision of significance is not driven by a slightly imprecise convergence process. While for few variables the estimated posterior inclusion probabilities

[68] As explained in section 3.2.3, the convergence criteria could also be further tightened. However, this unacceptably increases the computation time and is therefore no alternative.

indeed exhibit changes in the second decimal place for some prior model sizes, the overall findings on significance and robustness remain unchanged.

5.1.3 Alternative interaction selection strategies

While most of the interaction terms show posterior inclusion probabilities considerably above the prior, the results of 5 interactions ($PTX * POS$, $POS * UB45$, $UC * POS$, $EPLr * EPLt$, $EPLr * EB$) might bring about some doubts. Their posterior inclusion probabilities are only slightly above the corresponding prior inclusion probabilities for some prior model sizes. Furthermore, according to the comment of David Draper in Hoeting et al. (1999), a rather large number of explanatory factors together with a rather low number of observations can be a source of bias within the model averaging framework if some of the explanatory variables are highly correlated.[69] It is unknown when there are too many explanatory variables compared to the number of observations. In order to shed some light on this issue, the identification of robust and significant variables is repeated with a reduced set of explanatory factors.[70]

The reduction of the set of interactions is performed in the following way. For a pair of highly correlated interaction terms, one of the two is excluded. The calculation of the correlation coefficients for all pairs of interactions reveals a considerable degree of correlation between some interactions. There is no rule of thumb for choosing the threshold level for "high" correlation. In a first step,

[69]These correlations cannot be displayed in a table since this would require 91 rows and columns, respectively.

[70]Actually, 408 observations and 91 interactions are not expected to represent a problematic constellation. For instance, Sala-I-Martin et al. (2004) had 67 indicators and 88 observations and it is likely that some of these indicators have been considerably correlated. Nevertheless, dramatically different results of the model with a reduced number of interactions compared to the full-set model would underline the issue of highly correlated interactions. It would then be necessary to control for this in some other way.

the threshold level is set to be equal to ± 0.9, i.e. if $|\rho(i_1, i_2)| > 0.9$ then the two interaction terms i_1 and i_2 are highly correlated. Following this reasoning, 34 pairs of interactions are highly correlated.

Note that it is still unclear which of one of two highly correlated interactions should be excluded. For the exclusion decision, the results of the selection based on availability are used. The less significant interaction in the fully specified model of a pair of highly correlated interactions is excluded. 67 interactions remain since some interactions are highly correlated with more than one other interaction. Furthermore, a threshold level of 0.8 is also applied, resulting in the exclusion of 52 interactions. For this threshold level, 39 interactions remain to be estimated.

The problem with this approach is the purely technical exclusion decision. Even if two interactions are highly correlated, both could contribute to explain unemployment. Relevant interactions could be neglected when the selection based on correlations is applied. Additionally, the threshold level that determines exclusion is fully subjective and has no theoretical justification. Nevertheless, the findings of this strategy can help to evaluate the robustness of the approach including all interactions. If the majority of interactions exhibit a substantial loss in significance due to the exclusion of some interactions, it is questionable whether the *BACE* approach is appropriate to correctly assign importance to all included interactions.

The results for both threshold levels (the corresponding result tables are given in Appendix B) reveal that the identification of robust and significant variables based on all 91 interactions performs quite well. Nevertheless, some variables are no longer significant once highly correlated variables are excluded. Overall, 15 institutional interactions have a posterior inclusion probability above the prior inclusion probability independent of the prior model size. The

interaction between the payroll tax and the public ownership ($PTX*POS$) is excluded if the threshold for highly correlated interactions is set to 0.8, while the interaction between the union coverage and the entry barriers ($UC*EB$) is excluded for both threshold levels, i.e. 0.9 and 0.8. One interaction ($MW*EB$) is robust and significant for the 0.8 level, but not for the 0.9 level. Further 4 interactions which are robust and significant if all interactions are included ($CTX*EB$, $POS*UB45$, $UC*POS$, and $EPLr*EB$) turn out to be insignificant if a threshold level for highly correlated variables is introduced. These 4 variables all appear with posterior inclusion probabilities close to the prior as displayed in table 5.1 and the robustness of these variables is somewhat dubious.

Do the results imply that robust and significant interactions have been wrongly identified in the full model due to the joint inclusion of highly correlated variables? Given that the findings do not change much through the exclusion of several interaction terms and since the number of observations in the sample is considerably larger than the number of interactions, it is assumed that the fully specified model delivers a more reasonable approach. Hence, the risk of neglecting important determinants of unemployment is given a higher weight than the danger of wrongly estimating the significance of interactions due to high interaction term correlations. The following analysis therefore is based on the 22 robust and significant interactions derived from the fully specified model averaging approach including all 91 interactions.

5.2 Explanatory power of interactions

The model averaging approach applied in this chapter helps to evaluate whether bivariate interactions of institutional indicators are robustly and sig-

nificantly related to the unemployment rate. But does the inclusion of interacting variables really boost the explanatory power of macroeconomic unemployment models? In other words, does the model including interactions perform substantially better in explaining unemployment than the benchmark model without interactions? A comparison between both models gives an indication of the importance of interactions between individual institutional indicators. Table 5.2 displays the outcomes of classical fixed effects regressions with the unemployment rate as the left-hand variable.

The first model includes the 14 individual institutional indicators and the 5 control variables. No interactions are considered in this model. The second model adds the 22 institutional interactions identified as robust and significant within the model averaging framework. Due to space constraints, the 5 control variables as well as the unemployment benefit coverage are not displayed. Two results stand out. First, the model fit of the interaction model (model 2), measured by the adjusted R-squared, is substantially better than the model fit of model 1.[71] The goodness-of-fit increases from 0.51 to 0.79. Second, all 22 interaction terms show significance at least at the 5%-level, except for the interaction between the employment protection for regular and for temporary employment ($EPLr * EPLt$) which is only significant at the 10%-level.

[71]Note that the adjusted R-squared controls for the inclusion of additional explanatory variables. Of course, the explanatory power measured by the R-squared is usually larger for models considering more explanatory variables. The adjusted R-squared takes a penalty term for the number of explanatory factors into account and facilitates the goodness-of-fit comparison of models of different size. More specifically, the R-squared is defined as $1 - \frac{SSE}{TSS}$ with SSE being the sum of squared errors and TSS being the total sum of squares. The adjusted R-squared is calculated as $1 - \frac{SSE/(n-k)}{TSS/(1-n)}$ where n and k are the number of observations and the number of explanatory factors, respectively. Note that alternative model selection criteria like the Akaike information criterion or the Schwarz criterion lead to the same conclusion.

Table 5.2: Comparing the unconditional with the conditional model

Explanatory variables	Coefficient	Standard error	Coefficient	Standard error
	Model 1		Model 2	
PTX	0.251***	(0.036)	-0.225**	(0.1098)
ITX	0.145**	(0.059)	-0.123	(0.0801)
CTX	-0.297***	(0.047)	-0.039	(0.0628)
EPLr	0.981**	(0.425)	11.629***	(1.7362)
EPLt	-0.964***	(0.148)	-2.025***	(0.5899)
BCO	-0.436***	(0.113)	-1.773***	(0.3164)
MW	0.060	(0.072)	-0.658	(0.3455)
UC	0.063***	(0.016)	0.086	(0.0423)
EB	-0.438***	(0.157)	0.502	(0.4400)
POS	0.525**	(0.224)	4.093***	(0.5646)
UB1	0.035	(0.014)	0.087***	(0.0247)
UB23	0.002	(0.014)	-0.0441**	(0.0209)
UB45	-0.075***	(0.013)	0.208***	(0.0405)
PTX*ITX	-	-	0.037***	(0.0042)
PTX*MW	-	-	-0.089***	(0.0114)
PTX*POS	-	-	0.054***	(0.0171)
PTX*UB23	-	-	-0.015***	(0.0013)
PTX*UB45	-	-	0.019***	(0.0013)
ITX*MW	-	-	-0.052***	(0.0118)
CTX*MW	-	-	0.070***	(0.0144)
CTX*EB	-	-	-0.056***	(0.0097)
CTX*UB23	-	-	-0.004***	(0.0009)
EPLr*EPLt	-	-	-0.266*	(0.0205)
EPLr*UC	-	-	-0.118***	(0.0178)
EPLr*EB	-	-	-0.226**	(0.1074)
EPLr*UB1	-	-	-0.047***	(0.0113)
EPLt*MW	-	-	0.327***	(0.0495)
EPLt*UB23	-	-	0.087***	(0.0075)
EPLt*UB45	-	-	-0.079***	(0.0104)
BCO*UC	-	-	0.020***	(0.0042)
MW*EB	-	-	0.090***	(0.0212)
UC*EB	-	-	0.033***	(0.0050)
UC*POS	-	-	-0.028**	(0.0088)
EB*POS	-	-	-0.545***	(0.0620)
POS*UB45	-	-	-0.030***	(0.0093)
Adjusted R-squared	0.511499		0.792475	

*The dependent variable is the unemployment rate. The 5 control variables as well as the unemployment benefit coverage are included in both models, but not displayed. Fixed country- and time-specific effects are swept by using the Within transformation (see Baltagi (2003)). *** means significance at the 1%, ** at the 5%, and * at the 10%-level. The variable names follow the definition in the footnote to table 5.1.*

Note that the unconditional coefficients of model 1 and 2 are not directly comparable since the values in model 2 only tell the effect of a variable for the conditioning variable being zero.[72] For instance, the first year unemployment benefits ($UB1$) value of 0.087 is the impact of an unemployment benefit increase by 1 percentage point on unemployment if the conditioning variable is zero. First year unemployment benefits interact with employment protection for regular employment ($EPLr$). Since the employment protection for regular employment is an indictor ranging from 1 to 6, it can never be zero, and the $UB1$ coefficient has no actual meaning. For variables which appear in more than just one interaction term the unconditional coefficient gives the impact of a variable given that all conditioning variables are zero. In section 5.3, it will become clear how economically interpretable marginal effects of institutions on unemployment can be calculated.

5.3 Marginal institutional effects

The results of the previous estimations show that each institutional indicator is important as an interaction partner for at least one institution, except for the unemployment benefit coverage (UBC). The indicators which appear most frequently as interaction partners are the payroll tax (PTX), the minimum wage (MW), and the entry barriers (EB) with five appearances in interaction terms. Furthermore, the union coverage (UC) and the public ownership (POS) are included in four conditional relationships. The least important interaction partners, given the appearances in interaction terms, are the bar-

[72] Braumoeller (2004) shows that the coefficients and even the significance of the unconditional variables can be manipulated by simply adding a constant value to all observations of one variable as long as the interaction term including this particular variable is different from zero.

gaining coordination (BCO) and the first year unemployment benefits (UB1) with one interaction in each case.

From the results presented in table 5.2, it is generally possible to deduce the impact of an institutional change on the unemployment rate conditional on other institutional factors. The calculation of marginal effects of institutional changes needs to consider both the unconditional as well as the conditional effect. Hence, both coefficients of the individual indicators of model 2 in the upper part of table 5.2 as well as the interaction terms coefficients in the lower part of model 2 of table 5.2 are required.[73]

5.3.1 Calculating marginal effects

The coefficients of the individual effects show the impact of the particular variable given that the conditioning variable(s) are zero.

$$Y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_3 X_1 X_2 + \epsilon \tag{5.1}$$

gives an estimation equation with two institutional factors X_1 and X_2 as well as an interaction between both factors $X_1 X_2$. The marginal effect of X_1 on Y is described by

$$\frac{\partial Y}{\partial X_1} = \beta_1 + \beta_3 X_2. \tag{5.2}$$

β_1 gives the effect of X_1 for $X_2 = 0$, while β_3 shows the effect of a change in X_1 conditional on X_2. Given the equations (5.1) and (5.2), the results from table 5.2 provide enough information to calculate the marginal effects for

[73]Note that the values for the interaction coefficients could basically also be calculated within the model averaging framework. A comparison between the both methods shows minor differences. The results presented in the following would be only marginally affected.

all institutional indicators. The marginal effect of an indicator is calculated from the individual indicator coefficient plus the coefficients estimated for the interactions in which the particular indicator takes place. For example, the marginal effect of the bargaining coordination (BCO) is

$$\frac{\partial UE}{\partial BCO} = -1.773 + 0.020\ UC. \tag{5.3}$$

Equation (5.3) implies that the bargaining coordination has a negative impact on unemployment if union coverage (UC) is zero. All countries exhibit union coverage values larger than zero for all years. This is therefore an irrelevant case. For union coverage values larger than zero the effect becomes less negative or even positive. From equation (5.3) the union coverage level at which the marginal effect of bargaining coordination on unemployment becomes positive can be calculated. Some descriptive statistics are required. Table 5.3 delivers statistics for the individual indicators averaged across all 17 countries and over 24 years. According to equation (3.4), the threshold value for union coverage is $\frac{1.773}{0.020} = 88.7$. In countries with union coverage values larger than 88.7, the marginal effect of bargaining coordination is positive. For values below 88.7 it is negative. In other words, increasing bargaining coordination is beneficial in countries where union coverage is below 88.7.

A closer look at the data reveals that there are 88 observations above 88.7 for union coverage, and 320 below. Hence, whether an increase in the bargaining coordination has a positive or a negative impact on unemployment is clearly a country- and time-specific issue depending on the specific union coverage level. Similar to this, marginal effects can be calculated for all institutional indicators.

Table 5.3: Descriptive Statistics for selected institutions

Variable	Mean	Median	S.D.	Min	Max
Payroll tax	12.95	13.73	9.36	0.00	32.36
Income tax	15.74	15.91	6.64	0.01	33.80
Consumption tax	19.34	20.44	6.54	1.61	33.53
EPL regular	1.98	1.87	0.85	0.17	3.88
EPL temporary	2.15	1.88	1.44	0.25	5.38
Bargaining coordination	3.19	4.00	1.28	1.00	5.00
Minimum wage	3.50	2.00	2.86	0.00	8.00
Union coverage	67.90	75.00	24.84	13.70	99.00
Entry barriers	3.57	3.81	1.63	0.35	6.00
Public ownership	3.83	4.08	1.31	0.83	6.00
First year benefits	49.08	52.65	20.33	1.00	88.80
Second/third year benefits	25.43	25.90	20.19	0.00	68.50
Fourth/fifth year benefits	14.10	15.45	13.94	0.00	68.50

The underlying time frame ranges from 1982 to 2005. For the calculation of the mean, averages over all 24 years and all 17 countries have been constructed. S.D. = Standard Deviation, Min = Minimum value observation, Max = Maximum value observation.

From the example above it becomes clear that the calculation of threshold levels is helpful to evaluate the economic relevance of an interaction. If the threshold level of a conditioning variable is such that it lies outside the range of actual values, the economic relevance of this particular interaction is limited. For instance, a negative threshold level for a conditioning variable which can only take positive values is irrelevant in qualitative terms since no sign change for the marginal effect occurs. Marginal effects for a variable then only differ in quantitative terms depending on the conditioning factor. In order to shed some light on the issue of economic relevance, threshold levels need to be calculated for all conditioning factors. While calculating conditional marginal effects is easy for indicators included in only one interaction, it is more difficult for indicators that interact with more than one factor. This is shown in the following by calculating conditional marginal effects of income taxes (ITX) on

unemployment. The direct effect and the effects conditional on the minimum wage (MW) and the payroll tax (PTX) are required. The marginal effect of income taxes is

$$\frac{\partial UE}{\partial ITX} = -0.123 + 0.027\ PTX - 0.052\ MW. \tag{5.4}$$

If the minimum wage value was known, it would be feasible to calculate the threshold level for the payroll tax. Similarly, for a known value of the payroll tax, the threshold level for the minimum wage could be calculated. Hence, it is necessary to define values for the conditioning factors, except for the conditioning factor of interest in order to be able to determine threshold levels of this conditioning factors of interest. This could be done using averaged indicator values from table 5.3. If one is interested in the threshold level of minimum wages (MW) for the marginal effect of income taxes (ITX), the mean value for the payroll tax (PTX) over all countries and years can be inserted in equation (5.4). According to table 5.3, the mean value of the payroll tax is 12.95. Hence, the marginal effect of income taxes is

$$\frac{\partial UE}{\partial ITX} = -0.123 + 0.027 * 12.95 - 0.052\ MW = 0.226 - 0.052\ MW \tag{5.5}$$

and the threshold level for the minimum wage is $\frac{0.226}{0.052} = 4.3$. The minimum wage index can only be a natural number from the set $MW = \{1, 2, ..., 8\}$. Values of 1,2,3 and 4 assign a positive impact on unemployment to an income tax increase, while values of 5,6,7, and 8 mean a negative marginal effect of income taxes on unemployment, given that the payroll tax is at its mean across all countries over all years.

Threshold levels for all conditioning factors are calculated and presented in table 5.4. If there is more than just one interaction partner, the mean values from table 5.3 are taken to calculate the threshold level for the conditioning factor of interest. The table has to be interpreted as follows. The first and second column show the variables shaping the interaction term. The first column contains the variable for which the marginal effect is calculated. The second column (a) shows the conditioning variable of interest. Threshold levels are calculated for these factors. Column (b) displays the direct or individual effect for the variable of interest, and column (c) gives the corresponding conditional effect. Column (d) contains the threshold levels.

As an example, the threshold level for the payroll tax (PTX) in the interaction with fourth and fifth year unemployment benefits ($UB45$) is 4.04. This threshold value is calculated for the mean values for employment protection for temporary employment ($EPLt$) and public ownership (POS) which both also exhibit an interaction with fourth and fifth year benefits. This exercise shows at what payroll tax level a sign change for a reform of the fourth and fifth year benefits occurs for a hypothetic country with an average institutional level. If such a hypothetic country had a payroll tax above the threshold of 4.04, increasing the fourth and fifth year benefits would have a detrimental impact on the labor market. A payroll tax below the threshold means that increasing the benefits lowers unemployment. For most of the conditioning variables the threshold level is within the frontiers of actual possible values. Only for eleven values, which are written in *italics* in table 5.4, the threshold levels are not in the range of possible values when the other conditioning values are set to their mean.

Table 5.4: Calculation of threshold levels in the interaction model

Variable	(a)	(b)	(c)	(d)
Payroll tax (PTX)	ITX	-0.225	0.037	11.98
	MW		-0.089	5.06
	POS		0.054	1.25
	UB23		-0.015	34.71
	UB45		0.019	6.78
Income tax (ITX)	PTX	-0.123	0.037	8.24
	MW		-0.052	6.85
Consumption tax (CTX)	MW	-0.039	0.070	4.87
	EB		-0.056	1.86
	UB23		-0.004	1.52
EPL regular (EPLr)	UC	11.612	-0.118	67.17
	EB		-0.226	3.19
	UB1		-0.047	47.26
	EPLt		-0.266	1.83
EPL temporary (EPLt)	MW	-2.025	0.327	4.44
	UB23		0.087	28.98
	UB45		-0.079	10.19
	EPLr		-0.266	0.82
Bargaining coordination (BCO)	UC	-1.773	0.020	88.65
Minimum wage (MW)	EB	-0.658	0.090	*6.36*
	PTX		-0.089	10.13
	ITX		-0.052	10.92
	CTX		0.070	22.92
	EPLt		0.327	2.92
Union coverage (UC)	EB	0.086	0.033	5.79
	POS		-0.028	1.21
	BCO		0.020	*6.85*
	EPLr		-0.118	1.36
Entry barriers (EB)	CTX	0.502	-0.056	*-30.68*
	EPLr		-0.226	*-10.41*
	MW		0.090	*34.62*
	POS		-0.545	*-1.31*
Public ownership (POS)	PTX	4.093	0.054	*-5.85*
	UC		-0.028	*104.15*
	EB		-0.407	*6.06*
	UB45		-0.030	47.94
First year benefits (UB1)	EPLr	0.087	-0.047	1.85
Second/third year benefits (UB23)	PTX	-0.044	-0.015	4.38
	CTX		-0.004	*-12.80*
	EPLt		0.087	0.33
Fourth/fifth year benefits (UB45)	PTX	0.208	0.019	4.04
	EPLt		-0.079	4.29
	POS		-0.030	*9.47*

The table contains the estimated coefficients of model 2, table 5.2, with threshold levels calculated with the mean values of table 5.3.

This emphasizes the qualitative relevance of interaction terms for shaping institutional characteristics and conducting institutional reforms. Whether an institutional reform increases or decreases unemployment depends on the level of the conditioning institution(s).

5.3.2 Marginal effects for selected countries

Threshold levels are helpful to assess the economic relevance of interactions in general, but one of the major advantages of interactions is neglected through the incorporation of mean values across countries and over time. In principle, it is possible to calculate country-specific marginal effects for all institutional indicators. Instead of using averaged data, calculations can be based on the indicator values for a specific country, as well. Germany is used as an example to show the capability of the interaction model. First of all, the country specific descriptive statistics for the individual institutions are required. The necessary indicator values are given in table 5.5.

With the descriptive statistics for Germany, the calculations are carried out with the mean value over time as well as with the 2005 values. The mean value is selected since it is able to describe the average institutional level in Germany, while the 2005 value is the most recent observation point. The results on the impact of institutional changes enable us to draw a clear picture on which institutional reforms might be beneficial for the German labor market. Table 5.6 presents these marginal effects.

A positive value of the marginal effect means that an increase in the particular indicator raises the unemployment rate. For the mean value, five indicators have a positive and eight indicators a negative sign. If only the indicator values for 2005 are used, the results remain qualitatively the same. However,

Table 5.5: Descriptive statistics of institutions in Germany

Variable	Mean	Median	S.D.	Min	Max
Payroll tax	14.31	14.98	1.41	12.31	16.07
Income tax	17.18	17.34	0.72	15.55	18.23
Consumption tax	16.79	17.15	1.34	14.74	18.54
EPL regular	2.66	2.68	0.12	2.58	3.00
EPL temporary	2.96	3.63	1.00	1.25	3.75
Bargaining coordination	4.00	4.00	0.00	4.00	4.00
Minimum wage	1.00	1.00	0.00	1.00	1.00
Union coverage	69.32	69.20	5.20	63.00	76.80
Entry barriers	3.77	4.15	1.72	0.77	5.71
Public ownership	3.52	3.94	1.06	1.86	4.67
First year benefits	38.09	38.35	1.43	35.40	40.40
Second/third year benefits	22.01	23.10	4.81	0.00	24.40
Fourth/fifth year benefits	22.01	23.10	4.81	0.00	24.40

Averages over the period from 1982 to 2005 for Germany. S.D. = Standard Deviation, Min = Minimum value observation, Max = Maximum value observation.

in quantitative terms, the changes are considerable for some indicators. For instance, increasing employment protection for regular employment ($EPLr$) by one unit raises unemployment by only 0.03 percentage points for the mean values of the conditioning factors, but by nearly 1.8 percentage points for the 2005 values. A similar conclusion can be drawn for consumption tax (CTX), minimum wages (MW), public ownership (POS), and fourth and fifth year unemployment benefits ($UB45$). The decision of taking the mean or the 2005 values is therefore an important one. In the following, the focus is on 2005 since mean values neglect changes over time, and using means is equivalent to comparing synthetic averaged indicator values.

Table 5.6 displays what impact institutional reforms in Germany would have on the unemployment rate conditional on other institutions. Comparing these findings with other countries serves to assess the importance of the institutional framework for the success of institutional reforms. Hence, conditional marginal

Table 5.6: Marginal institutional effects for Germany

Variable	Mean value	Marginal effect	2005 value	Marginal effect
Payroll tax	14.31	0.600	15.29	0.367
Income tax	17.18	0.355	15.70	0.391
Consumption tax	16.79	-0.268	18.54	-0.012
EPL regular	2.66	0.003	3.00	1.797
EPL temporary	2.96	-2.230	1.25	-2.496
Bargaining coordination	4.00	-0.387	4.00	-0.513
Minimum wage	1.00	-0.343	1.00	-1.059
Union coverage	69.32	-0.122	63.00	-0.215
Entry barriers	3.77	-2.869	0.77	-2.140
Public ownership	3.52	0.729	1.86	2.840
First year benefits	38.09	-0.038	39.87	-0.054
Second/third year benefits	22.01	-0.069	0.00	-0.239
Fourth/fifth year benefits	22.01	0.141	0.00	0.344

Institutional marginal effects for Germany according to equation (5.2). The mean value is the average over all years for Germany.

effects are calculated for France, the United States, Japan, and Sweden, as well. For all these countries, only the conditioning values for 2005 are taken. The tables 5.7 and 5.8 display the marginal effects for all indicators for France and the United States, and Japan and Sweden.

The first and third column of tables 5.7 and 5.8 display the particular country-specific institutional level, while the second and the fourth column contain the country-specific marginal institutional effects. These values express the unemployment rate reaction to an institutional reform, and the marginal effects for each indicator are directly comparable across countries. This overview again emphasizes the importance of country-specific institutional characteristics for the overall effect of institutional indicators. For instance, the income tax (ITX) level in France and the United States is largely comparable with values of roughly 14% and 13%. However, the impact of an increase in income

Table 5.7: Marginal institutional effects for France and the United States

Variable	France		United States	
	2005 value	Marginal effect	2005 value	Marginal effect
Payroll tax	26.73	-0.241	6.41	-0.348
Income tax	14.32	0.554	13.28	-0.302
Consumption tax	24.62	0.153	9.99	0.405
EPL regular	2.47	-3.736	0.17	8.165
EPL temporary	3.63	1.137	0.25	0.590
Bargaining coordination	2.00	0.127	1.00	-1.499
Minimum wage	6.00	-0.746	8.00	-0.986
Union coverage	95.00	-0.230	13.70	0.102
Entry barriers	1.40	-3.045	1.68	-0.140
Public ownership	3.94	1.774	1.40	3.204
First year benefits	60.78	-0.029	29.45	0.079
Second/third year benefits	37.49	-0.228	5.56	-0.158
Fourth/fifth year benefits	17.79	0.311	5.56	0.268

Institutional marginal effects for France and the United States according to equation (5.2).

taxes on the unemployment rate is completely different. While an increase in income taxes in France increases the unemployment rate, the effect goes in the opposite direction for the United States. According to table 5.4, the impact of an income tax change depends on payroll taxes (PTX) and on minimum wages (MW). Since both countries have similarly rigid minimum wage settings, payroll taxes can be detected as the crucial conditioning factor.

Similarly, the impact of an increase in bargaining coordination (BCO) is distinct in Sweden and in Japan although the degree of coordination is the same. Again, the variation in the conditioning variable between both countries, the union coverage (UC) which is at 92% in Sweden and at about 16% in Japan, delivers the explanation for this effect.

Table 5.8: Marginal institutional effects for Japan and Sweden

Variable	Japan		Sweden	
	2005 value	Marginal effect	2005 value	Marginal effect
Payroll tax	8.94	-0.371	23.34	0.349
Income tax	13.14	-0.208	14.22	0.688
Consumption tax	13.90	0.418	31.66	-0.010
EPL regular	1.87	7.944	2.86	-3.317
EPL temporary	1.00	0.094	1.63	-2.190
Bargaining coordination	3.00	-1.445	3.00	0.067
Minimum wage	8.00	0.807	1.00	-0.680
Union coverage	16.41	-0.055	92.00	-0.270
Entry barriers	1.84	-0.782	0.51	-3.678
Public ownership	1.47	2.884	3.40	2.570
First year benefits	22.32	-0.001	74.99	-0.047
Second/third year benefits	0.00	-0.013	3.09	-0.379
Fourth/fifth year benefits	0.00	0.085	0.00	0.421

Institutional marginal effects for Japan and Sweden according to equation (5.2).

Another interesting point is the connection between employment protection and unemployment benefits. Table 5.4 shows that the impact of first year benefits ($UB1$) on unemployment only depends on employment protection for regular employment ($EPLr$). Comparing tables 5.7 and 5.8 points out the distinct effect of an employment protection reform in countries with different levels of first year unemployment benefits. While an increase in employment protection for regular employment in France and in Sweden (with benefit levels of roughly 60% and 75%) are linked to a fall in the unemployment rate, the same kind of reform increases unemployment in the United States and Japan (with benefit levels of roughly 30% and 22 %).

5.3.3 Economic significance and country heterogeneity

In order to check whether the results are economically significant, a closer look is taken at the fourth and fifth column of table 5.6 which contains the marginal effects for all 14 institutional indicators for Germany. The values for all conditioning variables are the observations for 2005. The results in table 5.6 emphasize that most of the coefficients are not only statistically but also economically significant. An increase in the payroll tax (PTX) by 1 percentage point increases the unemployment rate by 0.367 percentage points. Raising employment protection for regular employment ($EPLr$) by one unit, that is from 2 to 3, for example, increases the unemployment rate by roughly 1.8 percentage points.

In contrast, a 1 percentage point increase of employment protection for temporary employment ($EPLt$) lowers the unemployment rate by about 2.5 percentage points. The impact of a reform of the unemployment benefit system is relevant as well. An increase by 1 percentage point in first ($UB1$), and second and third year benefits ($UB23$) lowers the unemployment rate by 0.05 and 0.24 percentage points, respectively. In contrast, raising fourth and fifth year benefits ($UB45$) by 1 percentage point increases the unemployment rate by 0.34 percentage points.

The minimum wage (MW) results should be taken with care. This institutional factor is measured by an index that can take 8 values. This index says nothing about the level of the minimum wage. Yet, information on the level is crucial for a reasonable quantitative assessment of its labor market effect. The measure used here only allows statements regarding the impact of an institutional reform on the unemployment rate, conditional on the existence of

and the degree of government intervention, in the decision on a minimum wage setting.

Furthermore, as pointed out in section 5.1.1 the negligence of implicit interactions can result in biased interaction term coefficients. Recommendations from the result tables of particular institutional reforms should be interpreted carefully. Additionally, especially for the interactions including indicators which are approximated by indexes (minimum wages, bargaining coordination, employment protection, and product market regulation) estimated reform effects could suffer from imprecise estimates. This is obviously not an issue of the estimation strategy but of the data quality.

5.4 Discussion and classification

In comparison to the unconditional results, an economic classification of the conditional findings is more difficult for the following reason. The comparison with findings of the relevant empirical literature suffers from the fact that it is often infeasible to identify comparable interactions. Empirical studies generally consider just one indicator per institutional category although there might be several variables that capture different effects within the same category. For instance, one could use payroll, income and consumption taxes, or the sum of all three factors (the tax wedge) to describe the labor tax system. The preceding analysis has shown that all three tax measures exhibit robust and significant interdependencies with other institutional factors. Using an aggregate measure like the tax wedge would mean neglecting relevant available information. While the disaggregated variables can help to draw a more detailed picture of the conditional impact of institutions, the comparison with existing empirical examinations is difficult or sometimes even infeasible.

Concerning the classification of the conditional findings, one possibility is to stick to the organization of chapter 4.5 and to discuss the results for each category separately. However, theoretical guidance through the interaction effects separated by institutional categories is limited. This gives rise to the assumption that a more general perspective according to the major theoretical concepts about institutional interactions is better suited for the discussion and classification of the conditional results. The discussion is therefore organized using the four central theoretical concepts of institutional interactions which have already been described in chapter 2 and particularly in section 2.3. These concepts provide a broad perspective on theoretical mechanisms of the effect of institutional interdependencies on unemployment. It is acknowledged that further hypotheses for some specific interactions have also been discussed in the literature, but it is assumed that a broad theoretical perspective is better suited for the comprehensive macroeconomic empirical evaluation carried out in this chapter.

5.4.1 Testing four hypotheses

The four central hypotheses are briefly discussed in the context of the empirical findings given in this chapter. The first hypothesis refers to the compensating role of high bargaining coordination in the bargaining process. The second deals with the fact that bargaining power is crucial for the distribution of costs emerging from institutional rigidities. A complementary or substitutive relation between different institutional factors are the respective contents of hypotheses three and four.

The internalizing role of bargaining coordination

The first argument given by Baccaro and Rei (2007) highlights the potential interaction between the bargaining coordination and all other considered institutional factors. The idea is that a high degree of coordination enables the internalization of negative externalities stemming from institutional regulations. The higher the bargaining coordination, the less detrimental are the negative impacts (or the more beneficial are the positive impacts) of institutional rigidities on the unemployment rate. This corresponds to a negative sign for such interaction terms.[74] For instance, the negative labor market impact of high union bargaining power through extensive or even excessive wage demands is assumed to be compensated by a coordinated wage bargain. Coordination induces unions to care about the employment consequences of their wage demands. The advantage of a high union bargaining power in the wage bargain is then not fully utilized in the presence of coordination. Excessive wage claims are avoided.

The interaction between the bargaining coordination and the union coverage ($BCO * UC$) indeed appears to be robust and significant. However, the sign is positive and therefore counterintuitive to the theoretical predictions. This implies that the higher the degree of coordination, the more detrimental is an increase in the workers' bargaining power. One possible explanation for this is that an increase in coordination generally lowers the unemployment rate, but not so much when the union coverage is high since wage pressure is still strong.

[74]This holds as long as the increase of an indicator value corresponds to a rise in unemployment. Hence, according to the findings in chapter 4, interactions of the bargaining coordination with the consumption tax or the employment protection for temporary jobs should show a positive sign.

The existing literature provides inconclusive results for this hypothesis. The IMF (2003) presents a positive significant coefficient for the interaction between the union density and the bargaining coordination. This is opposed to what has been estimated by Baccaro and Rei (2007) who find a negative coefficient for the same interaction as the IMF (2003). Similarly, Belot and Van Ours (2004) report a negative coefficient for the interaction between the union density and the bargaining centralization.

No other interaction term including the bargaining coordination appears to be significantly related to unemployment. The hypothesis of a moderating bargaining coordination is not supported by the empirical evidence provided here. According to this, the prominent role bargaining coordination plays in the literature as a conditioning variable which causes the internalization of negative externalities seems to be overrated.

The distribution of costs

Daveri and Tabellini (2000) argue that institutions which cause labor cost increases interact with union bargaining power. The higher the union bargaining power, the more likely it is that additional costs are shifted to the employers. Labor demand is then negatively affected. Unemployment should rise more than in a situation with low union bargaining power. Employment protection for temporary and regular employment as well as payroll, income and consumption taxes are assumed to increase labor costs. Interactions of these factors with union coverage as the preferred measure for union bargaining power are expected to show a positive sign. For the consumption tax this assumption is questionable since in chapter 4 an unconditional negative impact on unemployment has been estimated for this factor. Consequently, a negative

sign is also possible for the interaction between consumption taxes and union coverage.

Very little evidence is found for the cost distribution hypothesis. Only the interaction between employment protection for regular employment and union coverage ($EPLr * UC$) is significantly related to unemployment, but this term has an unexpected negative sign. Increasing employment protection raises unemployment less for high union coverage levels. The remaining five relevant interactions are not significant.

From a theoretical point of view, the negative sign is counterintuitive, but it is largely in line with what has been reported in the empirical literature. Only Daveri and Tabellini (2000) find a positive and significant sign for the interaction between labor taxes and union density which is in line with theory. The IMF (2003) was not able to confirm these findings. Similar to the results presented here, the signs of the coefficients of the interactions between the union density and both the employment protection as well as the tax wedge are negative. Bassanini and Duval (2009) report an insignificant effect of both interactions. Hence, the empirical findings point to the invalidity of the cost distribution argument. The interaction between the employment protection for regular employment and the union coverage fits better in the context of complementary institutions. This argument is addressed in detail in the following.

Complementarity or substitutability of institutions

Both the first and the second theoretical argument have not found support from the empirical results of this chapter. The third and fourth argument provide an even more general perspective on interactions. For both hypotheses

it holds that interdependencies can be described by the interplay between determinants of the price-setting and the wage-setting curve. For the hypothesis construction, it is relied on the determinants of the wage-setting and price-setting equation given by Belot and Van Ours (2004). It is assumed that unemployment benefits, employment protection, bargaining power, and product market regulation affect the wage-setting while employment protection, labor taxes, and product market regulation influence the price-setting. Generally, interactions within one of the two groups as well as between them are possible. Two theoretical ramifications can be distinguished, the first provided by Bassanini and Duval (2006) emphasizing the complementary, and the second given by Fiori et al. (2012) underlining the substitutive character of institutions. While both provide reasonable theoretical explanations for interaction effects, the empirical literature has not been able to give unambiguous support for one of the two hypotheses. In the following, the empirical findings will be discussed in the light of the two opposing arguments.

In terms of complementary institutions, reforms that shift the wage-setting (price-setting) curve downwards (upwards) are more beneficial for the labor market if the corresponding price-setting or wage-setting curve is relatively flat. Given that deregulation leads either to a fall or to a flattening of the wage-setting or price-setting curve, a negative sign of the relevant interactions is predicted.[75] For instance, high unemployment benefits make employment reactions to a wage increase probably less elastic and employment rises only slightly if real wages are increased. Under such circumstances, a shift in the

[75]The preceding chapter 4 has shown that this is a simplifying assumption since the deregulation of institutions (expressed as a decrease in the corresponding indicator value) does not necessarily result in lower unemployment. However, the results of chapter 4 do not tell whether a deregulatory reform impacts on the slope or the level of the wage- and labor demand curves. Therefore, the simplifying assumption that deregulation makes the curves flatter or changes their level is kept up here.

price-setting curve through increased competition (what lifts real wages up) is less beneficial for the labor market compared to the situation with low unemployment benefits and a flat wage-setting curve.[76] According to this, interactions should be estimated with a negative sign.

In fact, through this derivation of institutional interactions, ten of the robust and significant interdependencies are covered. All four interactions including employment protection for regular employment reveal a complementary relationship between the respective indicators ($EPLr * UC$, $EPLr * EB$, $EPLr * UB1$, $EPLr * EPLt$). Similarly, three interactions including public ownership show a negative sign ($POS * UC$, $POS * EB$, $POS * UB45$), as well. Finally, payroll and consumption taxes interact with second and third year unemployment benefits ($PTX * UB23$, $CTX * UB23$), and employment protection for temporary employment interact with fourth and fifth year benefits ($EPLt * UB45$) in a complementary way. This indicates that deregulating reforms have a more beneficial effect if the conditioning institutions are at a deregulated level, as well. If it is further assumed that minimum wages could also be incorporated as a determinant of the wage-setting or price-setting equation, the interactions between minimum wages and the payroll as well as the income tax ($PTX * MW$ and $ITX * MW$) also fit to the hypothesis of complementary institutions.

From an economic point of view, the complementary character of institutions is comprehensible. For instance, deregulating the flexibility of temporary jobs ($EPLt$) leading to an increase in labor demand is more beneficial if unem-

[76] An alternative argumentation with the same result is that an institutional reform which raises labor demand only affects equilibrium unemployment if sufficient labor is supplied (if the wage-setting curve is flat). Similarly, a reform fostering labor supply calls for sufficient labor demand (for a flat price-setting curve) to be effective. An example for this, mentioned by Belot and Van Ours (2001), is a decrease of unemployment benefits. This has no effect if not enough jobs are provided due to high employment protection.

ployment benefits for long-term unemployed ($UB45$) are low enough to provide sufficient labor (or to make the wage-setting curve relatively flat). Similarly, a reduction of the job protection for regular employment ($EPLr$) brings about larger employment gains if short-term unemployment benefits ($UB1$) are sufficiently low.

The negative interaction between the employment protection for regular and temporary employment ($EPLr * EPLt$) is particularly interesting. The results of several microeconomic contributions studying this interaction finds support from this. The finding indicates that if protection for regular jobs is lowered, this is more beneficial if job protection for temporary employment is already relatively low. Increasing job protection for regular employment is less detrimental for the labor market if job protection for temporary employment is high, as well. The worst situation is a high regulation of one type of contract together with a rather flexible corresponding type of contract. A possible explanation for this is that joint deregulation prevents the creation of a segmented labor market with high transition rates between temporary jobs. Increasing the protection for temporary employment does no harm to the workforce if regular contracts are flexible enough to avoid worker allocation and adjustment processes. This does not mean that an increase in employment protection for regular employment is beneficial for the labor market if temporary employment is strictly regulated, but it can be claimed that such a regulation is less harmful compared to a situation with highly flexible temporary contracts.

Concerning substitutive institutions, it is argued that reforms lower unemployment more when the employment level is far from full employment. This situation probably occurs in the presence of highly regulated labor market institutions that have the ability to depress labor demand or labor supply. It is assumed that high unemployment is the product of inadequately regu-

lated labor and product markets. A reform carried out to reduce unemployment is assumed to be more successful when unemployment is considerably high. Then, the potential to reform is large enough to bring about a substantially lower unemployment rate. If unemployment is already low, the scope for unemployment-decreasing reforms is limited. As explained in chapter 2, this can be described with a convex wage-setting curve in the wage-setting/price-setting model.

For example, a reform which lowers payroll taxes is more beneficial if employment protection for regular contracts is strictly regulated. This happens due to the fact that high employment protection for regular employment is assumed to keep labor demand down. The tax reform enables firms to adjust their labor demand upwards. With low employment protection for regular employment labor demand would have been already high and the tax reform would not result in a substantial labor demand increase. If unemployment is high due to institutional rigidities which keep labor supply down, a reform increasing labor demand is also beneficial. Then, wages adjust upwards due to the competition for labor, and labor supply increases. If labor supply is already high, the additional employment gains through the reform increasing labor demand are smaller. According to this, a positive sign for interactions between determinants of the price-setting curve and/or the wage-setting curve is expected.

Consistent with this, the interactions between the payroll tax and fourth and fifth year benefits, public ownership, and the income tax, between employment protection for temporary employment and second and third year benefits, and between union coverage and entry barriers ($PTX*UB45$, $PTX*POS$, $PTX*ITX$, $EPLt*UB23$, and $UC*EB$) show positive coefficient signs. Here, reforms of one factor are more beneficial for the labor market if the conditioning

factor is strictly regulated. For instance, a reduction in the payroll tax causes larger employment gains when the degree of state control in the product market is high.

If it is further assumed that the minimum wage is a determinant of the wage-setting or the price-setting curve, three additional substitutive interactions arise. The minimum wage is substitutive to the consumption tax, employment protection for temporary employment and entry barriers ($MW * CTX$, $MW * EPLt$, $MW * EB$). Hence, deregulating one of these institutions is more beneficial if the minimum wage is at a high level.

To summarize, both the complements and the substitutes hypothesis receive support from the empirical findings estimated with the interaction model, and neither of these two hypotheses seems to be superior to the other. In contrast, theories based on the internalizing role of bargaining coordination and on the distribution of costs are not able to explain the estimated labor market impact of institutional interdependencies.

Chapter 6

Policy implications

The aim of this chapter is to assess the capability and the economic relevance of the institutional interaction model explaining unemployment developed in chapter 5. This is done by simulating the labor market impact of one component of the German Hartz reforms. These reforms which have been carried out between 2003 and 2006 caused a substantial modification of the institutional environment in the German labor market. First, it is described how the reform component appears in the macroeconomic indicators used in this study. Second, the simulation exercise is carried out to show what labor market impact would have been expected when the simulation was carried out before the reform component has taken place. A comparison with the actual evolution of the unemployment rate as well as with the related literature serves to classify the results. Third, the effect of joint reforms of unemployment benefits and employment protection on unemployment is discussed.

The German Hartz reforms in the data

The labor market reforms in Germany have been carried out in several steps. A brief description of the Hartz reforms can be found in Jacobi and Kluve

(2007), and a more extensive discussion is given by the German council of economic experts (2005). These Hartz reforms are comprised of various factors like policies improving activation and matching quality. However, while having macroeconomic labor market indicators which are comparable across countries as well as over time is a necessary condition for cross-country studies, this comes at the cost of high aggregation levels for these indicators. They effectively can provide only a highly stylized picture of institutional changes in specific countries, and some aspects of the Hartz reforms cannot be adequately considered with them.

Due to these data limitations, only one of the Hartz reform components is displayed within the interaction model of chapter 5. More specifically, the simulation is confined to the re-organization of the unemployment benefit system which means that benefit entitlement periods for unemployed are reduced from a maximum of 32 months to up to 18 months, depending on the age. More specifically, people aged below 55 are now entitled to unemployment benefits for a maximum of 12 months. This cut in entitlement periods shows up in two of the macroeconomic institutional indicators used in this thesis[77]: replacement rates for unemployment during the second and third year of unemployment, and during the fourth and fifth year of unemployment.[78] Both indicators exhibit a drop in the replacement rate from 2004 to 2005 from 24.4% to 0%.

[77]The calculation method for gross replacement rates is explained in greater detail in Appendix A.

[78]The reduction of benefit entitlement periods is part of the fourth step of the Hartz reforms. Hence, a piece of Hartz IV is simulated here, effectively.

Simulating a Hartz reform component

The simulation gives the hypothetic labor market effect of an unemployment benefit system reform in Germany in 2004 when all other things remain equal. It is carried out using the estimated coefficients in the interaction model of chapter 5. Table 6.1 shows that second and third year unemployment benefits ($UB23$) interact with the level of payroll (PTX) and consumption taxes (CTX) as well as with employment protection for temporary employment ($EPLt$). Fourth and fifth year unemployment benefits ($UB45$) show an interdependency with payroll taxes (PTX), employment protection for temporary employment ($EPLt$), and public ownership (POS). Column (b) of table 6.1

Table 6.1: Interaction effects for simulating the Hartz reforms

Variable	(a)	(b)	(c)
Second/third year benefits (UB23)	PTX	-0.044	-0.015
	CTX		-0.004
	EPLt		0.087
Fourth/fifth year benefits (UB45)	PTX	0.208	0.019
	EPLt		-0.079
	POS		-0.030

The table is a extract of table 5.4.

provides the marginal individual effect of the respective unemployment benefit indicator, and column (c) gives the interaction term coefficient. Both are necessary to evaluate the labor market impact of an unemployment benefit cut following equations (6.1) and (6.2).[79] The simulation can now be carried out using the results of table 6.1. According to this, the effect of a one percentage point reduction in the replacement rate during the second and third year of

[79]The calculation of marginal effects in the interaction model is explained in detail in chapter 5.

unemployment can be calculated as

$$-\frac{\partial UE}{\partial UB23} = -\beta_1 - \beta_2 PTX - \beta_3 CTX - \beta_4 EPLt. \tag{6.1}$$

During the fourth and fifth year of unemployment it reads

$$-\frac{\partial UE}{\partial UB45} = -\beta_1 - \beta_2 PTX - \beta_3 EPLt - \beta_4 POS. \tag{6.2}$$

Since the relevant reform has come into force in January 2004, the values for the interaction partners also refer to 2004.[80] Hence, the simulation shows how unemployment in Germany would have evolved when no other factors had changed after 2004. Introducing 2004 values for the interaction partners $PTX = 15.48$, $CTX = 18.30$, $EPLt = 1.25$, and $POS = 1.48$ gives the rounded marginal effects for the second and third year benefits

$$-\frac{\partial UE}{\partial UB23} = +0.044 + 0.015\ PTX + 0.004\ CTX - 0.087\ EPLt = +0.241 \tag{6.3}$$

and for the fourth and fifth year benefits

$$-\frac{\partial UE}{\partial UB45} = -0.208 - 0.019\ PTX + 0.030\ POS + 0.079\ EPLt = -0.359. \tag{6.4}$$

A one unit fall (which corresponds to a decrease by one percentage point) in $UB23$ and $UB45$ changes the unemployment rate by 0.241 and -0.359 percentage points, respectively. As already explained in the preceding section, the simulated Hartz reform component appears as a drop from 24.4% to 0%

[80] This is a simplified statement and refers to the available data. Actually, different steps of the reform have been carried out at different points in time.

in the replacement rates for both second and third ($UB23$) as well as fourth and fifth year ($UB45$) of unemployment. Marginal effects of both indicators therefore need to be multiplied by 24.4 to receive the simulated impact of the cut in benefit entitlement periods on the unemployment rate. It follows that $(0.241 - 0.359) * 24.4 = -2.887$ percentage points is the simulated change in the unemployment rate in Germany caused by the Hartz reform component.

The validity of this finding can be checked by comparing it with the actual evolution of unemployment in Germany. According to an unemployment rate of 9.78% in 2004, the simulation predicts a fall in unemployment by 2.887 percentage points to 6.89%.[81] In fact, after a slight increase in 2005, unemployment in Germany dropped to 8.4% and 7.3% in 2007 and 2008.[82]

The simulated fall in unemployment when interactions are considered is close to the effective evolution in the unemployment rate. However, it is difficult to decide how much of the actual reduction in unemployment indeed has been caused by the Hartz reforms. Actual unemployment after 2004 could also have decreased due to business cycle effects or other institutional reforms, which cannot be adequately considered in the simulation. Therefore, a comparison with existing findings of the literature on the labor market effect of the Hartz reform component can give additional insights on the validity of the results of the simulation exercise. Krebs and Scheffel (2010) set up a general equilibrium model to evaluate the impact of the reduction in the benefit en-

[81] Unemployment numbers correspond to the harmonized unemployment rates delivered by the OECD. National unemployment rates might differ.

[82] The strength of the interaction model can be illustrated with aid of the results of unconditional institutional effects on unemployment provided in chapter 4. If interdependencies between institutions are neglected, the reforms have a substantially different simulated impact. Chapter 4 gives the unconditional institutional effects of both the second and third year as well as the fourth and fifth year benefits. Wile the first indicator is unconditionally insignificant, the second indicator has a positive sign. Lowering fourth and fifth year benefits by 24.4 percentage points would result in an unemployment rate which is 0.7 to 1.7 percentage points higher than before the reform. It is therefore crucial to take institutional interdependencies into account when assessing institutional reform effects on unemployment.

titlement period as a component of the Hartz reforms in Germany in 2004. They identify a drop of the unemployment rate by approximately 1.1 percentage points. Krause and Uhlig (2012) use a search and matching model of the German labor market which can be applied to assess the labor market impact of the same Hartz reform component in Germany. They claim that the reform has lowered unemployment by approximately 3 percentage points. Especially the second finding is very close to the simulation results provided here.

An interesting aspect of the simulation exercise also deserves attention. A reduction in the unemployment benefits for the second and third year of unemployment results in increased unemployment, while the opposite is true for the unemployment benefits for the fourth and fifth year of unemployment. This could be explained by two opposing theoretical effects of unemployment benefits on unemployment; the insurance and the incentive effect. The incentive effect describes a reduction of unemployment benefits which is expected to lower unemployment due to an increased incentive to search for a job. The insurance effect refers to the assumption that high unemployment benefits ensure that the unemployed keep up their job network. Job matches with higher quality and prolonged job duration hold unemployment down. If the insurance effect is more important for medium-term unemployed, while the incentive effect prevails for long-term unemployment, the opposing unemployment benefit effects on unemployment can be explained. Apart from this argumentation, the interaction model can deliver additional insights as it relates unemployment benefit effects to the specific institutional setting in Germany. Accordingly, a country-specific explanation of both unemployment benefit effects in Germany is given in the following.

According to table 6.1, the labor market effect of a reduction of medium-term unemployment benefits in Germany depends on the strictness of employ-

ment protection for temporary employment. It holds that the higher employment protection for temporary employment, the less detrimental for unemployment is the reduction of medium-term unemployment benefits. Hence, the high flexibility for creating temporary jobs in Germany drives the negative labor market impact of the cut in medium-term replacement rates. A possible economic explanation for this relationship is that reducing medium-term unemployment benefits increases the search intensity of the unemployed due to the incentive effect. Since employment protection for temporary contracts in Germany is sufficiently low to allow for substantial creation of temporary jobs, job matches are generated and unemployment falls in the short-run. However, as temporary employment becomes increasingly important relative to permanent employment, unemployed are forced to accept temporary jobs more frequently. Flexible jobs exhibit high transition rates into and out of unemployment such that, in the end, the combination between reduced medium-term unemployment benefits and low employment protection for temporary jobs leads to higher unemployment.

Furthermore, table 6.1 indicates that reducing long-term unemployment benefits in Germany is the more beneficial, the lower employment protection for temporary jobs. The high flexibility to create temporary jobs in Germany thus seems to facilitate the integration of long-term unemployed, an issue which gains importance through the reduction of long-term unemployment benefits. At first, the incentive effect increases the search intensity of the long-term unemployed. New temporary job matches are created as employment protection for temporary jobs in Germany is low, and the average duration of unemployment reduces. This implies that the detrimental transition effect which is related to low employment protection for temporary jobs in Germany is less important for long-term unemployment than for medium term unemploy-

ment, and that labor market integration is more relevant for long-term than for medium term unemployed. In this context, it can be argued that medium-term unemployed have a higher likelihood to find a permanent job compared to long-term unemployed.[83]

An additional explanation for the beneficial labor market effect of a reduction in long-term unemployment benefits is that this reform leads to increased flows of long-term unemployed out of the labor force. Being long-term unemployed becomes less attractive when benefits are reduced, and the relative effort to remain entitled to unemployment benefits rises. It is probable that this movement out of the labor force affects long-term unemployment more than medium-term unemployment since the latter group exhibits a higher likelihood to find a permanent job.[84]

Benefits from joint reforms?

The reform process in the German labor market at the beginning of the century comprised several institutional adjustments. Besides the Hartz reforms which, amongst other things, changed the unemployment benefit system, employment protection for temporary jobs in Germany was reduced in 2003, as well. In the simulation exercise it was up to now assumed that the simulated reduction in unemployment of 2.887 percentage points in Germany is exclusively related to the decrease in unemployment benefits for medium- and long-term unemployment as part of the Hartz reforms in 2004. However, the reduction in

[83]Both factors, medium- and long-term unemployment benefits interact with additional institutions, but the main difference in the respective labor market effects caused by a reduction in medium- and long-term unemployment benefits occurs due to the dependence on employment protection for temporary employment. The other interactions are therefore not discussed in detail, here.

[84]It is expected that with a single indicator for long-term unemployment a similar drop of 3 percentage points in unemployment through the reduction of unemployment benefits is estimated since both groups are aggregated.

employment protection, which took place in 2003, could also have contributed to this simulated fall in unemployment after 2004 if the employment protection reform had an effect on unemployment which lasted more than one year. If this issue is neglected in the simulation, the labor market impact of the Hartz reform component could be over- or underestimated. Thus, the reduction of employment protection for temporary employment in Germany should also be considered in the simulations to disentangle the impact of both institutional reforms on the labor market.

Besides the direct effects of these two German reforms on unemployment, it is also very likely that the joint reduction of both unemployment benefits as well as employment protection for temporary employment in Germany reinforced or constrained each other. This argument is based on the findings of the interaction model of chapter 5 where unemployment benefits for medium- and long-term unemployment exhibit a significant interaction with employment protection for temporary employment. Since decreasing medium- and long-term unemployment benefits has an impact on unemployment dependent on the *level* of employment protection for temporary employment, a *change* in the employment protection level probably alters the unemployment benefit reform impact on unemployment, and vice-versa. In order to shed light on the benefits and drawbacks of such a potential joint reform effect, the simulation exercise is slightly extended to additionally explicitly consider the employment protection reform effect on unemployment. For this purpose, two distinct mechanisms have to be taken into account which influence the joint reform impact on unemployment. First, it is assumed that the simulated labor market effect of the Hartz reform component is now influenced by the employment protection reform, and, second, the employment protection reform itself also has an impact on unemployment in the simulation.

The first aspect can be easily analyzed by comparing the simulated effect of the reduction in unemployment benefits in Germany before and after the employment protection reform. Hence, the simulation of the Hartz reform simulation is carried out for two different values of employment protection for temporary employment: 1.5 to capture the employment protection level before the reform in 2003, and 1.25 for the employment protection level afterwards. Both levels are introduced in equations (6.2) and (6.3) for $EPLt$. Instead of a reduction by 2.887 percentage points, unemployment falls by 2.936 percentage points if the pre-reform level of employment protection for temporary employment is evaluated. This finding indicates that the reduction in unemployment benefits for medium- and long-term unemployment benefits results in a stronger simulated fall in unemployment for the pre-reform level of employment protection for temporary employment. According to this, the joint reform of both medium- long-term unemployment benefits as well as employment protection for temporary employment in Germany seems to have constrained each other. A final assessment of the joint reform impact also requires to simulate the direct effect of the employment protection reform in Germany, which is done in the following.

Similar to the simulation of the Hartz reform component, the interaction results for employment protection for temporary employment are required to perform this simulation. The corresponding findings of the interaction model of chapter 5 are given in table 6.2 where it is shown that employment protection for temporary employment ($EPLt$) affects unemployment conditional on the country-specific levels of the minimum wage indicator (MW), second and third year unemployment benefits ($UB23$), fourth and fifth year unemployment benefits ($UB45$), and employment protection for regular contracts ($EPLr$). The interactions of $EPLt$ with these factors have been identified as robust and

significant for unemployment with the *BACE* approach in chapter 5. In the

Table 6.2: Interaction effects for simulating the $EPLt$ reform

Variable	(a)	(b)	(c)
EPL temporary (EPLt)	MW	-2.025	0.327
	UB23		0.087
	UB45		-0.079
	EPLr		-0.266

The table is a extract of table 5.4.

style of equations (6.1) to (6.4), the impact of the reduction of employment protection for temporary employment by one unit is given by equation (6.5). The values for the conditioning variables refer to the German values for 2004 except for the medium and long-term unemployment benefits. For these two factors it is assumed that the reduction of the Hartz reform which caused the reduction of the benefit entitlement period for medium and long-term benefits had already been taken place to capture the incidence of joint reform. Hence, the levels for $UB23$ and $UB45$ are kept at zero, while the indicator values for MW and $EPLr$ are set to 1 and 3, respectively. The following equation (6.5) shows the marginal effect of a one unit reduction in employment protection for temporary employment on unemployment. It follows that

$$-\frac{\partial UE}{\partial EPLt} = 2.496. \tag{6.5}$$

Hence, a one unit decrease of $EPLt$ increases unemployment by 2.496 percentage points. The reform of employment protection for temporary employment in Germany appears with a drop of 0.25 units in the corresponding macroeconomic indicator. In order to determine the simulated effect of this reform, the marginal effect of $EPLt$ given in equation (6.5) needs to be multiplied by 0.25. According to this, the German employment protection reform in 2003

increased unemployment by $2.496 * 0.25 = 0.624$ percentage points. Given that the reform of the unemployment benefit system reduced unemployment in Germany by 2.887 percentage points, the joint impact of both reform types is $-2.887 + 0.624 = -2.263$, i.e. unemployment is approximately 2.3 percentage points lower subsequent to the joint reform.

In summary, while reforming the unemployment benefit system has been a success story, the additional reduction of employment protection for temporary employment failed to reduce unemployment further. Given that the latter reform increased unemployment, it would have been beneficial to leave employment protection for temporary employment at its pre-reform level. An economic argumentation, which has already been given in the preceding section, is that high flexibility to open up temporary jobs is detrimental if permanent jobs are to a large extent substituted by temporary ones. Such short-term contracts lead to higher unemployment since they result in increased transition rates from unemployment into temporary jobs and vice-versa.

Concluding remarks

In a nutshell, the interaction model is able to reasonably predict country-specific labor market effects of institutional reforms. With aid of a simulation exercise for the German labor market, interdependencies between institutions have shown to be crucial for the determination of country-specific reform effects on unemployment. The simulation of a German labor market reform of the unemployment benefit system in 2004 predicts a subsequent fall in unemployment in Germany which is both rather close to the actual evolution of unemployment as well as to the findings of two calibrated theoretical macroeconomic models.

In comparison to these models, the empirical interaction model developed in this thesis is able to give a much richer description of institutions, and the analysis of institutional reform effects is not restricted to the unemployment benefit system. This empirical interaction model allows the investigation of country-specific reform impacts of various institutional factors like employment protection, labor taxes, or bargaining power. Furthermore, this interaction model goes beyond the findings of existing empirical models since interdependencies between different institutional factors are taken into account in a systematic way for the first time. This allows the reliable evaluation of country-specific reform effects on the labor market as it can be abstracted from interpreting institutional effects as averages across countries and over time.

Chapter 7

Conclusions

The analysis of unconditional as well as conditional institutional effects on unemployment carried out in this thesis contributes substantially to a better understanding of the complex functioning of institutions. The discussion of theoretical mechanisms provided in chapter 2 reveals two important aspects. First, for all institutional categories, the theoretical literature has identified several mechanisms of how institutions have a direct impact on unemployment. Frequently, these mechanisms are working in opposite directions. This makes it difficult to establish clear-cut predictions of unconditional institutional impacts on unemployment. Second, much less theoretical work has dealt with conditional institutional effects. While interactions are potentially relevant for all institutions, few clear predictions can be derived from theory. The large number of potentially relevant interactions prevents the construction of a detailed and comprehensive macroeconomic model considering interdependent institutions. Answering the question of which institutions affect unemployment as well as of the role of interdependencies between institutions is therefore an empirical task.

However, the empirical literature as well provides inconclusive results in regards to unconditional institutional effects on unemployment at least for some institutions. While the institutional setting is generally identified as important for unemployment, it is still unclear as to which institutional factors actually matter. One of the main sources for this finding is the inconsistent use of institutional indicators. Subjective selections of institutions as explanatory variables cause the negligence of potentially relevant indicators. This can result in an omitted variable bias. In addition, the limited number of observations in macroeconomic studies complicates the inclusion of all available institutional factors. Degrees of freedom reduce substantially and inference from significance levels is therefore less reliable. Finally, improvements in indicator quality and availability showed that results of earlier studies are often unreliable.

The problem of limited observations and numerous potentially explanatory factors aggravates for the analysis of conditional effects. Constructing interaction terms increases the number of parameters to be estimated substantially. The comprehensive estimation of interaction effects is then infeasible. Consequently, the empirical literature provides results for few selected interaction terms only, and the validity of these findings has to be called in question. Systematic empirical approaches to estimate such interaction effects are missing.

The *Bayesian Averaging of Classical Estimates (BACE)* approach outlined in chapter 3 seems to be well-suited for the clear-cut identification of significant institutions. Bayesian model averaging techniques use a weighting scheme based on the marginal likelihood of a model to evaluate the quality of each considered model. Better models receive a higher weight. While a pure Bayesian approach requires the specification of prior information on the marginal likelihood of each model, the *BACE* approach uses the Bayesian information criterion as an approximation. With this approximation, the weighting scheme

can be adequately calculated without any prior specification, except for the prior model size. This parameter expresses the researcher's assumptions on the true model size before seeing the data. Alterations of this factor provide the opportunity checking the robustness of the results produced with the *BACE* approach.

The basic econometric model to which the *BACE* method is applied consists of the unemployment rate which is regressed on a set of explanatory variables. Macroeconomic control factors are included in all models. Country- and time-specific effects are wiped out through the *Within transformation* for a two-way error component regression model. The models actually differ only in the set of institutional variables, which are included. The significance of a variable is derived from the respective model qualities. Institutions which are assumed to be significant determinants of unemployment appear more often in models that receive a higher weight.

Data for 17 OECD countries from 1982 to 2005 is used for the empirical analysis. The dependent variable in all models is the harmonized unemployment rate which allows the comparison of labor market performance both across countries as well as over time. Overall, 19 institutional indicators which can be grouped in five categories are under inspection. An important prerequisite for the reliable estimation of institutional effects is the variation within as well as between countries for all institutional indicators. Invariant factors are wiped out by the *Within transformation*, and coefficients of nearly time-invariant indicators could be estimated with bias. While most institutional indicators show enough both within as well as between variation, five factors could be problematic. Indicators for bargaining centralization and coordination, minimum wages, and employment protection for both regular and

temporary employment do not exhibit substantial variation over time. Hence, results for these indicators have to be interpreted with care.

The analysis of unconditional institutional effects in chapter 4 shows that six institutional variables are robust and significant. Payroll and consumption taxes, fourth and fifth year unemployment benefits, union coverage, bargaining coordination, and employment protection for temporary employment are the indicators which are significant across all specifications. These factors are therefore clearly identified as determinants of unemployment. Four additional variables appear to be significant, but some doubts in terms of robustness surround the findings. Income taxes, employment protection for regular employment, entry barriers, and public ownership are significant indicators in some specifications, but not in all. The remaining nine institutional indicators (first year, second and third year, and aggregate unemployment benefits, unemployment benefit duration and coverage, bargaining centralization, aggregate product market regulation, minimum wages, and union density) are not significantly related to unemployment.

A single model estimation including merely the ten significant institutions is then carried out to perform several econometric tests. This is not easily feasible within the *BACE* framework. Controlling for heteroskedasticity, endogeneity, and outliers largely confirms the findings. Mainly the four institutional indictors which are not robust in some model averaging specifications also appear to be fragile here.

Nearly all of the ten significant institutions are also economically relevant. Lowering payroll and incomes taxes, union coverage, employment protection for regular contracts, and public ownership is linked to a fall in the unemployment rate. Conversely, a reduction in consumption taxes, employment protection for temporary contracts, bargaining coordination, fourth and fifth

year benefits, and entry barriers leads to an increasing unemployment rate. Concerns regarding the economic relevance exist for few factors. Coefficient signs of consumption taxes and entry barriers cannot be explained with theoretical arguments given in chapter 2. Hence, these factors are suspected to be estimated with bias due to potentially omitted variables and data measurement error.

Additional insights are generated when interdependencies between institutions are taken into account in chapter 5. 91 bivariate interaction terms are considered as varying explanatory variables in the *BACE* framework. Models now differ in the set of interactions that are included, while all relevant individual institutions as well as the macroeconomic control variables are fix regressors which appear in each model.

22 institutional interactions are identified as robust and significant determinants of unemployment. Interestingly, all included institutional variables except for the union bargaining coverage appear as an interaction partner in at least one robust and significant interaction term. This highlights the importance of such interdependencies between all institutional categories. The most important interaction partners in terms of appearances in robust and significant interaction terms are payroll taxes, minimum wages, and entry barriers with five appearances in each case.

Based on these findings, marginal institutional effects can be calculated for each country separately. The economic relevance of institutional interdependencies is then shown by means of comparing institutional marginal effects between selected countries. While this exercise can contribute to explaining differences in the unemployment rate across countries, it is particularly beneficial when it comes to predicting the labor market impact of institutional reforms. For each institution considered in the interaction terms, the hypo-

thetical effect of a reform on unemployment is identified conditional on the country-specific institutional framework. According to different theoretical approaches, institutions can either be substitutes or complements. Both arguments find empirical support from the results provided in chapter 5. The results illustrate that the institutions of nine of the 22 robust and significant interactions are substitutes, while the constitutive institutions of 13 interactions are complements.

Chapter 6 conducts a simple simulation exercise to evaluate the validity of the interaction model of chapter 5. The labor market impact of a reduction in the unemployment benefit entitlement period in Germany is simulated with this model. This reform has been a component of the German Hartz IV reforms and appears through a cut in the replacement rates for medium- and long-term unemployment in Germany. The model predicts a fall in unemployment of almost 3 percentage points subsequent to the reform. A comparison with the existing literature indicates that the model delivers reasonable and highly relevant results. It is therefore assumed to be well-suited for the analysis of reform effects on the labor market. The potential application for predicting the country-specific labor market impact of various institutional reforms makes it an appealing tool for policy-makers.

In summary, this thesis makes three important contributions. First, it is clearly identified which institutions drive unemployment in OECD countries. This issue has been discussed controversially in the literature up to now. Six factors, payroll and consumption taxes, long-term unemployment benefits, union coverage, bargaining coordination, and employment protection for temporary employment, are unequivocally identified as significant determinants of unemployment rates in 17 industrialized OECD countries from 1982 to 2005. Four additional indicators, income taxes, employment protection for regular

employment, entry barriers, and public ownership, also appear significant in some specifications. However, variations in the estimation setup affect the robustness of these four factors. The remaining nine institutional variables are not important for the unemployment rate. According to these findings, the inconclusive results of the literature can therefore at least partially be traced back to model misspecification.

Second, the *BACE* approach turns out to be an appropriate method for the valid identification of both unconditional as well as conditional institutional effects on unemployment. Model uncertainty which occurs when observations are limited and the number of explanatory variables is large can be explicitly taken into account.

Third, this approach also proves to be well-suited for the incorporation of institutional interdependencies which can be estimated systematically for the first time. 22 bivariate interactions are identified as significantly related to the unemployment rate in the 17 industrialized OECD countries. From these findings it is derived which institutional reforms are beneficial for the labor market in specific institutional environments. The outcome of this analysis allows the determination of potential reform effects for all 17 OECD countries separately. This is a substantial improvement over the existing empirical as well as theoretical literature since reform effects can be reliably determined for various institutions dependent on the respective country-specific institutional setting.

While this thesis makes important contributions to the literature, it has not been possible to consider all aspects related to the link between institutions and unemployment. One of the limitations of the empirical analysis is the confinement to five institutional categories what is caused by lacking data availability. Incorporating information on, for instance, family or migration

policies would provide additional important insights for the planning and design of labor market policy reforms. Similarly, it would be interesting to see whether the implications of this study also hold for emerging countries like the Central and Eastern European economies. Furthermore, the literature would profit considerably from the evaluation of higher-order interactions. Such interdependencies between 3 or more institutions could provide a more detailed perspective on the labor market impact of comprehensive reform packages.

Appendix A

Data sources and construction

A.1 Unemployment

The harmonized unemployment rate series for all countries come from the OECD key short-term economic indicators database. Some unemployment data is missing for earlier periods for some countries. For example, data on unemployment for Germany is missing on a harmonized basis due to the unification. However, the *BACE* approach requires the use of a balanced and comprehensive data set. To ensure consistent time series, the growth rates of the unemployment rates as a percentage of the civilian labor force (which are not harmonized over countries) are calculated and the harmonized unemployment rates are extended by concatenating the change of the country-specific non-harmonized unemployment rates.

This could give rise to criticism since it is doubtful as to whether the unemployment rates for early periods are comparable or not. However, only Austria from 1982 to 1992, Germany from 1982 to 1990 and Switzerland from 1982 to 1991 are affected by this adjustment. Therefore, it is preferred to have a larger data set at the expense of a potential measurement error.

A.2 Determinants of Unemployment

Labor tax system

The three taxes (payroll, income, and consumption tax) have been constructed according to the definitions given in Nickell and Nunziata (2001).[85] This data set was later updated and extended by Nickell (2006). Nevertheless, the original series have been compiled up to 2003 only and the information was incomplete for some countries. Note that a simple updating was not feasible for all necessary series since the OECD from where the meta-data has been taken re-organized and re-structured its database. As a result, some of the series Nickell (2006) used to construct the labor tax indicators were no longer available and had to be substituted. The calculations extended the three indicators for all countries for 2003, 2004, and 2005. Furthermore, the fragmentary series of Australia, Canada, Switzerland, Great Britain, Denmark, and Japan have been completed.

The payroll tax (PTX) is calculated as

$$PTX = \frac{ess}{ie - ess} \tag{A.1}$$

with ess equal to the employer's social security contributions and ie equal to the compensation of employees. Both series come from the OECD, the former from the taxation revenue statistics (Code 2200), the latter from the annual

[85] For Japan, it has to be taken into account that the OECD data is in billions, and not in millions as for the other countries. Hence, the Japan values for the payroll and the income tax have to be multiplied by 1000 to gain the true values.

national accounts (Code $D1$). The income tax (ITX) is

$$ITX = \frac{it}{hcr} \tag{A.2}$$

where it is the direct tax spending and hcr the household current receipt. it is the sum of the social security contributions of the employees, self-employed or non-employed and a statistical relic (codes 2100, 2300, and 2400), and the taxes on income and profits (code 1110). All data can be found in the Taxation Revenue Statistics of the OECD. The hcr series come from the OECD Economic Outlook 83 in the Economic Projections database. Note that a more recent edition of the Economic Outlook does not contain data for Spain which is why the 2008 version is used. Furthermore, no hcr data is available for Germany up to 1991 and for Switzerland up to 1990. The growth rates of the income tax series delivered by Nickell and Nunziata (2001) for the missing periods for both countries are used to extend the hcr series back by concatenating the growth rates of the default series for the missing observations for Germany and Switzerland. Finally, the consumption tax (CTX) is the result of

$$CTX = \frac{tls}{fce} \tag{A.3}$$

with tls equal to taxes less subsidies on products and imports and fce equal to the final consumption expenditure of households. Both series are from the OECD Annual National Accounts (codes $D21_D31$ and $D2_D3$).

For the majority of countries, the differences between the series calculated by Nickell and Nunziata (2001) and the newly calculated data are small. Differences are observable for the payroll tax for the Netherlands, for the income

tax for Belgium, Spain and Sweden, and for the consumption tax for most of the countries during the 80's. Especially the changes in the consumption tax can probably be traced back to data revisions or harmonizations of the OECD since the old and the new series are almost identical from the 90's on. The new payroll tax values for the Netherlands are between 3 and 10 percent points above the data from Nickell (2006), but from 1994 on, the difference almost disappears.

Bargaining system

Union density (UD), union coverage (UC), minimum wages (MW), bargaining coordination (BCO) and bargaining centralization (BCE) measures all have been taken from the Visser database (see Visser (2009)). The following definitions are taken from this source, as well.

The union density indicator consists of the percentage of wage and salary earners which are organized in a union. The union coverage indicator shows the percentage of employees whose wage bargaining is affected by wage bargaining agreements.

Bargaining coordination is an index ranging from 1 to 5. A value of 1 indicates fragmented bargaining, mostly at the company level. 2 means mixed firm-level bargaining, with weak enforceability of industry agreements. Industry bargaining with no or irregular pattern setting, limited involvement of central organizations and limited freedoms for company bargaining is represented by a value of 3. 4 refers to mixed industry and economy-wide bargaining: a) central organizations negotiate non-enforceable central agreements (guidelines) and/or b) key unions and employers associations set patterns for the entire economy. A value of 5 indicates economy-wide bargaining, based

on a) enforceable agreements between the central organizations of unions and employers affecting the entire economy or entire private sector, or on b) government imposition of a wage schedule, freeze, or ceiling.

Bargaining centralization is an indicator from 1 to 5 and shows the level at which bargaining is conducted. 1 means very low centralization at the local or company level. Sectoral or industry level, with additional local or company bargaining is displayed by a value of 2, while 3 refers to sectoral or industry level bargaining. 4 means bargaining at the national or central level, with additional sectoral/local or company bargaining, and 5 represents national or central level bargaining.

The minimum wage indicator ranges from 0 to 8. An indicator value of 0 means no national (cross-sectoral or inter-occupational) minimum wage. Minimum wages set by collective agreement or tripartite wage boards in (some) sectors is represented by a value of 1. 2 refers to minimum wages set by national (cross-sectoral or inter-occupational) agreement ("'autonomous agreement"') between unions and employers. 3 means that a national minimum wage is set by agreement (as for 2) but extended and made binding by law or Ministerial decree. A national minimum wage set through tripartite negotiations is displayed by a value of 4. 5 represents a national minimum wage set on fixed rule (index-based minimum wage) after negotiations or consultations with by the social partners. A national minimum wage set by government, but after (non-binding) tripartite consultations is reflected by a value of 6. A national minimum wage set by judges or expert committee, as in award-system refers to a value of 7. Finally, 8 means that a national minimum wage is set by the government, without fixed rule.

Unemployment benefit system

Data on unemployment benefits as a percentage of the previous income for the first, the second and third, and the fourth and fifth year of unemployment comes from Nickell (2006) up to 2003. The replacement rates which have originally been calculated by the OECD for every second year from 1961 to 2003 are interpolated for the missing even years. Different household and income situations are taken into account. More concretely, a single average worker, a married average worker without children where the spouse doesn't work, and an average worker with the spouse working are evaluated for two earning levels; $\frac{2}{3}$ of and exactly the average worker income. The replacement rate is then calculated as the average over all six household-income situations. For 2004 and 2005, the OECD Tax-Benefit Model from 2007 has been used for the calculations. First, the replacement rates for 2003 are calculated using the Tax-Benefit-Model, and compared to the values generated by the OECD. If both values are reasonably similar, the 2004 and 2005 values are calculated with the same methodology. For some countries, the OECD values and the values generated with the Tax-Benefit-Model show substantial differences. In this case, country-specific files available from the OECD and containing detailed information on the unemployment benefit system serves for the error search. In the following, the correction approaches for the flawed replacement rates are described explicitly. For Canada, the gross replacement rate has to be calculated manually by dividing the gross income by the average earning level. The replacement rates for the USA include social assistance payments which have to be considered when using the model. Similarly, the rent assistance needs to be included for Australia. However, this series is not available in the Tax-Benefit-Model. It has to be derived from the corresponding country

chapter information available from the OECD. For Italy, the model calculations are completely wrong. The gross replacement rates have to be calculated using the information provided in the corresponding country chapters. The same holds true for United Kingdom. Here, reasonable, but wrong values are provided by the Tax-Benefit-Model because unemployment insurance and unemployment assistance have shown the wrong underlying parameters.

The aggregate unemployment benefits (UBO) are the unweighted mean over the first ($UB1$), second and third ($UB23$), and fourth and fifth year benefits ($UB45$). The benefit duration (UBD) indicator equals

$$UBD = 0.6\frac{UB23}{UB1} + 0.4\frac{UB45}{UB1}. \tag{A.4}$$

Concerning the unemployment benefit coverage (UBC) series provided by the Fondazione Rodolfo de Benedetti, observations are missing for Belgium (2000-2005), Italy (1982-1989 and 2003-2005), Sweden (1982-1994), Switzerland (1982-1984) and the United Kingdom (1996). In order to construct a comprehensive data set without gaps, the same value as the first preceding or successive observation with a valid value are assigned to the missing observations. If both a preceding and successive value is available, the mean is constructed.

Employment protection

The employment protection indicator for regular employment ($EPLr$), delivered by the OECD, covers nine items: Notification procedures, delay involved before notice can start, length of the notice period, severance pay, definition of justified or unfair dismissal, length of trial period, compensation following unfair dismissal, possibility of reinstatement following unfair dismissal, and

maximum time to make a claim of unfair dismissal. The indicator for temporary employment ($EPLt$) involves eight items: Valid cases for use of fixed-term contracts (FTC), maximum number of successive FTC, maximum cumulated duration of successive FTC, types of work for which temporary work agency (TWA) employment is legal, restrictions on number of renewals, maximum cumulated duration of TWA contracts, does the set-up of a TWA require authorization or reporting obligations, and do regulations ensure equal treatment of regular and agency workers at the user firm. A score between 0 and 6 is assigned to each item according to its degree of regulation. The higher the value, the more regulated are labor contracts. The employment protection indicator is then constructed by combining the scores of all corresponding items. The weighting scheme can be found in Venn (2009). Note that the indicator updated by Venn (2009) ranges only from 1985 to 2005. Data from 1982 to 1984 which was generated by the OECD is available from the ifo Dice database which gathers cross-country information on institutional characteristics. Both series are concatenated by taking the growth rates from 1982 to 1984 to extend the new series backwards.

Product market regulation

Indicators for entry regulation, public ownership, market structure, vertical integration and separation as well as price controls are collected for seven sectors (telecom, electricity, gas, post, rail, air passenger transport, and road freight) by the OECD. Only three indicators, public ownership (POS), entry barriers (EB), and an overall indicator (PMR) for product market regulation are comprehensively available across countries. Nevertheless, data on entry barriers is missing for Austria from 1982 to 1990 and for Norway from 1999 to 2002, while the aggregate indicator shows missing observations for Austria from 1982 to

1990, Germany from 1991 to 1997, Norway 1999 to 2002 and Spain 1988 to 1997. The missing data has been added by interpolation. This is also possible for Austria since one observation for 1975 is available. The aggregate indicator is not just the average of the two sub-indicators, but includes additional indicators of regulation, where available.

Correlations between institutions

The following table A.1 presents the correlations between all 19 institutional indicators. The following name conventions hold. PTX, ITX and CTX give payroll, income and consumption taxes. $EPLr$ and $EPLt$ are employment protection for regular and temporary employment. BCO, BCE, MW, UD and UC represent bargaining coordination and centralization, minimum wages, union density and coverage. PMR is aggregate product market regulation, EB and POS show entry barriers and public ownership. $UB1$, $UB23$, $UB45$, UBD, UB and UBC are first, second and third, and fourth and fifth year benefits, unemployment benefit duration, aggregate unemployment benefits and unemployment benefit coverage.

Control variables

The four shock variables are calculated as follows. The real import price is the import price deflator divided by the GDP deflator. According to the following equation, the shock is the log change of the real import price (IPS) times the import share in GDP.

$$IPS = \frac{Imports}{GDP} log\left(\frac{IP_{deflator}}{GDP_{deflator}}\right) \tag{A.5}$$

with $IP_{deflator}$ being the import price deflator. The real interest rate is the long-term interest rate corrected for the current inflation rate. For the construction of the total factor productivity (TFP) shocks I follow Bassanini and Duval (2006) and calculate first the change in the log of TFP as

$$\Delta ln(TFP) = \frac{\Delta ln(Y) - \alpha \Delta ln(TE) + (1-\alpha)\Delta ln(K)}{\alpha} \tag{A.6}$$

with Y equal to the GDP in the business sector, TE is total employment, K the gross capital stock, and α the share of labor income in total business sector income. By cumulating the changes in the log TFP's over years I get the TFP in each year. Finally, the deviations from the TFP trend are taken to construct an index for TFP shocks by applying the Hodrick-Prescott filter with a λ of 100. The labor demand shock is the change in the residuals of a labor demand model to be estimated. Hence, the following equation is estimated for each country and take ε as the country-specific labor demand shock.

$$ln(TE_t) = \beta_0 + \beta_1 ln(TE_{t-1}) + \beta_2 ln(TE_{t-2}) + \beta_3 ln(TE_{t-3}) + \beta_4 ln(Y_t) + \beta_5 ln(LC_t) + \varepsilon_t. \tag{A.7}$$

Again, TE is total employment, Y is the real GDP and LC are the real labor costs per employee. The real labor costs are calculated as the total labor costs of the total economy divided by the number of dependently employed workers.

The sources and construction of the additional macroeconomic controls is described in the following. Data on the change in the inflation rate is taken from the OECD database. Following Baccaro and Rei (2007) this variable is constructed as $CPI_t - CPI_{t-1}$. The lagged labor productivity growth (LPG) is a series also provided by the OECD. For Austria, information is only available

from 1996 on. The Total Economy Database information on labor productivity (GDP per hour worked) is used and, using the OECD approach, the labor productivity growth series for Austria is extended as $(ln(LPG_t - ln(LPG_{t-1}) * 100$. Data on public spending on active labor market policy is provided by the OECD. For the credit constraints, data from Beck et al. (2009) is used. More specifically, the indicator for private credit by deposit money banks and other financial institutions over GDP is taken to represent financial market frictions. Finally, the output gap series are taken from the OECD data-base.

Table A.1: Correlation coefficients between all institutions

	PTX	ITX	CTX	EPLr	EPLt	BCO	BCE	MW	UD	UC	EB	POS	PMR	UB1	UB23	UB45	UBD	UB	UBC
PTX	1.00	-0.30	0.29	0.56	0.66	0.23	0.37	-0.14	0.11	0.56	0.17	0.40	0.30	0.24	0.03	-0.15	-0.20	0.08	-0.18
ITX		1.00	0.50	0.14	-0.15	0.08	0.22	-0.33	0.55	0.10	-0.13	0.21	-0.08	0.37	0.41	0.24	0.16	0.46	0.01
CTX			1.00	0.37	0.17	0.07	0.39	-0.27	0.63	0.52	-0.20	0.23	-0.08	0.34	0.40	0.31	0.32	0.46	-0.33
EPLr				1.00	0.51	0.52	0.55	-0.23	0.15	0.62	0.22	0.45	0.33	0.48	0.37	0.10	0.06	0.44	-0.14
EPLt					1.00	0.48	0.49	-0.25	0.17	0.57	0.53	0.63	0.64	0.21	0.29	0.05	-0.03	0.26	-0.12
BCO						1.00	0.63	-0.32	0.31	0.44	0.28	0.42	0.35	0.27	0.36	0.22	0.13	0.38	0.20
BCE							1.00	-0.44	0.51	0.69	0.19	0.54	0.30	0.40	0.51	0.29	0.33	0.53	0.02
MW								1.00	-0.55	-0.48	-0.02	-0.46	-0.14	-0.13	-0.27	-0.24	-0.32	-0.27	-0.28
UD									1.00	0.38	0.02	0.36	0.12	0.28	0.23	0.26	0.17	0.34	0.01
UC										1.00	0.20	0.71	0.38	0.31	0.57	0.48	0.48	0.58	0.02
EB											1.00	0.65	0.96	0.05	0.06	-0.02	-0.10	0.05	0.04
POS												1.00	0.79	0.36	0.45	0.24	0.18	0.47	0.21
PMR													1.00	0.13	0.16	0.07	-0.03	0.16	0.04
UB1														1.00	0.50	-0.03	-0.11	0.72	0.35
UB23															1.00	0.61	0.64	0.93	0.03
UB45																1.00	0.85	0.61	-0.06
UBD																	1.00	0.54	-0.09
UB																		1.00	0.16
UBC																			1.00

Appendix B

Additional results

B.1 Results for alternative selection strategies

The results given in the following belong to section 5.1.3. Instead of considering all available interaction terms, a distinct interaction selection strategy is followed. First, highly correlated interactions are identified. This is done by calculating the Pearson correlation coefficient for each pair of interaction terms. Two interaction terms are assumed to be highly correlated if the correlation coefficient ρ exceeds a value of 0.9 in the first step, and 0.8 in the second step. Second, the less significant interaction term in the baseline model given in table 5.1 of section 5.1.2 are excluded. Note that this can lead to the exclusion of interaction terms which are robust and significant in the baseline model. For a correlation threshold level of $\rho = 0.9$, a set of 67 interaction terms remains. For $\rho = 0.8$, 52 interaction terms are excluded and 39 remain to be estimated. The results for $\rho = 0.9$ and $\rho = 0.8$ are given in the tables B.1 and B.2.

Table B.1: Conditional effects for interaction term selection with $\rho = 0.9$

Variable						
Model size	k = 10	k = 15	k = 20	k = 25	k = 30	k = 40
PIB	(0.15)	(0.22)	(0.30)	(0.37)	(0.45)	(0.60)
EPLt*UB3	1.000	1.000	1.000	1.000	1.000	1.000
PTX*MW	1.000	1.000	1.000	1.000	1.000	1.000
EPLr*UC	1.000	1.000	1.000	1.000	1.000	1.000
PTX*POS	1.000	1.000	1.000	1.000	1.000	1.000
EPLt*UB2	1.000	1.000	1.000	1.000	1.000	1.000
BCO*UC	1.000	1.000	1.000	1.000	1.000	1.000
PTX*UB3	1.000	1.000	1.000	1.000	1.000	1.000
EB*POS	1.000	1.000	1.000	1.000	1.000	0.999
ITX*MW	0.999	0.999	0.999	0.999	0.999	0.999
PTX*ITX	0.980	0.991	0.995	0.998	0.999	1.000
EPLt*MW	1.000	0.999	0.999	0.992	0.983	0.966
PTX*UB2	0.949	0.970	0.981	0.988	0.991	0.995
EPLr*UB1	0.796	0.917	0.967	0.986	0.996	0.999
CTX*UB2	0.993	0.989	0.986	0.929	0.845	0.670
ITX*UB1	0.344	0.544	0.699	0.776	0.821	0.812
EPLr*MW	0.504	0.621	0.672	0.737	0.831	0.938
CTX*MW	0.226	0.425	0.593	0.720	0.809	0.877
EPLr*EPLt	0.671	0.637	0.587	0.605	0.703	0.878
PTX*UBC	0.328	0.368	0.383	0.446	0.574	0.807
BCO*UBC	*0.342*	*0.511*	*0.637*	*0.659*	*0.581*	0.398
MW*EB	0.128	*0.300*	*0.459*	*0.601*	*0.679*	*0.812*
ITX*EPLr	0.095	*0.250*	*0.411*	*0.510*	*0.533*	0.529
EPLr*UBC	*0.161*	*0.324*	*0.430*	*0.444*	0.374	0.217
EB*UB1	0.113	0.208	*0.325*	*0.441*	*0.521*	*0.673*
CTX*UC	0.073	0.165	0.266	*0.439*	*0.642*	*0.918*
EPLt*BCO	*0.562*	*0.466*	*0.373*	0.348	0.377	0.444
CTX*POS	0.055	0.092	0.128	0.209	0.399	*0.743*
ITX*POS	0.033	0.053	0.077	0.141	0.274	*0.603*
UC*UBC	*0.289*	0.222	0.164	0.136	0.129	0.150

The dependent variable is the unemployment rate. The five control variables as well as the 14 institutional indicators have been included in all regressions. Only variables with a posterior inclusion probability (PIB) above the prior inclusion probability for at least one prior model size specification are displayed in the table. PTX = Payroll tax, ITX = Income tax, CTX = Consumption tax, EPLr = Employment protection reg., EPLt = Employment protection temp., BCO = Bargaining coordination, MW = Minimum wage, UC = Union coverage, EB = Entry barriers, POS = Public ownership, UB1 = First year benefits, UB23 = Second/third year benefits, UB45 = Fourth/fifth year benefits, UBC = Unemployment benefit coverage.

Table B.2: Conditional effects for interaction term selection with $\rho = 0.8$

Variable					
Model size	k = 4	k = 7	k = 10	k = 15	k = 20
PIB	(0.10)	(0.18)	(0.26)	(0.38)	(0.51)
EPLt*UB23	1.000	1.000	1.000	1.000	1.000
EPLt*UB45	1.000	1.000	1.000	1.000	1.000
PTX*MW	1.000	1.000	1.000	1.000	1.000
EB*POS	1.000	1.000	1.000	1.000	1.000
PTX*UB45	1.000	1.000	1.000	1.000	1.000
BCO*UC	1.000	1.000	1.000	1.000	1.000
CTX*UB23	0.991	0.999	1.000	1.000	1.000
PTX*EB	0.999	1.000	1.000	1.000	1.000
PTX*ITX	0.997	0.999	1.000	1.000	1.000
ITX*MW	0.995	0.999	1.000	1.000	1.000
EPLt*MW	1.000	0.999	1.000	1.000	0.999
PTX*UB23	0.984	0.995	0.997	0.998	0.998
EPLr*UC	0.994	0.994	0.996	0.997	0.998
EPLr*UB1	0.942	0.978	0.987	0.992	0.996
BCO*UBC	0.855	0.961	0.982	0.991	0.993
MW*EB	0.595	0.810	0.891	0.953	0.976
CTX*MW	0.532	0.741	0.825	0.899	0.937
EPLr*UBC	0.552	0.729	0.787	0.830	0.838
EPLr*UB45	0.347	0.444	0.516	0.605	0.683
EPLr*EPLt	0.218	0.295	0.372	0.477	0.567
EB*UB1	*0.110*	0.157	0.199	0.259	0.315
ITX*UB45	*0.206*	*0.186*	0.176	0.187	0.196
PTX*UBC	*0.157*	0.153	0.158	0.183	0.229
EPLt*UBC	*0.179*	0.109	0.085	0.069	0.081

The dependent variable is the unemployment rate. The five control variables as well as the 14 institutional indicators have been included in all regressions. Only variables with a posterior inclusion probability (PIB) above the prior inclusion probability for at least one prior model size specification are displayed in the table. PTX = Payroll tax, ITX = Income tax, CTX = Consumption tax, EPLr = Employment protection reg., EPLt = Employment protection temp., BCO = Bargaining coordination, MW = Minimum wage, UC = Union coverage, EB = Entry barriers, POS = Public ownership, UB1 = First year benefits, UB23 = Second/third year benefits, UB45 = Fourth/fifth year benefits, UBC = Unemployment benefit coverage.

Bibliography

Acemoglu, Daron and Robert Shimer, "Efficient Unemployment Insurance," *Journal of Political Economy*, 1999, *107*, 893–928.

_ **and** _ , "Productivity gains from unemployment insurance," *European Economic Review*, 2000, *44*, 1195–1224.

Addison, John T., McKinley L. Blackburn, and Chad D. Cotti, "Do minimum wages raise employment? Evidence from the U.S. retail-trade sector," *Labour Economics*, 2009, *16*, 397–408.

Aidt, Toke S. and Zafiris Tzannatos, "Trade unions, collective bargaining and macroeconomic performance: a review," *Industrial Relations Journal*, 2008, *39*, 258–295.

Amable, Bruno, Lilas Demmou, and Donatella Gatti, "Employment performance and institutions: new answers to an old question," *IZA Discussion Papers No. 2731*, 2007.

Arulampalam, Wiji, "Is Unemployment Really Scarring? Effects of Unemployment Experiences on Wages," *Economic Journal*, 2001, *111*, 585–606.

Baccaro, Lucio and Diego Rei, "Institutional Determinants of Unemployment in OECD Countries: Does The Deregulatory View Hold Water?," *International Organization*, 2007, *61*, 527–569.

Baker, Dean, Andrew Glyn, David R. Howell, and John Schmitt, "Labor Market Institutions and Unemployment: A Critical Assessment of the Cross-Country Evidence," in David R. Howell, ed., *Fighting Unemployment: The Limits of Free Market Orthodoxy*, Oxford University Press, 2005, chapter 3.

Baltagi, Badi H., *Economic Analysis of Panel Data*, 3. ed., Wiley, 2003.

Bassanini, Andrea and Romain Duval, "The determinants of unemployment across OECD Countries: Reassessing the Role of Policies and Institutions," *OECD Economics Department Working Paper No. 486*, 2006.

_ **and** _ , "Unemployment, institutions, and reform complementaries: reassessing the aggregate evidence for OECD countries," *Oxford Review of Economic Policy*, 2009, *25*, 40–59.

Beck, Thorsten, Asli Demirgüç-Kunt, and Ross Levine, "Financial Institutions and Markets Across Countries and over Time: The Updated Financial Development and Structure Database," *World Bank Economic Review*, 2009, *24*, 77–92.

Belot, Michèle and Jan C. Van Ours, "Unemployment and Labor Market Institutions: An Empirical Analysis," *Journal of the Japanese and International Economies*, 2001, *15*, 403–418.

_ **and** _ , "Does the recent success of some OECD countries in lowering their unemployment rates lie in the clever design of their labor market reforms?," *Oxford Economic Papers*, 2004, *56*, 621–642.

Bennmarker, Helge, Erik Mellander, and Björn Öckert, "Do regional payroll tax reductions boost employment?," *Labour Economics*, 2009, *16*, 480–489.

Berger, Helge and Stephan Danninger, "The Employment Effects of Labor and Product Market Deregulation and Their Implications for Structural Reform," *IMF Staff Papers*, 2007, *54*, 591–619.

Berger, Tino and Gerdie Everaert, "A replication note on unemployment in the OECD since the 1960s: what do we know?," *Empirical Economics*, 2009, *36*, 479–485.

_ **and** _ , "Labour taxes and unemployment evidence from a panel unobserved component model," *Journal of Economic Dynamics and Control*, 2010, *34*, 354–364.

Bertrand, Marianne and Francis Kramarz, "Does Entry Regulation Hinder Job Creation? Evidence from the French Retail Industry," *Quarterly Journal of Economics*, 2002, *117*, 1369–1413.

Blanchard, Olivier, "European unemployment: the evolution of facts and ideas," *Economic Policy*, 2006, *45*, 5–59.

_ **and Augustin Landier**, "The Perverse Effects of Partial Labour Market Reform: Fixed-Term Contracts In France," *Economic Journal*, 2002, *112*, F214–F244.

_ **and Francesco Giavazzi**, "Macroeconomic Effects Of Regulation And Deregulation In Goods And Labor Markets," *Quarterly Journal of Economics*, 2003, *118*, 879–907.

_ **and Jean Tirole**, "The Joint Design of Unemployment Insurance and Employment Protection: A First Pass," *Journal of the European Economic Association*, 2008, *6*, 45–77.

_ **and Justin Wolfers**, "The Role of Shocks and Institutions In The Rise Of European Unemployment: The Aggregate Evidence," *Economic Journal*, 2000, *110*, C1–C33.

_ **and Pedro Portugal**, "What Hides Behind an Unemployment Rate: Comparing Portuguese and U.S. Labor Markets," *American Economic Review*, 2001, *91*, 187–207.

Boeri, Tito and Jan C. Van Ours, *The Economics of Imperfect Labor Markets*, Princeton University Press, 2008.

_ **and Pietro Garibaldi**, "Two Tier Reforms of Employment Protection: A Honeymoon Effect?," *Economic Journal*, 2007, *117*, F357–F385.

Brambor, Thomas, William Roberts Clark, and Matt Golder, "Understanding Interaction Models: Improving Empirical Analysis," *Political Analysis*, 2006, *14*, 63–82.

Braumoeller, Bear F., "Hypothesis Testing and Multiplicative Interaction Terms," *International Organization*, 2004, *58*, 807–820.

Bryant, Henry L. and George C. Davis, "Revisiting Aggregate U.S. Meat Demand With a Bayesian Averaging of Classical Estimates Approach: Do We Need a More General Theory?," *American Journal of Agricultural Economics*, 2008, *90*, 103–116.

Cahuc, Pierre and André Zylberberg, *Labor Economics*, The MIT Press, 2004.

_ **and Fabien Postel-Vinay**, "Temporary jobs, employment protection and labor market performance," *Labour Economics*, 2002, *9*, 63–91.

Calmfors, Lars and John Driffill, "Centralization of wage bargaining," *Economic Policy*, 1988, *3*, 13–61.

Clyde, Merlise, Heather Desimone, and Giovanni Parmigiani, "Prediction Via Orthogonalized Model Mixing," *Journal of the American Statistical Association*, 1996, *453*, 1197–1208.

Coe, David T. and Dennis J. Snower, "Policy Complementarities: The Case for Fundamental Labour Market Reform," *IMF Staff Papers*, 1997, *44*.

Conway, Paul and Guiseppe Nicoletti, "Product Market Regulation in the Non-Manufacturing Sectors of OECD Countries: Measurement and Highlights," *OECD Economics Department Working Paper No. 530*, 2006.

Daveri, Francesco, "Labor taxes and unemployment: A survey of the aggregate evidence," in Onotaro Castellino and Elsa Fornero, eds., *Pension Policy in an Integrating Europe*, Edward Elgar, 2003, chapter 3.

_ **and Guido Tabellini**, "Unemployment, growth and taxation in industrial countries," *Economic Policy*, 2000, *15*, 49–104.

Davidson, Russel and James G. MacKinnon, *Econometric Theory and Methods*, Oxford University Press, New York, 2004.

Dolton, Peter, Chiara Rosazza Bondibene, and Jonathan Wadsworth, "Employment, Inequality and the UK National Minimum Wage over the Medium-TermÅ," *Oxford Bulletin of Economics and Statistics, forthcoming*, 2011.

Doppelhofer, Gernot and Melvyn Weeks, "Jointness of Growth Determinants," *Journal of Applied Econometrics*, 2009, *24*, 209–244.

Dromel, Nicolas, Elie Kolakez, and Etienne Lehmann, "Credit Constraints and the Persistence of Unemployment," *Labour Economics*, 2010, *17*, 823–834.

Dube, Arindrajit, T. William Lester, and Michael Reich, "Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties," *Review of Economics and Statistics*, 2010, *92*, 945–964.

Ebell, Monique and Christian Haefke, "Product market deregulation and the US employment miracle," *Review of Economic Dynamics*, 2009, *12*, 479–504.

Eicher, Theo S., Alex Lenkoski, and Adrian Raftery, "Bayesian Model Averaging and Endogeneity Under Model Uncertainty: An Application to Development Determinants," *University of Washington Working Paper No. UWEC-2009-19-FC*, 2009.

Elmeskov, Jörgen, John P. Martin, and Stefano Scarpetta, "Key Lessons For Labour Market Reforms: Evidence From OECD Countries' Experience," *Swedish Economic Policy Review*, 1998, *5*, 205–252.

Felbermayr, Gabriel and Julien Prat, "Product Market Regulation, Firm Selection, And Unemployment," *Journal of the European Economic Association*, 2011, *9*, 278–317.

Feldmann, Horst, "Business regulation and labor market performance around the world," *Journal of Regulatory Economics*, 2008, *33*, 201–235.

Fernandez, Carmen, Eduardo Ley, and Mark F.J. Steel, "Benchmark priors for Bayesian model averaging," *Journal of Econometrics*, 2001, *100*, 381–427.

Fiori, Giuseppe, Giuseppe Nicoletti, Stefano Scarpetta, and Fabio Schiantarelli, "Employment Effects of Product and Labour Market Reforms: Are There Synergies?," *Economic Journal*, 2012, *122*, F79–F104.

Flanagan, Robert J., "Macroeconomic Performance and Collective Bargaining: An International Perspective," *Journal of Economic Literature*, 1999, *37*, 1150–1175.

Franz, Wolfgang, *Arbeitsmarktökonomik*, 6. ed., Springer, Berlin, 2005.

Freeman, Richard B., "Labor Market Institutions Around The World," *NBER Working Paper No. 13242*, 2007.

Gaure, Simen, Knut Røed, and Lars Westlie, "The Impacts of Labor Market Policies on Job Search Behavior and Post-Unemployment Job Quality," *IZA Discussion Paper No. 3802*, 2008.

German council of economic experts, *Die Chance nutzen - Reformen mutig voranbringen* 2005.

Granger, Clive W. J. and Harald F. Uhlig, "Reasonable extreme-bounds analysis," *Journal of Econometrics*, 1990, *44*, 159–170.

Griffith, Rachel, Rupert Harrison, and Gareth Macartney, "Product Market Reforms, Labour Market Institutions and Unemployment," *Economic Journal*, 2007, *117*, C124–C166.

Hall, Peter and David Soskice, *Varieties of Capitalism: The Institutional Foundations of Comparative Advantage*, Oxford University Press, 2001.

Hoeting, Jennifer A., David Madigan, Adrian E. Raftery, and Chris T. Volinsky, "Bayesian Model Averaging: A Tutorial," *Statistical Science*, 1999, *14*, 382–417.

Holmlund, Bertil, "Unemployment Insurance in Theory and Practice," *Scandinavian Journal of Economics*, 1998, *100*, 113–141.

Howell, David R. and Miriam Rehm, "Unemployment compensation and high European unemployment: a reassessment with new benefit indicators," *Oxford Review of Economic Policy*, 2009, *25*, 60–93.

_ **, Dean Baker, Andrew Glyn, and John Schmitt**, "Are Protective Labor Market Institutions at the Root of Unemployment? A Critical Review of the Evidence," *Capitalism and Society*, 2007, *2*, 1–71.

IMF, "World Economic Outlook," in "in" 2003, chapter 4.

Jacobi, Lena and Jochen Kluve, "Before and after the Hartz reforms: The performance of active labour market policy in Germany," *Zeitschrift für ArbeitsmarktForschung Journal for Labour Market Research*, 2007, *40*, 45–64.

Johnston, Jack and John DiNardo, *Econometric Methods*, 4. ed., McGraw-Hill, 1997.

Kahn, Lawrence, "The Impact Of Employment Protection Mandates on Demographic Temporary Employment Patterns: International Microeconomic Evidence," *Economic Journal*, 2007, *117*, F333–F356.

_ , "Employment protection reforms, employment and the incidence of temporary jobs in Europe: 1996-2001," *Labour Economics*, 2010, *17*, 1–15.

Kenworthy, Lane, "Wage-Setting Measures: A Survey and Assessment," *World Politics*, 2001, *54*, 57–98.

Kim, Jaewon, "Why do Some Studies Show that Generous Unemployment Benefits Increase Unemployment Rates? A Meta-Analysis of Cross-Country Studies," *Stockholm Research Papers in Economics No. 2011:18*, 2011.

Koop, Gary, *Bayesian Econometrics*, 1. ed., Wiley, 2003.

Krause, Michael U. and Harald Uhlig, "Transitions in the German labor market: Structure and crisis," *Journal of Monetary Economics*, 2012.

Krebs, Tom and Martin Scheffel, "A macroeconomic model for the evaluation of labor market reforms," *ZEW Discussion Papers No. 10-050*, 2010.

Kugler, Adriana D., "The Impact of Firing Costs on Turnover and Unemployment: Evidence from the Colombian Labour Market Reform," *International Tax and Public Finance*, 1999, *6*, 389–410.

_ **and Giovanni Pica**, "Effects of employment protection on worker and job flows: Evidence from the 1990 Italian reform," *Labour Economics*, 2008, *15*, 78–95.

Lalive, Rafael, "Unemployment Benefits, Unemployment Duration, and Post-Unemployment Jobs: A Regression Discontinuity Approach," *AEA Papers and Proceedings*, 2007, *97*, 108–112.

Lamla, Michael J., "Long-run determinants of pollution: A robustness analysis," *Ecological Economics*, 2009, *69*, 135–144.

Layard, Richard, Stephen Nickell, and Richard Jackman, *Unemployment: Macroeconomic Performance and the Labour Market*, 1. ed., Oxford University Press, 2003.

Lazear, Edward P., "Job Security Provisions And Employment," *Quarterly Journal of Economics*, 1990, *105*, 699–725.

Leamer, Edward E., "Let's Take the Con out of Econometrics," *American Economic Review*, 1983, *73*, 31–43.

_ , "Sensitivity Analyses Would Help," *American Economic Review*, 1985, *75*, 308–313.

_ **and Herman Leonard**, "Reporting the Fragility of Regression Estimates," *Review of Economics and Statistics*, 1983, *65*, 306–317.

Levine, Ross and David Renelt, "A Sensitivity Analysis of Cross-Country Growth Regressions," *American Economic Review*, 1992, *82*, 942–963.

Ley, Eduardo and Mark F. J. Steel, "On The Effect Of Prior Assumptions In Bayesian Model Averaging With Applications To Growth Regressions," *Journal of Applied Econometrics*, 2009, *24*, 651–674.

Ljungqvist, Lars, "How Do Lay-Off Costs Affect Unemployment?," *Economic Journal*, 2002, *112*, 829–853.

_ **and Thomas Sargent**, "The European unemployment dilemma," *Journal of Political Economy*, 1998, *7106*, 514–550.

_ **and** _ , "Understanding European unemployment with matching and search-island models," *Journal of Monetary Economics*, 2007, *54*, 2139–2179.

_ **and** _ , "Two Questions About European Unemployment," *Econometrica*, 2008, *76*, 1–29.

Maddala, G.S. and Shaowen Wu, "A Comparative Study Of Unit Root Tests With Panel Data And A New Simple Test," *Oxford Bulletin of Economics and Statistics (Special Issue)*, 1999, *61*, 631–652.

Manning, Alan, "How Do We Know That Real Wages Are Too High?," *Quarterly Journal of Economics*, 1995, *110*, 1111–1125.

McAleer, Michael, Adrian R. Pagan, and Paul A. Volker, "What Will Take the Con Out of Econometrics?," *American Economic Review*, 1985, *75*, 293–307.

Moral-Benito, Enrique, "Model Averaging in Economics," *CEMFI Working Paper No. 1008*, 2010.

_ , "Determinants of Economic Growth: A Bayesian Panel Data Approach," *Review of Economics and Statistics, forthcoming*, 2011.

Mortensen, Dale T., "Unemployment insurance and job search decisions," *Industrial and Labor Relations Review*, 1977, *30*, 505–517.

Nickell, Stephen, "Unemployment and Labor Market Rigidities: Europe versus North America," *Journal of Economic Perspectives*, 1997, *11*, 55–74.

_ **and Luca Nunziata**, "Labor Market Institutions Database," *CEP Discussion Paper No. 502*, 2001.

_ , _ , **and Wolfgang Ochel**, "Unemployment in the OECD since the 1960s. What do we know?," *Economic Journal*, 2005, *115*, 1–27.

_ , _ , _ , **and Glenda Quintini**, "The Beveridge Curve, Unemployment and Wages in the OECD from the 1960s to the 1990s," *CEP Discussion Paper No. 502*, 2002.

Nickell, William, "The CEP-OECD Institutions Data Set (1960-2004)," *CEP Discussion Paper No. 759*, 2006.

Nicoletti, Guiseppe and Stefano Scarpetta, "Product Market Reforms and Employment in OECD Countries," *OECD Economics Department Working Paper No. 472*, 2005.

OECD, *The OECD Job Study: Evidence and Explanations, Part II - The Adjustment Potential of the Labour Market*, OECD Paris, 1994.

_ , *OECD Employment Outlook*, OECD Paris, 1999.

_ , *OECD Employment Outlook*, OECD Paris, 2004.

_ , "Reassessing the Role of Policies and Institutions for Labour Market Performance: A Quantative Analysis," in "in," OECD Paris, 2006, chapter 7.

Ours, Jan C. Van and Milan Vodopivec, "Does reducing unemployment insurance generosity reduce job match quality?," *Journal of Public Economics*, 2008, *92*, 684–695.

Planas, Christophe, Werner Roeger, and Alessandro Rossi, "How much has labour taxation contributed to European structural unemployment?," *Journal of Economic Dynamics and Control*, 2007, *31*, 1359–1375.

Polavieja, Javier G., "Temporary Contracts and Labour Market Segmentation in Spain An Employment-Rent Approach," *European Sociological Review*, 2003, *19*, 501–517.

Raftery, Adrian E., "Bayesian Model Selection in Social Research," *Sociological Methodology*, 1995, *25*, 111–163.

Roed, Knut and Tao Zhang, "Does Unemployment Compensation Affect Unemployment Duration?," *Economic Journal*, 2003, *113*, 190–206.

Sabia, Joseph J., "The Effects of Minimum Wage Increases on Retail Employment and Hours: New Evidence from Monthly CPS Data," *Journal of Labor Research*, 2009, *30*, 75–97.

Sala-I-Martin, Xavier, "I Just Ran Two Million Regressions," *American Economic Review*, 1997, *87*, 178–183.

_ **, Gernot Doppelhofer, and Roland I. Miller**, "Determinants of Long-Term Growth: A Bayesian Averaging of Classical Estimates (BACE) Approach," *American Economic Review*, 2004, *94*, 813–835.

Scarpetta, Stefano, "Assessing the Role of Labour Market Policies and Institutional Settings on Unemployment: A Cross-Country Study," *OECD Economic Studies No. 26*, 1996.

Schiantarelli, Fabio, "Product Market Regulation and Macroeconomic Performance: A Review of Cross Country Evidence," *Boston College Working Papers in Economics No. 623*, 2008.

Schrimpf, Andreas, "International Stock Return Predictability Under Model Uncertainty," *Journal of International Money and Finance*, 2010, *29*, 1256–1282.

Schwarz, Gideon, "Estimating the Dimension Of A Model," *The Annals of Statistics*, 1978, *6*, 461–464.

Sørensen, Peter Birch, *Measuring The Tax Burden on Capital and Labor*, CESifo Seminar Series, 2004.

Soskice, David, "Wage Determination: The Changing Role of Institutions in Advanced Industrialized Countries," *Oxford Review of Economic Policy*, 1990, *6*, 36–61.

Stigler, George J., "The economics of minimum wage legislations," *American Economic Review*, 1946, *36*, 358–365.

Stock, James H. and Douglas Staiger, "Instrumental Variables Regression with Weak Instruments," *Econometrica*, 1997, *65*, 557–586.

Tatsiramos, Konstantinos, "Unemployment Insurance in Europe: Unemployment Duration and subsequent Employment Stability," *Journal of the European Economic Association*, 2009, *7*, 1225–1260.

Traxler, Franz, "Collective Bargaining: Levels and Coverage," *OECD Employment Outlook 1994*, 1994, pp. 167–194.

Venn, Danielle, "Legislation, collective bargaining and enforcement: Updating the OECD employment protection indicators," *OECD Social, Employment and Migration working papers No. 89*, 2009.

Visser, Jelle, "The ICTWSS Database: Database on Institutional Characteristics of Trade Unions, Wage Setting, State Intervention and Social Pacts in 34 countries between 1960 and 2007," 2009.

Wallace, T D and Ashiq Hussain, "The Use of Error Components Models in Combining Cross Section with Time Series Data," *Econometrica*, 1969, *37*, 55–72.

Zellner, Arnold, "On Assessing Prior Distributions and Bayesian Regression Analysis with g-Prior Distributions," in Prem Goel and Arnold Zellner, eds., *Bayesian inference and decision techniques: Essays in honor of Bruno de Finetti. Studies in Bayesian Econometrics and Statistics*, Amsterdam: North Holland, 1986, pp. 233–243.

Lebenslauf Andreas Sachs

Abschluss:	Diplom-Wirtschaftswissenschaftler
Geburtsdatum:	08.06.1981
Nationalität:	deutsch

Beruflicher Werdegang

02/2008-heute	Wissenschaftlicher Mitarbeiter in der Forschungsgruppe Wachstums- und Konjunkturanalysen am Zentrum für Europäische Wirtschaftsforschung, Mannheim
10/2002-12/2007	Studium der Wirtschaftswissenschaften mit Schwerpunkt Internationale Wirtschaft an der Universität Ulm, Diplomarbeit über *Wirtschaftliches Wachstum und Konvergenz - Methodische Konzepte und empirische Ergebnisse auf Basis des neoklassischen Wachstumsmodells*
09/2005-02/2006	Praktikum bei Porsche Iberica S.A. in Madrid, Spanien
03/2005-07/2005	Auslandssemester an der Universidad de La Laguna auf Teneriffa, Spanien
08/2001-05/2002	Zivildienst im Limeshof Welzheim
09/1991-06/2001	Max-Planck Gymnasium Schorndorf

Wissenschaftliche Publikationen

Publikationen in referierten Fachzeitschriften

Sachs, Andreas (2012), What really drives unemployment? A bayesian approach to determine the impact of institutions on the unemployment rate, *Economics Bulletin* 32(1), 1008-1019.

Diskussionspapiere

Sachs, Andreas (2011), *Institutions and Unemployment: Do Interactions Matter?*, ZEW Discussion Paper No. 11-057, Mannheim.

Sachs, Andreas (2010), *A Bayesian Approach to Determine the Impact of Institutions on the Unemployment Rate*, ZEW Discussion Paper No. 10-058, Mannheim.

Sachs, Andreas und Frauke Schleer (2009), *Labour Market Institutions and Structural Reforms: A Source for Business Cycle Synchronisation?*, ZEW Discussion Paper No. 09-008, Mannheim.

Expertisen

Projektgruppe Gemeinschaftsdiagnose (2012), Deutsche Konjunktur im Aufwind - Europäische Schuldenkrise schwelt weiter, Gemeinschaftsdiagnose Frühjahr, München.

Projektgruppe Gemeinschaftsdiagnose (2011), Europäische Schuldenkrise belastet deutsche Konjunktur, Gemeinschaftsdiagnose Herbst, Essen.

Kappler, Marcus, H. Bonin und A. Sachs (2011), Wertschöpfungseffekte der Fachkräftesicherung: Forschungsbericht, Mannheim.

Projektgruppe Gemeinschaftsdiagnose (2011), Aufschwung setzt sich fort -

Europäische Schuldenkrise noch ungelöst, Gemeinschaftsdiagnose Frühjahr, Halle.

Projektgruppe Gemeinschaftsdiagnose (2010), Deutschland im Aufschwung - Wirtschaftspolitik vor wichtigen Entscheidungen, Gemeinschaftsdiagnose Herbst, München.

Kappler, M., A. Sachs, A. Seymen, B. van Aarle und Klaus Weyerstrass (2008), Study on Economic Integration and Business Cycle Synchronization: Final Report, Bureau of Economic Policy Advisers (BEPA), Brüssel.

Vorträge auf wissenschaftlichen Konferenzen

03/2012	ifo Dresden Workshop Arbeitsmarkt- und Sozialpolitik, Dresden
10/2011	X^{th} Economic Policy Conference, Málaga
05/2011	Doctoral Meeting Montpellier, Montpellier
09/2010	AIEL Conference 2010, Pescara
06/2010	2^{nd} Conference on Recent Developments in Macroeconomics, Mannheim
04/2010	Tagung der ARGE-Institute, Berlin
04/2010	Spring Meeting of Young Economics 2010, Luxemburg
09/2009	Jahrestagung des Vereins für Socialpolitik, Magdeburg
05/2009	10 Years of the Euro - Adjustments in Capital and Labour Markets, Braga

Referee-Tätigkeit

Oxford Bulletin of Economics and Statistics