Do labor market institutions matter for business cycles?[*]

Stefano Gnocchi

Universitat Autonoma de Barcelona and BGSE

Evi Pappa[†]

European University Institute, Universitat Autonoma de Barcelona, and CEPR

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Abstract

We study whether labor market institutions matter for business cycles in 19 0ECD countries. Institutions affect mainly the cyclical behavior of the real wage. Wage bargaining reforms increase the volatility of the real wage and increase its correlation with labor productivity; Employment protection reforms increase the volatility of unemployment and decrease the correlation of wages with labor productivity; reforms affecting the replacement rates do not have significant impact on the cycle.

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[†]Corresponding author, mailing address: European University Institute, Department of Economics, Villa San Paolo, Via della Piazzuola 43, 50133 Florence - Italy, Email: evi.pappa@eui.eu Tel. [+39]055 4685 908 Fax [+39]055 4685 902.

1   Introduction

Business cycle models have been recently augmented with a variety of labor market frictions since Walsh (2005), Trigari (2006), Campolmi and Faia (2011), Krause and Lubik (2007), Blanchard and Gali (2010), Thomas (2008) and Gali and van Rens (2010) have argued that labor market frictions affect the short run responses of macroeconomic variables to shocks. 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; (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 some robust stylized facts about the relationship between labor market institutions (henceforth LMI) and business cycle fluctuations. In particular, we investigate how institutional arrangements, such as employment protection, replacement rates, union density and coverage shape the business cycle statistics in 19 OECD countries for 1970 to 2009. Rather than modelling explicitly the link between institutions, labor market frictions and macrovariables, we examine the direct relationship between business cycles and LMI. This is advantageous since it allows us to avoid controversial assumptions about how institutions map into labor market frictions and what is their functional form.

We use various indicators to capture the institutional characteristics of labor markets: the CEP-OECD Institutions Data Set (Nickell (2006)); the ICTWSS Database on Institutional Characteristics of Trade Unions, Wage Setting, State intervention and Social Pacts (Visser, 2009); the "Social Policy Reform Inventory" data assembled by the Fondazione Rodolfo DeBenedetti; the labor market reforms information provided by the OECD, the DICE, national statistical offices, and government and non-governmental agencies. The data covers a sufficiently long span of time to include both expansionary and recessionary periods. We have information about employment protection legislation, union density, union coverage, coordination and centralization of the wage bargaining process, replacement rates and employment benefit duration.

We look at the data from two different angles. First, motivated by the limited time variation of LMIs, we average the time series information so as to have one indicator per country. In addition, we collapse the information contained in the set of indicators using principal component analysis. We identify four main factors: (a) an "overall rigidity" factor, which is positively and highly correlated with all the institutional indices; (b) a "union"

factor, that correlates with union density and union centralization and coordination; (c) a "wage setting" factor, correlated with coverage and extension of wage agreements; (d) a "flow restrictions" factor, positively correlated with employment protection legislation and replacement rates. We then relate second moments of relative macro economic variables such as real GDP per capita, inflation, unemployment, employment, labor productivity and the real wage to these principal components in a number of ways, including graphical analysis, rank correlations and cross country regression analysis. Economies with tight labor market institutions on the wage bargaining have more volatile real wage and its correlation with labor productivity is low, while in economies with tight employment protection restrictions the correlation between wages and labor productivity is high. Surprisingly, these differences do not affect neither output, nor inflation, suggesting that economies with tight LMI have ways to manoeuvre their production process. These results are robust and hold regardless of the way we construct business cycle statistics, the sample we consider, the way factors are constructed and the control variables we include in the empirical model.

   We also look at specific reform episodes to identify which type of institutions are most influential for business cycles. Our sample features 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.

   Employment protection and wage bargaining characteristics matter for cyclical fluctuations. Reforms reducing employment protection make unemployment more volatile and decrease significantly the correlation of the real wage with labor productivity. Loosening wage bargaining reforms has positive effects in the volatility of the real wage and increases its correlation with labor productivity. On the other hand, reforms that lower benefits do not seem to have a significant impact on the cycle. In addition, for some variables, changes in cyclical volatility occur simply as a result of reversion to the mean.

   These facts have important implications for both business cycle models and policy making activities. First, to understand how the economy works it is important to model flow and wage restrictions. The two are not substitutes: flow restrictions affect movements of unemployment while wage restrictions affect the volatility of wages and each affect differently the relationship between the real wage and labor productivity. Loose bargaining increases the correlation between the real wage and labor productivity and loose employment protection decreases it. Second, output inflation and employment dynamics do not seem to be affected by the labor market characteristics of the different economies. Third, LMI 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 the macroeconomic turbulence. Finally, governments should target labor market reforms for affecting the dynamic behavior of the real wage without considering their effects on other macrovariables

such as inflation or output since according to our evidence reforms do not affect the output and inflation dynamics.

The literature investigating the relationship between institutions and labor market performance is vast (see, for instance, Layard, Nickell and Jackman (1991) and Nickell and Layard (1999), Blanchard and Wolfers (2000), Nunziata (2003), Nickell, Nunziata and Ochel (2005) and Costain and Reiter (2008)). A few studies, including Nunziata and Bowdler (2005), Merkl and Schmitz (2010), Fonseca 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[1]. 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 cycle statistics.

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

2   The data

2.1   The macroeconomic data

We use quarterly data from the OECD and the International Financial Statistics (IFS) of the IMF. We consider nineteen OECD countries. The time span is: Australia (1971:1-2009:4), Austria (1971:1-2009:4), Belgium (1981:1-2009:4), Canada (1971:1-2009:4), Denmark (1978:1-2009:4), Finland (1971:1-2009:4), France (1971:1-2009:4), Ireland (1973:1-2009:4), Italy (1972:1-2009:4), Japan (1971:1-2009:4), Netherlands (1978:1-2009:4), New Zealand (1971:1-2009:4), Norway (1976:1-2009:4), Portugal (1971:1-2009:4), Spain (1975:1-2009:4), Sweden (1981:1-2009:4), Switzerland (1971:1-2009:4), United Kingdom (1971:1-2009:4) and the United States (1971:1-2009:4). 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 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.

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[1]Canova, Ciccarelli and Ortega (2012) and Altug, Emin and Neyapti (2010) investigate the impact of monetary and fiscal institutions on cyclical fluctuations in a variety of developed and developing economies.

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, with inflation, 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 length in different units, 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 forth differencing the log of the raw data and check the robustness 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[2].

2.2   The labor market institutions data

We consider thirteen 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_S, 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

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[2]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.

2 to more than the 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 sectoral level [3]; (xiii) CENT, which measures concentration as CONC, but weights differently the national and the sectoral level according to their importance.

Most of these indicators are obvious and do not require 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's incentives to post vacancies. Indices of density, coverage, coordination, centralization and the concentration affect unions' power and may affect real wage adjustments.

Most of these indicators come from the CEP-OECD Institutions Data Set (see Nickell (2006)). The ICTWSS Database provides information about the main characteristics of wage bargaining systems [4]. For employment protection legislation we use the series constructed by the OECD. Replacement ratios are also from the OECD and we have one observation every two years for each country; the data is averaged over three family situations and two earnings levels and the benefits are measured as a percentage of average before tax earnings. The ranking of countries for the indices produced by the OECD and by Ochel (2001) coincide.

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[3]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.

[4]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.

3 Cross sectional evidence

We first document the correlations between business cycle statistics and some key labor market characteristics by exploiting cross country differences. 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. This 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, for example, Du Caju et al. (2010) and Fabiani et al. (2010)). We initially choose not to exploit the time series dimension of the data because as Figure 1 illustrates time variations in LMI are limited and when they occur they are abrupt and of a discrete nature. In the left panel, we present the countries for which employment protection and the LEVEL indicator do not significantly change over time; in the right panel the countries that exhibit some variations. The right column documents that some changes have affected the ranking of countries. Panel regressions are unlikely to be able to capture these variations. The approach discussed in section 4 is instead designed to deal with these specific type of changes.

3.1 Accounting for macroeconomic indicators

To control for factors that might affect business cycles characteristics but are not of direct interest in the investigation we use a number of macroeconomic variables. These include: (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 also control for the type of monetary policy regime adopted (with a dummy for countries that are inflation targeters in most of the sample) and for the association with the EMU (with an EMU dummy) since a priori these characteristics could matter for how business cycles have developed[5]. We summarize the information contained in the macroeconomic indicators by computing their first principal component which we label as the macroeconomic factor. The last columns of Table 1 display the correlation of the main macroeconomic factor with the original indicators. As the table makes clear, the macroeconomic factor is strongly positively correlated with openness. Figure 2 presents unconditional scatter plots of the average business cycle statistics with the macroeconomic factor. According to the figure, open economies tend to be more volatile. Hence, countries with different economic characteristics are likely to experience different cycles and controlling for this fact is important.

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[5]We have used alternative sets of macroeconomic indicators, for example, we have included real GDP per capita or 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.

3.2   Collapsing information

Since we are not exploiting the time series dimension of the data, we have 19 data points, 13 LMI indicators and 8 macroeconomic indicators making 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 interactions among 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 the eighty present of the variability of the original data. With this criterion we find that four principal components describe fairly well the sample of indicators.Also to control for endogeneity of LMI we construct principal components using the initial condition of institutions measured as the average value of institutions in the sample between 1960 and 1970.

The left panel of Table 1 displays the correlation of the factors with the original variables. The first factor is highly correlated with all labor market indicators, thus it can be interpreted as an **index of overall rigidity**. The second factor isolates the role of unions in an environment where employment protection is weak. The factor is negatively correlated with the employment protection indices and highly and positively correlated with union density, concentration and centralization and the minimum wage index. We label this factor as **the union factor**. Note that the union factor does *not* capture collective wage bargaining directly - the correlation between the index and union coverage or the level at which bargaining takes place is negative. Rather, it captures other activities that may indirectly affect wages and employment flows such as the power to organize strikes or the representation of workers in the organization of the production process. The third factor is labelled the **wage setting factor**, as it positively correlates with variables extension, EXT, coverage, UC, concentration, $CONC$, government intervention, $GOVINT$ and minimum wage legislations while it negatively correlates with coverage and replacement rates. The wage setting factor captures the role of unions in wage bargaining activities. Finally, the last factor strongly correlates with EPL in short term contacts, and the other EPL measures and replacement rates. Hence, this component may be interpreted as residually identifying a **flow restrictions factor**.

3.3   Cross country evidence

Table 2 presents the Spearman rank correlations between the business cycle statistics and alternative LMI factors. In constructing the business cycle statistics we have first conditioned out macroeconomic influences. That is, we have regressed business cycle statistics on the first principal component of macroeconomic indicators and then correlated the residuals with the principal component of the LMI indicators.

The overall index of rigidity does not have any effect on volatilities, but it significantly reduces the correlation between the real wage and labor productivity.The flow factor tends

to increase this correlation significantly. The union factor increases significantly the correlation between output and employment and the wage setting factor significantly increases the volatility of the real wage.

Thus, more rigid economies are more volatile in terms of real wages and exhibit higher wedges between the real wage and labor productivity. Our results differ from the existing results in the literature. We do not find any significant impact of institutions on inflation volatility (See Rumler and Scharler (2011)) and no evidence that institutions affect unemployment volatility (See Abbritti and Weber (2010)). The only variable that seems to be affected robustly and significantly from the presence of institutions is the real wage. Differences in the results could be due to the fact that these authors do not control for interactions among institutions and for the endogeneity of institutions in their regressions. In the sensitivity analysis that we report below, we reconcile those differences when we neglect the interaction among institutions.

3.4   Sensitivity

The results presented above are robust to a number of sensitivity tests we have performed. First, they are robust to the way we compute business cycle statistics.They continue to hold when we detrend the data with either HP, or BP filters (See online appendix, section A1), suggesting that results are not subject to measurement error. Second, results are robust to the way we have computed the LMI factors. When we use the average value of the LMI indices instead of their initial values to compute the principal components of the LMI indicators the Spearman rank correlations between the business cycle statistics and LMI factors are similar as in the baseline analysis (see, online appendix, section A2).

Third, to help the interpretation of the results we have also considered identifying factors using some economic intuition. To this end we have extracted a common factor from the series of indicators of employment protection, EPL, EPR and EPT, another from indicators such as COOD, GOVINT, LEVEL, EXT and MIN_W, characterizing the wage bargaining and one from the indicators that characterize the strength of unions. On the middle panel of Table 1 we present the correlation of the economic factors with the original indexes and on the right panel of Table 2 we present the Spearman rank correlations between the business cycle statistics and the economic factors we have extracted. Again the wage setting factor affects positively and significantly the volatility of the real wage, while the union and the wage setting factor both decrease the correlation of the real wage with labor productivity. In addition employment protection and replacement rates according to the economic factors seem to increase the correlation between inflation and output significantly.

Forth, we have also tried to examine the correlation of business cycle statistics with one LMI at the time independently. Results are presented in the online appendix, section A3. Again, the analysis confirms that the correlation of the real wage with labor productivity is the statistic that is affceted the most by institutional indicators. However, many other

statistics turn out to be significantly affected by LMI such as output an unemployment volatility in line with the results of previous studies.

Finally, instead of computing correlations we have regressed business cycle statistics on the institutional factors. The results of these regressions are very similar with the results obtained using rank correlations (see online appendix, section A4).

4   Labor markets reforms and business cycles

Cross country analysis is intuitively appealing but it may give a distorted picture of the relationship between LMI and business cycles if time variations change the cross sectional orderings or if omitted variables exist. As we have mentioned a panel regression approach is not suitable for the type of data we consider. Also, since we control for a number of additional factors, omitted variables may be less of a problem in our context. To look at the data from a different perspective we look at labor market reforms. We define reforms as a "treatment" administered to some countries but not others, and estimate the causal effect of the treatment through a difference-in-difference estimator. This methodology allows us to exploit both the time series and the cross sectional variation in the data.

4.1   The labor market reform data

Following Boeri and Garibaldi (2009), we gather information about major labor market reforms for the countries in our sample by using the information provided in the "Social Policy Reform Inventory" assembled by the Fondazione Rodolfo DeBenedetti (www.frdb.org), which covers European countries from the 1970 until 2009. This data set 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 4 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); finally, for the US we obtain information from various OECD Outlooks.

No formal data is available on specific reforms that change the wage bargaining structure for the 19 OECD countries we examine. To capture reforms that affect the wage bargaining process we used information from the time evolution of the LMI indices on 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[6].

4.1.1   Reforms on employment protection

Table 3a 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[7].

The resulting treatment and control groups are in the first column of Table 4. 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.

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[6]We considered also 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 dif-in-dif 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.

[7]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 in1981 and in the UK in 1983. We decided to present the results on reforms waves since by classifying reforms chronologically we can control also for time effects. Results obtained with this classification are available from the authors upon request.

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. Given that the sample is very limited even for the first wave, one should take the results with caution. Nevertheless, we are confident on the robustness of the conclusions since they do not change significantly when the sample is larger.

4.1.2   Non-employment benefits 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 3b 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 unemployed to actively look for a job.

To control for time effects we consider two waves of reforms. The third column of Table 4 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 unemployed at the beginning of the 2000s. However, given that Finland, Sweden and Austria have enacted other reforms at the same time period and given that the time series dimension for these reforms is short we decided to omit the second wave of reforms from the analysis.

4.1.3   Wage bargaining reforms

In Table 3c 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 mid90s.

The last two columns of Table 4 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.

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 timevarying variable, such as openness, the government size, or the exchange and monetary policy regime, could affect the two group of countries in very different ways. Luckily the countries in our sample are quite homogeneous since they are all developed economies and we include potentially relevant controls such as openness, government size or monetary policy regime in the regression. 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 it is standard, we control for the 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 a quite 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 have not 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 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 LMI and business cycles have changed in reformers relative to non-reformers. When more than one wave of reforms exist, 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:

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

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 was 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 the opposite direction. However, since the correlation works only through $X_{i,pre}$, controlling for the initial condition eliminates the bias.

4.3   Results

Table 5 considers reforms that reduce employment protection. Countries that have passed such reforms display a significant increase in the volatility of unemployment 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. All other statistics have not been significantly affected by the introduction of EPL reforms. The table also indicates that the initial condition is often negative and significant, signalling strong regression to the mean.

Table 6 considers the consequences of reforms that decreased non-employment benefits. Reforms that tightened the eligibility conditions for unemployment benefits have significantly increased the volatility of unemployment[8].

For the wave of reforms concerning the tightening of the replacement rates the initial conditions turn out to be very significant. Thus, the analysis suggests that a change in

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[8]This result is not robust since it does not hold when we use HP filter to detrend the data. See online appendix.

cyclical volatility may simply be the result of reversion to the average, rather than the effect of the reform in non-employment benefits.

In Table 7 we present the estimates for reforms concerning changes in the wage bargaining process. In accordance with the cross sectional analysis, wage bargaining reforms have affected mostly the cyclical behavior of the real wage and labor productivity. In particular, such reforms increased the correlation of the real wage with labor productivity and decreased the cyclical volatility of the real wage in the 1990s. In both waves of reforms the initial condition for the volatility of the real wage, unemployment and inflation volatility matter. Hence, we find a general tendency of the business cycle variability of these variables to decline independently of reforms.

4.4   Sensitivity

The experiments we undertake using reforms episodes have the disadvantage that they are performed over a very small sample making results subject to small sample bias. For that reason, we have also investigated how the results may change if we increase the number of countries in the treatment and control groups. To this end, we have reconstructed these groups selecting control and treatment groups based on a specific labor market reform unconditionally from other changes in the labor market structure in the economy. So, for example, when we consider the employment protection reform instead of considering only the US and Norway as the control group we also add Australia, Canada, New Zealand, and Switzerland in the control group since none of these countries has experienced a reform concerning EPL and Austria, Japan, France and Spain in the treatment group since those countries did experience EPL reforms unconditionally on other changes in the structure of their labor market. With this loser definition of treatment and control groups the sample size of the first wave of reforms increases from six to ten for the first wave of EPL reforms and from five to nine for the second (See online appendix B for details). The results from this exercise are very similar to the ones reported for the smaller samples confirming the validity of the baseline results.

4.5   Summary

To summarize, as with the cross country regressions, the analysis of labour market reforms confirms that real wage variability and the correlation of the real wage with labor productivity are the two statistics that are most importantly affected by the reforms. Finally, regardless of the empirical technique employed, wage bargaining and employment protection factors seem to be the most significant in explaining differences in business cycle statistics across countries. Again, the evidence suggests no changes in the dynamics of output, inflation and employment after labor market reforms. This counterintuitive finding might be explained by the fact that unions or workers find alternative channels to resist against the undone rigidity.

For example, facing a reform that decentralizes the wage bargaining process, making wages more flexible, might increase labor demand, but at the same time might discourage labor force participation reducing the labor supply. As a consequence the inwards shift in the labor supply can partially or completely undo the effects of the reform on employment and output.

5 Implications for theoretical models and policymaking

The results of our investigation have important implications for the development of macroeconomic models displaying labor market frictions and for labor market policies.

The literature has modelled labor market frictions due to institutional features via business cycle models with rigidities due to firing or hiring costs, or real wage inflexibilities. Our analysis instead suggests that the way the wage bargaining process is set up has important implications for labor market dynamics. Gertler and Trigari (2009) have taken a first step in this direction and proposed the explicit modelling of the nature of the wage contracting process. They show that a more realistic modelling of the wage contract can account better than the standard search and matching model for the relatively volatile behavior of labor market activity over the business cycle. On the other hand, rigidities in the flow of workers are also important determinants of the labor market dynamics, since EPL changes affect significantly the flow of unemployment and the real wage in the data, but surprisingly have little effects on output and inflation. Our results solicit further theoretical work on the mechanism that renders labor market institutions unable to affect the short run dynamics of output, employment and inflation.

As far as labor market policies are concerned, to understand how the economy works it is important to model flow and wage restrictions. The two are not substitutes: flow restrictions affect movements of unemployment while wage restrictions affect the volatility of wages and each of those restrictions affects inversely the relationship between the real wage and labor productivity. Any reform that affects the wage negotiation process increases the correlation of the real wage with labor productivity and the volatility of the real wage while reforms that reduce flow restrictions have the opposite effect.

Moroever, LMI 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 the macroeconomic turbulence. Reforms appear to occur at times of economic turbulence and, especially, when the volatility of real wages, inflation and unemployment is high.

Also, governments should target labor market reforms for affecting the dynamic behavior of the real wage without considering their effects on other macrovariables such as inflation

or output since according to our evidence labor market characteristics do not affect those dynamics.

6   Conclusions

This paper analyzed whether different labor market rigidities affected and in what way the macroeconomic performance of 19 OECD countries. Our main conclusion is that labor market institutional arrangements have important macroeconomic consequences. The cyclical behavior of real wages and its relation with labor productivity are the statistics mostly affected by labor market institutions. In terms of reforms, differences in wage bargaining and in employment protection have significant impact on business cycles. Our results are robust to the inclusion of a number of controls in the analysis and hold true when cyclical statistics are computed over different samples and prefiltering the data with a variety of methods.

The paper provides some important facts concerning the relationship between labor market institutions and business cycles. Our conclusions have relevant implications for the most recent macroeconomic literature. Many authors have suggested, in theory, that labor market rigidities can explain in part the nature of macroeconomics fluctuations. However, the empirical evidence in this respect is still limited. This paper shows that indeed labor markets institutions are important for determining the dynamics of the real wage and indicates which ones are the most important for shaping cyclical fluctuations in the real world. On the other hand, institutions do not affect output, employment and inflation dynamics. Future work in the area should try to explain this fact.

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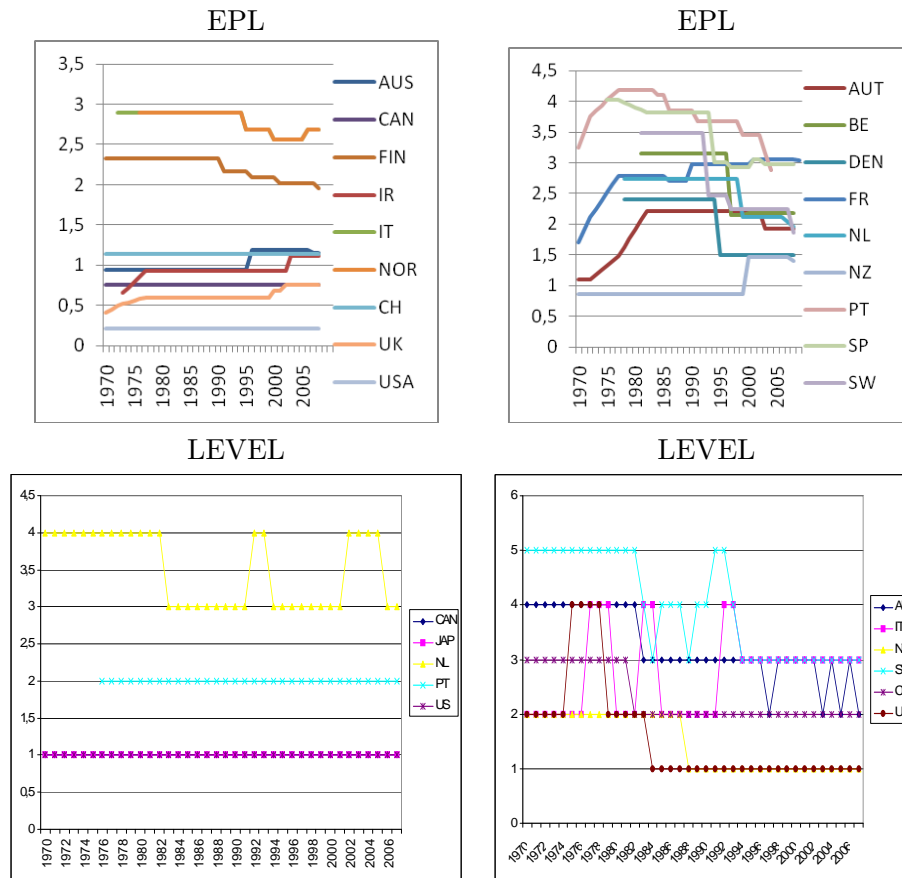
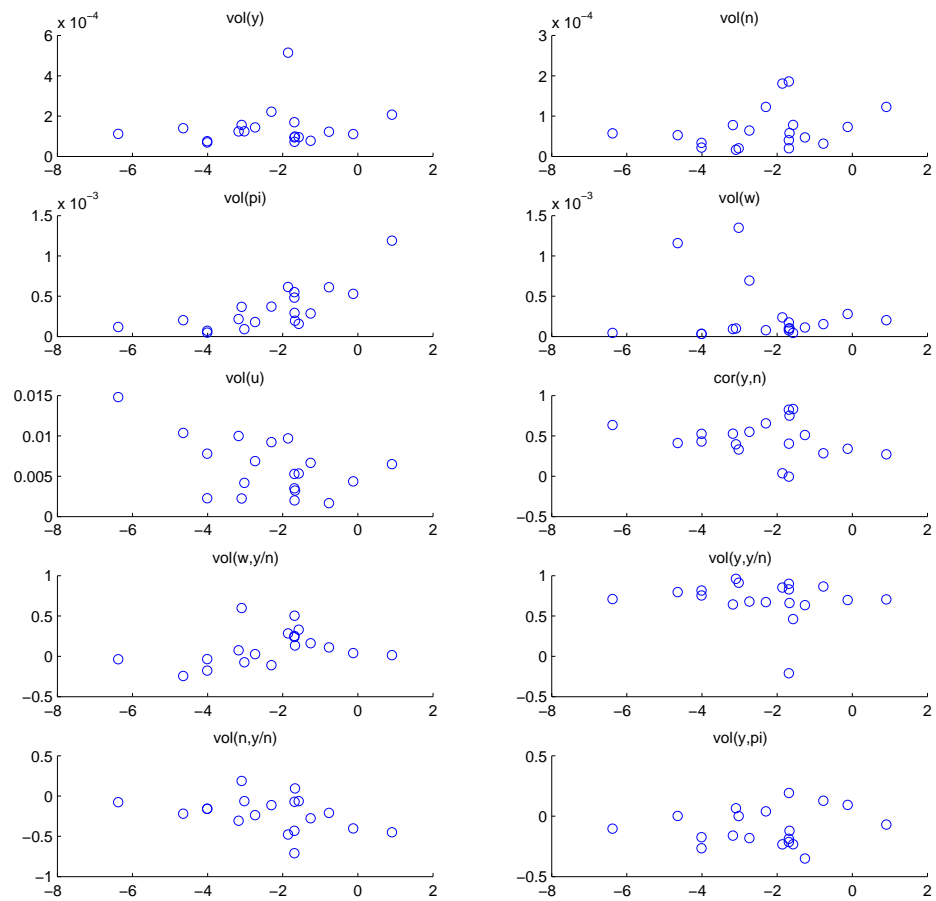# 8 Tables and Figures

Figure 1: EPL and Level. Changes over time.

Figure 2: Business cycle statistics and macoreconomic indicators.

Table 1: Correlation between factors and variables										
	Statistical factors				Economic Factors				Macro Factor	
VARIABLES	SF1	SF2	SF3	SF4	EF1	EF2	EF3	EF4	Variables	MPC1
EPL	0.73	-0.47	-0.19	0.16	0.92				CA/GDP	0.54
EPR$_{v1}$	0.30	0.21	0.24	**0.71**	0.25				(X+M)/GDP	0.88
EPT$_{v1}$	0.76	-0.30	0.17	0.19	0.93				G/GDP	0.41
RR	0.68	-0.27	-0.49	0.17		1.00			IT	0.50
COOD	0.73	0.19	-0.03	0.18			0.78		EMU	0.56
GOVINT	0.53	-0.39	**0.42**	-0.04			0.49			
LEVEL	0.85	0.22	-0.26	0.00			0.94			
EXT	0.44	-0.58	**0.36**	-0.29			0.26			
MIN_W	0.34	**0.70**	**0.43**	0.25			0.55			
UD	0.47	**0.50**	0.37	-0.31				0.82		
UC	0.59	-0.26	-0.03	-0.55				0.30		
CONC	0.21	**0.74**	**0.39**	0.06				0.78		
CENT	0.66	**0.54**	0.05	-0.08				0.84		

Table 2: Spearman rank correlations, forth differences								
	Statistical factors				Economic factors			
Index	SF1	SF2	SF3	SF4	EF1	EF2	EF3	EF4
Var(y)	-0.17	-0.07	0.19	-0.1	0.08	-0.10	0.15	0.10
Var(n)	0.03	0.07	0.27	0.17	0.18	-0.01	0.02	0.07
Var(u)	-0.27	0.06	-0.07	0.13	-0.07	-0.09	-0.16	0.26
Var(w)	0.17	-0.00	0.61*	-0.19	0.15	-0.03	-0.06	0.39*
Var(y/n)	-0.02	0.04	0.13	-0.07	0.14	0.02	-0.08	0.14
Var(π)	-0.06	0.32	0.06	0.06	0.21	0.14	-0.15	-0.13
cor(y,n)	0.07	0.44	0.08	0.24	0.30	0.16	0.06	0.30
cor(w,y/n)	-0.45*	0.26	-0.09	0.34*	-0.29	0.18	-0.60*	-0.55*
cor(y,y/n)	-0.21	0.18	0.07	0.08	0.05	0.04	-0.37	-0.09
cor(n,y/n)	-0.27	-0.11	-0.25	0.03	-0.21	-0.26	-0.23	-0.26
cor(y,π)	0.10	0.29	-0.44	0.03	0.36*	0.37*	0.03	-0.22

An * indicates correlations that are significant at the 5 percent level

Table 3a: 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. 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 3b: 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
		another 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 3c: 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 3d: 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 4: Treatment and control groups for reforms

Countries/Reforms	EPL1	RR1	WB1	WB2
Control	NOR(beg-91:4,92:1-end) US(beg-91:4,92:1-end)	NOR(beg-93:1,93:2-end) US(beg-93:1,93:2-end)	NOR(beg-84:1,84:2-end) US(74:1-84:1,84:2-end)	NOR(beg-92:3,92:4-end) US(74:1-92:3,92:4-end)
Treatment	SW(beg-93:4,94:4-00:4) FIN(beg-90:4,91:4-00:4) IT(beg-90:4,91:4-end) PT(beg-90:4,91:4-02:4)	AUT(beg-94:4,95:4-99:4) BE(beg-91:4,92:4-end) CAN(beg-95:4,96:4-end) DN(beg-93:4,94:4-02:4) FR(beg-90:4,91:4-end) NZ(beg-83:4,84:4-01:4) ESP(beg-91:4,92:4-99:4)	AUT(beg-81:4,82:4-00:4) CH(beg-80:4,81:4-04:4) DN(beg-87:1,88:1-end) IR(beg-87:1,90:4-end)	AUS(beg-95:4,96:4-00:4) JAP(beg-96:4,97:4-end) CH(81:4-94:4,95:4-end)

Table 5: Effects of EPL reforms on business cycles

mid80-90s

volatilities	vol(y)	vol(n)	vol(u)	vol(w)	vol(y/n)	vol(π)
D_i	0.41 (0.60)	3.72 (0.32)	0.73 (0.07)	−0.10 (0.56)	0.08 (0.87)	−0.01 (0.88)
$X_{i,pre}$	−0.41 (0.83)	1.71 (0.72)	−0.84 (0.10)	−0.95 (0.00)	1.28 (0.03)	−0.90 (0.00)
correlations	cor(y,n)	cor(w,y/n)	cor(y,y/n)	cor(n,y/n)	cor(y,π)	
D_i	0.04 (0.91)	−0.19 (0.06)	−0.28 (0.44)	0.24 (0.22)	−0.27 (0.50)	
$X_{i,pre}$	−0.58 (0.41)	−0.35 (0.15)	0.45 (0.74)	−0.54 (0.15)	−0.74 (0.51)	

p-values are in parenthesis

Table 6: Effects of RR/DU reforms on business cycles

volatilities	vol(y)	vol(n)	vol(u)	vol(w)	vol(y/n)	vol(π)
D_i	−0.07 (0.87)	−0.22 (0.53)	−0.86 (0.03)	−0.21 (0.23)	0.10 (0.86)	−0.05 (0.15)
$X_{i,pre}$	−1.04 (0.09)	−0.45 (0.15)	−1.16 (0.00)	−0.90 (0.00)	−0.62 (0.13)	−0.95 (0.00)
correlations	cor(y,n)	cor(w,y/n)	cor(y,y/n)	cor(n,y/n)	cor(y,π)	
D_i	−0.03 (0.83)	−0.01 (0.98)	0.36 (0.09)	0.31 (0.30)	−0.21 (0.44)	
$X_{i,pre}$	−0.70 (0.00)	−0.51 (0.42)	−0.51 (0.06)	−0.38 (0.44)	−1.82 (0.01)	

p-values are in parenthesis

Table 7: Effects of wage bargaining reforms on business cycles					
1980s					
volatilities	vol(y)	vol(n)	vol(u)	vol(w)	vol(y/n)	vol(π)
D_i	0.36 (0.41)	0.50 (0.80)	0.01 (0.95)	0.02 (0.45)	−0.08 (0.66)	−0.15 (0.33)
$X_{i,pre}$	−0.45 (0.04)	−1.63 (0.54)	−0.99 (0.00)	−0.95 (0.00)	1.29 (0.03)	−0.83 (0.00)
correlations	cor(y,n)	cor(w,y/n)	cor(y,y/n)	cor(n,y/n)	cor(y,π)	
D_i	−0.05 (0.65)	−0.17 (0.55)	0.28 (0.21)	0.24 (0.22)	0.02 (0.94)	
$X_{i,pre}$	−0.45 (0.04)	−0.84 (0.19)	−0.31 (0.62)	−0.55 (0.15)	−1.43 (0.04)	
1990s					
volatilities	vol(y)	vol(n)	vol(u)	vol(w)	vol(y/n)	vol(π)
D_i	0.07 (0.89)	−0.40 (0.35)	−0.25 (0.10)	−0.13 (0.02)	−0.06 (0.42)	−0.01 (0.57)
$X_{i,pre}$	0.45 (0.74)	−0.52 (0.32)	−0.88 (0.00)	−0.69 (0.00)	0.08 (0.44)	−1.02 (0.01)
correlations	cor(y,n)	cor(w,y/n)	cor(y,y/n)	cor(n,y/n)	cor(y,π)	
D_i	−0.23 (0.61)	0.48 (0.05)	0.20 (0.44)	0.03 (0.95)	−0.05 (0.90)	
$X_{i,pre}$	−0.78 (0.58)	−0.53 (0.16)	0.06 (0.74)	0.10 (0.93)	−1.66 (0.17)	

p-values are in parenthesis