Do labor market institutions matter for business cycles?*

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October 9, 2014

Abstract

Using panel data of 19 OECD countries observed over 40 years and data on specific labor market reform episodes we conclude that labor market institutions matter for business cycle fluctuations. Spearman partial rank correlations reveal that more flexible institutions are associated with lower business cycle volatility. Turning to the analysis of reform episodes, wage bargaining reforms increase the correlation of the real wage with labor productivity and the volatility of unemployment. Employment protection reforms increase the volatility of employment and decrease the correlation of the real wage with labor productivity. Reforms reducing replacement rates make labor productivity more procyclical.

JEL-Classification: E32, J01, J08
Key words: Labor market institutions, Business cycles, Principal component analysis, difference-in-difference regressions

*We would like to thank G. Bertola, A. Campolmi, F. Canova, J. J. Dolado, J. Gali, C. Merkl, M. Reiter, and T. Van Rens, and participants to CEPR/CREI Workshop on Changes in Labor Market Dynamics, Barcelona and "Labor market institutions and the macroeconomy", Friedrich-Alexander University, Nuremberg. The views expressed in this paper are those of the authors. No responsibility for them should be attributed to the Bank of Canada.

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1 Introduction

Many economists have argued that labor market frictions affect the short run responses of macroeconomic variables to shocks (see Walsh (2005), Trigari (2006), Campolmi and Faia (2011), Krause and Lubik (2007), Blanchard and Gali (2010), Thomas (2008) and Gali and van Rens (2010) among others). For that reason, business cycle models have been recently augmented with a variety of labor market frictions. Two broad categories of rigidities have been considered: (a) frictions limiting flows in and out of unemployment, such as hiring costs and employment protection legislation; and (b) rigidities preventing the adjustment of real wages, such as collective wage bargaining. Despite these theoretical developments, the empirical literature on the relevance of labor market frictions for business cycle fluctuations is rather scant. A number of business cycle models have been structurally estimated (see e.g. Tomas and Zanetti (2009), Krause, Lopez-Salido and Lubik (2008), Christoffel, Kuester and Linzert (2006)), but the focus of the investigation has been on the effects of labor market rigidities on inflation or the transmission of monetary policy decisions. In general, existing contributions look at the data through the lens of a model, so that the results are specific to the modelling assumptions and to the nature of the shocks included in (and excluded from) the analysis.

This paper provides stylized facts about the relationship between labor market institutions (henceforth LMIs) and business cycle fluctuations. We investigate how institutional arrangements, such as employment protection, replacement rates, union density and coverage shape business cycle statistics in a panel of 19 OECD countries using data from 1971 to 2007. Rather than modelling explicitly the link between institutions, labor market frictions and macro variables, we examine the direct relationship between business cycles and LMIs. This is advantageous because it allows us to avoid controversial assumptions about how institutions map into labor market frictions and what their functional form is.

We use various indicators to capture the institutional characteristics of labor markets obtained from the CEP-OECD Institutions Data Set (Nickell (2006)) and the ICTWSS Database on Institutional Characteristics of Trade Unions, Wage Setting, State Intervention and Social Pacts (Visser, 2009). The data covers a sufficiently long span of time to include both expansionary and recessionary periods. We have information about replacement rates, labor unions (density, coverage, concentration, centralization), the wage bargaining process (coordination, government involvement, the level at which bargaining takes place, and coverage extension of bargained wages), and employment protection legislation.

We look at the data from two different angles. Following Fonseca et al. (2009, 2010), we split the sample by decades and compute partial Spearman rank correlations between beginning of decade labor market characteristics and decade-average business cycle statistics, controlling for macroeconomic conditions. We collapse the information contained in the set of labor market indicators we have available using principal component analysis, and construct four "statistical" factors. We also construct factors using economic intuition, extracting a common factor separately from indicators that characterize (a) replacement rates, (b) the strength of unions, (c) wage bargaining, and (d) employment protection. We then relate second moments of macroeconomic variables such as real GDP per capita, inflation, unemployment, employment, labor productivity and the real wage to the statistical and economic principal components of the LMIs, controlling for macroeconomic conditions.

There is a strong positive correlation between the statistical factor which we call "overall rigid-

ity" and the variance of output, real wages, labor productivity, and inflation. The second statistical factor is also positively related to macroeconomic volatility but is negatively related to the correlation of output and employment. Hence, more flexible labor market institutions are generally associated with lower business cycle volatility. The results obtained using "economic factors" are similar: (i) replacement rates correlate positively with the volatility of unemployment, (ii) unions' power and wage bargaining both correlate positively with macroeconomic volatility (for output, wages, labor productivity, and inflation) and unions' power correlates negatively with the correlation between output and employment, and (iii) employment protection relates positively with the volatility of unemployment.

To sharpen our conclusions about the effects of LMIs on business cycles, we then turn to specific reform episodes. We assemble a new dataset using various existing data sources ("the Social Policy Reform Inventory" assembled by the Fondazione Rodolfo DeBenedetti; the Labor Markets Reforms Information provided by the OECD; the Database for Institutional Comparisons in Europe (DICE)), national statistical offices, and government and non-governmental agencies (Australian Council of Trade Unions, New Zealand Planning Council, Japan Institute for Labor Policy and Training, among others) that contains major labor market reforms for the countries of our sample between 1970 and 2007. We consider three broad categories of reforms: (i) those weakening employment protection, (ii) those reducing non-employment benefits, and (iii) those decentralizing the wage bargaining process. Reforms occur in time waves and we consider as many waves as possible for each of the reform categories to control for time effects. We cluster countries into two groups and contrast the macroeconomic performance of reformers and non-reformers using a difference-in-difference approach.

The dynamic evidence of reforms is strong and persuasive. Labor market institutions reforms affect crucially cyclical fluctuations. Reforms reducing employment protection make employment more volatile and decrease significantly the correlation of the real wage with labor productivity. Reforms reducing replacement rates increase the correlation of output with labor productivity. Reforms that render wage bargaining more flexible have positive effects on the volatility of unemployment and increase the correlation of real wages with labor productivity. In addition, for many macroeconomic variables, changes in cyclical volatility occur simply as a result of reversion to the mean. The fact that reforms are more likely to occur during times of macroeconomic turbulence is consistent with the positive correlation we observe between LMIs and business cycle volatility in the first part of our analysis.

The facts we collect have important implications for both business cycle models and policy making activities. On the one hand, to understand how the economy works, it is important to model flow and wage restrictions. Loose bargaining increases the volatility of unemployment and the correlation between the real wage and labor productivity, inducing a smaller wedge between real wages and labor productivity. Reforms on EPL seem to have the reverse effect on the correlation of the real wage with labor productivity and have significant effects on the volatility of employment. On the other hand, LMIs should not be taken as exogenous as their dynamics may be related to the dynamics of macro variables and the probability that institutional changes take place may be related to macroeconomic turbulence.

The literature investigating the relationship between institutions and labor market performance is vast.[1] A few studies, including Nunziata and Bowdler (2005), Merkl and Schmitz (2011), Fon-

[1]See, for instance, Layard, Nickell and Jackman (1991) and Nickell and Layard (1999), Blanchard and Wolfers

seca et al. (2010), Rumler and Scharler (2009), and Abbritti and Weber (2010) have analyzed the effects of certain labor market arrangements on inflation dynamics or output and unemployment volatility and on international business cycle correlations.[2] However, as far as we know, no study has yet systematically investigated the impact of labor market institutions on cyclical fluctuations, nor how a variety of labor market reforms affect the evolution of business cycles using a wide range of cyclical statistics.

The rest of the paper is organized as follows. The next section presents the data. Section 3 reports the results of Spearman rank correlations, while Section 4 focuses on the effects of labor market reforms. Section 5 discusses the implications of our work for business cycle models and for policy-making activities. Section 6 concludes.

2 The Data

2.1 Business cycle data

We use quarterly data from the OECD and the International Financial Statistics (IFS) of the IMF. The time span is 1971:1-2007:4 for most countries unless otherwise indicated. We consider nineteen OECD countries: Australia, Austria, Belgium (1981:1-2007:4), Canada, Denmark (1978:1-2007:4), Finland, France, Ireland (1973:1-2007:4), Italy (1972:1-2007:4), Japan, the Netherlands (1978:1-2007:4), New Zealand, Norway (1976:1-2007:4), Portugal, Spain (1975:1-2007:4), Sweden (1981:1-2007:4), Switzerland, the United Kingdom, and the United States. In the investigation we use time series for gross domestic product, employment, unemployment, real wages, labor productivity and inflation. Time series for real wages in Portugal and in Switzerland are unavailable for a consistent sample and are excluded from the analysis. GDP is measured in constant 2000 prices, employment measures total full and part time employment in thousands, while the unemployment rate measures average yearly rates. For real wages we use both relative unit labor costs adjusted for the real exchange rate, or nominal hourly earnings divided by a price deflator. Labor productivity is computed as the ratio between output and total employment and the CPI is used to construct the inflation series.

We summarize cyclical information using volatility and correlation measures. We compute the volatility of detrended real GDP per capita, y, of employment per capita, n, of real wages, w, of labor productivity, y/n, of annualized unemployment and inflation rates, u and π. We also compute the correlation of GDP per capita with employment and with labor productivity and the correlation of labor productivity with employment and the real wage.

We measure cyclical statistics in a number of ways. In the literature, it is common to filter out long and short frequency fluctuations and concentrate on fluctuations which, on average, last between 2 to 6 years. When a cross country point of view is taken, however, one has to worry about the fact that cycles may have different lengths in different countries, or that trends may not be common. For that reason, in cross-country studies, it is more typical to compute statistics using growth rates, or scaling variables by appropriate averages. As a benchmark, we compute cyclical statistics by differencing the log of the raw data (using a 4-quarter lag) and check the robustness

(2000), Nunziata (2003), Nickell, Nunziata and Ochel (2005) and Costain and Reiter (2008).

[2]Canova et al. (2012) and Altug et al. (2012) investigate the impact of monetary and fiscal institutions on cyclical fluctuations in a variety of developed and developing economies.

of our conclusions by filtering the data with the Hodrick-Prescott (HP) filter and Band-Pass (BP) filter. Besides spurious trend effects, one has also to worry about the presence of measurement error. As long as measurement error is uncorrelated with labor market rigidities, no systematic bias should emerge. However, measurement error may artificially increase the volatility of macro variables and reduce the power of our analysis. While there is little in principle one can do to eliminate this problem, comparing alternative de-trending procedures should help to quantify the importance of measurement error.[3]

2.2 Labor market institutions data

We consider 13 labor market indicators: (i) EPL, the strictness of employment protection legislation; (ii) EPR, employment protection on permanent contracts; (iii) EPT, employment protection on temporary contracts; (iv) RR, the replacement rate, defined as the ratio of disposable income when unemployed to expected disposable income, if beginning to work during the first year of unemployment; (v) UD, union density, measured as the percentage of workers affiliated to a union; (vi) UC, union coverage, measured as the percentage of contracts negotiated by unions; (vii) WCOORD, the degree of coordination in the bargaining process, both on workers' and firms' side, measured on a 0-5 scale; (viii) LEVEL, the degree of bargaining centralization, measured on a 0-5 scale, which captures the predominant level where bargaining takes place (e.g. firm level, industry level, and nation wide); (ix) GOVINT, government involvement in wage bargaining, measured on a 0-5 scale; (x) EXT, the extent to which collective agreements are applied to non-unionized workers, measured on a 0-2 scale, where 0 indicates a collective agreement applied only to union members and 2 indicates more than 10 percent of non-unionized workers; (xi) MINWAGE, the degree of government intervention in setting the minimum wage, measured on a 0-8 scale; (xii) CONC, average concentration measure (Herfindahl Index) of unionization at national and sectorial level; (xiii) CENT, which measures concentration as CONC, but weights differently the national and the sectorial level according to their importance. Higher values for these indicators imply more rigid labor markets.

Most of these indicators are quite standard and do not require a more detailed discussion. Coordination refers to the degree to which minor bargaining units follow the decision of major players, where major players may include union confederations (Norway, Netherlands, and Italy), leading unions and its employer counterpart (such as IG Metall in Germany) or confederations of large firms (as in Japan). Indices of coordination take into account the presence of coordinating activity by the major players. Examples of those activities are state-sponsored or state-imposed coordination.

We have chosen those indices since they account for rigidities that can affect both quantities and price adjustments. Employment protection is typically regarded as an important determinant of the incentives driving job creation and job destruction and, as a consequence, of labor market adjustments. Replacement rates and the presence of a minimum wage may have a direct impact both on the dynamics of the real wage by affecting workers' outside option, and on labor market flows, by affecting firms' incentives to post vacancies. Indices of density, coverage, coordination, centralization, and concentration affect unions' power and may affect real wage adjustments.

[3] In fact, while HP filtering leaves the importance of high frequency measurement errors unchanged, taking growth rates magnifies their importance, and BP filtering completely eliminates high frequency measurement errors.

The sources and definitions of LMI data are in Table 1. Most of these indicators come from the CEP-OECD Institutions Data Set (see Nickell (2006)) and the ICTWSS Database, which mostly provides information about the main characteristics of wage bargaining systems. The employment protection legislation series and replacement ratios are constructed by the OECD.

3 Evidence using Spearman Rank Correlation coefficients

We first document the correlations between business cycle statistics and some key labor market characteristics by exploiting variations across countries and decades. We depart from the existing literature (see, e.g., Rumler and Scharler (2011), Abbritti and Weber (2010), and Costain and Reiter (2008)) in several respects. We look at a larger number of cyclical statistics and we include a broader set of institutional indices. We allow for interactions of labor market indicators, which is important since several authors have suggested that institutions cannot be studied in isolation from each other and their interaction is crucial for determining macroeconomic outcomes (see, e.g., Du Caju et al. (2010) and Fabiani et al. (2010)). We control for a number of macroeconomic factors which may affect the relationship. Finally, we compute rank correlations to allow for potential non-linear effects.

3.1 Accounting for macroeconomic indicators

To control for factors that might affect business cycle characteristics but that are not of direct interest in the investigation we use a number of macroeconomic variables: (a) openness, measured as the sum of exports and imports as a percentage of GDP; (b) current account balance to GDP; and (c) the size of the government measured as the share of total government spending to GDP. We control for country characteristics such as the average inflation and unemployment rates and real GDP per capita; the type of monetary policy regime adopted (with a dummy for countries that are inflation targeters) and for the association with the European Monetary Union (with an EMU dummy), since a priori these characteristics could matter for how business cycles have developed.[4] We summarize the information contained in the macroeconomic indicators by computing their first principal component which we label "the macroeconomic factor." Table 2 displays the correlation of the macroeconomic factor with the original series.[5] As the table makes clear, the macroeconomic factor is strongly correlated with GDP per capita, openness, the current account, government spending, and average inflation. The correlation of the macroeconomic factor with business cycle statistics is high since all macroeconomic variables could be important determinants of business cycle fluctuations. For example, greater openness may reduce the severity of domestic cycles by providing more risk sharing opportunities, but it may also expose the country to international fluctuations. Countries with a larger government size may also be able to mitigate the cyclical impact

[4]Data on government spending comes from the Annual Macro-Economic database (AMECO) of the European Commission's DG ECFIN; data for all other macroeconomic indicators comes from the OECD and the International Finance Statistics (IFS) database of the IMF. We have used alternative sets of macroeconomic indicators, for example, we have included a G-7 dummy, or a human development index, and replaced the government spending with debt-to-GDP ratio, results are very similar and available upon request.

[5]We normalize all macroeconomic indicators to have positive factor loadings.

of technology shocks. For that reason, controlling for macroeconomic characteristics is important to avoid a spurious relationship between business cycles and LMIs.

3.2 Collapsing the information

We exploit the time series dimension of the data by considering decadal sub-samples. We have four decades of data (1970s, 1980s, 1990s, 2000s) and 19 countries, giving us a total of 76 observations. Controlling for 13 LMI indicators and 8 macroeconomic indicators would make the number of degrees of freedom prohibitively small. For this reason, we collapse the information contained in the institutional and macroeconomic variables into interpretable factors. Using factors, rather than the original variables saves degrees of freedom and allows us to take into account possible combinations of institutions, reducing the potential bias due to omitted variables. For the institutional data we select as many factors as needed to capture always at least 75 percent of the variability of the original data. With this criterion we find that four principal components describe fairly well the sample of indicators.[6] To control for potential endogeneity of LMIs we construct principal components using the value of institutions measured at the beginning of the decade. The business cycle statistics and macroeconomic control factors, instead, capture decade averages.

Table 3 displays the correlation of the four "statistical" factors with the original variables. The first factor, explaining 40% of the variance, is highly correlated with all labor market indicators, thus it can be interpreted as an index of overall rigidity. The second factor, explaining 18% of the variance, is negatively correlated with the employment protection indices and highly and positively correlated with union density, concentration, and centralization and the minimum wage index. The third and fourth factors, respectively, explain 9% and 7% percent of the variance and represent a mix of developments across labor market indicators.

3.3 Results

3.3.1 Statistical factors

Table 4 presents Spearman partial rank correlations between the business cycle statistics and alternative LMI statistical factors, when we condition on macroeconomic factors. These correlations, thus, slice the multivariate distribution to take into account macroeconomic conditions. Note also, that since rank correlations are computed, we allow for a non-linear relationship between business cycle statistics and LMIs.[7]

Formally, let S_i denote the statistical factor i and let B_j denote the business cycle statistic j, while M is the principal component of the macroeconomic variables. Then the Spearman's partial rank correlation coefficient can be expressed as:

$$r_{S_i,B_j|M} = \frac{r_{S_i,B_j} - (r_{B_j,M})(r_{M,S_i})}{\left[(1 - r_{B_j,M})^2(1 - r_{M,S_i})^2\right]^{\frac{1}{2}}}$$

[6] The principal components are extracted from non-orthogonal and standarized data.

[7] Spearman's correlation (unlike Pearson's correlation) is successful in small samples like ours, since normality is violated, and is robust to outliers. Moreover, since we can apply the method conditioning on more than one series, we have also investigated whether extracting more components from the macroeconomic variables, and conditioning on more macroeconomic factors as well as decade dummies would change the results. The results we present are robust to these changes.

where $r_{S_iB_j}$ is the Spearman correlation coefficient between statistical factor i and business cycle statistic j. Table 4 reports the p-values associated with testing the hypothesis whether an observed value of $r_{S_iB_j|M}$ is significantly different from zero using a small sample Wald-Mann test, when business cycle statistics are computed using log-differenced data. In Figure 1 we present partial rank correlations when we use different methods to detrend the data. In particular, we detrend the data with either HP (for $\lambda = 1600$) or BP filters (keeping cyclical movements of 6-32 quarters).

Independently of the filters used to construct the cycle, the overall index of rigidity is associated with higher volatility of the business cycle, evidenced by a positive and significant correlation with the variance of output, unemployment, real wages, labor productivity, and inflation. It is also associated with a lower correlation between the real wage and labor productivity, but the significance of this relation is not robust across detrending methods (see Figure 1).[8] The second statistical factor has a similar relationship with macroeconomic volatility (except for inflation's, which is insignificant) and it is significantly negatively related to the correlation between output and employment, but again this result is not robust across detrending methods (see Figure 1). Overall, the analysis of statistical factors suggests that labor market rigidities are significantly positively correlated with macroeconomic volatility.

3.3.2 Economic factors

Given the difficulties in interpreting the informational content of factors extracted with principal component analysis, we next identify factors using economic intuition. We consider separately the effect of the RR index and renamed it economic factor 1 (henceforth EF1); we have extracted a common factor from the indicators that characterize the strength of unions (EF2) namely UD, UC, CONC, and CENT; another factor from indicators such as COORD, GOVINT, LEVEL, EXT, and MIN-WAGE, characterizing wage bargaining (EF3) and one factor from the series of indicators of employment protection (EF4), EPL, EPR, and EPT. Table 5 presents the correlation of the economic factors with the original indices.

Table 6 presents Spearman partial rank correlations between the business cycle statistics and the economic factors when business cycle statistics are computed using the growth rates of the data, and in Figure 2 we plot partial rank correlation coefficients between economic factors and business cycle statistics for the alternative filtering methods.

A higher replacement rate (RR) is associated with more volatile unemployment and weaker correlation between output and productivity (for two out of the three filtering methods). Stronger unions (EF2) are associated with a significantly higher volatility of output, real wages, labor productivity, and inflation and a lower correlation between output and employment. Similarly, more wage bargaining (EF3) is also associated with significantly higher volatility of output and real wages, but a lower correlation between wages and labor productivity (this effect is, however, not robust across filters). Employment protection (EF4) is associated with higher volatility of unemployment and lower correlation between real wages and labor productivity (when we use growth

[8] In results we do not present here to economize space, we also consider, as an alternative to the overall index of rigidity (SF1), a simple average of all LMIs and obtain similar results. In addition to Spearman rank correlations, we consider a pooled regression specification that regresses business cycle statistics on SF1 and the macroeconomic factor as control variable. Overall rigidity of labor market institutions (SF1) relates significantly with higher macroeconomic volatility (for unemployment and real wages) and a lower correlation of real wages with labor productivity.

rates to detrend the data).[9] We conclude that, overall, there is strong evidence that LMIs relate to the business cycle. Labor market rigidities are associated with higher macroeconomic volatility and a lower correlation of output with employment and real wages with labor productivity.[10]

3.3.3 Considering LMIs separately

Finally, we have also tried to examine the correlation of business cycle statistics with one LMI at the time. Results are presented in Table 7 and confirm the previous evidence. The volatility of output, real wages, labor productivity, and inflation are the statistics that are most affected by institutional indicators. Replacement rates and employment protection comove positively with the volatility of unemployment. Several union and wage bargaining indicators relate negatively to the correlation of output with employment (as does EPL).

We observe that many LMIs turn out to be significantly related with business cycle statistics, such as output and unemployment volatility, in line with the results of previous studies. We also find that strong unionization (UD) relates positively with output volatility, as in Rumler and Scharler (2011), and in addition that this holds for coordinated wage bargaining (WCOORD). Also, in accordance with Abbritti and Weber (2010), we find that real wage rigidities (WCOORD, GOVINT, LEVEL, MIN-WAGE, among others) are associated with amplified business cycle fluctuations for more macroeconomic variables other than output, which they consider in their analysis.

As stressed by Du Caju et al. (2010) and Fabiani et al. (2010), the interactions of labor market institutions in forming business cycle outcomes are crucial. While results using individual LMIs do not account for such interactions or combinations, we observe that results are in general similar to those obtained using statistical and economic factors. Labor market rigidities correlate positively with macroeconomic volatility and negatively with correlations of output with employment and the real wage with labor productivity.

4 Labor Market Reforms and Business Cycles

Cross-country analysis is intuitively appealing but it may give a distorted picture of the relationship between LMIs and business cycles if time variations change the cross sectional orderings or if omitted variable problems are present. In the previous section we have taken into account the panel dimension of the data. However, the analysis abstracted from country fixed effects. Panel regressions yield virtually no significant results because of limited variation in the LMIs over time.[11]

[9] In addition to Spearman rank correlations, we consider a pooled regression specification which regresses business cycle statistics on the economic factors one at a time, and on the macroeconomic factor as control variable. The replacement rate (RR) continues to be associated with higher volatility of unemployment as well as a lower correlation of output with labor productivity. Stronger labor unions (EF2) continue to be associated with higher volatility of real wages and labor productivity, as well as lower correlation between output and employment. More wage bargaining (EF3) and employment protection (EF4) both continue to reduce the correlation of real wages with labor productivity. Finally, EF2 and EF3 also increase macroeconomic volatility for several other variables. Results are available from the authors upon request.

[10] These latter results depend on the data filtering and the way we construct factors.

[11] Pooled regressions, on the other hand, exploit more cross-country variations in LMIs and yield more significant results that are available from the authors upon request.

To potentially look at the data from a different perspective we consider labor market reforms. We define reforms as a "treatment" administered to some countries but not others, and estimate the causal effect of the treatment using a difference-in-difference estimator. This methodology allows us to exploit both the cross sectional and time series variation in the data. The question of interest that we investigate in this section is whether labor market reforms change the structure of cyclical fluctuations.

4.1 Labor Market Reform Data

Following Boeri and Garibaldi (2009), we gather information about major labor market reforms for the countries in our sample using the information provided in the "Social Policy Reform Inventory" assembled by the Fondazione Rodolfo DeBenedetti (www.frdb.org), which covers European countries from 1970 until 2009. This dataset draws on a variety of sources (including country economic reviews by the OECD, Income Data Source studies, EC-MISSOC reports, etc.) and gives information about the nature of the reforms carried out in Europe in the field of non-employment benefits and employment protection. It reports the date the bill was passed, a detailed description of the law and the scope of the reform, i.e. whether the law marginally affected the system already in place, or whether the law produced a structural change in the regulatory environment (marginal vs. structural reform). Moreover, the bill is qualified as two-tier, if it is targeting a particular segment of the labor market, such as the young unemployed or temporary workers, or complete, if it affects the whole labor force. We restrict attention to structural and complete reforms and in the sample there have been 25 of them. However, some reforms simply undid previous ones. In Table 8 we present the information concerning these reforms.

Since the "Social Policy Reform Inventory" does not include data for the non-European countries, we complement it with information from the OECD, from the DICE, and from national sources. For example, for Australia, we have used information from ACTU (Australian Council of Trade Unions) and the Australian Bureau of Statistics; for New Zealand we have used the information provided by the New Zealand Planning Council; for Japan we have used information coming from the Japan Institute for Labour Policy and Training and Neil et al. (2010); and finally, for the US we obtain information from various OECD Outlooks.

No formal data is available on reforms that change the wage bargaining structure for the countries we examine. To capture reforms that affect the wage bargaining process we used information contained in the time evolution of the LMI indices regarding the degree of coordination in the bargaining process, government involvement in wage bargaining, and the extent to which collective agreements are applied to non-unionized workers. We confirm the presence of reforms on these aspects of the labor market by comparing the dates suggested by the LMI indices with dates appearing in official data from national resources.[12]

[12]We also considered reforms that change the degree of union power, captured by changes in the indices of union density, coverage, and the average concentration measure. However, we could not conduct a difference-in-difference analysis based on such reforms, since when we controlled for other institutional reforms in the sample, we ended up with too few treatment and control groups.

4.1.1 Employment protection reforms

Table 8a lists the reforms on employment protection provided in the "Social Policy Reform Inventory". Three types of reforms are included: (a) reforms that remove or ease the costs of dismissal, (b) reforms that shorten the notice period, and (c) reforms that relax restrictions for fixed term contracts. Reforms appear to have happened in different waves: some in the middle of the 80s and the beginning of the 90s and the rest in the beginning of the 2000s. We pool reforms occurring approximately at the same date to control for time effects.

The resulting treatment and control groups are presented in the first column of Table 9. According to the Social Policy Reform Inventory and our own readings there were no significant employment protection legislation reforms in Australia, Canada, Ireland, New Zealand, Norway, Switzerland, and the US. However, some of these countries have experienced significant reforms in other labor market institutions. For that reason we restrict the control group of countries to Norway and the US. Austria, Finland, and Spain have performed reforms that eased dismissal; Finland, Spain, Japan, and Sweden had reforms that encouraged the creation of part-time contracts; Italy and Finland had undertaken reforms that reduced the notice period. Our goal is to measure whether cyclical statistics in the countries undertaking major reforms are different from those of countries where no significant reforms have taken place. Since we want to control for all subsequent institutional reforms in the sample we exclude Finland, France, and Japan from the treatment group. This leaves us with only six data points in the case of reforms of the first wave and five data points for the second wave of reforms and very small samples to summarize business cycle statistics for the second wave. For that reason, we exclude the second wave from the analysis. Finally, the employment protection reforms examined are pretty homogeneous. Apart from Sweden's, reforms involve changes that mainly decrease the costs of dismissals. We have investigated whether results depend crucially on the exclusion of Sweden and the main findings of this section are robust to this change.

4.1.2 Non-employment benefit reforms

For non-employment benefits there have been two waves of reforms, one at the beginning of the 90s and one at the beginning of the 2000s. The reforms reported in Table 8b affected different aspects of benefits and can be divided in three categories: (a) reforms that tighten the eligibility criteria for unemployment benefits, (b) reforms that reduced unemployment benefits, and (c) reforms that enforced the duty of the unemployed to actively look for a job.

To control for time effects we consider two waves of reforms. The second column of Table 9 presents the treatment and control groups for those reforms. Canada, France, and Spain in the 1990s have raised the contribution rates required to be eligible for benefits, while Austria and New Zealand have reduced unemployment benefits during the same time period. In addition, Belgium, Spain, Denmark, Finland, and Sweden have enforced the duty to actively look for a job for the unemployed at the beginning of the 2000s. However, given that Finland, Sweden, and Austria have enacted other reforms at the same time period we decided to omit the second wave of reforms from the analysis. Hence, the reforms we consider are quite homogeneous and adopt either stricter eligibility criteria or lower unemployment benefits.

4.1.3 Wage bargaining reforms

In Table 8c we summarize the information regarding reforms that affected the wage bargaining process. Countries like Australia, New Zealand, and Switzerland seem to have been repeatedly subject to significant reforms weakening the wage bargaining centralization. Again, we categorize reforms in two waves, one occurring in the 1980s and another in the mid 90s.

The last two columns of Table 9 present treatment and control groups for this last group of reforms considered. Since only minor reforms weakening the influence of unions in wage negotiations had taken place in the US in the beginning of the 1970s, we use the US cyclical data after 1973 to control for those changes. Reforms are rather homogeneous also in this case. The 1980s wave of reforms concerns falls in wage coordination and the 1990s wave of reforms involves falls in wage coordination as well as more decentralization in wage bargaining.

4.2 Measuring the impact of labor market reforms

The crucial identifying assumption in the estimation is that there is no unobserved variable correlated with business cycles that moves systematically over time differently between the treated and control groups. A violation of this assumption is more likely if the treated and control countries are very different from each other, because in this case any omitted time-varying variable, such as openness, the government size, or the exchange rate and monetary policy regime, could affect the two groups of countries in very different ways. Since all the countries in our sample are developed economies, we argue that they are relatively homogeneous and this is less of a concern. In Table 10 we present the means and standard errors for the control and treatment groups for the human development index (HD), the government size $\frac{G}{Y}$, real GDP per capita Y, the unemployment rate u, the inflation rate π, the degree of openness $\frac{(X+M)}{Y}$, and the current account to GDP ratio $\frac{CA}{Y}$, between 1995 and 2007.[13] Clearly, the hypothesis that the treated and the control groups are equal cannot be rejected for any pair and any characteristic we examine at the 5% level of confidence.

The identifying assumption could also be violated if reforms are not random and whatever triggers the reform also has a causal effect on business cycles; for instance, labor markets reforms might be systematically enacted during deep recessions, since it is easier to convince the public of the necessity of such reforms in bad times. As is standard, we control for potential endogeneity problems by including the initial condition as a regressor.

Implementing our estimation strategy requires care on a few other issues. First, some reforms take place very close to the end of the sample. Because the dependent variable measuring post-reform business cycle statistics spans quite a short time series we do not study the effects of such reforms. Second, an additional problem stems from the observation that labor market reforms may not have been independent from each other. As a result, if two reforms are undertaken simultaneously, the omission of one treatment may lead to bias in the estimated effect of the included one. For that reason we exclude from the sample countries that have enacted a number of labor market reforms simultaneously.

We proceed as follows: first, after identifying the dates when changes have taken place for each country, we divide countries in two groups. In the treatment group we include countries where labor market reforms have been passed. We classify as belonging to the control group all

[13] Results for earlier averages are similar but we have less time series available to control for different characteristics.

countries for which we have data and that did not implement any reform. Second, the sample is split into two sub-samples, the pre-reform period and the post-reform period, and we examine whether the relation between LMIs and business cycles have changed in reformers relative to non-reformers. For the non-reformers the pre and post reform samples are split using the average date of reforms in the treated group. So, for example, for EPL, reforms have taken place in Sweden in 1993:4, and in Finland, Italy, and Portugal in 1990:4, so that for the control group the pre-reform sample finishes in 1991:4 and the post reform samples starts in 1992:1. When more than one wave of reforms exists, we repeat the procedure for each wave in such a way that the post-reform period of a particular regulatory change does not overlap with the post-reform period of the subsequent one. For example, in the case of employment protection, reforms took place in Spain both in 1984 and 2002. In 1984 there have been reforms that encouraged part-time work, while the 2002 reform eased dismissal. To avoid overlapping samples, when testing the 2002 change we exclude the period before 1984. To allow for delays in the effects of reforms, we leave one year out of the post-reform sample. We select the post-reform periods for countries in the control group using the mean start date for the countries in the treatment group. The empirical model is:

$$X_{i,post} - X_{i,pre} = \alpha_0 + \alpha_1 D_i + \alpha_2 X_{i,pre} + \epsilon_{i,t} \tag{1}$$

where $X_{i,post}$ is the value of the cyclical statistic after the reform in a given country i, $X_{i,pre}$ is its value prior to the reform and D_i is a dummy variable equal to one only if country i has passed a reform. The inclusion of the initial condition in the model is necessary to obtain unbiased estimates of α_1. The intuition for why this is necessary is straightforward. In the analysis we are implicitly assuming that $X_{i,t}$ depends on a country effect, μ_i, a time period effect, η_t and a labor market reforms effect captured by a dummy $Q_{i,t}$ which takes the value of one if the reform under consideration has been passed and is in place in country i at time t.

$$X_{i,t} = \kappa + \beta Q_{i,t} + \mu_i + \eta_t + \nu_{i,t} \tag{2}$$

Defining $Q_{i,post} - Q_{i,pre} = D_i$, time differencing equation (2) and estimating it with OLS, it is easy to check that β is unbiased if the dummy is not correlated with the residual. However, the dummy may correlate with $X_{i,pre}$. For instance, countries with high standard deviation of unemployment could introduce reforms to reduce such volatility. If this were the case, the dummy would be negatively correlated with the error, thus inducing a downward bias in the OLS estimator; i.e., employment protection legislation would appear to reduce the volatility of unemployment even when the causal flow is going in the opposite direction. However, since the correlation works only through $X_{i,pre}$, controlling for the initial condition eliminates the bias.

Since the sample size available to estimate the coefficients in equation (1) is typically small, we report estimates and standard errors when we bootstrap the residuals of the equation, taking into account potential heteroskedasticity and serial correlation. We run 1,000 repetitions and report the mean value together with an estimate of dispersion of the estimated distribution.[14] The reader must remember to interpret reforms as decreases in labor market rigidities, thus expecting to find opposite signs when compared to the correlations obtained in section 3.

[14]To study the extent of small sample biases, we have also investigated how the results may change if we increase the number of countries in the treatment and control groups. To do this, we have constructed groups selecting control and treatment groups based on a specific labor market reform independently from other labor market changes. So, for example, when we consider the employment protection reform, instead of considering only the US and Norway as control group, we also add Australia, Canada, New Zealand, and Switzerland, since none of these countries has experienced an EPL reform. Similarly we add Austria, Japan, France, and Spain in the treatment group since those

4.3 Results

4.3.1 Employment protection reforms

Table 11 presents the estimated values and standard errors for parameters α_1 and α_2 for reforms that reduce employment protection and Figure 3 plots estimates for these parameters for all the methods we have used to detrend the data. Countries that have passed such reforms display a significant increase in the volatility of employment relative to countries that have not introduced the reforms during the mid 80s and the beginning of the 90s. In addition, in the countries where such reforms were passed, the correlation of the real wage with labor productivity has fallen significantly.[15] All other statistics have not been significantly affected by the introduction of EPL reforms (apart from the correlation of output and employment when we detrend the data with the HP filter and the correlation of employment with labor productivity when we log difference the data).

Given that we do not have an explicit theoretical framework, it is difficult to interpret these results. Some New Keynesian models with search and matching frictions or hiring costs (see, e.g., Abbritti et al. (2006) and Christoffel and Linzert (2005)) predict that changes in the separation rate induced by EPL reforms tend to increase macroeconomic volatility and reduce the volatility of inflation. In the data, EPL reforms seem to affect the volatility of employment and the correlation between real wages and labor productivity. However, these reforms have no statistically significant effect on the volatility of output nor inflation, as existing models would predict. This counterintuitive finding might be explained by the fact that workers change their effort or hours worked after the implementation of such reforms. Reducing the effort variability could explain both the lower correlation between real wages and labor productivity as well as the insignificant effect of the reforms on the volatility of output and inflation. Existing theoretical models do not incorporate variable effort. We conjecture that incorporating variable effort could help standard NK models with labor market imperfections capture the macroeconomic effects of labor market institutions.

4.3.2 Non-employment benefits reforms

Table 12 presents the estimated values of α_1 and α_2 for reforms that decreased non-employment benefits. Bootstrap standard errors are reported in parenthesis, while Figure 4 plots the values of estimated coefficients across different detrending methods.

Countries that tightened the eligibility conditions for unemployment benefits experienced a strong and significant increase in the correlation of output with labor productivity, irrespective of the filter we use to detrend the data (consistent with the results obtained in section 3). Earlier evidence (see, e.g., Shea (1990)) suggests that labor effort, measured as accident rates at work, is pro-cyclical. Our data suggests that workers, faced with a higher probability of not receiving

countries did experience EPL reforms. We have also tried to classify reforms according to the type of index they affected the most into three categories: (a) reforms that reduced the WCOORD index occurring in Australia in 1996, Austria in 1983, Japan in 1997, New Zealand in 1987, Ireland in 1987, Denmark in 1987, Switzerland in 1996, and the UK in 1980, (b) reforms that reduced the GOVINT index occurring in Australia in 1992, New Zealand in 1985, Spain in 1986, and the UK in 1980, and (c) reforms that reduced the extension of wage contracts in Austria in 1982, in New Zealand in 1987, in Switzerland in 1981, and in the UK in 1983. None of the conclusions we have reached is affected by those changes.

[15] We note that while the first result is not at odds with those found in section 3 (the correlation of EF4 and employment volatility has the same sign, although insignificant), the latter finding is when we look at growth rates.

benefits upon entering an unemployment spell, might have the incentive to vary effort over the cycle to a greater extent, making measured productivity more pro-cyclical.[16]

Three additional moments seem to be affected by this class of reforms: the volatility of labor productivity, output, and unemployment. As far as the first two moments are concerned, in line with the previous result, we again find some indirect evidence suggesting that workers are more willing to vary their effort over the cycle in countries with less generous unemployment benefits. Finally, the negative relation between replacement rates and the volatility of unemployment contrasts with the result by Hagedorn and Manovskii (2008). Although our evidence is in line with other empirical contributions, such as Faccini and Rosazza Bondibene (2012), the increase in volatilities is not robust to alternative detrending methods – it does not hold when we use growth rates to estimate (1). Hence, we remain cautious in interpreting this second set of results and we do not necessarily read our data as a rejection of the theoretical mechanism proposed by Hagedorn and Manovskii (2008).[17]

4.3.3 Wage bargaining reforms

In Table 13 and Figure 5 we present estimates for reforms concerning changes in the wage bargaining process. Such reforms increased the correlation of the real wage with labor productivity and the volatility of unemployment for both waves of reforms. The first finding is consistent with results we obtained from the Spearman correlation analysis. The wage bargaining reforms are targeting the "price" adjustment in the market. However, there seems to be no statistically significant effect of these reforms on real wages. A reform that decentralizes the wage bargaining process, making wages more flexible, might induce strong effects on labor force participation. For example, if the minimum wage is abolished more workers might enter the labor force if the market wage is high, decreasing the equilibrium wage, while if the market wage is low, agents might decide not to search for a job at all, decreasing their labor supply and reducing the pressure for wage changes in the market. Unfortunately, we do not have data on labor force participation to test this assumption. On the other hand, wage bargaining reforms increase unemployment volatility in both decades considered. This is in line with theoretical models (see Abbritti et al. (2012) and Christoffel and Linzert (2005)) that predict a negative relation between unemployment volatility and wage bargaining rigidities. In contrast with the existing theoretical models, reforms in wage bargaining surprisingly have no effect on the volatility of inflation. We infer that this is so because of the absence of any effects on the volatility of the real wage that in turn would affect marginal costs and inflation in equilibrium.

Results do not change dramatically going from the 1980s to the 1990s. The business cycle statistic that is affected significantly between the two decades is the correlation of output with employment. In the 1980s wage bargaining reforms increase significantly this correlation while in the 1990s this correlation is not affected by wage reforms. Again, this can be due to changes in

[16]There is an old literature that points to variations in effort as a source of pro-cyclicality in labor productivity. In addition to Shea (1990), earlier contributions include Fair (1969), Fay and Medoff (1985), Hall (1988), and Rotemberg and Summers (1990), among others. For a recent contribution reviving this literature, see for instance Gali and van Rens (2014).

[17]Trigari (2006) shows that lower unemployment benefits and workers' bargaining power generate more volatile responses in real wages and inflation after a monetary policy shock. Reforms to the non-employment benefits seem to affect neither the inflation nor the real wage volatility. Yet, our analysis is unconditional and does not necessarily contradict the theoretical predictions of Trigari (2006).

labor force participation. Womens' labor force participation increased significantly in the 1980s, while the increase is minor during the 1990s. Also, reforms in wage bargaining seem to increase significantly the procyclicality of labor productivity in the first wave of reforms, but have no significant effects on this correlation during the 1990s. Gali and Van Reens (2008) suggest that the decline in labor market turnover, by reducing hiring frictions, has eliminated the procyclicality of labor productivity. Their model incorporates hiring frictions, variable effort, and endogenous wage rigidities. Their model could explain our findings since in the 1980s hiring costs were high, employment could not adjust easily, and reforms that made the wage more adjustable could move the effort margin increasing the correlation of output with labor productivity without affecting employment. Instead in the 1990s the abolition of hiring costs implies more movements in the employment margin increasing the relative volatility of employment with respect to output. The increased volatility of employment in this decade could explain the absence of any correlation between wage bargaining reforms and the procyclicality of labor productivity.

5 Implications for Theoretical Models and Policy-making

Our analysis provides some insights that are of interest to both the empirical and the theoretical literature and allows to draw some conclusions that are relevant to policy-making.

First, the fact that more flexible LMIs are associated with lower macroeconomic volatility does not necessarily imply that reforms making labor markets more flexible are effective in dampening business cycle fluctuations. Spearman partial rank correlations suggest a negative relation between labor market rigidities and the volatility of output, unemployment, inflation, real wages, and labor productivity. However, we find much weaker effects of labor market reforms on macroeconomic volatility and, if anything, of opposite sign, as compared to correlations. For instance, if we restrict to the results that are robust across all filtering methods, weakening employment protection and the bargaining power of unions increases the volatility of employment and unemployment, respectively. One can conjecture that the correlation between LMIs and macroeconomic volatility could be attributed to the fact that reforms occur at times of economic turbulence. We observe indeed that reforms take place when the volatility of real wages, inflation, and unemployment is high.

Second, if the probability of institutional changes is related to macroeconomic variability, taking LMIs as given when modelling business cycles may be misleading, in particular when the goal of the exercise is to develop a theory about the effects of institutions on labor market dynamics.

In addition, our investigation has some interesting implications for macroeconomic models aiming at explaining labor market dynamics. In fact, it suggests that explicitly modelling effort might be important to understand business cycle fluctuations. For example, reforms that reduce employment protection reduce the correlation of the real wage with labor productivity and increase the volatility of employment, but we fail to find any significant effect on the volatility of the real wage, output, or inflation. These facts can be jointly explained by the endogenous response of effort. In the light of the early literature that considers variable effort as an important driver of labor productivity, we conjecture that augmenting business cycle models with this margin might be promising.

As far as labor market policies are concerned, our results indicate that it is important to model flow and wage restrictions. Reforms on flow and wage restrictions are not substitutes. The former affect employment and the latter mostly affect the volatility of real wages and unemployment.

Moreover, they have opposite effects on the correlation between wages and labor productivity. In fact, reforms to collective wage negotiation increase such correlation, while reforms to employment protection reduce it.

6 Conclusions

This paper analyzes whether and how different labor market rigidities affect the business cycle dynamics of 19 OECD countries. Our main conclusion is that labor market institutions matter. Evidence from Spearman rank correlations suggests more flexible LMIs lead to lower macroeconomic volatility and higher correlation of output with employment. Furthermore, labor market reforms have important macroeconomic consequences. The analysis of specific reform episodes indicates that differences in wage bargaining, in non-employment benefits, and in employment protection have significant impact on business cycles.

Our conclusions have relevant implications. Many authors have suggested that labor market rigidities can in theory explain in part the nature of macroeconomic fluctuations. However, the empirical evidence in this respect is still limited. This paper shows that labor markets institutions are in fact important for determining the dynamics of the real wage, employment, and unemployment and indicates wage bargaining and employment protection institutions as the most important for shaping cyclical fluctuations in the real world.

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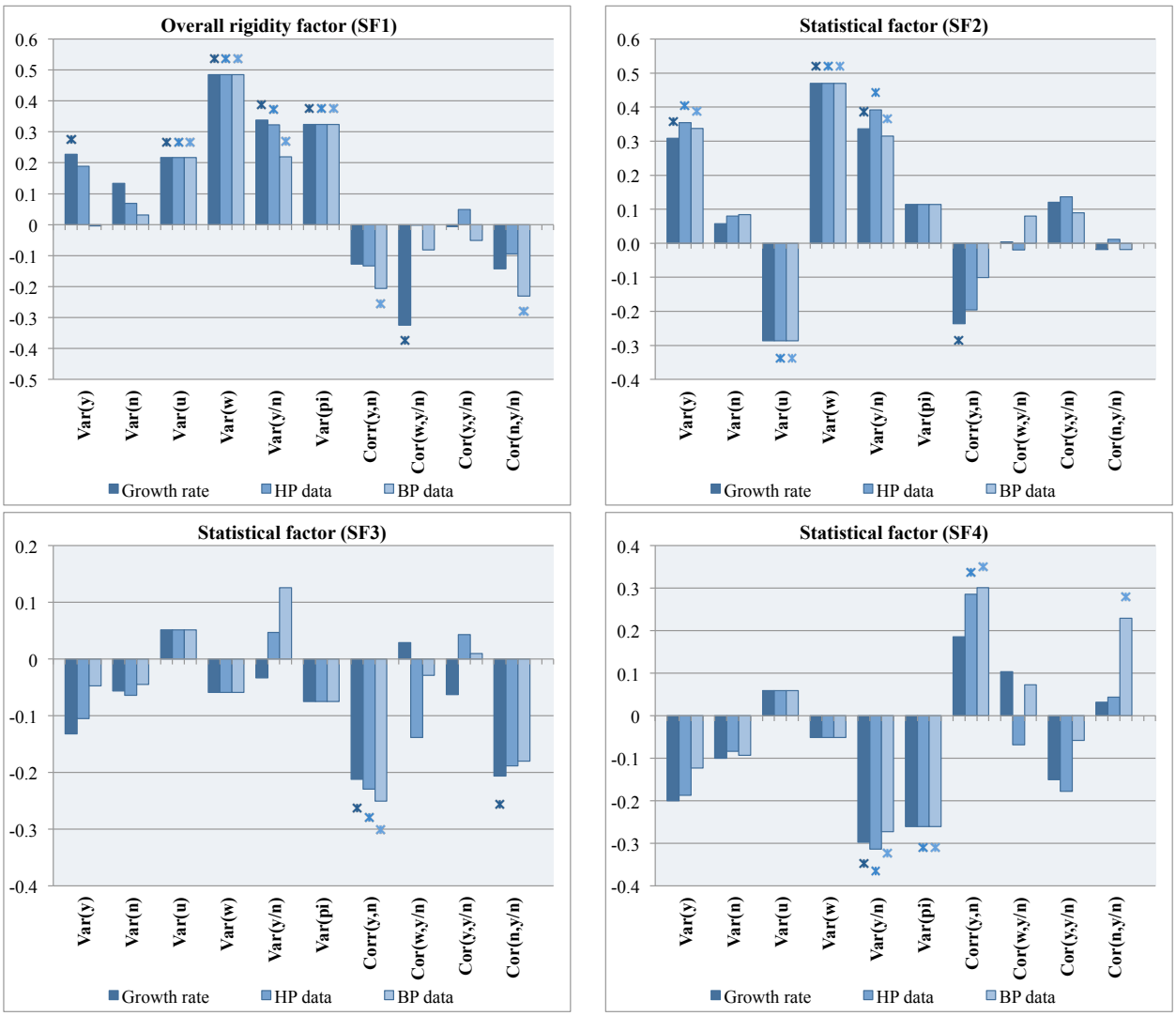
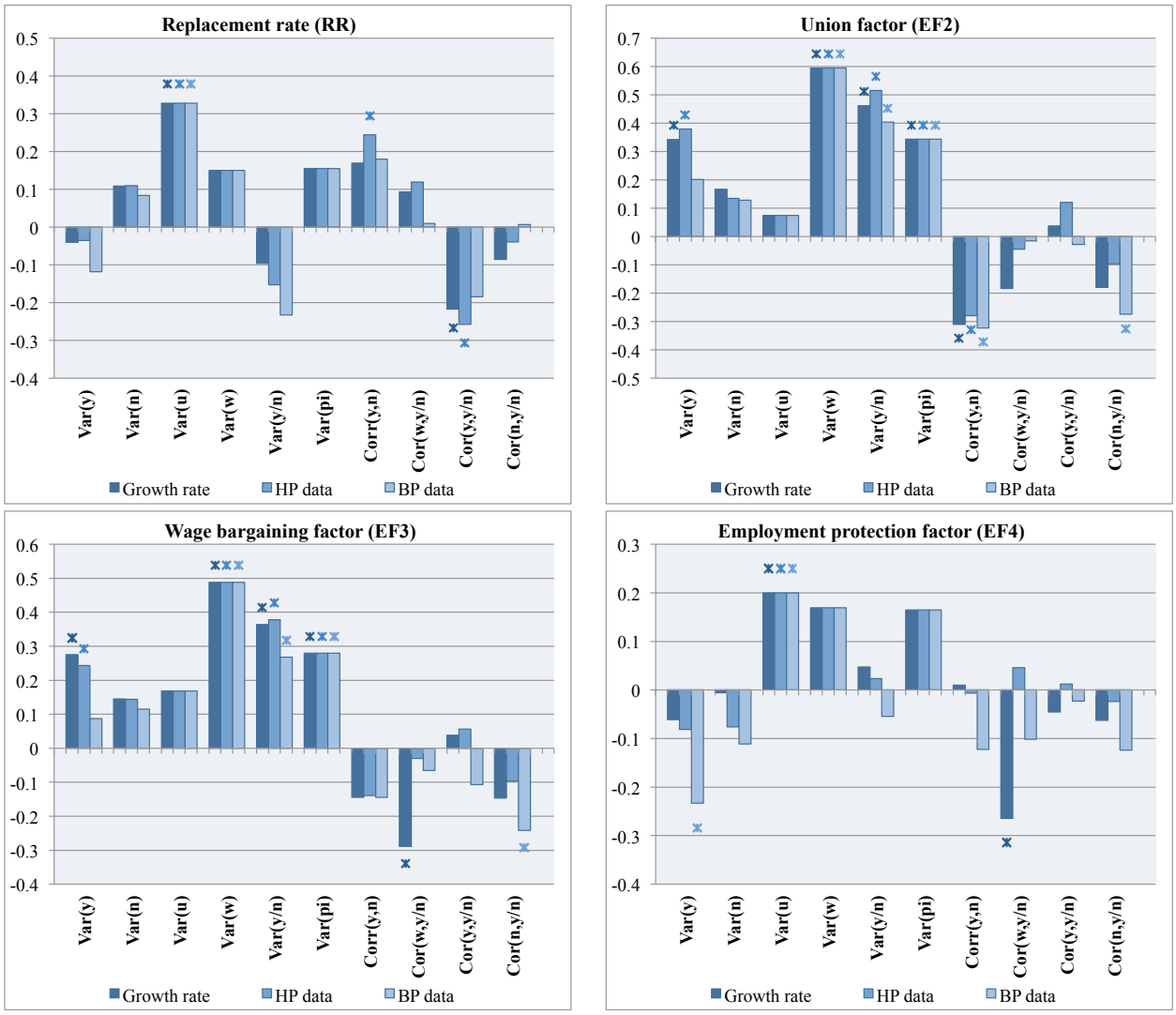
8 Tables and Figures

Figure 1: Spearman partial rank correlations between business cycle statistics and statistical factors, different detrending methods



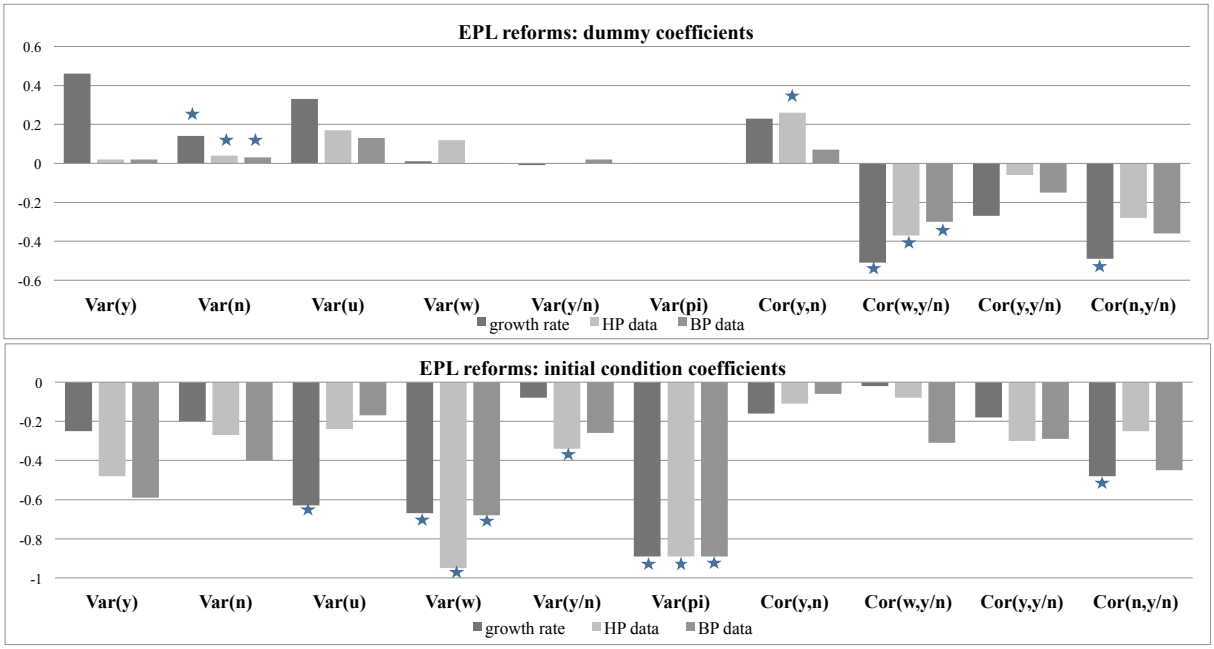
An * above/below a column indicates correlations that are significantly different from zero at the 10 percent level. Growth rate stands for log difference (4-period lag); HP data stands for Hodrick-Prescott filtered data; and BP data stands for data detrended using a Band Pass filter.

Figure 2: Spearman partial rank correlations between business cycle statistics and economic factors, different detrending methods



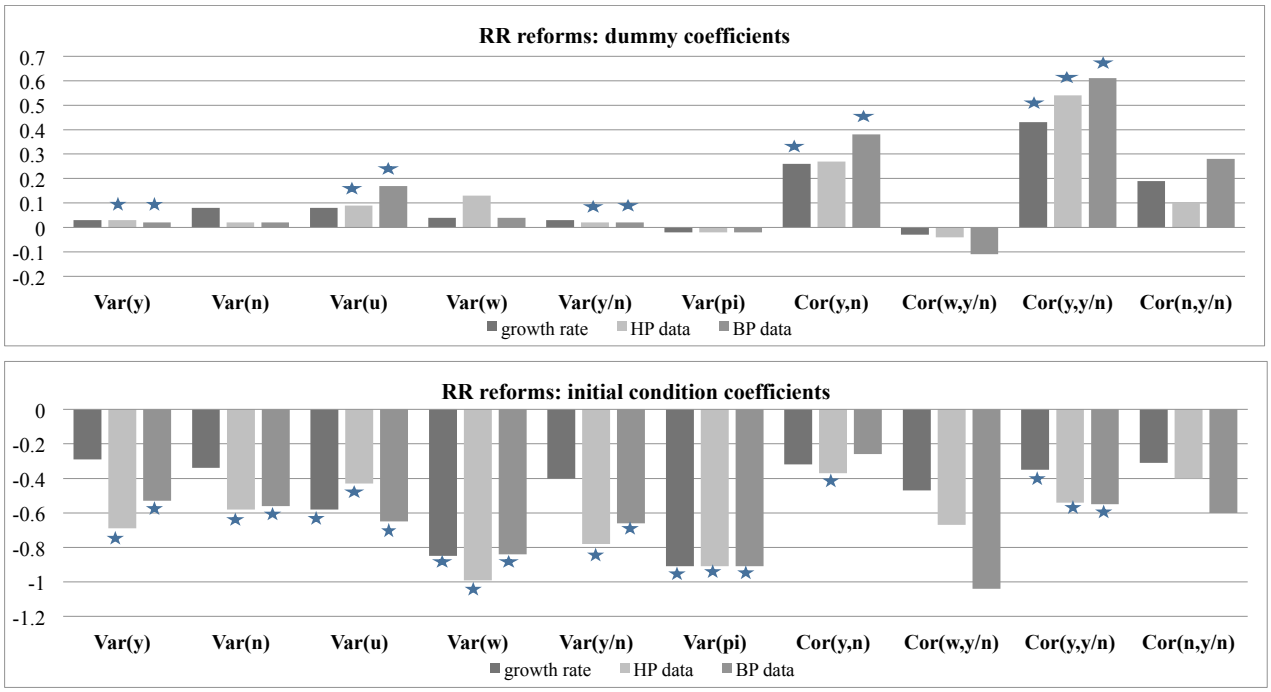
An * above/below a column indicates correlations that are significantly different from zero at the 10 percent level. Growth rate stands for log difference (4-period lag); HP data stands for Hodrick-Prescott filtered data; and BP data stands for data detrended using a Band Pass filter.

Figure 3: Effects of EPL reforms on business cycles, different detrending methods



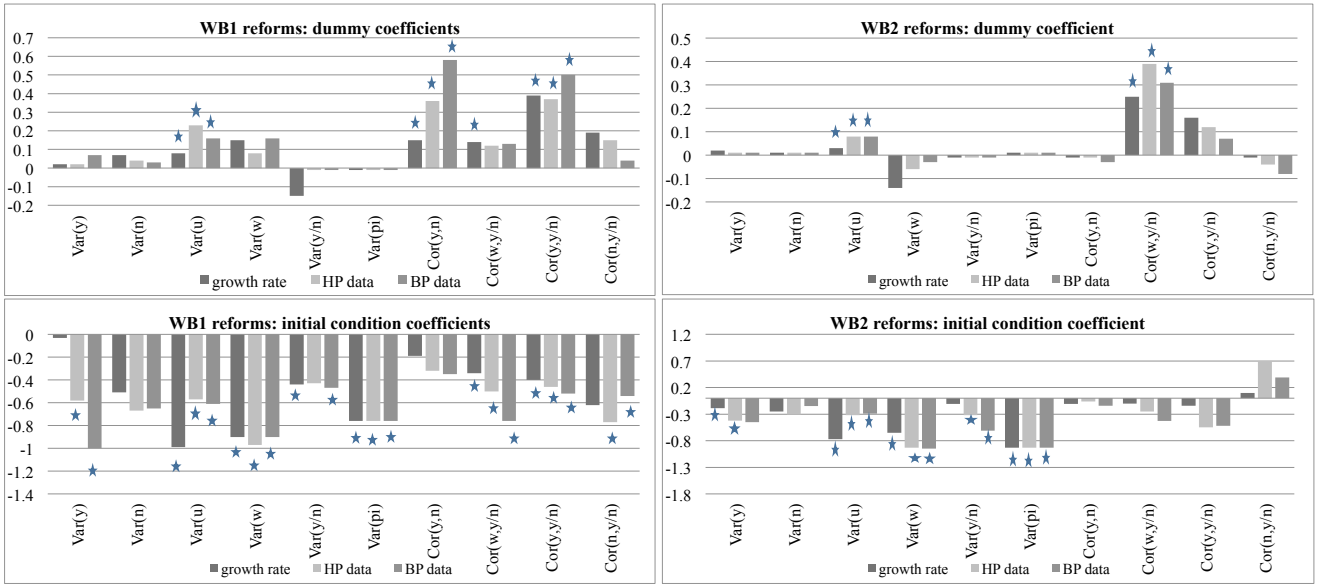
An * above/below a column indicates correlations that are significantly different from zero at the 5 percent level. Growth rate stands for log difference (4-period lag); HP data stands for Hodrick-Prescott filtered data; and BP data stands for data detrended using a Band Pass filter.

Figure 4: Effects of RR reforms on business cycles, different detrending methods



An * above/below a column indicates correlations that are significantly different from zero at the 5 percent level. Growth rate stands for log difference (4-period lag); HP data stands for Hodrick-Prescott filtered data; and BP data stands for data detrended using a Band Pass filter.

Figure 5: Effects of WB reforms on business cycles, different detrending methods



An * above/below a column indicates correlations that are significantly different from zero at the 5 percent level. Growth rate stands for log difference (4-period lag); HP data stands for Hodrick-Prescott filtered data; and BP data stands for data detrended using a Band Pass filter.

Table 1: Description of LMI data

LMI	Description	Source
Replacement rate (RR)	Unemployment benefits as a percentage of average earnings before tax. Data refer to first year of unemployment benefits and are averaged over three family situations and two earnings levels.	CEP-OECD Institutions Data Set (Nickell, 2006)
Union density (UD)	Percentage of workers affiliated to a union.	ICTWSS
Union coverage (UC)	Percentage of contracts negotiated by unions.	CEP-OECD Institutions Data Set (Nickell, 2006)
Union concentration (CONC)	Average concentration measure (Herfindahl index[1]) of unionization at national and sectorial level. Index takes values between 0-1.	ICTWSS
Union centralization (CENT)	Measures concentration as in CONC, but weights differently the national and the sectorial level according to their importance. Index takes values between 0-1.	ICTWSS
Wage bargaining coordination (WCO-ORD)	Degree of coordination in the bargaining process, both on workers' and firms' side, measured on 0-5 scale.	ICTWSS
Government bargaining involvement (GOVINT)	Government involvement in wage bargaining, measured on a 0-5 scale.	ICTWSS
Bargaining level (LEVEL)	Degree of bargaining centralization, measured on a 0-5 scale, which captures the predominant level where bargaining takes place (e.g. firm leve, industry level and nationwide).	ICTWSS
Wage bargaining extension (EXT)	The extent to which collective agreements are applied to non-unionized workers, measured on a 0-2 scale, where 0 indicates a collective agreement applied only to union members and 2 indicates more than 10 percent of non-unionized workers.	ICTWSS
Minimum wage (MIN-WAGE)	Degree of government intervention and discretion in setting the minimum wage, measured on 0-8 scale.	ICTWSS
Employment protection (EPL)	Overall strictness of employment protection legislation related to dismissals.	OECD
Employment protection for permanent contracts (EPR)	Stictness of employment protection on regular/permanent contracts related to dismissals. It incorporates 8 subitems each expressed on a 0-6 scale.	OECD
Employment protection for temporary contracts (EPT)	Stictness of employment protection on temporary contracts related to dismissals. It incorporates 6 subitems each expressed on a 0-6 scale.	OECD

Note: The Database on Institutional Characteristics of Trade Unions, Wage Setting, State Intervention and Social Pacts (Visser, 2009) covers 34 countries over the period 1960-2007 and gathers information about unionization, wage bargaining and social pacts. The information is summarized by 90 variables, including union density and coverage, coordination and centralization. The ICTWSS data set is one of the sources used to construct the data published by the OECD. Details about the CEP-OECD database are provided in Nickell (2006).

[1]The index is constructed as follows: first the share of workers represented by each union confederation relative to the represented labor force at the national level is computed and then the share of workers represented by each union affiliated to some confederation, relative to the represented labor force, is computed. Herfindahl indices are constructed by summing up the squared shares over confederations and unions respectively. Finally, the two indices are averaged.

Table 2: Correlation between the macroeconomic factor and variables

Macro variables	Macro PC1
Inflation Targeting dummy	0.25
European Monetary Union dummy	0.35
Government spending (% of GDP)	0.68
Trade openness (exports + imports as % of GDP)	0.60
Current account (% of GDP)	0.59
GDP per capita (%)	0.53
Inflation (%)	0.72
Unemployment rate (%)	0.17

Note: Macro PC1 denotes the first principal component extracted from the corresponding macro variables.

Table 3: Correlation between statistical factors and LMIs

	Statistical factors			
LMIs	Overall rigidity (SF1)	(SF2)	(SF3)	(SF4)
Replacement rate (RR)	0.35	0.03	-0.59	0.49
Union density (UD)	0.52	0.54	0.04	-0.28
Union coverage (UC)	0.82	-0.06	0.30	-0.14
Union concentration (CONC)	0.21	0.68	0.33	0.36
Union centralization (CENT)	0.63	0.53	0.21	0.27
Wage bargaining coordination (WCOORD)	0.68	0.20	-0.19	0.12
Government bargaining involvement (GOVINT)	0.71	-0.26	0.18	-0.20
Bargaining level (LEVEL)	0.84	0.20	-0.11	-0.07
Wage bargaining extension (EXT)	0.43	-0.54	0.64	0.13
Minimum wage (MIN-WAGE)	0.54	0.56	-0.16	-0.36
Employment protection (EPL)	0.76	-0.49	-0.22	0.02
Employ. protection - permanent contracts (EPR)	0.71	-0.37	0.00	0.38
Employ. protection - temporary contracts (EPT)	0.71	-0.39	-0.27	-0.27

Table 4: Spearman partial rank correlations: statistical factors

Bus. cycle indicators	Statistical factors: Overall rigidity (SF1)	(SF2)	(SF3)	(SF4)
Var(y)	0.23*	0.31***	-0.13	-0.20
Var(n)	0.13	0.06	-0.06	-0.10
Var(u)	0.22*	-0.29	0.05	0.06
Var(w)	0.49***	0.47***	-0.06	-0.05
Var($\frac{y}{n}$)	0.34***	0.34***	-0.03	-0.30**
Var(π)	0.32***	0.11	-0.07	-0.26
Cor(y,n)	-0.13	-0.24**	-0.21*	0.19
Cor(w,$\frac{y}{n}$)	-0.32**	0.00	0.03	0.10
Cor(y,$\frac{y}{n}$)	-0.01	0.12	-0.06	-0.15
Cor(n,$\frac{y}{n}$)	-0.14	-0.02	-0.21*	0.03

Note: ***,**,* indicate correlations that are significant respectively at the 1%, 5%, and 10% level. Business cycle data are detrended by differencing the log series.

Table 5: Correlation between economic factors and LMIs

LMIs	Economic factors: Replacement rate (RR)	Labor unions (EF2)	Wage bargaining (EF3)	Employment protection (EF4)
Replacement rate (RR)	1.00			
Union density (UD)		0.75		
Union coverage (UC)		0.68		
Union concentration (CONC)		0.69		
Union centralization (CENT)		0.86		
Wage bargaining coordination (WCOORD)			0.80	
Government bargaining involvement (GOVINT)			0.74	
Bargaining level (LEVEL)			0.93	
Wage bargaining extension (EXT)			0.27	
Minimum wage (MIN-WAGE)			0.59	
Employment protection (EPL)				0.98
Employ. protection - permanent contracts (EPR)				0.84
Employ. protection - temporary contracts (EPT)				0.89

Table 6: Spearman partial rank correlations: economic factors

Bus. cycle indicators	Economic factors: Replacement rate (RR)	Labor unions (EF2)	Wage bargaining (EF3)	Employment protection (EF4)
Var(y)	-0.04	0.34***	0.27**	-0.06
Var(n)	0.11	0.17	0.14	-0.01
Var(u)	0.33***	0.08	0.17	0.20*
Var(w)	0.15	0.60***	0.49***	0.17
Var($\frac{y}{n}$)	-0.10	0.46***	0.36***	0.05
Var(π)	0.15	0.34***	0.28**	0.16
Cor(y,n)	0.17	-0.31***	-0.14	0.01
Cor(w,$\frac{y}{n}$)	0.09	-0.18	-0.29**	-0.26**
Cor(y,$\frac{y}{n}$)	-0.22*	0.04	0.04	-0.05
Cor(n,$\frac{y}{n}$)	-0.09	-0.18	-0.15	-0.06

Note: ***,**,* indicate correlations that are significant respectively at the 1%, 5%, and 10% level. Business cycle data are detrended by differencing the log series.

Table 7: Spearman partial rank correlations: each LMI separately

	Business cycle indicators									
LMIs	Var(y)	Var(n)	Var(u)	Var(w)	Var($\frac{y}{n}$)	Var(π)	Cor(y,n)	Cor(w,$\frac{y}{n}$)	Cor(y,$\frac{y}{n}$)	Cor(n,$\frac{y}{n}$)
RR	-0.04	0.11	0.33***	0.15	-0.10	0.15	0.17	0.09	-0.22*	-0.09
UD	0.50***	0.21*	0.10	0.60***	0.59***	0.50***	-0.35***	-0.14	0.20*	-0.08
UC	0.03	0.03	0.15	0.18	0.17	0.19	-0.09	-0.23*	-0.04	-0.10
CONC	0.23**	0.13	0.12	0.37***	0.26**	0.08	-0.33***	0.09	0.03	-0.27**
CENT	0.24**	0.13	-0.06	0.49***	0.32***	0.24**	-0.19	-0.16	0.04	-0.10
WCOORD	0.25**	-0.03	-0.08	0.43***	0.32***	0.16	-0.06	-0.21*	0.19	0.11
GOVINT	0.20*	0.29**	0.37***	0.35***	0.29**	0.31***	-0.15	-0.21*	-0.16	-0.35***
LEVEL	0.23*	0.14	0.15	0.46***	0.29**	0.21*	-0.09	-0.28**	0.00	-0.13
EXT	-0.30***	-0.11	0.18	-0.11	-0.20*	-0.14	0.06	-0.12	-0.18	-0.13
MIN-WAGE	0.27**	-0.00	-0.08	0.45***	0.46***	0.26**	-0.38***	-0.19	0.25**	-0.12
EPL	-0.09	0.00	0.21*	0.15	0.02	0.16	0.05	-0.27**	-0.08	-0.06
EPR	-0.08	-0.07	0.07	0.10	0.06	0.07	-0.06	-0.19	0.02	-0.09
EPT	-0.08	-0.02	0.14	0.15	0.02	0.16	0.05	-0.22	-0.08	-0.02

Note: ***,**,* indicate correlations that are significant respectively at the 1%, 5%, and 10% level. Business cycle data are detrended by differencing the log series.

Table 8a: Countries reducing EPL

Country	Date	Reform
Austria	2002	Reform of the severance pay system: right to a severance pay upon contract termination after 3 years with the same employer replaced by retirement accounts, removing the specific costs of dismissals.
Finland	1991	The notice period was shortened from 2 months to 1-2 weeks.
Finland	2001	The employer has the right to dismiss an employee with notice if the work in question has decreased substantially and permanently for economic and production-related reasons.
France	1986	The administrative authorization in case of individual dismissal for economic reasons is abolished.
Italy	1991	Law on collective redundancies establishing weaker standards related to notice and union consultation. It concerns companies with more than 15 employees.
Japan	1986	Private temporary staffing agency activity was partially legalized in 1986 with the advent of the Worker Dispatching Law (WDL).
Portugal	1991	Several restriction on lay-off legislation are phased out. Dismissals for unsuitability are authorized.
Portugal	2003	Employers now have the right to oppose the reinstatement of workers in dismissal cases under certain conditions, such as in cases where it would harm or disrupt business activity.
Spain	1984	Restrictions for fixed-term contracts are substantially relaxed. Legal norms establishing the conditions under which a fixed term contract can be stipulated are overridden by the principle of promoting employment through the extension of contracts between 6 months and 3 years.
Spain	2002	The employer is allowed to immediately deposit in court an amount equal to unfair dismissal severance payment in order to avoid paying interim wages.
Sweden	1993	Time work agencies were permitted. The last-in-first-out rule was relaxed: employers may retain two workers of their own choice in redundancy situations.

Table 8b: Countries reducing RR and/or DU

Country	Date	Reform
Austria	1995	Unemployment benefits have been reduced. As alternative to benefits, early retirement is allowed for women from the age of 54 and for men from the age of 59.
Austria	2000	Replacement rates are lowered and eligibility criteria are stricter.
Belgium	1992	Duty to actively seek for a job is enforced. Eligibility for long-term unemployed is made stricter.
Denmark	1994	Duty to actively seek for a job is enforced after 6 months of unemployment. Duration is reduced. Possibility to combine benefits with wage income.
Denmark	2003	Duty to actively seek for a job and accept an offer, if received, are introduced immediately after the first day of unemployment.
Finland	2001	Duty to actively seek for a job is enforced for unemployed receiving social assistance.
France	1991-93	Contribution required to be eligible for unemployment insurance is raised and duration of benefits is lowered.
Spain	1992-93	Contribution rates and period required to be eligible for benefits is raised. Duration is reduced.
Spain	2000	Duty to actively seek for a job is enforced. Unemployed rejecting three suitable job offers loose the benefit. An offer is suitable if job is identical to previous jobs. After 12 months, unemployed must accept any other job after retraining.
Sweden	2000	Duty to actively seek for a job is enforced. Unemployed rejecting three job offers loose the benefit.
New Zealand	1989-92	Reduction of benefits and stricter eligibility.
Canada	1996	Contribution rates required to be eligible for benefits is raised. RR lowered for unemployed with higher income during contribution period prior to dismissal.

Table 8c: Countries reducing centralization of wage bargaining

Country	Date	Reform
Australia	1996	The Workplace Relations Act 1996 decollectivates Australian labour law and re-regulates industrial relations to the level of the workplace.
Australia	2001	Reforms promote flexibility and workplace level wage bargaining.
Austria	1982	Fall in wage coordination.
Japan	1997	The management of the labour market has been delegated to regional labour market councils.
New Zealand	1987	Labor Relations Act reduces centralization and coordination.
Denmark	1987	Bargaining shifts down to the industry and/or firm level.
Ireland	1987	Program of National Recovery in wage bargaining.
New Zealand	2005	Labor Relations Act is replaced, replacement regulation increases wage decentralization.
Spain	1986	Liberalization of fixed-term contracts, reductions in government intervention.
Switzerland	1981	Fall in the degree of bargaining centralization.
Switzerland	1995	Fall in wage coordination.
UK	1980	Thatcher reforms reduce government intervention in wage bargaining and reduce wage coordination.

Table 8d: Countries reducing union power

Country	Date	Reform
Australia	1975	Fall in unionization.
Australia	1996	Conservative Howard Government elected - introduces Workplace Relations Act, aimed at prohibiting compulsory unionization and making the transition to non-award coverage easier.
Denmark	1987	The management of the labor market has been delegated to regional labour market councils.
Japan	1987-90	In 1987 two labor federations were dissolved and amalgamated into newly established National Federation of Private Sector Unions (Rengo) and in 1990 Sohyo affiliates merged with Rengo.
New Zealand	1985	Labour Relations Act required unions to have minimum 1,000 members (compared to 30 members under previous legislation).
New Zealand	1991	Abolished compulsory unionism and decentralization.
Spain	1990	Fall in union density.
UK	1980	Thatcher's labor market reforms.

Table 9: Treated and control groups for reforms

	Reforms			
	Empl. protection (EPL)	Replacement rate (RR)	Wage bargaining (WB1)	Union power (WB2)
Control	Norway (beg-91:4,92:1-end) US (beg-91:4,92:1-end)	Norway (beg-93:1,93:2-end) US (beg-93:1,93:2-end)	Norway (beg-84:1,84:2-end) US (74:1-84:1,84:2-end)	Norway (beg-92:3,92:4-end) US (74:1-92:3,92:4-end)
Treatment	Sweden (beg-93:4,94:4-00:4) Finland (beg-90:4,91:4-00:4) Italy (beg-90:4,91:4-end) Portugal (beg-90:4,91:4-02:4)	Austria (beg-94:4,95:4-99:4) Belgium (beg-91:4,92:4-end) Canada (beg-95:4,96:4-end) Denmark (beg-93:4,94:4-02:4) France (beg-90:4,91:4-end) New Zealand (beg-83:4,84:4-01:4) Spain (beg-91:4,92:4-99:4)	Austria (beg-81:4,82:4-00:4) Switzerland (beg-80:4,81:4-04:4) Denmark (beg-87:1,88:1-end) Ireland (beg-87:1,90:4-end)	Australia (beg-95:4,96:4-00:4) Japan (beg-96:4,97:4-end) Switzerland (81:4-94:4,95:4-end)

Table 10: Treated and control groups' mean characteristics

	HD	$\frac{G}{Y}$	Y	u	π	$\frac{X+M}{Y}$	$\frac{CA}{Y}$
Control group	0.92 (0.023)	0.18 (0.03)	2.09 (0.42)	7.77 (1.04)	1.94 (0.06)	0.41 (0.32)	0.05 (0.12)
Empl. protection (EPL) reforms	0.87 (0.04)	0.22 (0.04)	1.58 (0.22)	8.48 (0.20)	2.87 (1.21)	0.53 (0.18)	0.009 (0.06)
Replacement rate (RR) reforms	0.89 (0.012)	0.21 (0.03)	1.71 (0.24)	14.2 (7.38)	2.12 (0.70)	0.63 (0.31)	−0.01 (0.03)
Wage bargaining (WB1) reforms	0.90 (0.01)	0.19 (0.06)	1.81 (0.17)	11.9 (4.76)	1.91 (0.71)	0.82 (0.15)	0.04 (0.06)
Union power (WB2) reforms	0.91 (0.015)	0.14 (0.029)	1.93 (0.06)	12.5 (4.3)	1.65 (0.75)	0.44 (0.22)	0.04 (0.08)

Note: Standard errors are in parentheses. HD: human development index; $\frac{G}{Y}$: government size; Y: GDP per capita; u: unemployment rate; π: inflation rate; $\frac{X+M}{Y}$: trade openness; $\frac{CA}{Y}$: current account-to-GDP.

Table 11: Effect of EPL reforms on business cycles

	Var(y)	Var(n)	Var(u)	Var(w)	Var($\frac{y}{n}$)	Var(π)	Cor(y,n)	Cor(w,$\frac{y}{n}$)	Cor(y,$\frac{y}{n}$)	Cor(n,$\frac{y}{n}$)
D_i	0.46	0.14*	0.33	0.01	−0.01	0.01	0.23	−0.51*	−0.27	−0.49*
	(0.40)	(0.06)	(0.21)	(0.10)	(0.03)	(0.12)	(0.14)	(0.06)	(0.24)	(0.13)
$X_{i,pre}$	−0.25	−0.20	−0.63*	−0.67*	−0.08	−0.89	−0.16	−0.02	−0.18	−0.48
	(0.30)	(0.71)	(0.17)	(0.03)	(0.24)	(0.03)	(0.23)	(0.12)	(0.25)	(0.41)

Note: * indicates coefficients that are significant at the 5% level. Bootstrap standard error in parenthesis.

Table 12: Effect of RR/DU reforms on business cycles

	Var(y)	Var(n)	Var(u)	Var(w)	Var($\frac{y}{n}$)	Var(π)	Cor(y,n)	Cor(w,$\frac{y}{n}$)	Cor(y,$\frac{y}{n}$)	Cor(n,$\frac{y}{n}$)
D_i	0.03	0.08	−0.08	0.24*	0.03	−0.02	0.26*	−0.03	0.43*	0.19
	(0.03)	(0.12)	(0.07)	(0.05)	(0.16)	(0.05)	(0.12)	(0.12)	(0.09)	(0.16)
$X_{i,pre}$	−0.29	−0.34	−0.58*	−0.85*	−0.40	−0.91*	−0.32	−0.47	−0.35*	−0.31
	(0.22)	(0.15)	(0.05)	(0.05)	(0.29)	(0.02)	(0.18)	(0.42)	(0.14)	(0.30)

Note: * indicates coefficients that are significant at the 5% level. Bootstrap standard error in parenthesis.

Table 13: Effect of wage bargaining reforms on business cycles

	Var(y)	Var(n)	Var(u)	Var(w)	Var($\frac{y}{n}$)	Var(π)	Cor(y,n)	Cor(w,$\frac{y}{n}$)	Cor(y,$\frac{y}{n}$)	Cor(n,$\frac{y}{n}$)
1980s										
D_i	0.02	0.07	0.08*	0.15	−0.15	−0.01	0.15*	0.14*	0.39*	0.19
	(0.13)	(0.06)	(0.02)	(0.13)	(0.08)	(0.03)	(0.07)	(0.05)	(0.14)	(0.11)
$X_{i,pre}$	−0.03	−0.51	−0.99*	−0.90*	−0.44*	−0.76*	−0.19	−0.34*	−0.40*	−0.62
	(0.14)	(0.70)	(0.01)	(0.05)	(0.18)	(0.05)	(0.10)	(0.09)	(0.13)	(0.31)
1990s										
D_i	0.02	0.01	0.02*	−0.14	−0.01	0.01	−0.01	0.25*	0.16	−0.01
	(0.15)	(0.01)	(0.00)	(0.14)	(0.02)	(0.01)	(0.13)	(0.08)	(0.14)	(0.10)
$X_{i,pre}$	−0.19	−0.25*	−0.77*	−0.65*	−0.11	−0.93*	−0.12	−0.09	−0.14	0.09
	(0.16)	(0.09)	(0.07)	(0.02)	(0.06)	(0.05)	(0.31)	(0.16)	(0.15)	(0.96)

Note: * indicates coefficients that are significant at the 5% level. Bootstrap standard error in parenthesis.