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Andrea Bassanini

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SUMMARY

I examine the effect of labour market policies and institutions on the transmission of macroeconomic shocks to the labour market, using both aggregate and industry-level annual data for 23 OECD countries, 23 business-sector industries and up to 29 years. I find that high and progressive labour taxes and generous unemployment benefits amplify labour income fluctuations. By contrast, statutory minimum wages reduce the difference in the sensitivity of wages to aggregate shocks between low-wage and high-wage industries. Dismissal regulations are found to mitigate the impact of shocks on both earnings and employment. Moreover, this mitigation effect is greater in industries where firms have a greater propensity to make staffing changes through dismissals. Stringent dismissal regulations also appear to reduce the counter-cyclicality of the earnings dispersion between high and low-educated labour.

RÉSUMÉ

J'examine l'effet des politiques et des institutions du marché du travail sur la transmission des chocs macroéconomiques au marché du travail, en utilisant à la fois des données agrégées et sectorielles pour 23 pays de l'OCDE, 23 secteurs et sur une période maximale de 29 années. Je trouve qu'une fiscalité du travail élevée et progressive ainsi que des allocations chômage généreuses amplifient les fluctuations des revenus de travail. Par contre, le salaire minimum légal réduit les écarts de sensibilité des salaires aux chocs agrégés entre les secteurs où sont observés de bas salaires et les autres secteurs. La réglementation sur les licenciements semble réduire les effets des chocs à la fois sur les salaires et sur l'emploi. De plus, cet effet d'atténuation est plus important dans les secteurs où les entreprises ont une plus grande propension à ajuster la main-d'œuvre par des licenciements. Une réglementation rigoureuse des licenciements semble aussi réduire l'effet contra-cyclique de la dispersion des salaires selon le niveau d'éducation.

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AGGREGATE EARNINGS AND MACROECONOMIC SHOCKS: THE ROLE OF LABOUR MARKET POLICIES AND INSTITUTIONS

Introduction

There is an increasingly large empirical literature that investigates cross-country differences in the way employment and unemployment react to macroeconomic shocks (Blanchard and Wolfers, 2000, Nickell *et al.*, 2005, Bassanini and Duval, 2006, Porter and Vitek, 2008). Many studies also point to cross-country differences in the resilience of employment to shocks – most prominently between the United States and Continental European countries (Burgess *et al.*, 2000, Balakrishnan and Michelacci, 2001, Amisano and Serrati, 2003, Dustmann *et al.*, 2010, Ormerod, 2010). In this context, previous research suggests that structural policy settings might amplify or mitigate the employment effects of shocks and make them more or less persistent (Blanchard and Wolfers, 2000, Bassanini and Duval, 2006).

By contrast, the literature on cross-country differences in the response of aggregate earnings to shocks is comparatively smaller (see e.g. Balmaseda *et al.*, 2000, Messina *et al.*, 2009, Dustmann *et al.*, 2010, Kandil, 2010). A key issue for workers' well-being, however, is the extent to which cyclical downturns result in fluctuations in labour market earnings – that is the *combined* effect of changes in *employment*, *hours worked* and *wages*. Indeed, a recession can impact the labour income of employees even if they do not lose their job, by affecting the number of paid hours of work (through lower paid overtime or temporary cuts to working hours) and/or by reducing their real hourly wage (generally by compressing nominal wage growth). In addition, recessions can have long-term consequences on the labour income of employees through the effect on the length of non-employment spells and wage levels at re-employment (see *e.g.* Jacobson *et al.*, 1993, Farber, 2005, Krebs, 2007, and Schmieder *et al*., 2010). Quantifying the effect of policies and institutions in shaping the costs of a recession for workers involves, at the very least, assessing their effect on all sources of loss of labour income. This is also of crucial importance to the government budget in downturns insofar as reductions in gross labour income are directly reflected in falling government revenues and greater demand for social spending. In order to make some steps in filling this gap, by making use of aggregate and industry-level data, this paper investigates the role for labour market policies and institutions in influencing how aggregate earnings adjust over the cycle and the relative importance of different adjustment margins. Moreover, by breaking down adjustment patterns by level of education, I also examine the effect of the business cycle on earnings inequality, a key issue for social cohesion that has so far been investigated for only a few countries.

In order to identify the effect of policies and institutions I use both a standard cross-country/time-series approach and an industry-level difference-in-difference approach. Using aggregate cross-country/time-series data makes it possible to exploit the large variation in policies across countries and over time and examine general equilibrium effects. Yet, a key problem with aggregate analysis is that it is difficult to control for an exhaustive list of confounding factors. I circumvent this problem by exploiting the fact that cross-country comparable time-series data on earnings and employment are available at the industry level and that, while labour market policies and institutions are defined at the aggregate level, the impact of a few of them (notably the minimum wage and employment protection, EP hereafter) is likely to differ across industries. Within this context, I use a difference-in-difference strategy in the spirit of Rajan and Zingales (1998). The basic premise is that EP and the minimum wage are more likely to be binding in some industries than others. Therefore, if these policies have an impact on the transmission of aggregate shocks to earnings fluctuations, this impact will be greater in these so-called policy-binding industries. For example, reforms of dismissal regulations are likely to have a greater impact on the labour market adjustment in industries where, in the absence of regulations, firms rely on layoffs to make staffing changes, rather than in industries where internal labour markets or voluntary turnover are more important. We can use these other industries as a control group for EP-binding industries. In following this strategy, we will at worst underestimate the true effect of EP on earnings fluctuations. The same methodology can be followed for the minimum wage, by defining as minimum-wage-binding industries those industries that typically employ low-pay workers – that is, workers for which high minimum wages are more likely to constrain downward wage adjustments.

By looking simultaneously at the adjustment of wages and employment, this paper also complements the micro-literature on wage cyclicality. Estimates based on microdata consistently indicate a greater pro-cyclicality of individual wages than those based on macrodata (see *e.g.* Abraham and Haltiwanger, 1995; Brandolini, 1995; Devereux, 2001; Devereux and Hart, 2007), particularly for new hires (see Pissarides, 2009, for a survey). Nonetheless, the literature have clearly shown that nominal wages for incumbents tend to be rigid downward (see among others Nickell and Quintini, 2003, Gottschalk, 2005, Dickens *et al.*, 2007, Messina *et al.*, 2010 and the December 2010 special issue on price and wage dynamics on the *Scandinavian Journal of Economics*). A more infant literature has also related downward wage rigidity with upward wage rigidity: in order to cope with worker resistance to wage cuts, the optimal reaction of firms would be to temper wage increases in boom times (Bewley *et al.*, 2000, Elsby, 2009). The consequence of this literature is that wages might adjust less and more slowly. Institutions, and notably wage bargaining institutions and employment protection legislation are typically considered to explain cross-country differences in wage rigidity patterns (Bertola and Rogerson, 1997, Bertola, 1999, Babecký *et al.* 2009, 2010).

The paper is divided as follows: Section 1 details the empirical strategy. Section 2 describes the data. Section 3 presents the empirical results concerning average earnings, wages and employment, while results concerning the earnings distribution are presented in Section 4. Concluding remarks follows

1. Empirical strategy

A very simple and widely-used way to measure the impact of cyclical output fluctuations on a given aggregate variable (*e.g.* log total earnings) is to measure the covariation of the output gap and the cyclical component of that variable (see *e.g.* Abraham and Haltiwanger, 1995). Let us consider the following simple multiplicative model:

$$\log W_{it} = \theta \log W_{it}^{*} + \sum_{l} \varphi_l OGAP_{it-l} + \varepsilon_{it} \qquad [1]$$

where log W is the log of total earnings, * indicates its non-cyclical (*i.e.* trend or potential) component, *OGAP* is the output gap that is assumed to capture all business-cycle-related macroeconomic shocks, i and t index country and time and ε is an error term capturing shocks that are unrelated to the business-cycle. The non-cyclical component of total earnings is disentangled from the cyclical component through a Hodrick-Prescott (HP) filter (see, Hodrick and Prescott, 1997).[1] Hereafter, we will refer to the non-cyclical

1 . HP-filtered series are estimated by minimising a weighted average of the square of the growth of the trend component and its quadratic difference from the actual series. As standard for annual data, I set the relative weight of the growth term to 100. One problem with the HP filter is that it performs poorly around the beginning and the end of each time series. The Baxter-King filter (Baxter and King, 1999), by "passing" only frequencies between a low and high thresholds (reflecting the idea that business cycles are fluctuations of a certain frequency), performs better but at the cost of eliminating a few observations around the endpoints. Usual thresholds for the Baxter-King filter are 2 and 8 years, which is what I use

component of a variable as its trend and to the cyclical component as its gap, noting that the sum of the trend and gap yields the actual value by construction. To the extent that the trend captures all structural long-run determinants of the variable, including *e.g.* population growth and institutions, and shocks are stationary (with zero mean), θ can be set equal to 1 and the above equation becomes:

$$\log WGAP_{it} = \sum_l \varphi_l OGAP_{it-l} + \varepsilon_{it}, \qquad [2]$$

where log *WGAP* is the gap of log *W*. The sum of φs represents the long-run elasticity of fluctuations in log *W* to business-cycle fluctuations as measured by the output gap. In this paper we are interested not only to total earnings but also to its components (average hourly wage, total hours worked and/or total dependent employment) as well as distribution variables such a the earnings ratio between high and low educated. I will apply the empirical models presented in the next subsection alternatively to all these variables.

1.1. Aggregate cross-country/time-series analysis

As mentioned in the introduction, the goal of this paper is to estimate the impact of labour market institutions in shaping the reaction of aggregate earnings to macroeconomic shocks. In order to assess the amplification/mitigation effects of policies or institutions, the latter are assumed to affect the elasticity of fluctuations to the output gap as specified in equations [1] and [2]. More precisely, let us start with the following static model:

$$\log W_{it} = \theta \log W_{it}^* + \varphi_0 OGAP_{it-l} + \sum_k \varphi_k (P_{it}^k - \overline{P}^k) OGAP_{it} + \text{Other covariates} + \varepsilon_t \qquad [3]$$

where log *W* is the logarithm of total earnings, hours worked, or hourly wages, * indicates their respective trend values, *OGAP* is the output gap, *i* and *t* index country and time, respectively, *P* stands for policies and institutions, indexed by *k*, a bar above a variable indicates its sample average and ε is an error term capturing shocks that are unrelated to the business-cycle. Other covariates include country and time dummies, and the level of each included institution (for identification of the interaction terms).[2] As above, to the extent that the trend captures all structural long-run determinants of the dependent variable and shocks are stationary, θ can be set equal to 1 and the above equation becomes:

$$\log WGAP_{it} = \varphi_0 OGAP_{it-l} + \sum_k \varphi_k (P_{it}^k - \overline{P}^k) OGAP_{it} + \text{Other covariates} + \varepsilon_t, \qquad [4]$$

where log *WGAP* is the gap of log *W*. The hypothesis θ=1 can be easily tested and in fact is never rejected in the specifications presented in this paper. Insofar as institutions are included as deviations from their sample means, φ_0 captures the elasticity of the dependent variable with respect to the output gap for an "average country", assuming no lagged effects. The model can, however, be easily extended to include lagged effects. A positive estimated sign of φ_k for a given policy P_k implies that the policy significantly amplifies output shocks, while a negative sign means that the policy exerts a smoothing effect on output

here. In order to preserve sample size, I mainly use the HP filter in this paper, but all results are qualitatively robust to the use of a Baxter-King filter.

2 . Following a standard approach in aggregate unemployment regressions (see for example Biagi and Lucifora, 2008), in order to capture unusually large swings in Sweden and Finland at the beginning of the 1990s, I include specific dummies for Sweden and Finland in 1991-1992 and the subsequent period.

fluctuations.[3] The ratio φ_k / φ_0 gives a quantitative assessment of the proportional increase (if positive) or decrease (if negative) of the elasticity to the output gap if the policy P_k is raised by one unit from the OECD average.

An adverse shock might not only compress earnings and reduce employment. Its effects might also persist over time, and the degree of persistence is likely to be affected by policies and institutions. In order to assess amplification versus persistence effects of shocks, a dynamic version of the baseline model described above is needed. The simplest one is the following error-correction model:

$$\Delta \log W_{it} = -\phi(\log W_{it-1} - \theta \log W^*_{it-1}) + \kappa \Delta \log W^*_{it} + \sum_k \varphi_k (P^k_{it} - \overline{P}^k) OGAP_{it} + \text{Other covariates} + \varepsilon_{it},$$

where ϕ is a non-negative coefficient that captures persistence mechanisms (the greater it is, the less persistence). Under the same assumptions as above, this can be rewritten as:

$$\Delta \log WGAP_{it} = -\phi(\log WGAP_{it-1}) + b \Delta \log W^*_{it} + \sum_k \varphi_k (P^k_{it} - \overline{P}^k) OGAP_{it} + \text{Other covariates} + \varepsilon_{it}, \quad [5]$$

with $b = \kappa$ - 1 and θ = 1. ϕ can also be modeled as dependent on institutions:

$$\phi = \gamma_0 + \sum_k \gamma_k (P^k_{it} - \overline{P}^k),$$

where γ_0 captures the persistence of the average country (the smaller its value, the greater the degree of persistence). From a qualitative point of view, the interpretation of the γ coefficients is simple: a negative value γ_k indicates that a deviation of the policy P_k from the sample average is estimated to increase persistence. From a quantitative point of view, the interpretation is somewhat more complex. The equation above is justified by a model in which, after a one-period transitory shock, if no other shock occurs, the dependent variable goes back to its trend level following an exponential time path:

$$\log WGAP_{it} - \log WGAP_{i0} = -(1 - e^{-\lambda t})(\log WGAP_{i0}),$$

where λ is a parameter describing the speed of convergence to the equilibrium trend. Since the empirical model above is estimated on annual data, it follows that $\lambda = -\ln(1-\phi)$. Persistence is typically measured in terms of the half-life of a shock – that is the number of years required to reduce the initial impact of a shock by 50% – which is equal to $\ln(2)/\lambda$. Therefore the impact on the half-life of a shock of the increase in the policy X_k from the OECD average can be written in terms of semi-elasticity or proportional effect as:

$$\frac{\partial H / H}{\partial X_k} = \frac{\log(1-\gamma_0)}{\log(1-\gamma_0-\gamma_k)} - 1.$$

Insofar as certain policies might have opposite amplification and persistence effects, these two effects must be combined in some way in order to assess the consequences of policy actions in terms of labour-income smoothing. In turn this requires making assumptions about the discount rate and, if individual labour-income risk is not insurable, the degree of risk aversion. Let us assume a discount rate equal to

3. In principle amplification (and persistence – see below) parameters can be different in expansionary and contractionary stages of the cycle. However, in all the specifications considered in this paper, statistical tests can never reject the insignificance of possible differences. For this reason, this issue is not explored further in this paper and is left for future research.

δ and linear utility (that is no risk aversion), and consider the total cumulated impact, denoted C_0, of a shock resulting in a one-period transitory deviation of output from its trend by one percentage point. Its actual value in the average country at the time of the shock can be computed as:

$$C_0 = \int_0^{+\infty} \varphi_0 e^{-(\lambda_0+\delta)t} dt = \frac{\varphi_0}{\lambda_0 + \delta},$$

where $\lambda_0 = -\ln(1-\gamma_0)$ and γ_0 and φ_0 are defined as above. The overall effect of a policy on volatility can be measured in this context. Define the estimated proportional effect of a one unit change of a given policy on φ_0 and λ_0, as ξ and ζ, respectively. In other words, for a one-point increase in that policy, the immediate effect of a one percentage point shock on the dependent variable will be $(1+\xi)\varphi_0$, while the half-life of the effects of that shock will be $\ln(2)/(1+\zeta)\lambda_0$. The cumulated impact C of the shock after the policy reform will be:

$$C = \int_0^{+\infty} \varphi_0 (1+\xi) e^{-(\lambda_0(1+\zeta)+\delta)t} dt = (1+\xi)\frac{\varphi_0}{(1+\zeta)\lambda_0 + \delta}. \qquad [6]$$

The overall impact of the policy on the cumulated effect of the shock, expressed as a proportion of the cumulated effect of that shock for the average country – that is $(C-C_0)/C_0$, can be written as:

$$c = (1+\xi)\frac{\lambda_0 + \delta}{(1+\zeta)\lambda_0 + \delta} - 1. \qquad [7]$$

In other words in the case of an adverse shock, c multiplied by 100 gives an estimate of the difference (in percentage points) between the total cost of that shock and the cost in a country that has the same institutions as in the average country except for a one-unit greater level of the policy of interest. To the extent that all these parameters, with the exception of the discount rate, are obtained from the estimated equation, a confidence interval for c can be derived, and related statistical hypotheses tested.

For a policy with significant effects on both amplification/mitigation and persistence, two observations are however in order. First, equations [6] and [7] show that the greater the discount rate, the smaller the importance of the persistence effect of a policy as regards total costs/benefits of shocks. Moreover, second, the higher the degree of risk aversion, the greater the demand for consumption smoothing and the greater the weight of the mitigation effect of the policy in the determination of the total costs of adverse shocks.

One key identification problem in estimating equations [4] and [5] is that policies might be endogenous and, in particular, may be adapted during severe recessions. However, in the sample, the fluctuation of policies over time is much smaller than their variance across countries. Insofar as the effect of policies is identified also through their cross-country variation, reverse causality issues appear somewhat minor. By contrast, as in standard aggregate cross-country/time-series analyses, it is more difficult to control for an exhaustive list of confounding factors. In fact, due to the high correlation across institutions (see for example Bassanini and Duval, 2009), it is quite likely that a number of institutions that are omitted from the above equations will be simultaneously correlated with included policies and affect the transmission of macroeconomic shocks to the labour market. In order to reduce these concerns, I also use an industry-level difference-in-difference approach in the case of specific policies, such as employment protection (EP hereafter) and the minimum wage, whose effect is likely to differ across industries. This approach is described in the following subsection.

1.2. Industry-level difference-in-difference analysis

In the industry-level difference-in-difference approach, originally suggested by Rajan and Zingales (1998), the idea is to look at *within-country* industry differences in the effect of an aggregate policy variable. The identifying assumption is that if a policy *P* has an impact on an economic variable, this impact – whatever its sign – is greater in industries where *P* is more likely to be binding – hereafter called "policy-binding" (or *P*-binding) industries. I apply this idea to policy determinants of the transmission of macroeconomic shocks to labour market fluctuations. For example, EP-binding industries will be those where firms typically need to lay off workers to restructure their operations in response to changes in technologies or product demand and where, therefore, high firing costs are likely to slow the pace of reallocation of resources. In these industries, one can expect that EP has the greatest impact, if any, on cyclical fluctuations. By contrast, in industries where firms can restructure through internal adjustments or by relying on natural attrition of staff, changes in EP for open-ended contracts can be expected to have little impact. Following Bassanini *et al*. (2009), I use average dismissal rates by industry in the United States, the least regulated OECD country, as a benchmark to measure the layoff propensity of each industry in the absence of regulation. Similarly, in the case of the minimum wage, the estimation is based on the assumption that changes in minimum wages have a greater impact on wage and earnings cyclicality in industries that are more heavily reliant on low-wage labour. In this case, as suggested by Bassanini and Venn (2007), I identify low-wage industries based on the incidence of low-wage workers by industry in one specific country, the United Kingdom, prior to the introduction of statutory minimum wages in that country in 1999, when the *de facto* minimum wage can be assumed to be arbitrary small.[4]

The advantage of this estimation strategy is that it controls for policies or institutions that influence cyclical fluctuations in the same way in all industries. More precisely, all factors and policies that can be assumed to have, on average, the same effect on the dependent variable in policy-binding industries as in other industries can be controlled for by country-by-time dummies. In practice, the same models as in the previous subsection can be used to estimate the elasticity of industry-specific fluctuations to aggregate shocks. In that case, in equations [1] to [5], log *WGAP* will be an industry-by-country-specific time-varying variable. Nevertheless, to the extent that we want to estimate the reaction to aggregate shocks rather than to industry-specific reallocation shocks, *OGAP* must be the aggregate output gap and will therefore remain a country-specific time-varying variable. By contrast, the effect of policies on the elasticity of log *WGAP* to *OGAP* will be assumed to be industry-specific, that is I will estimate:

$$\log WGAP_{ijt} = \alpha B_j P_{it} + \beta B_j OGAP_{it} + \varphi B_j (P_{it} - \overline{P}) OGAP_{it} + X_{ijt}\delta + \eta_{it} + \eta_{jt} + \eta_{ij} + \varepsilon_{ijt} \qquad [8]$$

where *i*, *j* and *t* index country, industry and time, respectively, *P* stands for the policy of interest, a bar above a variable indicates its sample average, *B* is the benchmark used to classify industries,[5] *X* stands for additional covariates (including other interactions), ηs are bi-dimensional fixed effects (estimated by including the corresponding bi-dimensional dummies in the specification) that capture all aggregate effects as well industry-specific trends[6] and ε is an error term capturing idiosyncratic shocks that are unrelated to the business cycle. The parameter of interest is φ. A positive sign for φ would suggest that output-gap fluctuations result in bigger fluctuations of the dependent variable in *P*-binding industries when *P* is high. Such a finding, given the identification assumptions made above, would imply an amplification effect of *P*. By contrast, a negative sign would imply a mitigation effect of *P*. A reasonable estimate of the absolute

4. Draca *et al*. (2011) use a similar identification strategy at the firm level.

5. That is, either the industry-specific US dismissal rate proxying the dismissal propensity of an industry in the absence of regulation, in the case of EP, or the share of low-pay workers in the UK prior 1999, proxying the natural propensity to employ low-paid workers in the absence of a minimum wage.

6 . Country-by-industry effects are less justified and are included only in sensitivity analyses.

effect of a one-unit increase of P on the elasticity to the output gap for an average industry of an average country is given by φ multiplied by the average value of the benchmark B. This relies on the reasonable assumption that the effect of P is 0 in an hypothetical industry for which B is equal to 0. Moreover, if β is precisely estimated, the ratio φ/β multiplied by 100 would provide an estimate of the percentage effect of EP on the elasticity of fluctuations in the dependent variable with respect to the output gap, in the same way as φ_k/φ_0 in the previous subsection.[7]

The same framework can be used to study the effect of P on persistence, by assuming that persistence is likely to be greater (whatever its sign) in P-binding industries. In that case, the equivalent of equation [5] is:

$$\Delta \log WGAP_{it} = -\phi(\log WGAP_{it-1}) + \kappa\Delta \log W_{it}^{*} + (\beta + \varphi(P_{it} - \overline{P}))B_j OGAP_{it} + \text{Other covariates} + \varepsilon_{ijt} \qquad [9]$$

where

$$\phi = \gamma_0 + \gamma_P (P_{it} - \overline{P}) + \gamma_B B_j + \gamma_{PB} B_j (P_{it} - \overline{P})$$

The parameter of interest is γ_{PB}. A negative sign of γ_{PB} would suggest that dismissal regulations increase persistence more in P-binding industries than in other industries, which, given the identification assumptions made above, would imply that the policy P raises aggregate persistence. The effect on the half-life and the cumulated impact of a shock can be obtained in the same way as in the previous subsection.

Finally, policies and institutions might affect the magnitude of fluctuations in the aggregate output gap. Using this difference-in-difference methodology, it is possible to identify the direct effect of P on output fluctuations: in fact, if P had an impact on value-added fluctuations, one would expect this effect to be greater in EP-binding industries. For example, suppose that stringent EP dampens GDP fluctuations, then one would expect EP to reduce the difference between EP-binding and other industries in the elasticity of fluctuations of industry-specific value-added to the aggregate output gap. The same argument can be made for persistence. This would imply that the sign of the effects of EP on aggregate GDP fluctuations can be identified by substituting the logarithm of the industry-specific value-added gap for the dependent variable in equations [8] and [9] above.

2. Data

I draw value added, total earnings, average hourly wages, total hours worked and total employment from the EU KLEMS database (www.euklems.net), except for Norway, for which data come from the OECD STAN Database. Both databases are designed to provide cross-country comparable data at the industry-level that are consistent with national accounts. This allows me to have comparable data on these variables for 23 business-sector industries, 23 countries and up to 22 years (1986-2007).[8] I obtain aggregate data on these economic variables by summing them over business sector industries, which allows me to have aggregate time series that span over 29 years (1979-2007). Labour market data refer to wage and salary employees, except in the case of Norway, however, where data refer to total employment. Earnings and wage data are deflated based on private consumption deflators (drawn from the OECD EO

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7 . Assuming that $\alpha = 0$ and is precisely estimated. This condition always holds at standard confidence levels in the specifications estimated in this paper (likely because log *WGAP* has close-to-zero mean by construction).

8. The list of industries is reported in Table A1. Available countries are Australia, Austria, Belgium, Canada, Czech republic, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Korea, the Netherlands, Norway, Poland, Portugal, Slovak Republic, Spain, Sweden, the United Kingdom and the United States

database), which is preferred to the consumer price index because it is available for a larger number of countries. Results presented in the paper are however robust to changes in the deflator. Real value added is obtained by deflating nominal value added in each industry with the industry-specific double deflator. I also also use the distributions by educational attainment of earnings, wage, and hours drawn from the EUKLEMS database. Education is divided into three categories: low-education (less than upper secondary); medium education (upper secondary); and high education (more than upper secondary). Yet, in the case of data by educational attainment, the business sector is partitioned in only 9 industries for reasons of data reliability.[9] EUKLEMS data that in the original dataset are obtained through interpolation and/or estimated on the basis of conjectures, identified from Timmer *et al.* (2007), Baldwin (2009) and the related EUKLEMS documentation, were removed from the sample. The aggregate output gap comes from the OECD EO database and refers to the whole economy. In the case of Korea, due to missing data, the output gap is obtained by filtering real GDP through an HP filter with standard parameters.[10]

In terms of policies and institutions, I focus in this paper on the standard set of policy and institutional variables (henceforth, institutions for brevity) for that have been widely used in previous empirical analyses of unemployment (see *e.g.* Blanchard and Wolfers, 2000; Nickell *et al.*, 2005; Bassanini and Duval, 2006). These are: the tax wedge between labour cost and take-home pay (for a single-earner couple with two children, at average earnings levels); a summary measure of unemployment benefit generosity (an average of gross replacement rates across various earnings levels, family situations and durations of unemployment); the degree of stringency of employment protection (EP) and its subcomponent on dismissal regulations concerning permanent contracts (EPR hereafter); the ratio of the statutory minimum wage to median wage of full-time workers;[11] collective bargaining coverage rates; and the degree of centralisation/co-ordination of wage bargaining, a proxy for the concept of “corporatism” which has received widespread attention in the comparative political economy literature. I use two mutually exclusive measures of corporatism: one is drawn from Bassanini and Duval (2006) and takes only three values (low, intermediate and high), while the other is drawn from the ICTWSS database and is more detailed. I also include in most specifications the average degree of stringency of product market regulation (PMR) across seven non-manufacturing industries.[12]

In the case of the tax wedge and of unemployment benefits, more interesting indicators are available for a shorter period and I use their country average in sensitivity analyses. In particular, OECD data on net replacement rates are available since 2001. Similarly, data on marginal tax rates are available for eight income levels and family situations since 2000. These rates refer to the marginal tax of the principal earner in the following situations: single person at 167% of average earnings and no child; Single person at 100% of average earnings and no child; two-earner married couple, one at 100% of average earnings and the other at 33 %, with no child; two-earner married couple, one at 100% of average earnings and the other

9. Available industries correpond to the following ISIC codes (cf. Table A1): 10-14 and 40-41; 15-19 and 36-37; 20-28; 29-35; 45; 50-52 and 55; 60-64; 65-67; and 70-74.

10. Results are robust to using HP filtering of GDP time series to derive output gaps for all countries.

11. Comparable time-series on minimum wages are available for only the subset of countries where they are imposed by law or regulation, rather than being set by collective bargaining among social partners. These countries are Australia, Belgium, Canada, Czech republic, France, Greece, Japan, Korea, the Netherlands, Poland, Portugal, Slovak republic, Spain, the United Kingdom and the United States.

12. This PMR indicator is used because it is available over a long time-series for many OECD countries, unlike the economy-wide indicator which covers only 3 years in the period 1998-2008. One drawback is that changes in the PMR indicator for non-manufacturing industries do not incorporate all aspects of regulatory reforms that have been undertaken by a number of OECD countries in the past decades, such as administrative reforms affecting all sectors. As a result, the effects of regulatory reforms may not be fully captured by the econometric estimates presented in this paper.

at 67 %, with two children; two-earner married couple, one at 100% of average earnings and the other at 33%, with two children; single person at 67% of average earnings, with two children; one-earner married couple at 100% of average earnings, with two children; and single person at 67% of average earnings, no child. The simple average of all eight marginal rates yields a rough indicator of the level of average marginal tax rates on labour income. Furthermore, I will define hereafter the simple average of the first four in the above list as marginal tax wedge on "relatively high income levels".

Two industry benchmarks are used for the difference-in-difference analysis: the industry-specific US dismissal rate, which is drawn from Bassanini *et al.* (2010)[13] and is derived from various waves of the CPS Displaced Workers Supplement; and the industry-specific share of low-paid workers in the United Kingdom prior to the introduction of the minimum wage in 1999 obtained from quarterly UK Labour Force Surveys as the average share of low-pay workers in each industry over all available quarters between 1994 and 1998.[14] Both measures appear to be stable over time.[15]

More details on variable construction and sources and descriptive statistics are reported in the Annex. Other specific data, used as additional controls in certain specifications, are discussed in the next sections.

3. Earnings fluctuations

3.1. Aggregate analysis

To begin, I estimate the extent to which selected policies and institutions appear to amplify or mitigate the impact of output shocks on total earnings, average wages and total hours worked by fitting a simple *static* cross-country/time-series model (cf. equation [4]).[16] Results of this estimation exercise are presented in Table 1. The first line reports elasticities at the sample average. [17] For a country with average institutions, both average hourly wages and total hours of work appear to fluctuate procyclically, resulting in strong procyclicality of total earnings (with an average elasticity of 1.05). However, the elasticity of wages (0.14) is much smaller than the elasticity of hours (0.92).[18]

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13. Data available at *www.oecd.org/dataoecd/28/30/46825863.zip*.

14. In each quarter, low-paid workers are defined as those with gross hourly wages less than two-thirds of the median wage of the quarter for the whole economy.

15. Stability of the industry distribution of US dismissal rates is discussed in Bassanini *et al.* (2009). As regards the UK share of low-pay workers, Fisher and Spearman's correlation coefficients between any pair of quarters are never smaller than 0.95.

16. Institutions are included both in levels and in interaction with the output gap but, as expected, coefficients of levels are insignificant in all specifications.

17. Since institutions are included in deviation from the sample mean, the coefficient on the output gap shows average elasticities.

18. This difference is, by and large, the result of the exclusion of lagged effects. In fact, OECD (2011) shows that if longer lags are allowed in the specification, the elasticity of wages to output shocks becomes much greater. Two reasons might explain the small contribution of contemporaneous wage fluctuations. First, there is evidence that the sensitivity of employment to downturns is greater among low-paid workers (youth, low-skilled and temporary workers, see *e.g.* Abraham and Haltiwanger, 1995, OECD, 2010, Heathcote *et al.*, 2010, Robin, 2011). Therefore, given the size of the employment elasticity, the low aggregate wage elasticity might reflect a compositional effect, with the average hourly wage remaining relatively unchanged when adverse shocks drive a large numbers of youth, low-paid and temporary workers into unemployment. Second, when contracts cannot be re-negotiated each year, any short-run measure of the cyclicality of real wages tends to be dominated by changes in the consumption price deflator (see *e.g.* Messina *et al.*, 2009). Moreover, even when contracts are frequently negotiated, there is

Table 1**. Institutions and shock amplification/mitigation**

Estimated average elasticity to the output gap and estimated effect of institutions on this elasticity

	Total earnings gap				Hourly wage gap				Total hours worked gap			
	(1)		(2)		(1)		(2)		(1)		(2)	
Output gap	1.055	***	1.051	***	0.139	**	0.136	**	0.916	***	0.915	***
	(11.991)		*(11.703)*		*(2.441)*		*(2.363)*		*(14.072)*		*(13.894)*	
EP	-0.194	*	-0.196	*	-0.059		-0.081		-0.136		-0.115	
	(1.922)		*(1.872)*		*(0.853)*		*(1.075)*		*(1.404)*		*(1.150)*	
Average tax wedge	0.032	***	0.032	***	0.02	***	0.021	***	0.012		0.011	
	(3.302)		*(3.172)*		*(2.835)*		*(2.879)*		*(1.513)*		*(1.324)*	
PMR	0.034		0.039		0.009		0.012		0.026		0.027	
	(0.416)		*(0.477)*		*(0.165)*		*(0.226)*		*(0.422)*		*(0.455)*	
Bargaining coverage	-0.008	*	-0.007		0.000		-0.000		-0.008	**	-0.007*	
	(1.687)		*(1.431)*		*(0.068)*		*(0.050)*		*(2.088)*		*(1.736)*	
ARR	0.023	***	0.021	***	-0.006		-0.005		0.029	***	0.025	***
	(3.329)		*(3.345)*		*(1.000)*		*(1.038)*		*(5.278)*		*(5.789)*	
Corporatism (BD)	-0.050				0.059				-0.110			
	(0.484)				*(0.843)*				*(1.293)*			
Corporatism (ICTWSS)			-0.023				0.042				-0.065	
			(0.398)				*(1.093)*				*(1.475)*	
Level effect of institutions	Yes		Yes		Yes		Yes		Yes		Yes	
Country dummies	Yes		Yes		Yes		Yes		Yes		Yes	
Time dummies	Yes		Yes		Yes		Yes		Yes		Yes	
Observations	**449**		**449**		**449**		**449**		**449**		**449**	
R-squared	**0.654**		**0.652**		**0.225**		**0.224**		**0.702**		**0.701**	

Note: In the first row the table reports the elasticity to the output gap for each dependent variable, estimated at the sample average of each institution. The other rows report the estimated effect of a one unit change of each institution on this elasticity. The term gap indicates the log difference between actual and trend values. EP: Employment Protection, measured on a 0-6 scale. PMR: Product Market Regulation (time-varying index), measured on a 0-6 scale. Two mutually exclusive measures of corporatism are included: BD: Bassanini and Duval index, measured on a 1-3 scale; ICTWSS: ICTWSS index measured on a 0-5 scale. All other variables are measured in percentages. ARR: Average gross unemployment benefit replacement rate. Absolute values of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively.

In the other lines, Table 1 shows baseline aggregate estimates of the amplification/mitigation effects of included institutions.[19] A positive coefficient implies that the policy significantly amplifies output shocks, while a negative sign means that the policy exerts a smoothing effect on output fluctuations. The tax wedge and the generosity of unemployment benefits unambiguously amplify the impact of output-gap fluctuations on total annual earnings. Taken at face value, estimates imply that a 5 percentage-point increase in average replacement rates – that is, about one standard deviation of the distribution, considering only time series variation – from the OECD average raises the elasticity of the total earnings gap to the output gap by between 0.10 and 0.12 (that is, about a 10% increase), depending on the specification. Consistent with previous findings (Blanchard and Wolfers, 2000, Bassanini and Duval, 2006), this effect appears to be entirely due to the fact that, *ceteris paribus*, the employment impact of shocks tends to be

evidence that nominal wages tend to be rigid both downward and upward, so that adjustments are delayed for several periods, particularly in times of low inflation when these rigidities bind (see in particular Elsby, 2009). This issue is not analysed further here, but must be kept in mind in interpreting the results.

19 . In order to preserve sample size, statutory minimum wages, which are available only for few countries, are not included in the specification. I perform a specific analysis of the minimum wage in Section 3.3.

larger in countries where unemployment benefits are more generous, while unemployment benefits do not appear to affect wage cyclicality.[20]

Two mechanisms might explain the amplification effect of benefit generosity on unemployment fluctuations. First, a number of empirical studies suggest that longer durations of generous benefits tend to reduce job-search effort and make the unemployed more choosy about job offers, thereby lengthening the duration of unemployment spells (see *e.g.* OECD, 2006; Boeri and van Ours, 2008 for surveys), although a few recent studies have questioned these results.[21] Statistically, this would imply that in the year in which an adverse shock occurs, those who become redundant would remain in the unemployment pool longer, thereby dampening further average employment in that year (and possibly in subsequent years). Second, generous unemployment benefits might reduce workers' resistance to job loss, making them less inclined to challenge dismissals in courts. In fact, Bassanini *et al.* (2010) show that dismissals leading to unemployment spells are more common in countries with generous unemployment benefits.

Gross replacement rates are used for reasons of time-series availability but net rates would be more meaningful from a theoretical point of view. If an interaction between the 2001-2007 country average of *net* replacement rates and the output gap is also included, the effect of gross rates becomes insignificant while that of net rates is significant at the 1% level (see Table 2). This suggests, not surprisingly, that net rather than gross unemployment benefits are responsible for the amplification of business-cycle fluctuations.

By contrast, the effect of the average tax wedge on labour income appears to be essentially due to its role in amplifying gross wage fluctuations, while no significant impact on employment fluctuations is detected. One possible explanation of this finding could be that average tax wedges are higher in countries where marginal tax wedges are more progressive. In turn, progressive labour taxes make labour supply more inelastic and/or the wage-setting curve steeper (see *e.g.* Guo and Lansing, 1998; Dromel and Pintus, 2008), at least when the latter is defined in terms of gross wages, thereby facilitating wage adjustments (and, possibly, restraining employment adjustments) whenever firms need to compress unit labour costs. In this interpretation, the effect of the average tax wedge would reflect the impact of the marginal tax wedge, which is omitted from the main empirical specifications due to lack of data on marginal tax rates for the whole time period under examination. This explanation is supported by the strong correlation between average tax wedge and the marginal tax wedge on relatively high income levels in the eight years for which both are available (correlation coefficient 0.64). I therefore test this explanation by including in the regressions, as an additional covariate, the interaction between the output gap and country-specific averages of the indicator of the marginal tax rate for higher income levels computed for the period for which it is available. Consistent with the above interpretation, the marginal tax wedge on relatively high income appears, in this specification, to amplify the wage effect of a shock and conditional on the inclusion

20. Some caution is in order here because, due to composition effects (see above), the effect of an institution on the elasticity of wage fluctuations might be biased whenever the same institution has a strong effect on the elasticity of employment adjustments.

21. Recent findings suggest that one needs to be cautious about the interpretation of the empirical relationship between benefit generosity and the duration of unemployment spells. For example, using Austrian data, Card *et al.* (2007) argue that unemployment exit spikes at benefit exhaustion are mainly due to leaving the unemployment system and becoming inactive rather than to job-finding. Using US time-use data, Krueger and Mueller (2010) show that there is not much difference in average job-search effort between UI eligible and non-eligible job seekers, but the profile of job-search intensity of the former depends on time to benefit exhaustion. Moreover, the effect of unemployment insurance on search effort seems to be confined only to those job seekers that are liquidity-constrained, whom UI enables to smooth consumption and thus reduces the pressure to rush back to work (Chetty, 2008). By contrast, those with access to a secondary income source are more likely to maintain consumption during a spell of unemployment and thus are less responsive to unemployment benefits.

of the marginal rate, the estimated coefficient of the interaction between the output gap and the average tax wedge becomes insignificant (Table 2, Panel B).[22]

Table 2. **Institutions and amplification/mitigation of shocks, additional results on unemployment benefits and the tax wedge**

Panel A. **Amplification effect of unemployment benefits on the total earnings gap**			Panel B. **Amplification effect of the tax wedge on the hourly wage gap**		
	(1)	**(2)**		**(1)**	**(2)**
Output gap	1.061 ***	1.06 ***	Output gap	0.104 *	0.105 *
	(12.081)	*(11.791)*		*(1.850)*	*(1.843)*
ARR (gross)	0.013 *	0.011	Average tax wedge	0.005	0.006
	(1.743)	*(1.500)*		*(0.751)*	*(0.923)*
ARR (net)	0.013 ***	0.013 **	Marginal tax wedge on	0.041 ***	0.041 ***
	(2.631)	*(2.521)*	high incomes	*(4.452)*	*(4.407)*
Country dummies	Yes	Yes	Country dummies	Yes	Yes
Time dummies	Yes	Yes	Time dummies	Yes	Yes
Observations	**449**	**449**	**Observations**	**449**	**449**
R-squared	**0.659**	**0.657**	**R-squared**	**0.263**	**0.263**

Note: the table reports the estimated effect of the output gap on each dependent variable at the sample average of each institution as well as the effect of a change of each institutions on this effect. The term gap indicates the log differences between actual and trend values. : ARR: Average unemployment benefit replacement rate; net rates are 2001-2007 averages. The marginal tax wedge on high income is the simple average of the marginal tax rate of the principal earners in the four following situations: single person at 167% of average earnings and no child; single person at 100% of average earnings and no child; two-earner married couple, one at 100% of average earnings and the other at 33 %, with no child; and two-earner married couple, one at 100% of average earnings and the other at 67 %, with two children. In addition this tax wedge is averaged over 2000-2007. All specifications include the institutions reported in Table 1 as well as their interaction with the output gap. The BD and ICTWSS indexes of corporatism are used in Column 1 and 2, respectively. All variables are measured in percentages. Absolute values of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively.

These results appear to be reasonably confirmed by the estimation of dynamic models allowing institutions to affect the degree of persistence in the labour market (cf. equation [9] above). Indeed, both the tax wedge and the replacement rate are significant at least at the 10% level in the preferred specification, that is excluding country fixed effects[23] and insignificant interactions between institutions and the lagged dependent variable (columns 2 and 4 of Table 3).[24] Yet, neither the tax wedge nor the replacement rate appears to have any effect on the persistence of macroeconomic shocks.

By contrast, employment protection (measured through the overall indicator of EP stringency) appears to increase the persistence of the effect of shocks on earnings while having a mitigating effect on their short-run impact. Indeed, there is a large theoretical literature suggesting that firms' optimal behaviour in the presence of positive firing costs is to compress both job creation and destruction at any stage of the business-cycle (see Bentolila and Bertola, 1990, Bertola, 1990 and Mortensen and Pissarides, 1999). To the extent that EP shelters insiders against the risk of job loss, they can also resist downward adjustment of wages after an adverse shock (Bertola and Rogerson, 1997, Bertola, 1999). However, aggregate estimates

22. The estimated coefficients for both the tax wedge and the replacement rate are robust to the exclusion of other co-variates as well as the exclusion of countries one-by-one (results available from the author upon request).

23. Note that similar estimates for the coefficient of the lagged dependent variable are obtained with or without the inclusion of country fixed effect. As suggested by Angrist and Pischke (2009), this is an indication that estimates obtained without including fixed effects are consistent and more efficient.

24. Stringent anti-competitive product market regulation also appears to amplify the effect of shocks, according to dynamic models, but this is not detectable in static models. Therefore, no robust conclusion can be drawn from these estimates on the effect of these regulations on the transmission of shocks to the labour market.

presented in Table 1 are inconclusive as regards whether the mitigating impact of EP is mainly due to a wage or an employment effect. In the next subsection, I will refine the identification strategy, which will allow us to get sharper conclusions on employment protection, even though only as regards dismissal regulations.

Table 3. **Institutions and the persistence of the effects of shocks on the total earnings gap**

Cross-country/time-series aggregate estimates, dynamic specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lagged dep. variable (LDV)	-0.380 ***	-0.382 ***	-0.374 ***	-0.380 ***	-0.367 ***	-0.364 ***	-0.362 ***	-0.360 ***
	(9.412)	*(10.339)*	*(9.532)*	*(10.083)*	*(8.567)*	*(9.232)*	*(8.640)*	*(9.172)*
EP x LDV	0.111 ***	0.070 ***	0.146 ***	0.069 ***	0.106 ***	0.070 ***	0.127 ***	0.071 ***
	(3.404)	*(3.288)*	*(3.812)*	*(3.329)*	*(3.369)*	*(3.360)*	*(3.272)*	*(3.419)*
Average tax wedge x LDV	-0.002		-0.005		-0.002		-0.003	
	(0.532)		*(1.174)*		*(0.485)*		*(0.832)*	
PMR x LDV	-0.012		-0.010		-0.011		-0.010	
	(0.301)		*(0.248)*		*(0.278)*		*(0.251)*	
Bargaining coverage x LDV	-0.001		-0.001		-0.001		-0.001	
	(0.623)		*(0.324)*		*(0.488)*		*(0.354)*	
ARR x LDV	0.002		0.001		0.002		0.001	
	(0.501)		*(0.362)*		*(0.529)*		*(0.291)*	
Corporatism (BD) x LDV	-0.054				-0.048			
	(1.019)				*(0.844)*			
Corporatism (ICTWSS) x LDV			-0.055 *				-0.036	
			(1.697)				*(1.040)*	
Output gap	0.722 ***	0.707 ***	0.720 ***	0.709 ***	0.769 ***	0.749 ***	0.764 ***	0.749 ***
	(11.301)	*(11.297)*	*(11.261)*	*(11.367)*	*(11.193)*	*(11.245)*	*(10.965)*	*(11.006)*
EP x Output gap	-0.141 **	-0.119*	-0.149 **	-0.122 *	-0.122 *	-0.095	-0.137 *	-0.112
	(-2.262)	*(-1.865)*	*(2.313)*	*(1.810)*	*(1.792)*	*(1.408)*	*(1.897)*	*(1.521)*
Average tax wedge x output gap	0.016 **	0.015 **	0.015 **	0.014 **	0.010	0.008	0.010	0.008
	(2.578)	*(2.297)*	*(2.438)*	*(2.191)*	*(1.499)*	*(1.269)*	*(1.525)*	*(1.304)*
PMR x Output gap	0.170 ***	0.164 ***	0.183 ***	0.177 ***	0.144 **	0.135 **	0.143 **	0.136 **
	(2.742)	*(2.694)*	*(3.011)*	*(2.971)*	*(2.311)*	*(2.232)*	*(2.342)*	*(2.279)*
Bargaining coverage x Output gap	-0.003	-0.004	-0.004	-0.004	-0.003	-0.004	-0.004	-0.004
	(0.937)	*(1.229)*	*(1.166)*	*(1.384)*	*(0.901)*	*(1.197)*	*(1.112)*	*(1.352)*
ARR x Output gap	0.007	0.009*	0.007	0.008 *	0.009 *	0.010 **	0.009 *	0.009 *
	(1.446)	*(1.738)*	*(1.522)*	*(1.682)*	*(1.755)*	*(2.025)*	*(1.871)*	*(1.961)*
Corporatism (BD) x Output gap	-0.007	-0.035			-0.022	-0.053		
	(0.110)	*(0.547)*			*(0.312)*	*(0.804)*		
Corporatism (ICTWSS) x Output gap			0.011	-0.011			0.003	-0.013
			(0.285)	*(0.299)*			*(0.079)*	*(0.348)*
Country dummies	No	No	No	No	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	**447**	**447**	**447**	**447**	**447**	**447**	**447**	**447**
R-squared	**0.689**	**0.685**	**0.693**	**0.686**	**0.715**	**0.713**	**0.717**	**0.713**

Note: The table reports the coefficient of the lagged dependent variable and of the output gap estimated at the sample average of each institution as well as the estimated effect of a one unit change of each institution on these coefficients. The term gap indicates the log differences between actual and trend values of total earnings. EP: Employment Protection, measured on a 0-6 scale. PMR: Product Market Regulation (time-varying index), measured on a 0-6 scale. Two mutually exclusive measures of corporatism are included: BD: Bassanini and Duval index, measured on a 1-3 scale (Panel A); ICTWSS: ICTWSS index measured on a 0-5 scale (Panel B). All other variables are measured in percentages. ARR: Average gross unemployment benefit replacement rate. All specifications control for the linear effect of institutions. Absolute values of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively.

3.2. Industry-level difference-in-difference analysis: Dismissal regulation

As pointed out in Section 1.2, in the case of dismissal rules for permanent workers (EPR hereafter), it is possible to improve upon the estimation strategy by following an industry-level difference-in-difference approach. The identification strategy is based on the assumption that the effect of EPR on the responsiveness of hours and wages to aggregate business-cycle shocks varies across industries and that these regulations are more likely to be binding in industries with a greater dismissal propensity (EP-binding industries). In order to reduce bias due to the possible relationship between EPR stringency and the cross-industry distribution of dismissals, I identify EP-binding industries based on dismissal rates by industry in the United States (*i.e.* the least regulated country).

This approach has become increasingly popular in the literature concerning the impact of EP on several performance variables (see *e.g.* Micco and Pages, 2006; Haltiwanger *et al.*, 2008; Bassanini *et al.*, 2009; Cingano *et al.*, 2010; and Subramanian and Megginson, 2011). However, when the analysis focuses on the cyclical fluctuations induced by aggregate shocks, this intuitive identification strategy might be problematic. In fact, those industries that do not usually adjust through dismissals in normal times might disproportionately increase their dismissal rate during severe recessions, at least in the absence of regulations, so that the identification strategy might not be appropriate in bad times. For the identification strategy to be valid, one must assume that, in the absence of regulations, dismissals in EP-binding industries are no less anti-cyclical than in other industries. This assumption can be tested using industry data for the United States by estimating the following (difference-in-difference) specification:

$$\log WGAP_{jt} = \beta B_j OGAP_t + \eta_t + \eta_j + \varepsilon_{ijt}$$

where log *WGAP* is the industry-specific time-varying dependent variable, *OGAP* is the aggregate output gap, *j* and *t* index industry and time, respectively, *B* is the industry-level average of dismissal rates, ηs are fixed effects that capture all industry-specific time-invariant effects and aggregate time-varying factors, including nation-wide institutional reforms and the average effect of the output gap, and ε is an error term capturing idiosyncratic shocks that are unrelated to the business cycle. A positive sign of β implies greater pro-cyclicality of high dismissal industries.

Results of this exercise are presented in Table 4. In the United States, dismissal-intensive industries appear to have more counter-cyclical dismissal rates and more pro-cyclical employment, total hours worked and real value added than non-binding industries. These findings suggest that the identification assumption spelled out above is unlikely to be weaker in bad times.

Table 4. **Effect of dismissal intensity on the amplification/mitigation of shocks in the United States.**

	Value added (volume)	Total earnings	Hourly wages	Hours worked	Dependent employment	Dismissals
output gap X avg.	53.66 ***	30.56 ***	1.589	28.97 ***	29.98 ***	-22.77 **
dismissal rate	*(4.654)*	*(3.810)*	*(0.382)*	*(4.340)*	*(4.588)*	*(2.059)*
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	**462**	**462**	**462**	**462**	**462**	**143**
R-squared	**0.120**	**0.367**	**0.441**	**0.561**	**0.523**	**0.686**

Note: All dependent variable are gaps between the log of the actual and trend values of each variable. The average dismissal rate is expressed as the average of the ratio of dismissals to dependent employment for each available year (1996-2006, even years). Its global average is 0.0518. Absolute value of robust t-statistics in parentheses. ***, **: statistically significant at the 1% and 5% level, respectively.

Panel A of Table 5 presents baseline results obtained by estimating the simplest static difference-in-difference model corresponding to equation [8]. Column 1 includes only country and industry dummies and, for identification purposes, controls for the direct effect of the output gap and EPR (not shown in the table). This specification provides a useful benchmark to check that the inclusion of time dummies, by sweeping away common cyclical components, does not alter our estimates of the amplification/mitigation effect of dismissal regulations. The disadvantage of this specification is, obviously, that it is potentially affected by omitted variable biases. By contrast, in all subsequent columns, specifications include country-by-time dummies, thereby controlling for all aggregate effects, including the direct effect of the output gap and EPR. The specification corresponding to Column 2 controls only for aggregate country-specific time-varying factors but not for industry-specific trends or for time-invariant heterogeneity across countries and industries, which is done in Columns 3-5. Column 6 replicates the exercise reported in Column 5 in first differences, thereby looking at the effect of EPR on the elasticities of changes in log industry earnings to changes in the aggregate output gap. Results are remarkably consistent across specifications and confirm the shock-mitigating short-run impact of EP. Taken at face value, a one-point increase in EPR (approximately corresponding to half of the distance between the United States and the OECD average) would imply a reduction of between 15% and 17% in the elasticity of total earnings to the output gap.[25]

Table 5. **Industry-level total earnings fluctuations and dismissal regulations.**

Panel A. **Static models**	(1)	(2)	(3)	(4)	(5)	(6)
Output gap	-0.822 ***					
	(2.757)					
Output gap x DR	34.833 ***	34.939 ***	35.869 ***	36.752 ***	38.662 ***	33.449 ***
	(6.709)	*(7.310)*	*(7.506)*	*(7.436)*	*(7.818)*	*(5.428)*
EPR x DR x output gap	-5.343 **	-5.380 ***	-5.889 ***	-5.699 ***	-6.355 ***	-5.621 **
	(2.400)	*(2.654)*	*(3.075)*	*(2.738)*	*(3.250)*	*(2.311)*
Country dummies	Yes	No	No	No	No	No
Industry dummies	Yes	Yes	No	No	No	No
Country x time dummies	No	Yes	Yes	Yes	Yes	Yes
Industry X time dummies	No	No	Yes	No	Yes	Yes
Country x industry dummies	No	No	No	Yes	Yes	No
Observations	**8,604**	**8,604**	**8,604**	**8,604**	**8,604**	**8,582**
R-squared	**0.229**	**0.406**	**0.478**	**0.417**	**0.488**	**0.336**

Panel B. **Dynamic models**	(1)	(2)	(3)	(4)	(5)	(6)
Lagged dep. variable	-0.581 ***	-0.567 ***	-0.596 ***	-0.578 ***	-0.564 ***	-0.593 ***
	(16.787)	*(15.526)*	*(16.565)*	*(16.422)*	*(15.080)*	*'(16.234)*
Output gap x DR	23.439 ***	26.532 ***	24.061 ***	31.308 ***	30.168 ***	29.869 ***
	(5.704)	*(6.474)*	*(5.660)*	*(5.411)*	*(5.360)*	*(5.059)*
EPR x DR x output gap	-3.529 **	-4.444 ***	-3.709 **	-5.249 **	-4.710 *	-5.219 **
	(2.208)	*(2.677)*	*(2.305)*	*(2.179)*	*(1.932)*	*'(2.175)*
Lagged output gap x DR				-10.005 *	-5.678	-7.709
				(-1.688)	*(-0.960)*	*(-1.271)*
Lagged EPR x DR x output gap				1.770	0.464	1.724
				(0.727)	*(0.191)*	*(0.711)*
Country dummies	No	No	No	No	No	No
Industry dummies	No	No	No	No	No	No
Country x time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry X time dummies	Yes	No	Yes	Yes	No	Yes
Country x industry dummies	No	Yes	Yes	No	Yes	Yes
Long-term coeff. of EPR x output gap	-6.074	-7.831	-6.227	-6.023	-7.528	-5.891
long-term EPRB coeff.: p-value	0.0264	0.00768	0.0204	0.0432	0.0171	0.0439
Observations	**8,582**	**8,582**	**8,582**	**8,582**	**8,582**	**8,582**
R-squared	**0.540**	**0.513**	**0.563**	**0.541**	**0.513**	**0.563**

Note: The dependent variable is the industry-specific gap between the logs of actual and trend total earnings. DR: average US Dismissal rate, by industry (average = 0.0518). EPR: employment protection for regular contracts. Other interactions required for identification are included. All variables are in levels except in Column 6 of Panel A where they are in first-differences. Absolute values of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively.

These results are broadly confirmed by dynamic specifications reported in Panel B of Table 5, which allow for persistence.[26] In particular, two remarks are in order: i) the percentage impact of EPR on the

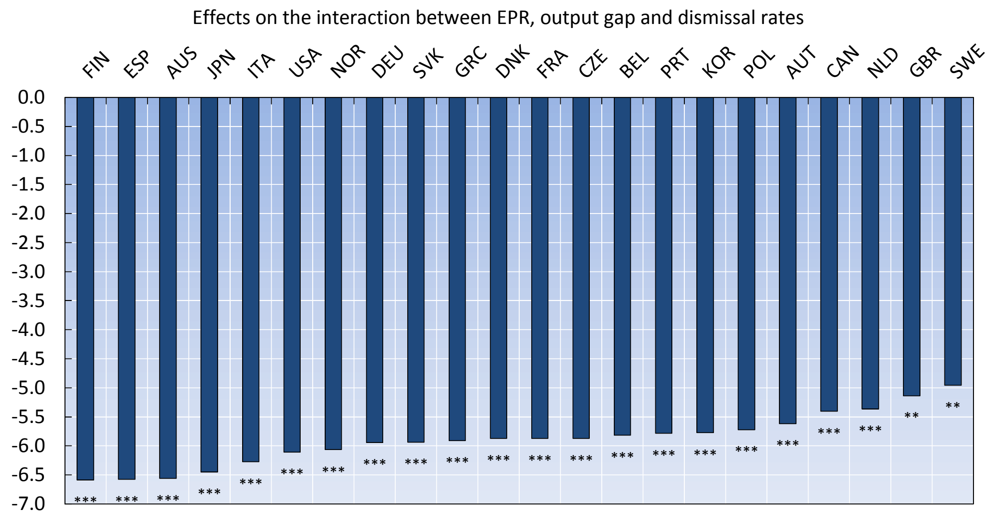
25. Remember that, as discussed in Section 1.2, one can obtain the estimated percentage impact on the elasticty by multiplying by 100 the ratio between the third and the second row of Panel A of Table 5.

26. In addition, Columns 4 to 6 also allow for a one-period lagged impact of aggregate shocks, which is assumed to vary as a function of layoff-intensity and EPR. This is important given that the evidence

elasticity of industry-level earnings fluctuations to the aggregate gap is close to that estimated through static models; and ii) the long-run impact of EPR on this elasticity – that is the effect that would be realized if the shocks were permanent rather than transitory is somewhat larger than what is obtained in static models – but the difference does not appear significant. Interestingly, the coefficient of the lagged dependent variable is very precisely estimated and shows no variation across models, which suggests that omitting country-by-industry fixed effects is preferable (see Angrist and Pischke, 2009). This does not appear surprising insofar as these fixed effects are in general insignificantly different from zero. Indeed, if this were not the case, this would imply that certain countries/years have a gap persistently greater or smaller than zero, which is ruled out by the fact that gaps are obtained through HP filtering.

Insofar as employment protection does not vary much over-time, cross-country variation is key to identify the effect. We might therefore worry that specific countries might drive the results. I therefore drop countries one by one and re-estimate my specifications. Figure 1 reports results for my preferred specification (Column 3 of Panel A). It appears that no significant differences emerge as regards the coefficient of the interaction between EPR, the output gap and the US dismissal rate.

Figure 1. **Sensitivity analysis of the mitigation effect of EP when countries are excluded one-by-one from the sample**



Note: The figure refers to the specification reported in Columns 3 of Panel A of Table 5. Coefficient estimates are obtained by excluding indicated countries one-by-one. ***, **: statistically significant at the 1%, and 5% level, respectively.

I argued that one of the key advantages of our difference-in-differences approach is that it allows us to control for other aggregate confounding factors, including other institutions and policies, some of which are not easy to quantify. This claim is correct provided that there is no reason to believe that the impact of aggregate institutions on earnings and employment fluctuations varies, on average, between EP-binding and other industries and/or proportionally to the industry layoff propensity. For institutions that have no direct bearing on layoffs, it is difficult to think of convincing reasons for such a differential impact. Yet, can we provide stronger evidence that this is the case? In order to do so, I augment my preferred specification with interactions between our quantitative indicator of layoff propensity and the aggregate indicators of labour market institutions and product market regulations, which are typically used in

suggests that the measured elasticity of total earnings to aggregate shocks is greater if shocks are allowed to have a delayed impact on the labour market. In these models persistence is assumed to be the same across countries and industries, except for the one-period lagged effect of the shock. See below for a more general treatment of the possible effect of EPR on persistence.

aggregate unemployment equations and which we already considered in Section 3.1.[27] Table 6 shows that our estimates are sensitive to the simultaneous inclusion of all these institutions interacted with dismissal rates (Column 1 and 2). However, sequential elimination of the least significant co-variates leads to a specification in which only EPR is significant, suggesting that the insignificance of this variable in the first columns is mainly due to a multicollinearity problem.[28]

Countries that have stringent dismissal regulations typically have rigid legislation for hiring on temporary contracts. Therefore, one can ask whether the estimated effect of EPR in Table 5 is not in fact due to regulation for temporary contracts. In order to check for this, I include the index of employment protection concerning regulations for temporary contracts (EPT) interacted with the output gap and US dismissal rates in Column 7 of Table 6. The estimated effect turns out to be even greater for EPR, thereby confirming our previous results.[29]

Short-time work schemes, which played a key role in mitigating the labour market effects of the 2008/09 recession, are typically more intensively used in countries with stringent EP (see Hijzen and Venn, 2010). Unfortunately, cross-country comparable data on these schemes for most OECD countries are available only since 2007. In order to check that observed mitigation effects of EPR are not due to these schemes, I perform two alternative sensitivity exercises. Insofar as many countries that did not have such schemes introduced them only after the onset of the 2008/09 crisis, I include a dummy for existence of a scheme in 2007 (interacted with output gap and US dismissal rates), which is a noisy indicator of the existence of short-time work schemes during the whole period of analysis. The disadvantage of this indicator is that it does not take into account that take-up rates differ markedly across schemes and over time (see Hijzen and Venn, 2010). As an alternative exercise, I therefore exclude all countries that had already implemented a short-time work scheme by 2007. In both cases, estimates of the mitigating effect of EPR appear robust (Table 6, Columns 8 and 9), suggesting that the omission of an adequate control for such short-time work schemes does not impair the reliability of results presented in Table 5.

27. Insofar as these controls are only used to check that the estimate of the relevant coefficient for EPR are not due to confounding factors, and EPR and product market regulation (PMR) are especially correlated in cross-section, I prefer to include here the aggregate PMR indicator for 1998, which is based on all industries and aspects of anti-competitive regulation.

28. It is also reassuring that none of these covariates turn out significant if included in the specification without including EPR (results available from the author upon request).

29. Care is however required in interpreting these results, insofar as the EPT indicator does not capture cross-country differences in enforcement of regulations. In fact, EP is typically enforced by individuals who consider themselves as victims and lodge a complaint with the competent tribunals or courts. In the case of dismissals, potential plaintiffs are easily identified and able to react, whereas victims of breaches of rules on temporary contracts (particularly in the case of violations of hiring restrictions under such contract) are much less likely to make a complaint. As a consequence, enforcement problems are particularly important in the case of EPT (see Bassanini *et al.*, 2010, for an extensive discussion). For this reason, in another specification, I also include the trend aggregate share of temporary workers as a substitute for EPT, which arguably capture all determinants of temporary contracts. Results for EPR remain broadly unchanged (available from the author upon request).

Table 6. **Industry-level total earnings fluctuations and dismissal regulations, including additional controls.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
EPR x DR x output gap	-3.131	-3.299	-3.099	-5.705 *	-4.576 *	-5.441 **	-7.381 ***	-5.864 ***	-9.690 **	-9.774 **
	(1.013)	*(1.038)*	*(0.996)*	*(1.891)*	*(1.813)*	*(2.157)*	*(3.501)*	*(3.045)*	*(2.440)*	*(2.232)*
Corporatism (BD) x DR x output gap	0.405									
	(0.132)									
PMR x DR x output gap	-8.102	-9.450 *	-7.725	2.509						
	(1.345)	*(1.686)*	*(1.501)*	*(0.546)*						
ARR x DR x output gap	0.244	0.231	0.264	0.138	0.120					
	(1.027)	*(1.090)*	*(1.267)*	*(0.660)*	*(0.593)*					
Tax wedge x DR x output gap	0.237	0.246	0.229							
	(0.853)	*(0.890)*	*(0.824)*							
Corporatism (ICTWSS) x DR x output gap		1.271								
		(0.700)								
Bargaining coverage x DR x output gap	-0.163	-0.170	-0.166	-0.071	-0.073	0.005				
	(1.209)	*(1.283)*	*(1.276)*	*(0.611)*	*(0.633)*	*(0.057)*				
EPT x DR x output gap							2.301 *			
							(1.717)			
STW scheme x DR x output gap								1.876		
								(0.514)		
Wage indexation x DR x output gap										-0.043 ***
										(5.027)
Country dummies	No	No	No	No	No	No	No	No	No	No
Industry dummies	No	No	No	No	No	No	No	No	No	No
Country x time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country x industry dummies	No	No	No	No	No	No	No	No	No	No
Observations	**7,042**	**7,042**	**7,042**	**7,394**	**7,394**	**7,526**	**8,604**	**8,604**	**2,354**	**2,966**
R-squared	**0.521**	**0.521**	**0.521**	**0.503**	**0.503**	**0.504**	**0.478**	**0.478**	**0.536**	**0.467**

Note: The dependent variable is the industry-specific gap between the logs of actual and total earnings. EPR: employment protection for regular contracts. EPT: employment protection for temporary contracts. PMR: Product Market Regulation (aggregate 1998 index). BD: Bassanini and Duval index. ICTWSS: ICTWSS index. ARR: Average gross unemployment benefit replacement rate. STW Scheme: dummy for short-time scheme in 2007. Wage indexation: percentage of firms covered by an indexation scheme in 2005-2008. In Column 9 countries with short-time scheme in 2007 are excluded. DR: average industry-specific US dismissal rate. Other interactions required for identification are included. All variables are in levels. Absolute values of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively

Finally, there is evidence that, in countries with restrictive dismissal regulations, firms with a larger share of permanent workers and/or a greater share of blue-collar and low-skilled white-collar workers tend to have more rigid wage-setting schemes (Babecký *et al.* 2009, 2010). In order to check that our results are not due to the correlation between EPR and wage-rigidity, I include the percentage of firms covered by a wage indexation scheme in 2005-2008 (the only period for which data are available) interacted with US dismissal rates and the output gap.[30] Reassuringly, the inclusion of this additional control does not reduce the estimated effect of EPR (Table 6, Column 10).

Is the mitigation effect of dismissal restrictions on the transmission of output shocks due mitigation of employment or hourly wage fluctuations? Table 7 looks at this issue in detail by estimating separate equations for average hourly wages, total hours worked and employee headcounts. While no significant effect emerge as regards hourly wages, a strong impact is estimated for employee headcounts, with a one unit increase in the EPR indicator from the OECD average leading to about a 35% reduction in the elasticity of employment headcounts to aggregate output shocks. Interestingly, the effect on total hours fluctuations is insignificantly different and even slightly smaller, suggesting that all the effect of EPR is concentrated in retaining workers into their jobs during downturns, consistent with theoretical priors.

Up to this point, I have implicitly assumed that policies and institutions do not affect the magnitude of the fluctuations of aggregate output. In general, this requires some caution in interpreting the quantitative estimates presented so far, because a policy could have opposite effects on output and the labour market transmission of output fluctuations. From a qualitative point of view, the impact of EP on the output gap can be examined using the same methodology as above. If EP does have a mitigating impact on the output gap, one would expect this impact to be greater in EP-binding industries (see also Section 1.2). Evidence presented in Table 8 suggests that in countries with stringent EP, an aggregate GDP swing would translate in smaller differences in value-added fluctuations between EP-binding and other industries. In other words, EP for regular contracts appears to have a mitigation effect both on output and on the transmission of output fluctuations to the labour market. Overall, this suggests that we can consider the shock-mitigation effect of EP for regular contracts derived from Table 5 as providing a lower-bound estimate of the true effect.

30. Data on wage indexation were collected the Eurosystem's Wage Dynamics Network (WDN) coordinated by the European Central Bank (ECB), which administered in 2007 and 2008 a firm-level survey including, *inter alia*, questions concerning wage-adjustment procedures in use in the firm with reference to the last years before the survey (see Druant *et al.*, 2009, and Babecký *et al.*, 2009, for more details). The ECB and the WDN network kindly made available these data at the level of country-by-industry cells (with the business sector being disaggregated into 5 industries). The analysis made here assumes that the frequency of each policy in the survey period is representative of the true frequency in the past 20 years.

Table 7. **Industry-level wage and employment fluctuations and dismissal regulations.**

Panel A. **Hourly wage**

	(1)	(2)	(3)	(4)
Output gap x DR	-0.219	-0.092	-0.439	-0.807
	(0.182)	*(0.061)*	*(0.358)*	*(0.519)*
EPR x DR x output gap	1.698	1.175	1.693	1.188
	(1.128)	*(0.760)*	*(1.112)*	*(0.752)*
Country dummies	No	No	No	No
Industry dummies	Yes	No	No	No
Country x time dummies	Yes	Yes	Yes	Yes
Industry x time dummies	No	Yes	No	Yes
Country x industry dummies	No	No	Yes	Yes
Observations	**8,582**	**8,582**	**8,582**	**8,582**
R-squared	**0.212**	**0.262**	**0.227**	**0.276**

Panel B. **Hours worked**

	(1)	(2)	(3)	(4)
Output gap x DR	23.869 ***	23.562 ***	25.202 ***	26.031 ***
	(16.003)	*(13.051)*	*(16.102)*	*(14.317)*
EPR x DR x output gap	-7.113 ***	-7.061 ***	-7.416 ***	-7.520 ***
	(3.604)	*(3.627)*	*(3.680)*	*(3.826)*
Country dummies	No	No	No	No
Industry dummies	Yes	No	No	No
Country x time dummies	Yes	Yes	Yes	Yes
Industry x time dummies	No	Yes	No	Yes
Country x industry dummies	No	No	Yes	Yes
Observations	**8,582**	**8,582**	**8,582**	**8,582**
R-squared	**0.380**	**0.444**	**0.391**	**0.457**

Panel C. **Employee headcount**

	(1)	(2)	(3)	(4)
Output gap x DR	21.361 ***	21.017 ***	22.226 ***	22.669 ***
	(14.030)	*(10.736)*	*(13.735)*	*(10.956)*
EPR x DR x output gap	-6.940 ***	-7.430 ***	-7.157 ***	-7.728 ***
	(3.696)	*(3.977)*	*(3.726)*	*(4.092)*
Country dummies	No	No	No	No
Industry dummies	Yes	No	No	No
Country x time dummies	Yes	Yes	Yes	Yes
Industry x time dummies	No	Yes	No	Yes
Country x industry dummies	No	No	Yes	Yes
Observations	**8,604**	**8,604**	**8,604**	**8,604**
R-squared	**0.346**	**0.410**	**0.362**	**0.425**

Note: The dependent variables are industry-specific gaps between logs of actual and trend values. EPR: employment protection for regular contracts. DR: average industry-specific US dismissal rate. Other interactions required for identification are included. All variables are in levels. Absolute values of robust t-statistics in parentheses. ***: statistically significant at the 1% level.

Table 8. **Industry-level real value added fluctuations and dismissal regulations.**

	(1)	(2)	(3)	(4)
Output gap x DR	26.326 ***	24.059 ***	27.977 ***	26.704 ***
	(11.966)	*(8.839)*	*(12.054)*	*(9.220)*
EPR x DR x output gap	-8.127 ***	-8.395 ***	-8.272 ***	-8.779 ***
	(3.481)	*(3.774)*	*(3.427)*	*(3.834)*
Country dummies	No	No	No	No
Industry dummies	Yes	No	No	No
Country x time dummies	Yes	Yes	Yes	Yes
Industry x time dummies	No	Yes	No	Yes
Country x industry dummies	No	No	Yes	Yes
Observations	**8,194**	**8,194**	**8,194**	**8,194**
R-squared	**0.15**	**0.239**	**0.163**	**0.253**

Note: The dependent variable is the industry-specific gap between the logs of actual and trend value added (in volume terms). EPR: employment protection for regular contracts. DR: average industry-specific US dismissal rates. Other interactions required for identification are included. All variables are in levels. Absolute values of robust t-statistics in parentheses. ***: statistically significant at the 1% level.

As already discussed, an adverse shock might not only compress earnings and reduce employment. Its effects might also persist over time, and the degree of persistence is likely to be affected by policies and institutions. Aggregate dynamic models presented in Table 3 above, where the speed of shock re-absorption is assumed to depend on policies and institutions, suggest that EP significantly affects the persistence of shocks. The impact of EP for regular contracts on persistence is confirmed by industry-level analysis, using again a difference-in –difference approach (see Section 1.2). Indeed, Table 9 shows that more stringent dismissal regulation is associated with greater persistence of total earnings in industries where US dismissal rates are greater. Taken at face value, these estimates imply that the half-life of the effect of a temporary macroeconomic shock on total earnings would be 13% smaller in a country where the EPR indicator is one unit below the OECD average, than in an average OECD country. These estimates also appear robust to changes in model specifications, in particular to the inclusion of additional institutional controls (Table 10).[31]

Moreover, firing restrictions delay the re-absorption of the initial effect of shocks on total earnings mainly through their effect on the speed of adjustment of wages, while no significant effect is found on hours or employment. Indeed, Table 11 shows that more stringent dismissal regulation is associated with greater persistence of average wages in industries where US dismissal rates are greater, while I find no significant cross-industry difference for employment and hours. This finding suggests that stringent dismissal regulations could be among the factors behind the slow reaction of wages to shocks. Indeed, as discussed above, economic theory and the available empirical evidence support the conclusion that wage rigidity is likely to be more widespread when firing restrictions are high, since strong insiders can more easily resist real wage cuts.

Finally, there is also no evidence of differences in the persistence of value-added deviations from their respective trends between EP-binding and other industries (see Table 11). This cautiously suggests that EP for regular contracts has no consequence in terms of the speed of re-absorption of the impact of macroeconomic shocks on GDP (and therefore the output gap). As a consequence, estimated impacts reported above need not be compounded with an additional effect on GDP persistence.

31. They are also robust to excluding countries one-by-one from the sample (results available from the author upon request).

Table 9. **Dismissal regulations and persistence of industry-level earnings fluctuations**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lagged dep. variable (LDV)	-0.562 ***	-0.586 ***	-0.571 ***	-0.601 ***	-0.557 ***	-0.582 ***	-0.567 ***	-0.598 ***
	(15.726)	*(17.038)*	*(15.401)*	*(16.856)*	*(15.349)*	*(16.733)*	*(15.044)*	*(16.569)*
LDV X EPR X DR	1.134 *	1.294 **	1.052	1.213 *	1.651 **	1.764 **	1.585 *	1.673 ***
	(1.649)	*(1.980)*	*(1.463)*	*(1.790)*	*(2.123)*	*(2.387)*	*(1.954)*	*(2.193)*
LDV X EPR	0.028	0.044	0.028	0.047	0.029	0.045	0.029	0.048
	(0.746)	*(1.277)*	*(0.715)*	*(1.280)*	*(0.756)*	*(1.289)*	*(0.730)*	*(1.293)*
LDV x DR	0.424	0.673	0.769	1.101	1.569 **	1.656 **	1.791 **	1.934
	(0.614)	*(0.917)*	*(1.072)*	*(1.458)*	*(2.112)*	*(2.141)*	*(2.327)*	*(2.440)*
EPR X DR X output gap	-5.741 ***	-5.364 ***	-6.084 ***	-5.508 ***	-3.560	-4.168 *	-3.806	-4.248
	(3.196)	*(3.072)*	*(3.284)*	*(3.119)*	*(1.446)*	*(1.712)*	*(1.534)*	*(1.759)*
Lagged EPR X DR X output gap					-3.726	-3.089	-3.882	-3.028
					(1.246)	*(1.049)*	*(1.284)*	*(1.030)*
Industry dummies	Yes	No	No	No	Yes	No	No	No
Country X time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry X time dummies	No	Yes	No	Yes	No	Yes	No	Yes
Country X industry dummies	No	No	Yes	Yes	No	No	Yes	Yes
Observations	**8,582**	**8,582**	**8,582**	**8,582**	**8,582**	**8,582**	**8,582**	**8,582**
R-squared	**0.492**	**0.542**	**0.513**	**0.565**	**0.494**	**0.543**	**0.515**	**0.566**

The dependent variable is the industry-specific gap between the logs of actual and total earnings. DR: average US dismissal rate (by industry). EPR: employment protection for regular contracts. Other interactions required for identification are included. Absolute values of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively.

Table 10. **Dismissal regulations and persistence of industry-level earnings fluctuations, including additional controls**

	(1)	(2)
Lagged dep. variable (LDV)	-0.551 ***	-0.559 ***
	(14.757)	*(14.746)*
EPR x DR x LDV	2.145 *	2.137 **
	(1.931)	*(2.023)*
Corporatism (BD) x DR x LDV	-1.198	
	(1.017)	
PMR x DR x LDV	1.068	1.137
	(0.446)	*(0.487)*
ARR x DR x LDV	0.082	0.057
	(1.056)	*(0.904)*
Tax wedge x DR x LDV	-0.034	-0.048
	(0.344)	*(0.462)*
Corporatism (ICTWSS) x DR x LDV		-0.726
		(1.075)
Bargaining coverage x DR x LDV	-0.040	-0.025
	(0.803)	*(0.477)*
Country dummies	Yes	Yes
Industry dummies	Yes	Yes
Observations	7,020	7,020
R-squared	0.557	0.559

Note: The dependent variable is the industry-specific gap between the logs of actual and total earnings. EPR: employment protection for regular contracts. PMR: Product Market Regulation (aggregate 1998 index). BD: Bassanini and Duval index. ICTWSS: ICTWSS index. ARR: Average gross unemployment benefit replacement rate. Other interactions required for identification are included. All variables are in levels. Absolute values of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively.

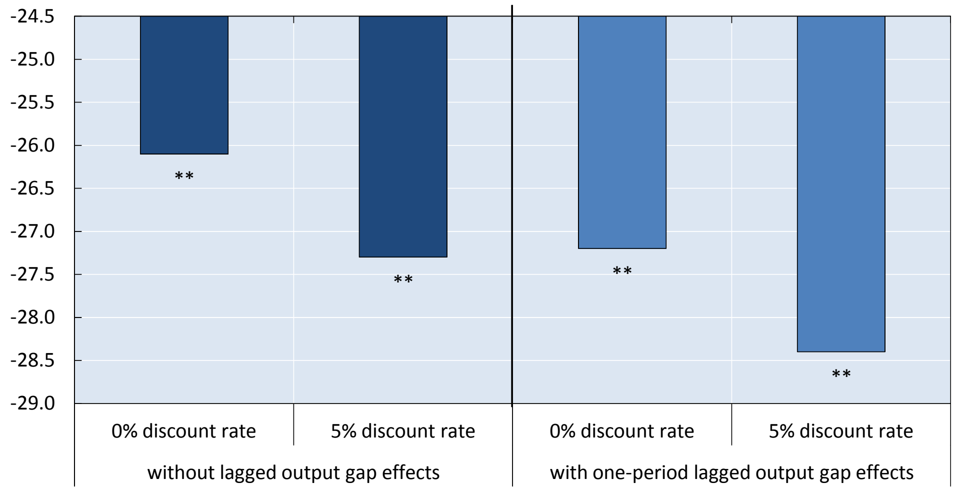
What do the counteracting effects of firing restrictions on shock amplification and persistence imply for labour-income smoothing? Subject to reasonable assumptions (see Section 1.2), it is possible to derive, from the econometric estimates presented above, the effect of EP on the *cumulated* impact of an adverse temporary shock on total earnings, measured under different assumptions about the discount rate (Figure 2). These estimates suggest that, in a country where the indicator of stringency of EP for regular contracts is one unit below the OECD average – *i.e.*, approximately the level of the United Kingdom, the actual value of the total cumulated loss of labour income due to a one-time adverse macroeconomic shock would be about 27% larger than in the average OECD country. This effect would result from the combination of larger employment fluctuations partially compensated by a more rapid adjustment of hourly wages to the equilibrium.

Table 11. **Dismissal regulations and persistence of other industry-level variables**

	Hourly wage gap		Hours worked gap		Employee headcount gap		Value added gap	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Lagged dep. variable (LDV)	-0.688 ***	-0.688 ***	-0.550 ***	-0.557 ***	-0.584 ***	-0.590 ***	-0.686 ***	-0.688 ***
	(20.196)	*(20.187)*	*(22.417)*	*(22.746)*	*(21.894)*	*(22.229)*	*(13.886)*	*(13.952)*
LDV x EPR x DR	2.299 *	2.316 **	-0.250	0.251	0.090	0.448	-0.536	-0.325
	(1.929)	*(2.007)*	*(0.226)*	*(0.248)*	*(0.082)*	*(0.452)*	*(0.339)*	*(0.229)*
LDV x EPR	0.071 *	0.071 *	0.060 *	0.061 **	0.052 *	0.053 *	-0.008	-0.007
	(1.855)	*(1.862)*	*(1.935)*	*(1.978)*	*(1.775)*	*(1.805)*	*(0.174)*	*(0.163)*
LDV x DR	0.323	0.424	2.193 **	0.766	2.662 **	1.684 *	1.652	0.601
	(0.260)	*(0.352)*	*(2.280)*	*(0.885)*	*(2.302)*	*(1.650)*	*(1.366)*	*(0.504)*
EPR x DR x output gap	4.257 **	1.017	-9.008 ***	-5.285 ***	-8.817 ***	-5.826 ***	-7.410 **	-4.972 **
	(2.117)	*(0.744)*	*(3.796)*	*(3.135)*	*(3.759)*	*(3.642)*	*(2.379)*	*(2.213)*
Lagged EPR x DR x output gap	-4.077 *		4.571 *		3.515		1.961	
	(1.787)		*(1.826)*		*(1.425)*		*(0.502)*	
Industry dummies	No	No	No	No	No	No	No	No
Country x time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country x industry dummies	No	No	No	No	No	No	No	No
Observations	**8,560**	**8,560**	**8,560**	**8,560**	**8,582**	**8,582**	**8,172**	**8,172**
R-squared	**0.496**	**0.495**	**0.511**	**0.507**	**0.490**	**0.488**	**0.473**	**0.472**

Note: The dependent variable is the industry-specific gap between the logs of actual and total earnings. DR: average US dismissal rate (by industry). EPR: employment protection for regular contracts. Other interactions required for identification are included. Absolute value of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively.

Figure 2. **Percentage effect of dismissal regulations on the total cumulated loss of labour income due to an adverse shock**



Note: Estimated percentage effect of a one unit EPR shift from the OECD average on the actual value of the total cumulated loss (gain) in labour income due to a one-shot adverse (favourable) macroeconomic shock. Effects are computed assuming linear utility and a discount rate of, alternatively, 0% and 5%. Without lagged effects means that the direct effect of the output gap is assumed to be only contemporaneous. **: statistically significant at the 5% level, respectively.

3.3. Industry-level difference-in-difference analysis: Statutory minimum wages

By preventing downward adjustment at the bottom of the distribution, minimum wages can be expected to significantly affect wage adjustments in the aftermath of an adverse aggregate shock. Whether the lack of wage adjustment will be reflected in stronger adjustments in employment or along other margins remain an open question. I have not considered the minimum wage so far because comparable time-series are available for only the subset of countries where minimum wages are imposed by law or regulation, rather than being set by collective bargaining among social partners.[32] In this section, I analyse the effects of the minimum wage in these countries through an industry-level difference-in-difference approach similar to that implemented in the case of EP. The identifying assumption is that changes in minimum wages have a greater impact on wage and earnings cyclicality in industries that are more heavily reliant on low-wage labour. In order to reduce bias due to the possible relationship between minimum wages and the distribution of low-wage employment, low-wage industries are identified based on the incidence of low-wage workers by industry in one specific country, the United Kingdom, prior to the introduction of statutory minimum wages in that country in 1999.[33]

I measure minimum wages as the economy-wide ratio of the gross statutory minimum wage to the median wage. This ratio, however, could be endogenous, due to the cyclical fluctuations of median wages.

32. These include Australia, Belgium, Canada, Czech republic, France, Greece, Japan, Korea, the Netherlands, Poland, Portugal, Slovak republic, Spain, the United Kingdom and the United States.

33. In practice, it is assumed that the greater the fraction of low-paid workers in the United Kingdom between 1994 and 1998, the larger the extent to which the minimum wage is binding. In sensitivity analyses, I alternatively classify minimum-wage-binding industries using the share of those without any diploma and the share of those with low educational attainment. Results presented in this section are robust to changes in the benchmark used to classify industries.

Therefore, I estimate baseline specifications using both OLS and instrumental variables (IV) approaches, using the logarithm of the real minimum wage in 2000 US dollars purchasing power parities interacted with the output gap as an instrument for the ratio of the minimum wage to median earnings interacted with the output gap. However, exogeneity tests[34] could never reject the hypothesis that the interaction between the output gap and the ratio of the minimum wage to median earnings is exogenous, which suggests that OLS estimates are consistent and more efficient. Thus, in the following, I will discuss only results based on OLS.

Difference-in-difference estimates suggest that minimum wages mitigate the impact of macroeconomic shocks on the cyclicality of hourly wages (Table 12). A ten percentage point increase in the ratio of minimum to median wages from the OECD average appears to reduce the elasticity of hourly wages to the output gap by 0.18 in the average industry (whose share of low-pay workers is about 0.14). This is a noteworthy effect from an economic point of view, taken into account the relatively low elasticity of wage fluctuations (see for example Table 1, above).[35] However, possibly due to the heterogeneous impact of the minimum wage on the cyclicality of employment and hours worked, I find no significant impact on the transmission of GDP shocks onto total earnings. I also cannot find any effect of minimum wages on real value added fluctuations.[36]

The analysis of the impact of institutions presented so far has considered average effects on earnings, hours worked, employment and wages. However, these averages can hide large asymmetries in adjustment patterns, particularly in the case of employment fluctuations. For this reason, in the next section, I turn to examine the effect of institutions on fluctuations of the earnings distribution.

4. The business cycle and the earnings distribution

One key finding of the recent US-based literature on earnings inequality is that the dispersion of the wage and salary annual earnings distributions (and to a minor extent of that of hourly wages) is counter-cyclical (see e.g. Heathcote *et al*., 2010). That is, the distribution of annual earnings becomes less equal during recessions and more equal during booms. This has been attributed to spikes in the incidence of unemployment for low-skilled (low-paid) workers around business-cycle troughs. Trivially, for a given hourly wage, the longer the time an individual spends jobless, the lower his/her annual earnings. To the extent that low-paid workers appear to suffer from greater increases in joblessness hazards in a recession, this would explain why the effect is more evident within the earnings rather than the wage distribution (see e.g. Robin, 2011).

34. Available from the author upon request.

35. These results are also robust to exclusion of countries one-by-one from the sample and inclusion of the additional institutional co-variates that are included in the baseline aggregate specification of the previous section (results available from the author upon request).

36. In addition, I have also examined the impact of minimum wages on the persistence of shocks and found no significant effect.

Table 12. **Statutory minimum wages and industry-level fluctuations**

	Total earnings		Hourly wage		Hours worked		Employee headcount		Value added	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Output gap x Low pay	6.615 **	5.773 **	6.363 **	6.073 **	0.191	-0.354	1.727	1.116	-4.306	-4.918
	(2.514)	*(2.126)*	*(2.240)*	*(2.132)*	*(0.075)*	*(0.140)*	*(0.706)*	*(0.468)*	*(1.294)*	*(1.409)*
Minimum wage x Low pay x output gap	-9.155	-6.507	-12.951 **	-12.349 *	4.025	6.015	-1.363	0.578	10.258	12.665
	(1.478)	*(0.999)*	*(2.028)*	*(1.877)*	*(0.699)*	*(1.023)*	*(0.246)*	*(0.104)*	*(1.359)*	*(1.630)*
Country x time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country x industry dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	5,543	5,543	5,520	5,520	5,589	5,589	5,612	5,612	5,602	5,602
R-squared	0.408	0.424	0.268	0.285	0.371	0.387	0.332	0.352	0.247	0.265

Note: The dependent variables are industry-specific gaps between logs of actual and trend values. Minimum wage: ratio of statutory minimum to median wage. Low pay: 1994-1998 share of low-pay workers (by industry) in the United Kingdom (average: 0.14). Other interactions required for identification are included. All variables are in levels. Absolute values of robust t-statistics in parentheses. **, *: statistically significant at the 5% and 10% level, respectively.

Data on the earnings distribution at a relatively high frequency (at least annual) are not available for many countries, which makes it difficult to see whether this phenomenon occurs outside the United States. One alternative way to look at this issue – that is exploited in this section – is to examine the distribution of total gross real annual earnings of wage and salary employees by level of education using data derived from the EUKLEMS database (see Section 2 above). In fact, to the extent that differences in employment, hours worked and pay across different educational attainment levels are among the main drivers of earnings disparities, the ratio between total gross annual earnings of the high- and low-educated workers[37] provides a measure of the dispersion of the earnings distribution, which compounds the impacts of relative wage and employment fluctuations.

Figure 3 shows the elasticity of the cyclical component of this ratio with respect to the output gap. Several elements emerge from it:

- First, in most countries, relative earnings by educational attainment appear to fluctuate counter-cyclically, although with important cross-country differences. In other words the earnings distribution becomes more unequal around the troughs of the business cycle. This has important equity consequences. To the extent that low-educated/low-paid workers are less able to shield themselves against income shocks, they will suffer a greater welfare reduction in bad times than high-educated/high paid workers, in the absence of policy interventions to compensate their loss of labour income.

- Second, cyclical fluctuations in total hours levels by education are the main driver of cyclical fluctuations of the earnings distribution (Canada being the only exception), confirming the generality of similar findings of the US literature. This is generally true both at the top and bottom end of the distribution (see Panels B and C). These findings appear consistent with the fact that the lower the level of educational attainment, the greater the risk of incurring in spells of joblessness – and therefore of working few or no hours in a year and having thus low labour income – in bad times.[38] Allowing for lagged effects of macroeconomic shocks does not alter this result.[39]

- Third, in the United States, patterns of fluctuations of the earnings distribution by educational attainment appear to replicate those reported in the literature for the percentiles of the earnings distribution. In particular, the distribution by level of educational attainment appears to fluctuate counter-cyclically, and the elasticity with respect to the output gap seems greater at the bottom end of the distribution.

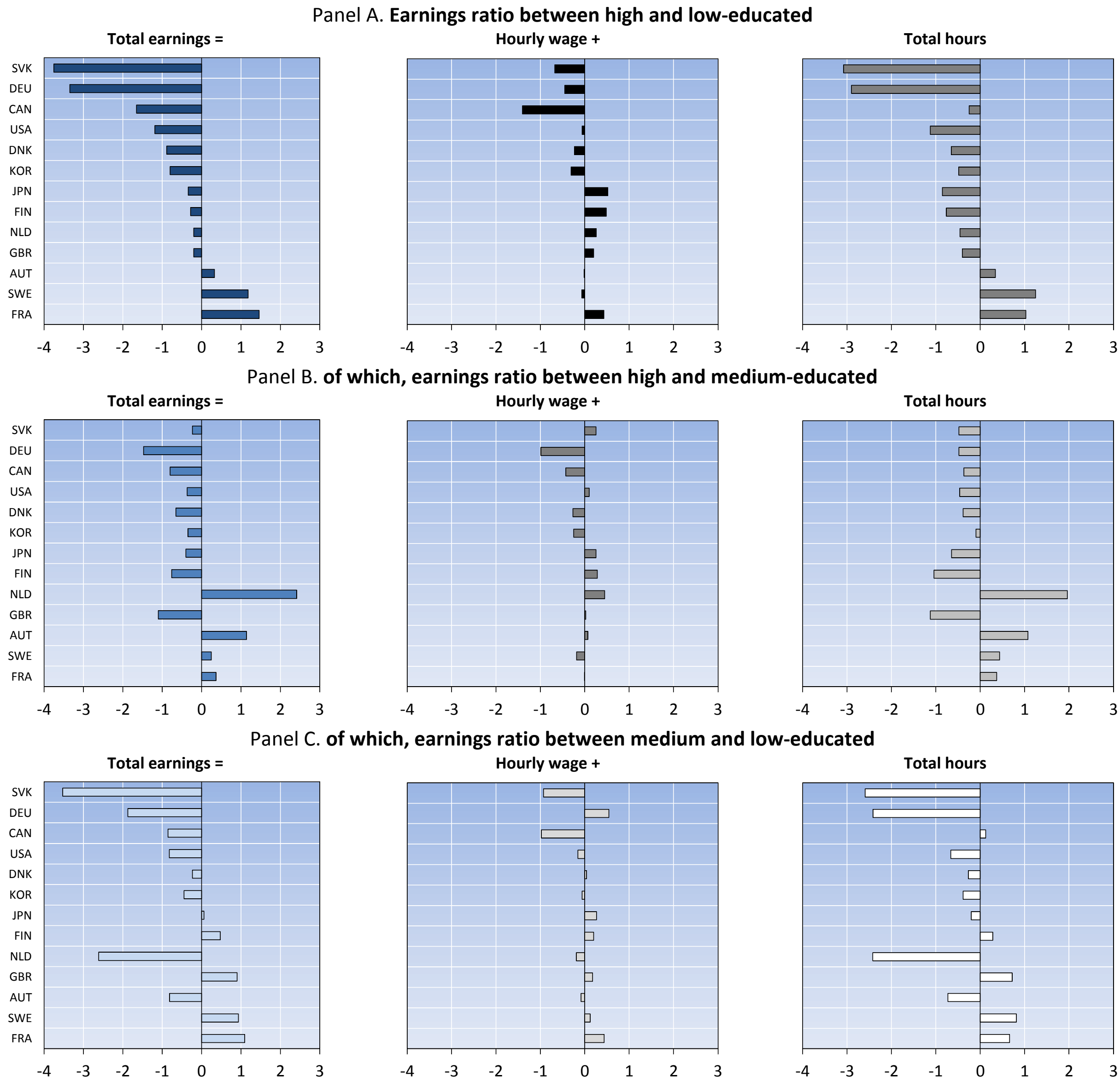
- Fourth, countries differ markedly in the relative sensitivity to shocks at the bottom and at the top of the distribution. Beside in the United States, the earnings distribution appears to be more counter-cyclical at the bottom end in only five other countries (Slovak Republic, Germany, Korea, Netherlands, and Austria). In the other countries, fluctuations of the top segment of the distribution appear to dominate those of the bottom end, suggesting that both the medium- and the low-educated are more affected by adverse shocks than the high-educated.

37. The term "high-educated" identifies here those with more than upper secondary education, the "low-educated" are those with less than upper secondary education, while "medium-education" denotes those with upper-secondary education.

38. These findings appear consistent with the few available studies in the literature (see for example Dustmann *et al.*, 2010).

39. Detailed figures are available from the author upon request

Figure 3. **Elasticity of the cyclical component of the earnings ratio between high and low-educated workers to the output gap**



Note: 1974-2004 for the United States; 1980-2003 for Denmark; 1982-2002 for France; 1984-2003 for Austria; 1987-2003 for the United Kingdom; 1987-2004 for Finland; 1991-2004 for Canada; 1992-2002 for Japan; 1992-2002 for the Netherlands; 1992-2004 for Germany; 1993-2004 for Sweden; 1993-2005 for Korea; and 1999-2004 for the Slovak Republic.

As a first step into the investigation of the effect of labour market institutions on the transmission of aggregate shocks to the earnings distribution, I estimate aggregate regressions – based on the same methodology as in Sections 1.1 and 3.1 above – where the dependent variable is the ratio of earnings of the high-educated to the low educated. Table 13 presents aggregate cross-country/time-series evidence on the extent to which specific institutions amplify or mitigate the counter-cyclicality of relative earnings, wage and hours across educational attainment groups. I include in the specifications those institutions considered in the analysis of institutional determinants of aggregate earnings fluctuations (Section 3.1). Positive coefficients imply that the related policy mitigates the tendency of the business-cycle to make these distributions more unequal in bad times (cf. Figure 3). However, great caution is required when drawing conclusions from these findings because the sample size is small and estimates are, therefore, relatively imprecise. Moreover, the small sample size suggests that it is important to be parsimonious in terms of covariates.[40] For this reason, country and year dummies are not included in the estimated specifications, given that statistical tests suggest that they are jointly insignificant (and due to the way the dependent variable is constructed there is no strong theoretical argument to include them in aggregate regressions). Nevertheless, all significant coefficients in Table 13 appear robust to exclusion of insignificant covariates. Moreover, even though these coefficients might become insignificant upon exclusion of specific countries in the sample (which is not surprising given the small number of countries and observations), the impact on point estimates of the exclusion of countries one-by-one always remains within one standard error of parameters estimated on the full sample.[41]

As discussed in the previous section, the tax wedge and unemployment benefit generosity have a strong shock-amplification effect on average wages and earnings. By contrast, evidence presented in Table 13 tentatively suggests that, while they have an impact on the cyclicality of relative hourly wages, they do not have any significant effect on the cyclicality of relative total earnings, possibly due to opposite or heterogeneous effects on fluctuations of relative hours worked. In fact, there is some evidence that the average tax wedge makes the hourly wage premium to education less counter-cyclical (or more pro-cyclical) while the opposite is found in the case of unemployment benefit generosity. To the extent that the marginal tax wedge on relatively high incomes is a key driver of the pro-cyclicality of gross wages, it is intuitive that this effect is likely to be greater for the highly-educated. By contrast, the negative relationship between unemployment benefit generosity and the cyclicality of the wage distribution is consistent with those equilibrium models of the labour market in which the slope of the wage curve depends on the level of the replacement rate (e.g. Belot and van Ours, 2004). To the extent that the replacement rate is likely to be higher for low-paid workers, employment fluctuations of a similar magnitude across groups are likely to translate in larger wage fluctuations for low-educated/low-skilled workers, thereby making the wage distribution more unequal during recessions.

There is also some evidence that a greater degree of centralisation/coordination of the wage bargaining reduces the pro-cyclicality of relative wages, while making the relative position of the low-educated worse in bad times. One could interpret this finding as being due to the fact that negotiated minimum wages are more rigid in the short-run under centralised industrial relations regimes, because contracts cannot be re-negotiated every year. This makes short-term wage adjustment at the bottom of the wage distribution more sluggish, with consequent greater employment adjustment. However, the estimated coefficients of corporatism become insignificant if employment protection or unemployment benefits are dropped from the specification (not shown in the table), suggesting that coefficients on coordination presented in Table 13 might reflect multi-collinearity, rather than a true causal effect.

40. Time series on earnings by educational attainment are also too short to analyse persistence, therefore dynamics models are not estimated in this section.

41. Detailed results available from the author upon request.

Table 13 **Institutions and amplification/mitigation of the cyclicality of earnings ratios by educational attainment**

	Relative earnings gap		**Relative hourly wage gap**		**Relative hours worked gap**	
	(1)	**(2)**	**(1)**	**(2)**	**(1)**	**(2)**
Output gap	-0.295	-0.306	-0.034	0.061	-0.261	-0.367
	(1.006)	*(1.126)*	*(0.189)*	*(0.369)*	*(1.084)*	*(1.544)*
EP	1.028 **	0.923 *	-0.219	0.049	1.247 ***	0.874 **
	(2.119)	*(1.907)*	*(0.679)*	*(0.150)*	*(3.491)*	*(2.473)*
Average tax wedge	0.008	0.008	0.066 **	0.063 **	-0.058	-0.055
	(0.193)	*(0.180)*	*(2.363)*	*(1.976)*	*(1.375)*	*(1.206)*
PMR	-0.207	-0.256	-0.073	-0.084	-0.134	-0.172
	(0.551)	*(0.692)*	*(0.641)*	*(0.699)*	*(0.347)*	*(0.449)*
Bargaining coverage	0.014	0.012	-0.001	-0.006	0.015	0.018
	(1.228)	*(1.005)*	*(0.130)*	*(0.928)*	*(1.370)*	*(1.616)*
ARR	-0.031	-0.035	-0.034 ***	-0.021 *	0.002	-0.014
	(0.784)	*(0.893)*	*(2.830)*	*(1.762)*	*(0.060)*	*(0.332)*
Corporatism (BD)	-0.531		0.542 **		-1.073 ***	
	(1.601)		*(2.145)*		*(4.438)*	
Corporatism (ICTWSS)		-0.186		0.184		-0.37 **
		(0.893)		*(1.116)*		*(2.194)*
Level effect of institutions	Yes	Yes	Yes	Yes	Yes	Yes
Country dummies	No	No	No	No	No	No
Time dummies	No	No	No	No	No	No
Obsrevations	**187**	**187**	**187**	**187**	**187**	**187**
R-squared	**0.126**	**0.119**	**0.136**	**0.111**	**0.117**	**0.09**

Note: In the first row the table reports the elasticity to the output gap for each dependent variable, estimated at the sample average of each institution. The other rows report the estimated effect of a one unit change of each institution on this elasticity. For each variable, the term "relative gap" indicates the log difference between actual and trend values of the high/low-educated ratio for that variable. EP: Employment Protection, measured on a 0-6 scale. PMR: Product Market Regulation (time-varying index), measured on a 0-6 scale. Two alternative measures of corporatism are included: BD: Bassanini and Duval index, measured on a 1-3 scale; ICTWSS: ICTWSS index measured on a 0-5 scale. All other variables are measured in percentages. ARR: Average gross unemployment benefit replacement rate. Absolute values of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively.

Finally, Table 13 shows that EP is the only policy for which I estimate an unambiguous effect on the elasticity of the earnings distribution by educational attainment to the output gap. Yet, point estimates appear somewhat too large to be plausible. However, I implement an industry-level difference-in-difference analysis of the effect of EP based on the same methodology as in Section 3.2 above (see also Section 1.2).[42] Estimates confirm that EPR dampens the tendency of the earnings distribution to become more unequal around business-cycle troughs (Table 14, Panel A).[43] Moreover, point estimates also appear more realistic. Taken at face value, the estimates suggest that in a country where the indicator of stringency of EPR is one unit above the OECD average, fluctuations of the earnings ratio between the high- and low-educated to the output gap would be 32% less counter-cyclical than in the average OECD country.[44] This pattern appears to be almost equally due to the effects of dismissal regulations on the wage and

42. Again, in order to make the model as parsimonious as possible, I include the minimal set of dummies required for indentification.

43. However, to avoid relying on those EUKLEMS data that are based on interpolations and conjectures (see Section 2), in comparison with what used in Table 13, a smaller number of countries (those in Figure 3 except Canada and the Netherlands) is considered.

44. As discussed in Section 3.2 this figure is obtained from the ratio of the first and the second row in column 1 of Panel A of Table 14.

employment distribution. In fact, the effect of a one-unit shift of the EPR indicator on mitigating the tendency of the ratio between the high- and low-educated to become greater in bad times is as large as 40% and 28% in the case of hourly wages and total hours, respectively. [45]

Table 14**. Dismissal regulation and industry-level fluctuations in the earnings ratio between high and low-educated workers**

Panel A. **Ratio of high to low-educated**

	Total earnings	**Hourly wages**	**Hours worked**
EPR x DR x output gap	27.012 ***	11.077 *	15.937 *
	(2.737)	*(1.916)*	*(1.670)*
DR x output gap	-83.791 ***	-27.677 **	-56.117 ***
	(3.599)	*(2.088)*	*(2.588)*
Industry dummies	yes	yes	yes
Country x time dummies	yes	yes	yes
Observations	**1,458**	**1,458**	**1,458**
R-squared	**0.259**	**0.285**	**0.200**

Panel B. **Ratio of high to medium-educated**

	Total earnings	**Hourly wages**	**Hours worked**
EPR x DR x output gap	6.639	6.376 *	0.265
	(0.843)	*(1.859)*	*(0.035)*
DR x output gap	-36.760 *	-17.685 **	-19.080
	(1.889)	*(2.075)*	*(0.998)*
Industry dummies	yes	yes	yes
Country x time dummies	yes	yes	yes
Observations	**1,458**	**1,458**	**1,458**
R-squared	**0.239**	**0.307**	**0.235**

Panel C. **Ratio of medium to low-educated**

	Total earnings	**Hourly wages**	**Hours worked**
EPR x DR x output gap	20.372 **	4.700	15.672 *
	(2.537)	*(1.101)*	*(1.828)*
DR x output gap	-47.031 **	-9.993	-37.037 *
	(2.428)	*(1.064)*	*(1.909)*
Industry dummies	yes	yes	yes
Country x time dummies	yes	yes	yes
Observations	**1,458**	**1,458**	**1,458**
R-squared	**0.257**	**0.174**	**0.222**

Note: The dependent variables are industry-level gaps defined as differences between the logs of actual and trend values. EPR: employment protection for regular contracts. DR: average industry-specific US dismissal rate. Other interactions required for identification are included. Absolute values of robust t-statistics in parentheses. ***, **, *: statistically significant at the 1%, 5% and 10% level, respectively.

The available data also allow me to analyse separately the effect of firing restrictions on the cyclicality of earnings inequality in the top and bottom halves of the distribution (Table 14, Panels B and C). The estimates suggest that dismissal regulations have a strong dampening impact on the counter-cyclicality of earnings inequality in the bottom half of the earnings distribution, but have no significant impact on fluctuations in the top half, notably because of the lack of any effect on relative employment

45. These estimates appear robust to excluding countries one-by-one from the sample and including additional controls. More precisely, if the full list of institutions of Table 13 is added to the specification (interacted with US dismissal rates and the output gap), all co-variates become insignificant, possibly due to multicollinearity and the small sample size. However, if other institutions are included one-by-one, they are never significant both with and without the simultaneous inclusion of EPR.

fluctuations in this segment of the distribution. Taking estimates at face value, they suggest that a one-unit increase of the EPR indicator from the OECD average would reduce the tendency of the bottom halves of the distributions of earnings, wages and hours worked to widen in recessions by between 42% and 47%.

Concluding remarks

This paper has investigated the role of labour market institutions in propagating macroeconomic shocks to the labour market and explaining earnings fluctuations at the aggregate levels. There is some evidence that generous unemployment benefits and progressive labour taxes amplify the effect of macroeconomic shocks on labour income. This suggests that, even if the tax and transfer system mitigates the transmission of individual earnings volatility onto household disposable income (see e.g. OECD, 2011), providing adequate income security is more difficult in a recession. By amplifying the effects of shocks on gross labour income, and therefore government revenues, these measures can be extremely costly for the government budget, underlying the importance for countries to achieve a sound fiscal stance during periods of growth, so as to have the fiscal capacity to sustain income support for workers during a crisis.

By contrast, policies that keep workers in their current jobs, such as employment protection for regular workers, are likely to mitigate the loss of labour income in downturns. The reduction in the risk of job and earnings losses associated with stricter dismissal regulations appears to be particularly large for workers at the bottom of the earnings distribution. However, the evidence also suggests that these policies risk prolonging the effects of adverse aggregate shocks. But the estimates presented in this paper show that the dampening effect outweighs the persistence effect and employment protection for regular workers is likely to reduce the cumulated loss of labour income brought about by a downturn. This suggests that policy-makers should set their stance, as regards labour regulation, by striking a balance between the gains in efficiency associated with lower employment protection (see Bassanini *et al.*, 2009) and the labour-income smoothing associated with higher employment protection.

Annex: Data construction, sources and descriptive statistics

Industry-level data

Earnings and hourly wage data refer to total gross annual earnings and average hourly wages, respectively of wage and salary employees. Employment refers to annual averages for wage and salary employees. Hours worked refers to total annual hours for the same employees. Real value added is obtained by deflating nominal value added in each industry with the industry-specific double deflator. Data are from the EUKLEMS Database except for Norway, where they come from the OECD STAN Database and refer to total employment. EUKLEMS data obtained through interpolation and/or estimated on the basis of conjectures, identified from Timmer *et al.* (2007), Baldwin (2009) and the related EUKLEMS documentation, were removed from the sample. Data are aggregated at the level of the business sector to be used in aggregate regressions. The list of industries used in the industry-level analysis is reported in Table A1.

The distributions by educational attainment of earnings, wage, and hours also come from the EUKLEMS database. Again, data obtained through interpolation and/or on the basis of conjectures were removed from the sample. Education is divided into three categories: low-education (less than upper secondary); medium education (upper secondary); and high education (more than upper secondary). The business sector, in this case, is partitioned in 9 industries for reasons of data reliability (refer to ISIC codes reported in Table A1): 10-14 and 40-41; 15-19 and 36-37; 20-28; 29-35; 45; 50-52 and 55; 60-64; 65-67; and 70-74.

The industry-specific US dismissal rate is from Bassanini *et al.* (2010; *www.oecd.org/dataoecd/28/30/46825863.zip*) and is derived from various waves of the CPS Displaced Workers Supplement (2000-2006, even years). An individual is considered to have been dismissed if he/she lost his/her job in the most recent year covered by each survey, because of plant closing or moved, insufficient work, or position or shift abolished. Only wage and salary employees in the private-for-profit sector are considered.

Table A1. **List of industries (with ISIC rev. 1 code)**

Isic Rev.1 code	Industry label
10-14	Mining
15-16	Food , beverages and tobacco
17-19	Textiles, leather and footwear
20	Wood and manufacturing of wood and cork
21-22	Pulp, paper, printing and publishing
23	Coke, refined petroleum and nuclear fuel
24	Chemicals and chemical products
25	Rubber and plastics
26	Other non-metallic mineral products
27-28	Basic metals and fabricated metal
29	Machinery, not elsewhere classified
30-33	Electrical and optical equipment
34-35	Transport equipment
36-37	Other manufacturing; Recycling
40-41	Electricity, gas and water supply
45	Construction
50-52	Wholesale and retail trade
55	Hotels and restaurants
60-63	Transport and storage
64	Post and telecommunications
65-67	Financial intermediation
70	Real estate activities
71-74	Other business services

The share of low-paid workers in the United Kingdom prior to the introduction of the minimum wage in 1999 is the average share of low-pay workers in each industry over all available quarters between 1994 and 1998. In each quarter, low-paid workers are defined as those with gross hourly wages less than two-thirds of the median wage of the quarter for the whole economy. The source is the UK Labour Force Survey.

Wage indexation is the employment-weighted share of firms having a policy of adjusting base wage to inflation in 2007-2008. Data are disaggregated into five industries and were kindly provided by the WDN and the ECB. The source is Druant *et al.* (2009).

Table A2 reports summary statistics of the main industry-level variables.

Table A2. **Summary statistics, industry-level variables, main sample**

Variable	Observations	Mean	Std. Dev.	Min	Max
Log earnings gap	8998	0.001144	0.057396	-0.88136	0.489028
Log wage gap	8998	0.000187	0.04373	-0.90662	0.560124
Log hours gap	8998	0.000991	0.049829	-0.39632	0.5378
Log employment gap	8998	0.000614	0.049404	-0.50383	0.459586
Log value added gap	8568	0.00124	0.086022	-1.32804	1.265822
US layoff rate	8604	5.18404	1.6671	2.226535	8.995686
UK low pay rate	8998	0.144562	0.084716	0.032433	0.390982
Wage indexation	2966	50.19957	29.07121	0	100

Institutional variables

EP indicators come from the OECD Indicators of Employment Protection (*www.oecd.org/employment/protection*). All indicators vary from 0 to 6 from the least to the most stringent. In aggregate regressions, data are extended backward by making them constant between 1978 and 1985.

UB generosity is measured on the basis of average replacement rates (in percent of pre-displacement wage), defined as average unemployment benefit replacement rate across two income situations (100% and 67% of average worker earnings), three family situations (single, with dependent spouse, with spouse in work) and three different unemployment durations (first year, second and third years, and fourth and fifth years of unemployment). Net benefits, available between 2001 and 2007, are net of taxes and transfers, but exclude means-tested social assistance. The source is the OECD Benefits and Wages database (*www.oecd.org/els/social/workincentives*).

The indexes of anti-competitive product market regulation come from the OECD Regulatory Database (*www.oecd.org/document/1/0,3746,en_2649_ 37421_2367297_1_1_1_37421,00.html*). They vary from 0 to 6 from the least to the most restrictive. Time-invariant aggregate regulation data refer to 1998. Time-varying aggregate data are based on two 1-digit industries (Energy and Transport and Communications).

Minimum wages are measured as the ratio of the statutory minimum wage to median wage of full-time workers. For exogeneity tests, the deviation of the logarithm of the real minimum wage in 2000 USD purchasing power parities from the OECD average of each year is used an instrument. The source of all these variables is the OECD Employment Database (*www.oecd.org/els/employment/database*).

The tax wedge considered in this paper is the wedge between the labour cost for the employer and the corresponding net take-home pay of the employee for single-earner couples with two children earning 100% of average worker earnings. It is expressed as the sum of personal income tax and all social security contributions as a percentage of total labour cost. Data are retropolated using tax wedges for average production workers between 1978 and 1982 for most countries. Data on marginal tax rates are available for eight income levels and family situations from 2000 to 2007. These rates refer to the marginal tax of the principal earner in the following situations: single person at 167% of average earnings and no child; Single person at 100% of average earnings and no child; two-earner married couple, one at 100% of average earnings and the other at 33 %, with no child; two-earner married couple, one at 100% of average earnings and the other at 67 %, with two children; two-earner married couple, one at 100% of average earnings and the other at 33%, with two children; single person at 67% of average earnings, with two children; one-earner married couple at 100% of average earnings, with two children; single person at 67% of average earnings, no child. The first four in the above are defined as relatively high income levels for the purpose of the analysis of this paper. Indicators of the level of average marginal tax rates on labour income are obtained by simple average of the different components. The source of all these variables is the OECD Taxing Wages Database (*www.oecd.org/ctp/taxingwages*).

Collective bargaining coverage is the share of workers covered by a collective agreement, in percentage. The source is the ICTWSS database (*http://www.uva-aias.net/207*). Data were averaged or interpolated when information is not available at the annual level.

The degree of corporatism is proxied with two mutually exclusive variables. The BD index takes values 1 for decentralised and uncoordinated wage-bargaining processes, and 2 and 3 for intermediate and high degrees of centralisation/co-ordination, respectively. Data were extended to cover the period 2003–2007. The source is Bassanini and Duval (2009; *www.oecd.org/dataoecd/25/25/37431112.zip*). Alternatively, the ICTWSS index of coordination is used, which takes values from 1 to 5 from the least to the most coordinated. The source of this variable is the ICTWSS database (*http://www.uva-aias.net/207*).

The presence of short-time work scheme, used in a few robustness checks, is codified through a dummy for existence of these schemes in 2007. The source is Hijzen and Venn (2010).

Other aggregate variables

The output gap is the OECD measure of the gap between actual and potential output as a percentage of potential output. In the case of Korea, due to the lack the OECD measure of the output gap, an HP filter of GDP in volume terms is used to derive the output gap. The source is OECD Economic Outlook (EO) database.

Earnings and wage data are deflated using the private consumption deflator, drawn from the OECD EO Database

The aggregate share of temporary workers is drawn from labour force surveys. Missing years were interpolated. Temporary workers are those whose job’s termination is determined by objective conditions such as reaching a certain date, completion of an assignment or return of another employee who has been temporarily replaced. Included in these groups also are: a) persons with a seasonal job; b) persons engaged by an employment agency or business and hired out to a third party for the carrying out of a "work mission" (unless there is a work contract of unlimited duration with the employment agency or business); and c) persons with specific training contracts. The source is the OECD Employment Database (*www.oecd.org/els/employment/database*).

Table A3 reports summary statistics of the main aggregate variables.

Table A3. **Summary statistics, main aggregate variables**

Variable	Observations	Mean	Std. Dev.	Min	Max
Log earnings gap	531	-0.00029	0.038413	-0.14573	0.155526
Log wage gap	531	-0.00059	0.020864	-0.076	0.086785
Log hours gap	531	0.000305	0.029727	-0.12681	0.105689
Output gap	539	-0.00191	0.02113	-0.08676	0.064828
EP	539	2.124898	1.004258	0.21	4.1
EPR	449	2.16029	0.907506	0.17	4.83
EPT	449	2.042739	1.401948	0.25	5.38
ARR (gross)	516	28.47152	13.70659	0.347222	64.94407
ARR (net, average)	539	40.77312	20.74327	5.654741	67.16872
PMR (time-varying)	539	3.70764	1.346185	0.938546	6
PMR (time-invariant, 98)	539	2.019111	0.573705	1.0698	3.969725
Minimum wage	319	0.456871	0.101906	0.2185	0.6745
Average tax wedge	473	29.69772	9.438382	1.918403	60.81406
Marginal tax wedge on high incomes (average)	539	48.85163	9.547817	24.32234	67.5211
Bargaining coverage	481	67.87409	24.86884	10	99
Corporatism (BD)	524	2.227099	0.865722	1	3
Corporatism (ICTWSS)	524	3.139313	1.358234	1	5
STW scheme	526	0.749049	0.433973	0	1
Year	539	1994.356	8.223821	1979	2007

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