The Economic Journal, **117** (*June*), F279–F301. © The Author(s). Journal compilation © Royal Economic Society 2007. Published by Blackwell Publishing, 9600 Garsington Road, Oxford OX4 2DQ, UK and 350 Main Street, Malden, MA 02148, USA.

JOB FLOW DYNAMICS AND FIRING RESTRICTIONS: EVIDENCE FROM EUROPE*

Julián Messina and Giovanna Vallanti

We exploit homogeneous firm level data of manufacturing and non-manufacturing industries to study the impact of firing restrictions on job flow dynamics across 14 European countries. Our results suggest that more stringent firing laws dampen the response of job destruction to the cycle, thus making job turnover less counter-cyclical. Moreover, stricter EPL reduces both the creation and destruction of jobs in declining sectors relative to expanding sectors, implying that faster trend growth attenuates the impact of firing costs on firm's hiring and firing decisions.

How does the reallocation of factors of production behave along the business cycle? Are there significant differences across countries? Which are the determinants of such differences? Following Davis and Haltiwanger's (1990, 1992) seminal work, a large literature has emphasised the importance of labour reallocation and microeconomic heterogeneity for macroeconomic fluctuations. While the direction of causality is debatable,[1] the study of the behaviour of job reallocation over the business cycle is fundamental in order to understand economic fluctuations. Moreover, even if reallocation is just a consequence of the business cycle, understanding the nature and timing of job reallocation remains crucial to design the appropriate policy responses to recessions and, more generally, to business cycle fluctuations. Several studies in Anglo-Saxon countries clearly suggest that the reallocation of jobs presents a counter-cyclical pattern. During slumps the rate at which jobs are destroyed increases rapidly. Perhaps more surprisingly, job creation reacts slowly to economic downturns, sometimes even not declining at all. As a result, job reallocation (the sum of job creation and job destruction) is clearly counter-cyclical.[2]

This set of facts spurred the proposal of different theories consistent with the counter-cyclicality of reallocation. Caballero and Hammour (1994) show, within a vintage model of process and product innovation, that declines in demand are only partly accommodated by a reduction of job creation when fast creation of jobs in an industry is costly due to increasing creation costs. As a consequence, job creation is smoothed over the business cycle and job destruction is concentrated in recessions, implying a counter-cyclical pattern in the reallocation of jobs. In Mortensen and Pissarides (1994), counter-cyclical movements of job reallocation are generated by the

* We are specially indebted to Ramon Gomez-Salvador for his feedback at an early stage of this project. We thank two anonymous referees, Samuel Bentolila, Giuseppe Bertola, Dimitrios Christelis, Giulio Fella, Larry Kahn, Steve Nickell, Christopher Pissarides, Anna Sanz de Galdeano, Uwe Sunde and seminar participants at the IZA Workshop on Employment Protection and EEA Meetings in Madrid for helpful comments. Giovanna Vallanti thanks the Economic and Social Research Council for financial support. The work of Julián Messina is partially supported by Research Grant SEJ2004-03276/ECON from the Spanish Ministry of Education and Science. All errors are ours.

[1] See Schuh and Triest (1998) for a discussion of the causality links between reallocation and business cycle fluctuations.

[2] See Davis and Haltiwanger (1992) and Davis *et al.* (1996) for the US manufacturing sector, Baldwin *et al.* (1998) for Canada and Konings (1995) for the UK.

time required to establish a profitable job-worker match. Intuitively, during upturns it takes time to fill in vacancies while during downturns job destruction occurs immediately. Moreover, the profitability of a vacancy increases during recessions, as a larger unemployed pool facilitates finding workers, stabilising job creation along the cycle. As a result, job turnover is counter-cyclical.

In Continental Europe the evidence on the behaviour of job reallocation along the business cycle is more limited, suggesting that job reallocation tends to be a-cyclical or even slightly pro-cyclical.[3] Garibaldi (1998) takes stock of the dichotomy in job flow dynamics between Anglo-Saxon and Continental European countries and shows that extending the Mortensen and Pissarides (1994) framework to allow for the presence of fixed adjustment costs associated with dismissals can explain the apparent a-cyclicality of job reallocation in the latter group. In this setting, when firing is costly and time-consuming the asymmetry in the cyclical pattern of job creation and job destruction disappears, as job destruction becomes less responsive to the cycle. Thus, Garibaldi (1998) concludes that cross-country differences in job flow dynamics can be accounted for by differences in the relative stringency of employment protection legislation (EPL).

A competing explanation of these cross-country differences relies on limitations in the data coverage and sampling frame across studies. While evidence for the US, Canada and the UK is mostly based on establishment data for the manufacturing sector, studies for continental European countries typically rely on firm level data including manufacturing and service industries. Boeri (1996) and Foote (1998) claim that the apparent counter-cyclicality of reallocation in US data appears to be a peculiarity of the manufacturing sector. Foote (1998) shows that service industries in the US instead present a higher variability of job creation over the business cycle, resulting in pro-cyclical job reallocation. He further argues that the asymmetric behaviour of job creation and destruction between manufacturing and service sectors is broadly consistent with an (S,s) model of micro-level employment adjustment featuring trend growth. In this context, the interaction of (S,s)-type adjustment and negative trend growth implies that job destruction becomes a more important margin of adjustment in declining sectors, and hence more volatile, explaining the apparent counter-cyclicality of reallocation in US manufacturing. Similarly, in sectors featuring a positive trend growth such as most service industries, the volatility of job creation over the business cycle increases, resulting in pro-cyclical reallocation of jobs.

This article brings these two competing hypothesis to the data. For this purpose, it overcomes previous problems of cross-country comparability of job flow dynamics by using a unique homogenous firm-level data set that covers the whole spectrum of productive sectors for 14 European countries during the 1990s and early 2000s. This allows for the first examination of differences and similarities across countries and sectors in the cyclical properties of job creation and destruction within a comprehensive framework. Relying on comparable data, we examine empirically the impact of

[3] In particular, an a-cyclical pattern has been found in Austria (Stiglbauer *et al.*, 2002), Italy (Contini *et al.*, 1995), Spain (Dolado and Gomez-Salvador, 1995) and Germany (Boeri and Cramer, 1992) while a slightly pro-cyclical pattern has been documented for France (Lagarde *et al.*, 1994) and Sweden (OECD, 1994). However, most of these studies present simple correlations between job reallocation and the cycle at the aggregate level, relying on a limited number of years.

employment protection on the behaviour of reallocation along the business cycle. A contribution of the article relates to its identification strategy of the impact of EPL in labour market outcomes, which avoids the problems of lack of degrees of freedom typically encountered in the empirical macro literature when identifying the impact of labour market institutions. While most of the literature focuses on the direct impact of EPL on job flows, our focus here is on the differential impact of the legislation across sectors and phases of the business cycle.

Our findings indicate that firing restrictions play a significant role in shaping labour reallocation along the business cycle, while sectoral trend growth is less important. When firing is costly and time consuming, firms respond by smoothing the destruction of jobs over the business cycle. Hence, job destruction becomes less responsive to economic fluctuations, providing support to the hypothesis put forward by Garibaldi (1998). Simulation of these empirical results show that the impact of firing restrictions on the behaviour of reallocation along the cycle is large, EPL differences being able to account for cross-country patterns of job reallocation. A closely related result relates to the differential impact of EPL across sectors. Bentolila and Bertola (1990) show that higher trend growth is expected to dampen the impact of firing restrictions on job flows. Consistent with their model, our findings suggest a differential impact of EPL across sectors within a given country, depending on sectoral employment trend growth.

These findings relate to a recent literature that highlights the differential impact of EPL across demographic groups. Bertola *et al.* (2007) find that EPL increases the youth unemployment rate relative to adults using aggregate data for OECD countries. This is consistent with results from individual data in the US, analysed by Autor *et al.* (2006). Similarly, Kahn (2007) finds that non-employment is relatively higher among the young, women and less skilled workers in high EPL countries using international individual household data. We take here the perspective of the firm, and show important heterogeneity across firms in the incidence of dismissal restrictions depending on the sector of operation and phase of the business cycle. Hence, previous studies that failed to control for differences across countries in aggregate trend growth or the business cycle might have missed an important element when evaluating the impact of firing restrictions on labour market dynamics.

The rest of the article is organised as follows. The next Section presents the main characteristics of the data. Section 2 sets out the empirical methodology. The main results of the article are presented in Section 3. Section 4 performs a series of robustness checks and Section 5 concludes.

1. The Data

The main data source in this study is Amadeus, a firm-level data base collected by the Bureau van Dijk (BvD) from balance sheet data in European countries.[4] The information is collected by the national Chambers of Commerce and homogenised by BvD applying uniform formats to allow accurate cross-country comparisons. The period of analysis used for this study spans from 1992 to 2001 and varies slightly depending on

[4] There are several versions of Amadeus, depending basically on the number of firms covered. Ours is the top 1,000,000 firms.

the country. The sample includes all EU-15 countries with the exception of Luxemburg and Ireland, plus Norway.

The Amadeus database has several important advantages for the study of job flow dynamics within and across countries. Previous studies usually suffer from differences across countries in the source of the data (administrative versus survey), unit of observation (firms versus establishments), sectoral coverage (manufacturing versus services), and period of observation (expansion versus recessions), which may have led to misleading interpretations of the cross-country cyclical patterns of job flows (OECD, 1994). Instead, in Amadeus the data collection is relatively homogeneous across countries. Moreover, firms' information is classified on narrowly defined sectors (2-digit NACE classification) and data from both manufacturing and non-manufacturing sectors are reasonably representative.

One limitation of Amadeus is that it does not allow accurate identification of the birth and death of firms. Therefore we restrict our analysis to continuing firms, e.g. firms that are in the sample for at least two consecutive periods. This is an important limitation for the purpose of comparison of job turnover rates from Amadeus with that from other sources. However, the exclusion of entry and exit should be less problematic at the time of evaluating the impact of EPL on employment dynamics, because the component of total job turnover that is more likely to be affected by firing restrictions is precisely job turnover of continuing firms (OECD, 1999).[5]

Gómez-Salvador *et al.* (2004) show that the sectoral distribution of employment in Amadeus is very similar to the actual distribution of employment as measured by the national labour force surveys (LFS). Perhaps more convincingly, they show that aggregate employment growth rates from Amadeus follow the growth rate of employment in the LFS quite closely, suggesting that the sample in Amadeus is representative of the total firms' population. Figure 1 shows annual employment growth in 24 different sectors and 14 countries as measured in Amadeus, against employment

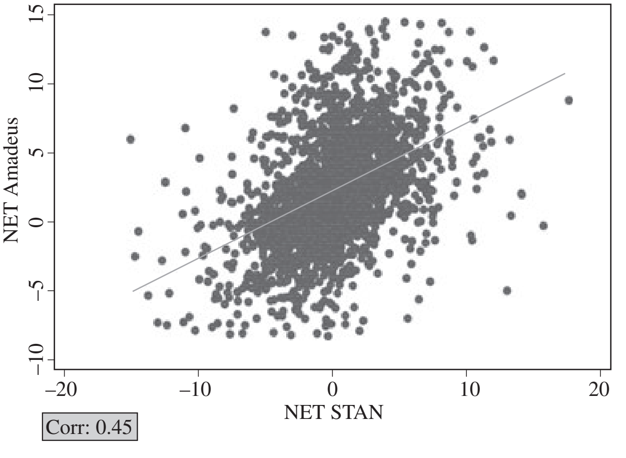

Fig. 1. *Sectoral Employment Growth. STAN vs. AMADEUS*

[5] See Koeniger and Prat (2007) for a discussion of the role of firing restrictions and product market regulations in firm's adjustment at the intensive and extensive margin.

growth measured in those sectors by STAN, the Structural Analysis Database constructed by the OECD. We do not expect a perfect correlation, as Financial sectors and, more importantly, public employees are not covered by Amadeus. However, the positive and significant association (corr. 0.45) between both sources is reassuring.

Job flow statistics from Amadeus are merged with employment and value added data at the sectoral level from STAN. To this purpose, we construct using Amadeus annual job flow statistics between 1992 and 2001 for 24 sectors, which are those covered in STAN.[6] The advantage of STAN is that it contains long time series (1970–2003) of annual value added at the sectoral level, which we use to construct a sectoral output gap indicator as our main measure of the business cycle.[7]

A second limitation in Amadeus relates to the sampling procedure, which introduces a bias against very small firms.[8] This is common in firm level data sets, but is potentially important when measuring job flows since a sizable fraction of job turnover occurs within this segment of the size distribution. Moreover, in some countries firms below a certain size-threshold are exempted from firing restrictions.[9] It could well be the case that firms more prone to labour turnover limit their size to slightly below the threshold in order to avoid falling under the legislation.[10] Similarly, the data are available at the firm rather than the establishment level. Measuring job flows at the firm level understates the actual magnitude of total gross flows between plants. However, cross-country comparisons of establishment data pose serious difficulties since there is important heterogeneity in the definition of establishment across data sets and countries (OECD, 1994). This is less of a problem with firm data. Moreover, in most countries employment protection legislation applies to the firm rather than to the establishment.[11]

One advantage of the country-sectoral panel that we build for the analysis is that we can control for different sets of fixed effects in an attempt to assess the robustness of our results. Some of our empirical specifications include country and sector fixed effects as well as their interaction. Hence, identification in this more restrictive specification comes from within sector and country variation in time. Such a specification

[6] The sectors are: Agriculture, forestry and fishing; Mining and quarrying; Food, beverages and tobacco; Textiles; Wood products; Paper products, publishing and printing; Refined petroleum, nuclear fuel and chemical products; Rubber and plastic products; Other non-metallic products; Basic metals and fabricated metal products; Machinery and equipment; Electrical and optical equipment; Transport equipment; Other manufacturing sectors; Electricity, gas and water supply; Construction; Wholesale and retail trade, Repairs; Hotels and restaurants; Transport and communications; Financial intermediation and insurance; Real estate and renting, Computer and related activities, Research and development; Public Administration, defence and education; Health and social work; Other community, social and personal services

[7] The output gap is constructed applying the Hodrick-Prescott filter with a smoothing parameter $\lambda = 100$, to annual value added series in each sector over the period 1970–2003. Section 4 provides sensitivity analysis with respect to the smoothing parameter and alternative measures of the output gap.

[8] Typically, firms below 10 employees are excluded from the sample.

[9] For a rationale for such differential legislation see Boeri and Jimeno-Serrano (2005).

[10] Evidence suggests that threshold effects are present, although are quantitatively small. See Borgello *et al.* (2002), Schivardi and Torrini (2004), Boeri and Jimeno-Serrano (2005) for a discussion of the Italian case, and Vereck (2004) for Germany.

[11] Known exceptions are Germany and Austria. In the UK, the application of collective dismissal legislation is identified at the establishment level. Finally, Italy is a mixed case, where the exception of EPL is applied to firms below 15 employees, but the threshold defining a collective dismissal applies to the establishment (OECD, 2004*b*).

would wipe out possible biases in the construction of sectoral job flows due to misrepresentation of small firms, as long as the bias remains constant over time.

There are several indices of employment protection in the literature. Our preferred indicator is the latest index developed by the OECD (2004*a*), which ranges theoretically from 0 to 6, and empirically from 0.6 to 3.7, according to the increasing strictness of EPL. This is the most comprehensive index of EPL, covering several aspects of employment protection including regulation for individual and collective dismissals and differences across regular and temporary contracts. However, this index presents little time variation within the sample period.[12] An alternative measure of employment protection was first developed by Blanchard and Wolfers (2000) and updated by Nickell *et al.* (2001) and Gómez Salvador *et al.* (2004). This index is also scaled from 0 to 6 and in principle has the virtue of providing greater variability over time. However, to a large extent this variability is due to the interpolation of previous measures. We provide some robustness checks using this index below.

Table 1 provides descriptive statistics for the EPL index, the cycle indicator and a sectoral trend growth indicator (that is measured as average employment growth in each sector and country over the sample period). Concerning the EPL index, there are some relevant cross-country differences in the strictness of employment protection. The UK is the least regulated country while stricter employment protection is a feature of Portugal, Greece and France. Turning to the business cycle and trend growth, we observe substantial heterogeneity in both indicators across countries and sectors, and this variation will be used to identify the parameters of interest in the econometric analysis that follows.

Table 1
Summary Statistics

	EPL		Cycle		Trend growth	
	max	min	mean	sd	mean	sd
Austria	2.2	2.2	−0.31	3.88	1.23	2.52
Belgium	3.2	2.2	−0.04	4.02	1.07	2.16
Denmark	1.4	1.4	0.01	6.27	2.78	2.29
Finland	2.1	2.1	1.19	6.84	3.65	3.19
France	3	3	−1.80	3.94	1.79	1.5
Germany	3.09	2.5	1.34	5.19	0.48	2.32
Greece	3.5	3.5	−1.88	6.71	2.20	2.01
Italy	3.6	2.59	−0.94	3.54	4.52	1.70
Netherlands	2.7	2.1	−0.66	4.41	1.77	2.39
Norway	2.7	2.6	0.45	7.64	3.33	2.83
Portugal	3.85	3.7	−2.56	5.62	1.75	3.22
Spain	3.11	2.9	−1.64	3.66	4.43	2.56
Sweden	2.2	2.2	−0.54	7.49	3.53	2.82
UK	0.68	0.6	−0.05	5.32	1.29	2.79

[12] Over the sample period, seven countries have experienced a relaxation of the rules governing EPL: Belgium (1997), Germany (1997), Italy (1997 and 2000), Netherlands (1999), Norway (2000), Portugal (1996) and Spain (1997). The UK has moved to the opposite direction, the EPL index changing from 0.6 to 0.68 in 2000. Summary statistics on the EPL index for each country in the sample are reported in Table 1.

2. Empirical Model

We calculate yearly job creation (*JC*), job destruction (*JD*) and job reallocation (*JR*) rates at the sectoral level for a total of 24 sectors. We follow the standard definitions of job flow measures described in Davis and Haltiwanger (1990). JC_{ijt} in period t, country j and sector i equals the weighted sum of employment gains over all growing firms in sector i and country j between $t-1$ and t. Similarly JD_{ijt} equals the sum of employment losses (in absolute value) over all contracting firms between $t-1$ and t. It follows that net employment can be obtained as $NET_{ijt} = JC_{ijt} - JD_{ijt}$ and the job reallocation rate is defined as $JR_{ijt} = JC_{ijt} + JD_{ijt}$. Descriptive statistics for job reallocation, job creation and job destruction are presented in Table 2.

Our basic empirical strategy is based on the following reduced-form specification

$$\begin{aligned} JF_{ijt} = {} & \alpha + N_{ijt}\beta + E_{jt}\gamma + G_{ij}\delta + (N_{ijt} \times E_{jt})\zeta + (N_{ijt} \times G_{ij})\eta + \\ & + (E_{jt} \times G_{ij})\theta + (N_{ijt} \times E_{jt} \times G_{ij})\vartheta + \mathbf{D}\boldsymbol{\xi} + \varepsilon_{ijt} \end{aligned} \tag{1}$$

for $i = 1, ..., 24$, $t = 1992, ..., 2001$ and $j = 1, ..., 14$
where Greek letters are parameters of the model, JF_{ijt} denotes job flows (JR_{ijt}, JC_{ijt} or JD_{ijt} depending on the specification), N_{ijt} is a business cycle indicator, E_{jt} denotes the index of employment protection legislation and G_{ij} is the sectoral trend employment growth. The matrix $\mathbf{D}$ encompasses a set of dummies that generally includes country, time and sectoral fixed effects, and their interactions depending on the specification.

We run a fully interacted model, including interaction terms between the cycle, EPL and trend growth as well as the triple interaction between these three covariates. Hence, we in principle allow for the effects of EPL on the cyclicality of job flows to depend on sectoral trend growth and *vice versa*, we allow for the effects of sectoral growth on the cyclicality of job flows to depend on EPL.

Let us concentrate on JR for a moment. Taking the derivative of (1) with respect to the business cycle, we derive an expression for the cyclicality of job flows

Table 2
Summary Statistics on Job Flows

	JR		JC		JD	
	mean	sd	mean	sd	mean	sd
Austria	7.96	3.74	4.60	2.94	3.37	2.24
Belgium	8.73	3.23	4.90	2.73	3.83	1.72
Denmark	9.87	2.59	6.33	2.27	3.54	1.95
Finland	10.06	3.84	6.85	3.27	3.21	2.37
France	8.30	2.80	5.04	2.55	3.25	1.24
Germany	8.10	2.53	4.29	2.47	3.81	1.64
Greece	9.36	7.22	5.78	5.18	3.57	3.01
Italy	11.91	2.99	8.22	2.65	3.69	2.13
Netherlands	10.14	3.86	5.95	3.20	4.18	1.97
Norway	11.35	5.98	7.34	4.65	4.01	2.75
Portugal	8.30	3.20	5.06	3.40	3.25	1.97
Spain	11.25	3.12	7.84	3.05	3.41	1.44
Sweden	11.11	3.77	7.32	3.03	3.79	2.18
UK	10.51	2.36	5.90	2.57	4.61	2.00

Note: Gross job flows are calculated from continuing firms, hence not accounting for firm demographics.

$$\frac{\partial JR_{ijt}}{\partial N_{ijt}} = \beta + E_{jt}\zeta + G_{ij}\eta + \left(E_{jt} \times G_{ij}\right)\vartheta, \tag{2}$$

which depends both on EPL and sectoral trend growth. Our primary interest relates to the marginal effects of EPL and trend growth on the cyclicality of job flows, hence to $\partial^2 JR_{ijt}/\partial N_{ijt}\partial E_{jt}$ and $\partial^2 JR_{ijt}/\partial N_{ijt}\partial G_{ij}$ respectively. Thus, taking the derivative of (2) with respect to EPL, a test for the hypothesis put forward by Garibaldi (1998) can be simply expressed as:

$$H_0 : \frac{\partial^2 JR_{ijt}}{\partial N_{ijt}\partial E_{jt}} = \zeta + G_{ij}\vartheta = 0; \quad H_1 : \frac{\partial^2 JR_{ijt}}{\partial N_{ijt}\partial E_{jt}} > 0. \tag{3}$$

If we reject the null at standard confidence levels this would suggest that more stringent EPL increases the cyclicality of job turnover. Obviously, this test will generally depend on G_{ij}, and consequently will be evaluated at different values of trend growth. Similarly, taking the derivative of (2) with respect to G_{ij} we can derive a test for Foote's hypothesis,

$$H_0 : \frac{\partial^2 JR_{ijt}}{\partial N_{ijt}\partial G_{ij}} = \eta + E_{jt}\vartheta = 0; \quad H_1 : \frac{\partial^2 JR_{ijt}}{\partial N_{ijt}\partial G_{ij}} > 0, \tag{4}$$

where a rejection of the null would imply that the cyclicality of job reallocation is higher in sectors with higher trend growth such as service industries.

In order to make inference about country patterns, we weight our regressions by the relative number of employees in each cell with respect to the total number of employees in the country. Thus, each country has equal weight in the final regressions.[13] Note that our EPL indicator misses the sectoral dimension in the panel, only varying across countries and over time. This might imply that observations are not independent within time and country clusters, biasing the standard errors in the regressions. All our specifications present heteroscedasticity robust standard errors clustered at the year and country level.[14]

3. Empirical Results

3.1. *The Cyclical Patterns of Job Reallocation*

We start the analysis by illustrating the cyclical patterns of job turnover. Following most of the literature, Table 3 shows Spearman correlations between job turnover and the output gap indicator. The pooled correlations are reported for five different groups: all sectors, services, manufacturing, growing sectors (those whose average growth rate is above the country average) and contracting sectors (those whose average growth rate is below the country average). As noted before, the period of observation spans at most

[13] Alternatively, one may argue that cells constructed from a larger number of firm observations are less likely to be affected by noise, and thus more likely to be representative of the sectoral employment dynamics. We have experimented with relative firm rather than employment weights in the regressions. The results, available from the authors, are very similar to those presented in the text.

[14] One might argue that EPL has limited variation over time and hence the relevant cluster might be the country rather than country and year. Results with robust standard errors to country clustering are virtually identical to those presented in the article.

Table 3

Spearman Correlations between Job Reallocation and Cycle

	All sectors	Services	Manufacturing	Growing	Contracting
Austria	0.018	0.186	−0.064	0.061	−0.136
Belgium	−0.059	−0.101	−0.067	−0.031	−0.099
Denmark	0.003	0.117	−0.054	0.071	−0.177
Finland	0.001	0.000	0.127	0.019	−0.415*
France	0.115	0.014	0.042	0.059	0.223
Germany	0.165*	0.197	−0.010	0.281*	−0.048
Greece	0.192*	0.365*	0.073	0.235*	−0.297
Italy	−0.102	0.038	−0.063	−0.100	0.067
Netherlands	−0.049	0.278*	−0.112	−0.012	−0.077
Norway	−0.109	0.175	−0.148	−0.132	−0.261
Portugal	0.129	0.155	0.232	0.064	0.089
Spain	−0.136*	−0.317*	0.061	−0.160*	−0.326*
Sweden	−0.110	0.137	−0.092	−0.106	−0.255
UK	−0.225*	0.081	−0.286*	−0.123	−0.418*

Note: *denotes significant at the 5% level. Correlations of job reallocation and the output gap across different groups, pooling the data from all sectors belonging to each group. The data are yearly observations for a total of 24 sectors. For a definition of the sectors see Footnote 6. Growing (contracting) sectors are those whose average growth rate is above (below) the country average.

between 1992–2001, and differs across countries and sectors. Job reallocation is in most cases a-cyclical with the clear exception of the UK and (perhaps more surprisingly) Spain, where the correlation between job reallocation and the indicator of the cycle is negative and statistically significant.[15] These correlations are in line with previous studies, suggesting a-cyclical labour flows in continental Europe in contrast with counter-cyclical patterns in the Anglo-Saxon countries. The cross-country differences are even more apparent when comparing country averages within manufacturing and services industries, or expanding and contracting sectors. With the sole exception of Spain, job reallocation is a-cyclical or pro-cyclical in growing sectors, but either a-cyclical or counter-cyclical in sectors with an average growth below the country mean. A somewhat similar pattern arises if the distinction is made between service and manufacturing sectors, the former group tending to present more pro-cyclical correlations. In all columns, the UK presents a lower correlation between JR and the cycle. Indeed, although differences across sectors are apparent, the ranking of countries is relatively stable across the different columns. Spearman pairwise correlations across the groups in the different columns are always positive and statistically significant, suggesting the importance of country effects.

3.2. *The Cyclical Patterns of Job Flows and Firing Restrictions*

Can firing restrictions account for the differences in the cyclicality of job turnover? Table 4 presents OLS estimates following (1) for JR. Column 1 includes year and

[15] Spain is characterised by a relatively stringent EPL. However, there is evidence suggesting that this legislation is to a large extent bypassed by the use of temporary employment contracts (Dolado *et al.*, 2002), which incidence is the highest in Europe. We examine the role of temporary contracts in the cyclicality of job flows in Section 4.

Table 4

Employment Protection and the Cyclical Behaviour of Job Reallocation

	(1) JR	(2) JR	(3) JR	(4) JR	(5) JR
Cycle	−0.169	−0.176	−0.183	−0.191	−0.147
	(2.44)*	(3.20)**	(2.34)*	(2.85)**	(3.18)**
EPL	−0.619	−0.806	−0.661	−0.871	−0.570
	(1.53)	(1.06)	(1.59)	(1.10)	(0.70)
Cycle × *EPL*	0.060	0.061	0.066	0.068	0.052
	(2.29)*	(2.95)**	(2.33)*	(2.72)**	(2.99)**
Cycle × *TrendG*	0.001	0.021	−0.002	0.020	−0.009
	(0.08)	(1.32)	(0.08)	(1.07)	(0.50)
TrendG	0.153	0.014	0.114	−0.016	
	(1.29)	(0.15)	(0.99)	(0.18)	
Cycle × *EPL* × *TrendG*	−0.001	−0.006	0.000	−0.005	0.009
	(0.15)	(0.94)	(0.01)	(0.70)	(1.00)
EPL × *TrendG*	0.173	0.112	0.188	0.122	
	(3.94)**	(2.97)**	(4.31)**	(3.35)**	
Intercept	11.958	10.098	9.275	8.039	10.785
	(11.96)**	(4.95)**	(9.37)**	(3.32)**	(3.15)**
Sector Dummy	Yes	Yes	Yes	Yes	Yes
Year Dummy	Yes	Yes	Yes	Yes	Yes
Country Dummy	No	Yes	No	Yes	Yes
Year × *Sector Dummy*	No	No	Yes	Yes	No
Country × *Sector Dummy*	No	No	No	No	Yes
Observations	2,080	2,080	2,080	2,080	2,080
R^2	0.40	0.46	0.44	0.50	0.56

Note: Robust standard errors clustered on country year. t-statistics in parenthesis. * and ** denote significant at the 5% and 1% level respectively. Data are weighted using sectoral employment weights adjusted such that each country has the same weight in the regression.

sectoral fixed effects but excludes country effects. Column 2 adds country effects to this basic specification. Column 3 adds sector and year interactions but excludes country dummies, hence accounting for any specific sectoral trends present in the data, while column 4 allows for both country effects and sector specific non-parametric trends. Finally, column 5 replaces the sector and time interactions with a set of country and sector interactions. In this restrictive specification, the effects of interest are identified from time variation within country by sector idiosyncratic patterns. Note that the interaction of country and sector fixed effects is perfectly collinear with *TrendG* and almost perfectly collinear with the interaction *EPL*× *TrendG*, which consequently are dropped from the regression in column 5. Results are very similar across the different specifications. Tests of the joint significance of the different sets of fixed effects suggest that country, sector, year and the interaction of sector and year fixed effects cannot be rejected at standard confidence levels. Results for the restricted set of covariates that are included in the regression with country and sector interactions are very similar to those of previous specifications. Hence, we will concentrate our discussion on the results presented in Column 4, including year, sector, country and the interaction of sector and year fixed effects in the regression.

In line with our previous discussion, the interaction term *Cycle* × *EPL* is positive and statistically significant at the 5% level. Note however that this term captures the response of JR to the cycle as a function of EPL for a sector with zero trend growth. In order to evaluate the impact of EPL on the cyclicality of job turnover we need to take into account also the term *Cycle* × *EPL* × *TrendG*. The first panel of Table 7 follows expression (5) and presents statistical tests of the significance of the marginal effect of EPL on the cyclicality of job flows for three different values of trend growth: zero, the median trend growth in sectors experiencing negative growth (from now on, declining sectors) and the median trend growth in sectors experiencing positive growth (from now on, expanding sectors). In all three cases the marginal effect of EPL on the cyclicality of JR presents a similar positive magnitude and is statistically significant at the five% level. Hence, we conclude that, as suggested by Garibaldi (1998), EPL increases the cyclicality of JR.

Is the magnitude of this marginal effect economically relevant? Figure 2*a* provides an illustration of the predicted effect of EPL on the cyclicality of JR following (2). We concentrate on the specification presented in Column 4 of Table 4 for a sector with zero *TrendG*, but a very similar picture would be obtained for expanding or declining sectors. The thick line stands for the predicted response of the cyclical behaviour of JR to changes in EPL, and the dotted lines are 95% confidence intervals. Note that the slope of this thick line is precisely $\partial^2 JR_{ijt}/\partial N_{ijt}\partial E_{jt}$, the marginal effect of EPL on the cyclicality of JR. According to Figure 2*a*, the predicted response of JR to the cycle is negative and statistically significant in relatively low EPL countries such as the UK and Denmark, and to a lesser extent Finland and Sweden. Instead, in most continental European countries (whose EPL index scores values larger than 2.5) we cannot statistically reject the a-cyclicality of JR. Hence, cross-country differences in the cyclicality of job turnover can be accounted for by differences in EPL.

Tables 5 and 6 show estimates of (1) for JC and JD following the same set of specifications discussed above regarding JR. As before, results are very similar independently of the set of dummies included in the regression, allowing us to concentrate directly on our preferred specification including year, sector, country and the interaction of year and sector fixed effects (column (4) in both Tables). The related marginal effects of EPL on the cyclicality of JC and JD are presented in columns (2) and (3) of Table 7, where the p-values refer to the significance of these marginal effects.

Let us concentrate first on column 3 of Table 7, which displays the results for JD. The marginal effect of EPL on the cyclicality of JD is positive and statistically significant (p-value = 0.007) when evaluated at zero *TrendG*. Hence, the apparent counter-cyclicality of JD is weakened as EPL increases. The magnitude of this response is higher in declining sectors and lower in growing sectors, becoming non statistically significant in the latter group (p-value = 0.211). Thus, the impact of EPL on job flow dynamics depends also on sectoral trend growth in a meaningful fashion. We will come back to this issue below. Figure 2*b* shows the predicted response of JD to the cycle as a function of EPL. In line with previous evidence for Anglo-Saxon countries, JD is strongly counter-cyclical in the UK, where EPL is lowest. However, as EPL increases above 2.5 we cannot reject the a-cyclicality of JD. Finally, the results in Table 7 suggest that the cyclicality of JC also increases as we move from low to high EPL countries, at least in growing and zero growth sectors. However, there is too much uncertainty regarding the

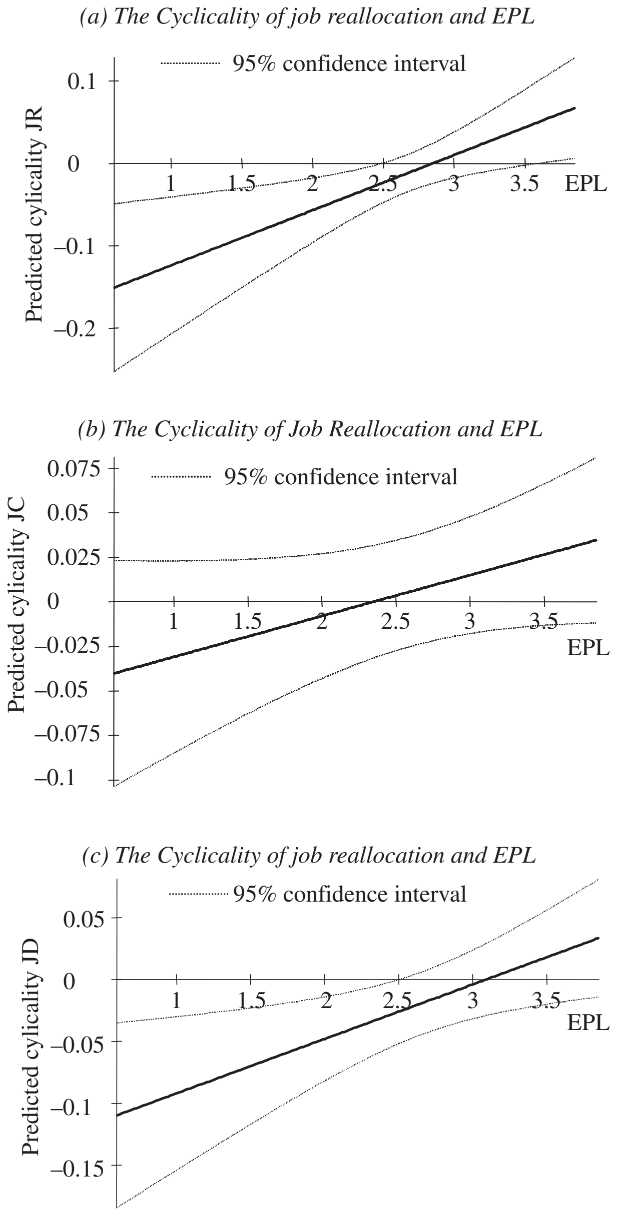

Fig. 2. *Predicted Responses of the Cyclicality of Job Flows to Changes in EPL.*

response of JC to the cycle. As Figure 2*c* shows, when we evaluate the predicted effects of EPL on the cyclicality of JC we can never reject an a-cyclical response.

3.3. *Job Flows, Sectoral Trend Growth and Firing Restrictions*

Let us turn now to the effect of trend growth on the cyclicality of job turnover. The coefficient of the interaction term *Cycle* × *TrendG* in Table 4 is positive, in line with

Table 5

Employment Protection and the Cyclical Behaviour of Job Creation

	(1) JC	(2) JC	(3) JC	(4) JC	(5) JC
Cycle	−0.025	−0.032	−0.047	−0.054	−0.004
	(0.58)	(0.89)	(1.08)	(1.35)	(0.11)
EPL	−0.350	−0.361	−0.391	−0.459	−0.246
	(1.43)	(0.61)	(1.57)	(0.77)	(0.37)
Cycle × *EPL*	0.014	0.017	0.021	0.023	0.009
	(0.90)	(1.27)	(1.38)	(1.65)	(0.68)
Cycle × *TrendG*	−0.022	−0.009	−0.022	−0.009	−0.039
	(1.79)	(0.83)	(1.64)	(0.74)	(2.38)*
TrendG	0.500	0.497	0.453	0.451	
	(5.88)**	(6.34)**	(5.07)**	(5.21)**	
Cycle × *EPL* × *TrendG*	0.008	0.005	0.008	0.005	0.019
	(1.65)	(1.10)	(1.48)	(0.97)	(2.40)*
EPL × *TrendG*	0.107	0.059	0.122	0.071	
	(3.56)**	(1.99)*	(3.86)**	(2.27)*	
Intercept	5.319	3.899	4.085	3.038	2.737
	(7.41)**	(2.37)*	(3.63)**	(1.47)	(1.33)
Sector Dummy	Yes	Yes	Yes	Yes	Yes
Year Dummy	Yes	Yes	Yes	Yes	Yes
Country Dummy	No	Yes	No	Yes	Yes
Year × *Sector Dummy*	No	No	Yes	Yes	No
Country × *Sector Dummy*	No	No	No	No	Yes
Observations	2080	2080	2080	2080	2080
R^2	0.56	0.59	0.60	0.63	0.63

Note: Robust standard errors clustered on country year. t-statistics in parenthesis. * and ** denote significant at the 5% and 1% level respectively. Data are weighted using sectoral employment weights adjusted such that each country has the same weight in the regression.

Foote (1998) hypothesis, but not different from zero at standard levels of statistical significance. The same conclusion is reached when we test for the fully interacted model following expression (4), where we evaluate the marginal effect at different values of EPL taking into account the term *Cycle* × *EPL* × *TrendG*. These results are presented in the second panel of Table 7. For all plausible values of EPL we can never reject the null of $\partial^2 JF_{ijt}/\partial N_{ijt}\partial G_{ij} = 0$. Hence, our results provide no support for Foote's hypothesis, suggesting that in our sample the cyclicality of job flows is similar in sectors with positive and negative trend growth.

This last result does not imply that sectoral trend growth is unimportant for the determination of job flows. Tables 4, 5 and 6 show instead that differences across sectors in trend growth are crucial for understanding the *level*, rather than the cyclical patterns of reallocation. Similarly, they illustrate an interesting interaction between trend growth and EPL in the determination of the level of job turnover. Note first that JC (JD) is apparently higher (lower) in faster growing sectors as suggested by the positive (negative) and statistically significant sign of *TrendG* in the regressions.[16] More interestingly, EPL mediates the response of the reallocation of labour to sectoral trend growth in a very similar fashion as it acts on the cyclicality of job turnover. The inter-

[16] Note that *TrendG* captures the effect of trend growth for zero EPL and zero output gap. However, once taking into account all plausible values of output gap and EPL we still find that JC (JD) increases (declines) with sectoral trend growth.

Table 6

Employment Protection and the Cyclical Behaviour of Job Destruction

	(1) JD	(2) JD	(3) JD	(4) JD	(5) JD
Cycle	−0.144	−0.144	−0.135	−0.136	−0.144
	(3.23)**	(3.60)**	(2.67)**	(2.86)**	(3.67)**
EPL	−0.269	−0.445	−0.270	−0.412	−0.324
	(1.45)	(0.81)	(1.40)	(0.70)	(0.55)
Cycle × *EPL*	0.045	0.044	0.046	0.044	0.043
	(2.64)**	(2.85)**	(2.41)*	(2.47)*	(2.64)**
Cycle × *TrendG*	0.023	0.030	0.021	0.029	0.030
	(2.52)*	(3.28)**	(1.90)	(2.50)*	(2.59)*
TrendG	−0.348	−0.483	−0.339	−0.467	
	(5.00)**	(8.64)**	(4.75)**	(7.92)**	
Cycle × *EPL* × *TrendG*	−0.009	−0.011	−0.008	−0.010	−0.010
	(2.75)**	(3.24)**	(2.21)*	(2.51)*	(1.98)
EPL × *TrendG*	0.066	0.053	0.066	0.051	
	(2.50)*	(2.32)*	(2.46)*	(2.19)*	
Intercept	6.639	6.198	5.190	5.001	8.048
	(8.29)**	(3.58)**	(11.34)**	(3.19)**	(3.25)**
Sector Dummy	Yes	Yes	Yes	Yes	Yes
Year Dummy	Yes	Yes	Yes	Yes	Yes
Country Dummy	No	Yes	No	Yes	Yes
Year × *Sector Dummy*	No	No	Yes	Yes	No
Country × *Sector Dummy*	No	No	No	No	Yes
Observations	2080	2080	2080	2080	2080
R^2	0.17	0.24	0.23	0.31	0.37

Note: Robust standard errors clustered on country year. t-statistics in parenthesis. * and ** denote significant at the 5% and 1% level respectively. Data are weighted using sectoral employment weights adjusted such that each country has the same weight in the regression.

Table 7

Marginal Effects

	JR		JC		JD	
	marginal effect	p-value	marginal effect	p-value	marginal effect	p-value
(*a*) *Marginal effect of EPL on the cyclicality of job flows*						
Declining sectors*	2.509	0.006	1.110	0.133	2.672	0.004
Zero trend growth	2.719	0.033	1.651	0.049	2.473	0.007
Growing sectors†	2.146	0.016	2.112	0.017	0.801	0.211
(*b*) *Marginal effect of trend growth on the cyclicality of job flows*						
EPL = 0.6	1.161	0.122	−0.641	0.739	2.465	0.007
EPL = 2.7	0.907	0.182	0.946	0.172	0.222	0.412
EPL = 3.7	0.061	0.475	1.073	0.141	−1.912	0.971
(*c*) *Marginal effect of EPL on the response of job flows to trend growth*						
Recession‡	3.462	0.000	1.523	0.064	3.137	0.001
Zero output gap	3.349	0.000	2.269	0.012	2.191	0.014
Expansion§	2.547	0.005	2.620	0.004	0.955	0.169

Note: All calculations correspond to the specifications presented in columns 4 of Tables 4, 5 and 6 for JR, JC and JD respectively. *Evaluated at the median of trend growth of declining sectors (−0.97%). †Evaluated at the median of trend growth of expanding sectors (3.09%). ‡Evaluated at the median value of negative output gap observations (−2.83%). §Evaluated at the median value of positive output gap observations (2.52%).

action $EPL \times TrendG$ is positive and statistically significant in all specifications for either JC, JD or JR. However, before making any inference on the effect of EPL on the response of job flows to the sectoral patterns of trend growth, we need to take into account the interaction $Cycle \times EPL \times TrendG$. From (1), we can write the response of job flows to trend growth as

$$\frac{\partial JF_{ijt}}{\partial G_{ij}} = \delta + N_{ijt}\eta + E_{jt}\theta + \left(N_{ijt} \times E_{jt}\right)\vartheta, \tag{5}$$

which is a function of EPL and the business cycle. The marginal effect of EPL in the response of job flows to trend growth ($\partial^2 JF_{ijt}/\partial G_{ij}\partial E_{jt}$) can be easily derived as $\theta + N_{ijt}\vartheta$. Hence, a test of the positive effects of EPL on the response of job flows to trend growth can be written as

$$H_0 : \frac{\partial^2 JF_{ijt}}{\partial G_{ij}\partial E_{jt}} = \theta + N_{ijt}\vartheta = 0; \quad H_1 : \frac{\partial^2 JF_{ijt}}{\partial G_{ij}\partial E_{jt}} > 0. \tag{6}$$

The third panel of Table 7 presents the test of statistical significance represented in (8) for JR, JC and JD evaluated at different values of the business cycle. Let us first concentrate on JD. When the output gap is zero a clear message arises: EPL makes the response of JD to sectoral trend growth less negative (p-value = 0.014). In order to illustrate this result we present in Figure 3*c* the response of JD to trend growth as a function of EPL as represented by (5).[17] The slope of the curve here is precisely $\partial^2 JD_{ijt}/\partial G_{ij}\partial E_{jt}$. As expected, the relationship between JD and trend growth is always negative, suggesting that JD in declining sectors is higher than in expanding sectors. However, the gap between expanding and declining sectors decreases as EPL becomes more stringent. According to Figure 3*c*, the gap between expanding and declining sectors in the level of JD approximately halves when one moves from the UK to Portugal, the two extremes regarding EPL provisions. To grab the intuition of this result imagine an extreme case where firing is forbidden and credibly enforceable by legislation. In this hypothetical country we would still observe some destruction of jobs, due to retirement of workers and voluntary quits. However, firms in sectors where demand is contracting would not be able to fire additional workers, implying that this labour attrition (the only source of job destruction in this example) is likely to be similar across expanding and declining sectors. Finally, coming back to Table 7, note that the magnitude of the slope coefficient increases in recessions and declines in expansions (to become actually non-significantly different from zero). These two findings point towards the same direction, suggesting that when firing is costly the burden of legislation falls on firms in declining sectors, and becomes even larger during recessions.

Something similar applies to JC. Figure 3b shows the response of JC to sectoral trend growth as a function of EPL. As the sign of $Cycle \times TrendG$ suggests, JC is positively related to trend growth. Moreover, the upward trend with respect to EPL, which is statistically significant according to the tests reported in Table 7, shows that the gap in JC between expanding and declining sectors widens as EPL increases. The

[17] This response is evaluated at zero output gap.

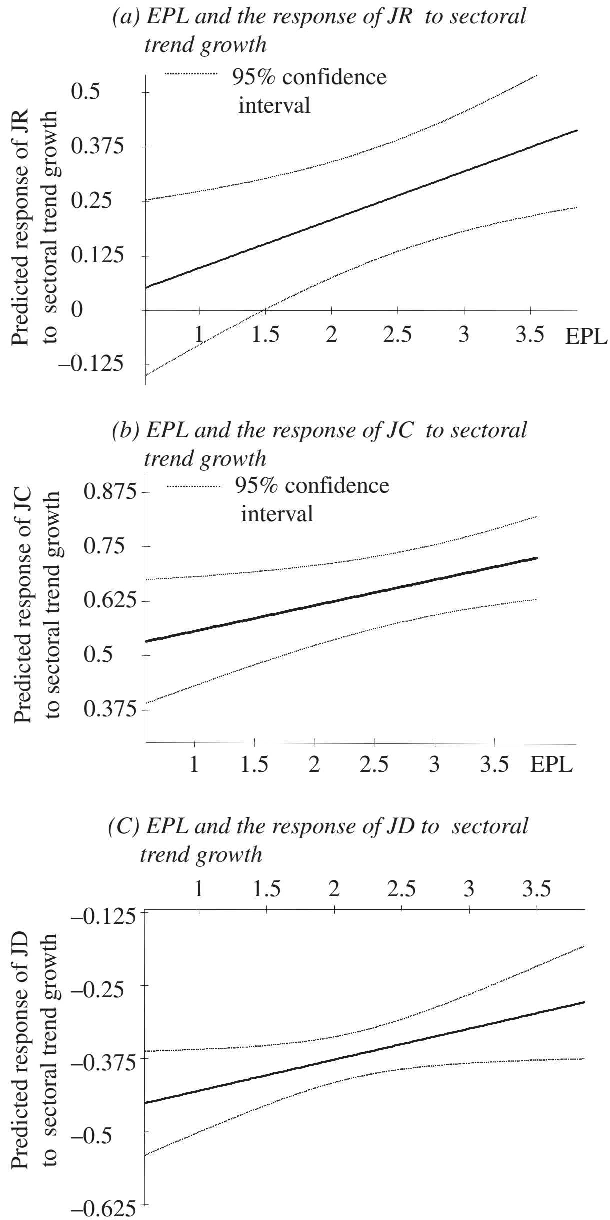

Fig. 3. *The Effect of EPL on the Response of Job Flows to Sectoral Trend Growth.*

rationale is similar to that discussed regarding JD. As long as more stringent EPL reduces the responsiveness of job destruction to trend growth, firms in declining sectors will accommodate a declining trend in demand by constraining the creation of new jobs, further reducing the job creation rate relative to firms operating in growing sectors. The effects of trend growth on JC and JD translate into JR. As the first column of Table 7 suggests, more stringent EPL reduces job reallocation in declining sectors relative to expanding sectors. These findings provide empirical support for models of adjustment costs featuring aggregate as well as idiosyncratic shocks such as Bentolila and Bertola (1990), suggesting that faster trend growth in a sector or country dampens the impact of firing costs on firm's hiring and firing decisions.

So far we have uncovered important heterogeneities on the effects of EPL in job flows depending on the sectoral business cycle and trend growth. When we evaluate instead the effects of EPL on the level of JR, there is unfortunately little we can say. As predicted by theory, EPL presents a negative sign in all the specifications presented in Table 4. When we evaluate the marginal effect of EPL for different values of the cycle and sectoral trend growth taking into account all the interactions in the regression the negative impact persists. However, in most cases we cannot reject at standard confidence levels that this negative effect is statistically different from zero. This is not surprising, since we have a relatively short sample and a reduced number of countries in order to evaluate the average impact of EPL on the level of job flows.[18] Whether this effect is actually equal to zero or just reflects the imprecision of our estimates is something we cannot address with our data.

4. Robustness Checks

In this Section we present a number of robustness checks of the main results presented above. Due to length restrictions, we focus on job reallocation. We first check for the robustness of the results with respect to alternative measures of employment protection and the business cycle. Our specifications allow for time and sector dummies and their interactions, as well as country effects. Column 1 of Table 8 present these estimates for the EPL measure developed by Blanchard and Wolfers (2000) and extended by Nickell *et al.* (2001) and Gómez-Salvador *et al.* (2004). With the new measure, the interaction terms *Cycle* $\times$ *EPL* and *EPL* $\times$ *TrendG* are positive and statistically significant at the 1% level. The message is not altered when we take into account the triple interaction *Cycle* $\times$ *EPL* $\times$ *TrendG*. We now turn to alternative measures of the cycle. Columns 2 and 3 test for the sensitivity of our results to the smoothing parameter of the HP filter. As suggested by Ravn and Uhling (2002), the smoothing parameter λ is set here to 6.5. As before, *Cycle* $\times$ *EPL* and *EPL* $\times$ *TrendG* remain positive and statistically significant for the EPL variable used in the text (column 2) and the new EPL variable (column 3). Finally, we introduce a new output gap measure using a Band-Pass filter as suggested by Baxter and King (1999) in columns 4 and 5. As standard in the literature we remove fluctuations that are too short (less than 2 years) or too long (more than 8 years) to be considered business cycle frequencies. Both effects of interest are signed as expected. However, while the

[18] The available empirical evidence on the effect of EPL on the level of job flows is mixed. Studies exploiting cross-country variation in EPL show a negative (although weak) association between EPL and job flows (OECD, 1999; Blanchard and Portugal, 2001; Gomez-Salvador *et al.*, 2004). Contrasting results are obtained in the micro empirical literature. Exploiting changes in regulations and differences in the enforcement of regulation as sources of identification, Bauer *et al.* (2004) and Kugler and Pica (2007) find no effects of changes in dismissal protection legislation on job turnover in Germany and Italy, respectively. Instead, Autor *et al.* (2007) find lower employment flows in US states following the adoption in court of wrongful-discharge protections laws.

Table 8

Robustness Check. Alternative EPL and Cycle Measures

Cycle Variable	HP ($\lambda = 100$) GS (2004)	HP ($\lambda = 6.25$) OECD(2004)	HP ($\lambda = 6.25$) GS (2004)	BK OECD(2004)	BK GS (2004)
EPL Variable	JR	JR	JR	JR	JR
Cycle	−0.183	−0.216	−0.207	−0.218	−0.202
	(2.81)**	(2.16)*	(2.15)*	(1.99)*	(1.94)
EPL	−2.011	−0.883	−1.991	−0.827	−1.937
	(1.57)	(1.12)	(1.56)	(1.09)	(1.57)
Cycle × *EPL*	0.065	0.075	0.071	0.077	0.069
	(2.73)**	(2.03)*	(2.04)*	(1.91)	(1.85)
Cycle × *TrendG*	0.028	0.020	0.028	0.018	0.024
	(1.47)	(0.73)	(1.09)	(0.64)	(0.94)
TrendG	0.045	−0.001	0.056	0.009	0.064
	(0.50)	(0.01)	(0.65)	(0.09)	(0.70)
Cycle × *EPL* × *TrendG*	−0.008	−0.003	−0.007	−0.003	−0.005
	(1.12)	(0.33)	(0.70)	(0.23)	(0.51)
EPL × *TrendG*	0.097	0.115	0.092	0.110	0.088
	(2.66)**	(3.15)**	(2.56)*	(2.94)**	(2.39)*
Intercept	11.421	7.950	11.228	7.983	11.249
	(3.01)**	(3.19)**	(2.92)**	(3.36)**	(3.07)**
Sector Dummy	Yes	Yes	Yes	Yes	Yes
Year Dummy	Yes	Yes	Yes	Yes	Yes
Country Dummy	Yes	Yes	Yes	Yes	Yes
Year × *Sector Dummy*	Yes	Yes	Yes	Yes	Yes
Observations	2080	2080	2080	2074	2074
R^2	0.50	0.50	0.50	0.50	0.50

Note: Robust standard errors clustered on country year. t-statistics in parenthesis. * and **denote significant at the 5% and 1% level respectively. GS (2004) refers to Gomez-Salvador *et al.* (2004). HP refers to the Hodrick-Prescott filter. BK refers to the Baxter-King band-pass filter. Data are weighted using sectoral employment weights adjusted such that each country has the same weight in the regression.

interaction *EPL* × *TrendG* remains significant at the 5% level, the interaction of EPL with the cycle is now significant at the 10% level.

In the next exercises we exclude the triple interaction *Cycle* × *EPL* × *TrendG*, which was never significant in our previous JR regressions. This greatly simplifies the interpretation of the hypotheses we want to contrast, inasmuch as the test of the effects of EPL on the cyclicality of JR represented in (3) boils down to a test of the sign and significance of the interaction term *Cycle* × *EPL*. Similarly, if the response of job flows to sectoral trend growth increases with EPL as suggested by the alternative hypothesis in (6), we should find a positive and significant coefficient of the interaction term *EPL* × *TrendG*.

Next, we explore the sensitivity of our results with respect to the number of sectors and countries included in the regressions. While our empirical strategy is expected to suffer less from this factor than standard cross-country regressions, it might still be the case that some of our results are driven by the inclusion of some specific country or sector. Our strategy follows Sala-i-Martin (1997) but focusing on the number of countries and sectors included in the regression rather than on the set of control variables. Very briefly, we look at the distribution of the coefficients of interest across the full set of regressions that result from dropping any combinations of three countries (or sectors) in our baseline specification (Column 4 in Table 4). Taking into account that the full sample of countries (sectors) is 14 (24), the resulting number of regressions is 560 (2600). We take next the averages of the estimated coefficients and their standard deviations across the different regressions. Under the assumption of normality, these two statistics are sufficient to calculate the cumulative distributive function (CDF_N) of the estimates and apply standard confidence levels. However, even if the estimates in every regression follow a t-Student distribution, it might be the case that the distribution of the estimates is not normal. Following Sala-i-Martin (1997), in this case we can still compute their cumulative distributive function (CDF_{NN}) as the average of the individual cumulative distributive functions.

The first panel of Table 9 shows the effects of changing the number of countries on our baseline results. Independently of the normality assumption, the null of each coefficient equal to zero would be rejected at the 5% level in the two cases of primary interest: *Cycle* × *EPL* and *EPL* × *TrendG*. As the second panel of Table 9 clearly shows, none of these results is either affected by the exclusion of different combinations of sectors.

Our final set of robustness checks concentrates on the possible role of competing institutional factors. Gómez-Salvador *et al.* (2004) find a negative impact of unemployment benefits, union co-ordination and the tax wedge on the level of JR within a cross-country framework. Other institutional indicators included in the analysis are the incidence of temporary contracts and the generosity of employment subsidies. In principle, we have no reason to expect any of these institutions to have a role on the determination of the cyclical behaviour of job flows, with the possible exception of

Table 9

Robustness Check. Sensitivity with Respect to the Number of Countries and Sectors Included in the Regression

	Mean	s.d	CDF_N	CDF_{NN}
Combining Countries (560 regressions)				
Cycle	−0.163	0.074	0.986	0.973
EPL	−0.864	0.932	0.823	0.807
Cycle × *EPL*	0.056	0.026	0.983	0.959
Cycle × *TrendG*	0.006	0.007	0.811	0.798
TrendG	−0.007	0.119	0.524	0.668
EPL × *TrendG*	0.121	0.047	0.995	0.970
Combining Sectors (2600 regressions)				
Cycle	−0.166	0.064	0.995	0.991
EPL	−0.870	0.795	0.863	0.862
Cycle × *EPL*	0.058	0.024	0.993	0.986
Cycle × *TrendG*	0.006	0.007	0.825	0.812
TrendG	−0.013	0.097	0.552	0.674
EPL × *TrendG*	0.122	0.038	0.999	0.998

Note: The results refer to all the regressions resulting from dropping any combinations of 3 countries (first part of the Table) or 3 sectors (second part of the Table) in the specification presented in Table 4, Column 4. CDF_N: cumulative distributive function under normality. CDF_{NN}: cumulative distributive function under non-normality assumption. All the specifications include time, sector and country fixed effects and a full set of time and sector interactions. Data are weighted using sectoral employment weights adjusted such that each country has the same weight in the regression.

temporary contracts. Temporary contracts might replace permanent employment when the latter is heavily protected by firing restrictions. Thus, we might expect that a higher incidence of temporary contracts counter-balances the positive role of EPL on the cyclical behaviour of JR.

Columns 1 and 2 in Table 10 present a full set of interactions between labour market institutions and the cycle variable. The set of institutional variables includes the EPL index, an index of the generosity of unemployment benefits, the tax wedge, the share of temporary contracts in total employment and the generosity of employment subsidies.[19] Hence, we add all institutional variables considered in Gómez-Salvador *et al.* (2004) with the exception of union coordination, which is time invariant within the sample period. The first aspect worth noting is that the interaction term *Cycle* × *EPL* is positive and statistically significant at the 1% level in both specifications, thus confirming our previous results. Contrary to our expectations, we cannot reject that *Cycle* × *Temp* is statistically equal from zero. A possible reason is that temporary contracts act only on the cyclicality of job flows through its complementarity with employment protection. Hence, once EPL is accounted for they have no role in the determination of JR. As expected, other institutions do not seem to affect the cyclicality of JR, with the exception of benefit duration that presents a positive and statistically significant effect (only when country dummies are present) on the cyclicality of JR. Column 3 shows that these conclusions are unaltered when a full set of country by sector fixed effects are included in the regression. Finally, columns 4 and 5 add the interactions between the institutional variables and trend growth. Note that their inclusion does not alter the positive and significant coefficient of *Cycle* × *EPL*. The interaction term *EPL* × *TrendG* is positive and statistically significant, but is not robust to the inclusion of country dummies, while the impact of the remaining institutional variables (with the exception of temporary contracts) on JR does not seem to be affected by sectoral trend growth.

5. Conclusions

The primary aim of this article is to evaluate the impact of employment protection legislation (EPL) on the cyclicality of job turnover. To this end, we build a firm level dataset for 14 European countries and 24 industries, which overcomes previous problems of comparability of job flow statistics, and allows to extend the analysis of employment dynamics to manufacturing and non-manufacturing sectors. Our empirical strategy does not suffer from the small sample problems typically encountered in cross-country studies, since we focus on the differential impact of EPL on the employment adjustment in different sectors and phases of the business cycle.

[19] The index of the duration of unemployment benefits (Nickell *et al.*, 2001) is defined as a weighted average of benefits received during the second, third and fifth year of unemployment divided by the benefits in the first year of unemployment. It ranges from 0 (if benefit provision stops after 1 year) to 1 (for a constant benefit after 5 years). The tax wedge (Nickell *et al.*, 2001) measures the difference between the real (monetary) labour cost faced by the firms and the consumption wage received by the employees, and is calculated as the sum of employment tax rate, the direct tax rate and inderect tax rate normalised by GDP. The indicator of temporary contracts is the share of workers holding temporary contracts in the total number of employees at the ISIC-1 sectoral level (source: LFS). The sectoral employment subsidies indicator is the share of sectoral and *ad hoc* state aid as a percentage of GDP (*Source*: Eurostat).

Table 10

The Role of Other Labour Market Institutions in the Determination of Job Turnover

	(1) JR	(2) JR	(3) JR	(4) JR	(5) JR
Cycle	−0.120	−0.189	−0.235	−0.257	−0.269
	(0.75)	(1.48)	(1.78)	(1.74)	(2.39)*
Cycle × TrendG	0.008	0.014	0.013	0.013	0.016
	(1.28)	(2.62)*	(2.13)*	(1.84)	(3.05)**
TrendG	0.444	0.318	0.713	−0.234	−0.117
	(9.68)**	(7.46)**	(1.59)	(0.86)	(0.54)
Cycle × EPL	0.121	0.104	0.123	0.132	0.114
	(3.40)**	(3.02)**	(3.34)**	(3.84)**	(3.30)**
EPL	−0.839	−0.752	−0.838	−1.132	−0.892
	(1.16)	(0.85)	(0.92)	(1.52)	(1.01)
Cycle × Temp	−0.001	0.001	0.004	0.001	0.002
	(0.33)	(0.40)	(1.51)	(0.26)	(0.85)
Temp	0.051	0.128	0.061	−0.009	0.075
	(2.40)*	(8.44)**	(0.95)	(0.29)	(2.99)**
Cycle × Benefits	0.196	0.197	0.210	0.233	0.220
	(1.65)	(2.00)*	(2.20)*	(2.04)*	(2.33)*
Unemployment Benefits	−2.591	−10.582	−9.896	−2.288	−10.194
	(3.36)**	(2.83)**	(2.38)*	(1.89)	(2.66)**
Cycle × TaxWedge	−0.003	−0.002	−0.001	−0.001	−0.001
	(1.27)	(0.95)	(0.47)	(0.52)	(0.52)
TaxWedge	−0.017	−0.228	−0.231	−0.038	−0.234
	(0.53)	(1.71)	(1.61)	(1.13)	(1.64)
Subsidies × Cycle	−0.172	−0.139	−0.218	−0.183	−0.153
	(1.78)	(1.83)	(2.57)*	(1.97)	(2.02)*
Employment Subsidies	−0.208	−0.892	−0.791	0.234	−0.762
	(0.16)	(0.77)	(0.68)	(0.16)	(0.61)
EPL × TrendG				0.169	0.083
				(2.73)**	(1.33)
Temp × TrendG				0.012	0.009
				(3.19)**	(2.65)**
Benefits × TrendG				−0.013	−0.056
				(0.05)	(0.31)
TaxWedge × TrendG				0.007	0.004
				(1.91)	(0.91)
Subsidies × TrendG				−0.185	−0.026
				(1.17)	(0.16)
Intercept	11.945	23.560	30.909	13.124	24.086
	(6.96)**	(2.67)**	(3.26)**	(6.16)**	(2.70)**
Country Dummy	No	Yes	Yes	No	Yes
Year × Sector Dummy	Yes	Yes	No	Yes	Yes
Country × Sector Dummy	No	No	Yes	No	No
Observations	1972	1972	1972	1972	1972
R^2	0.53	0.59	0.66	0.54	0.60

Note: Robust standard errors clustered on country year. t-statistics in parenthesis. * and **denote significant at the 5% and 1% level respectively. All the specifications include time and sector dummies.

We find that EPL induces a positive co-movement of job turnover with different indicators of the cycle. This positive co-movement is mainly driven by the behaviour of job destruction. In line with Garibaldi (1998) theoretical predictions, we show that firing restrictions dampen the volatility of job destruction during the cycle, having a milder effect on job creation. These results are statistically significant and robust to different specifications including country, sectoral and time effects and their interactions. Moreover, the estimated effects of firing restrictions on employment dynamics are large in magnitude and can account for observed cross-country differences in the cyclical patterns of job flows.

Our estimates further suggest that the impact of EPL on job turnover is closely related to trend growth in the sector. Accordingly, the burden of legislation falls on firms in declining sectors, implying that differences in reallocation across countries with different degrees of employment protection are likely to be more noticeable in contracting sectors, such as manufacturing, than in growing sectors, such as most service industries.

Our results have potentially important policy implications. Understanding the behaviour of gross job flows over the cycle and its determinants is fundamental for the assessment of the extent and need of stabilisation policies. Our findings strongly suggest a role for EPL in stabilising employment fluctuations along the business cycle. In countries with little protection of jobs, recessions are times of strong reallocation. On the contrary, when firing a worker is costly and time consuming the reallocation of labour is smoothed along the business cycle. The welfare impact of such insulation of job destruction to economic downturns will depend on several factors, including the availability of alternative insurance mechanisms and possible productivity losses due to legislation as those documented in Autor *et al.* (2007). A fully fledged cost benefit analysis of this stabilising device constitutes a promising line for further research.

Universitat de Girona and IZA
Università Luiss "Guido Carli"

References

Autor, D.H., Donohue, J.J. III, and Schwab, S.J. (2006). 'The costs of wrongful-discharge laws', *Review of Economics and Statistics*, vol. 88(2), pp. 211–31.
Autor, H., Kerr, W.R. and Kugler, A.D. (2007). 'Does employment protection reduce productivity? Evidence from US States', Economic Journal, vol. 117, pp. F189–F217.
Baldwin, J., Dunne, T. and Haltiwanger, J. (1998). 'A comparison of job creation and job destruction in Canada and the United States', *Review of Economics and Statistics*, vol. 80(3), pp. 347–56.
Bauer, T.K., Bender, S. and Bonin, H. (2004). 'Dismissal protection and worker flows in small establishments', IZA Discussion Papers, No. 1105, April, Bonn.
Baxter, M. and King, R.G. (1999). 'Measuring business cycles: approximate band-pass filters for economic time series', *Review of Economics and Statistics*, vol. 81(4), pp. 575–93.
Bentolila, S. and Bertola, G. (1990). 'Firing costs and labour demand: how bad is eurosclerosis?', *Review of Economic Studies*, vol. 57, pp. 381–402.
Bertola, G., Blau, F. and Kahn, L.M. (2007). 'Labour market institutions and demographic employment patterns', *Journal of Population Economics*, forthcoming.
Blanchard, O. and Portugal, P. (2001). 'What hides behind an unemployment rate? Comparing Portuguese and U.S. labor markets', *American Economic Review*, vol. 91(1), pp. 187–207.
Blanchard, O. and Wolfers, J. (2000). 'The role of shocks and institutions in the rise of european unemployment: The aggregate evidence', Economic Journal, vol. 110, pp. C1–33.
Boeri, T. (1996). 'Is job turnover countercyclical?', *Journal of Labour Economics*, vol. 14, pp. 603–25.
Boeri, T. and Cramer, U. (1992). 'Employment growth, incumbents and entrants: evidence from Germany', *International Journal of Industrial Organization*, vol. 10, pp. 545–65.
Boeri, T. and Jimeno-Serrano, J.F. (2005). 'The effects of employment protection legislation: learning from variable enforcement', *European Economic Review*, vol. 49(8), pp. 2057–77.
Borgello, A., Garibaldi, P. and Picelli, L. (2002). 'Employment protection and the size of firms', LABORatorio Revelli, Torino.
Caballero, R. and Hammour, M. (1994). 'The cleansing effect of recessions', *American Economic Review*, vol. 84(5), pp. 1250–368.

Contini, B., Pacelli L., Filippi, M., Lioni G. and Revelli, R. (1995). 'A study on job creation and job destruction in Europe', Study for the Commission of the European Communities, DGV, Torino.

Davis, S.J. and Haltiwanger, J. (1990). 'Gross job creation and destruction: microeconomic evidence and macroeconomic implications', *NBER Macroeconomic Annual*, vol. 5, pp. 123–68.

Davis, S.J. and Haltiwanger, J. (1992). 'Gross job creation, gross job destruction and employment reallocation', *Quarterly Journal of Economics*, vol. 107(3), pp. 819–63.

Davis, S.J. and Haltiwanger, J. and Schuh, S. (1996), *Job Creation and Destruction*, Cambridge, MA: The MIT Press.

Dolado, J. García-Serrano, C. and Jimeno-Serrano, J.F. (2002). 'Drawing lessons from the boom of temporary jobs in Spain', ECONOMIC JOURNAL, vol. 112(480), pp. F270–95.

Dolado, J.J. and Gómez-Salvador, R. (1995). 'Creación y destrucci ón de empleo en el sector privado manufacturero español: un análisis descriptivo', *Investigaciones Económicas*, vol. 19(3), pp. 371–93.

Dolado, J.J., Jansen, M. and Jimeno-Serrano, J.F. (2005). 'Dual employment protection legislation: a framework for analysis', CEPR Discussion Paper No. 5033, CEPR.

Foote, C.S. (1998). 'Trend employment growth and the bunching of job creation and job destruction', *Quarterly Journal of Economics*, vol. 113(3), pp. 809–34.

Garibaldi, P. (1998). 'Job flows dynamics and firing restrictions', *European Economic Review*, vol. 42(2), pp. 245–75.

Gómez-Salvador, R., Messina, J. and Vallanti, G. (2004). 'Gross job flows in Europe', *Labour Economics*, vol. 11, pp. 469–85.

Kahn, L.M. (2007). 'The impact of employment protection mandates on demographic temporary employment patterns: international microeconomic evidence', ECONOMIC JOURNAL, vol. 117, pp. F333–56.

Koeniger, W. and Prat, J. (2007). 'Employment protection, product market regulation and firm selection', ECONOMIC JOURNAL, vol. 117, pp. F302–32.

Konings, J. (1995). 'Job creation and job destruction in the UK manufacturing sector', *Oxford Bulletin of Economics and Statistic*, vol. 57, pp. 5–24.

Kugler, A.D. and Pica, G. (2007). 'Effects of employment protection on worker and job flows: evidence from the 1990 Italian reform', *Labour Economics*, forthcoming.

Lagarde, S., Maurin, E. and Torelli, C. (1994). 'Créations et suppressions d'emplois en France: une étude de la période 1984-1992', *Economie et Prévision*, vol. 113-4 (2/3), pp. 67–88.

Mortensen, D.T. and Pissarides, C.A. (1994). 'Job creation and job destruction in the theory of unemployment', *Review of Economic Studies*, vol. 61(3), pp. 349–415.

Nickell, S., Nunziata, L., Wolfgang, O. and Quintini, G. (2001). 'The Beveridge curve, unemployment and wages in the OECD from the 1960s to the 1990s', CEP Discussion Paper DP0502, July.

OECD (1994). *Employment Outlook*. Paris: OECD.

OECD (1999). *Employment Outlook*. Paris: OECD.

OECD (2004*a*). *Employment Outlook*. Paris: OECD.

OECD (2004*b*). 'Detailed description of current employment protection legislation', Background material for Chapter 2 of *OECD Employment Outlook*, Paris: OECD.

Ravn, M. and Uhlig, H. (2002). 'On adjusting the HP-filter for the frequency of observations', *Review of Economics and Statistics*, vol. 84(2), pp. 371–6.

Sala-i-Martin, X. (1997). 'I just ran two million regressions', *American Economic Review*, vol. 87(2), pp. F178–83.

Schivardi, F. and Torrini, R. (2004). 'Threshold effects and firm size: the case of firing costs', Temi di Discussione della Banca d'Italia no. 504, June.

Schuh, S. and Triest, R. K. (1998), 'Job reallocation and the business cycle: new facts for an old debate', in *Beyond Shocks: What Causes Business Cycles*? Proceedings from the Federal Reserve Bank of Boston Conference Series no. 42.

Stiglbauer, A.M., Stahl, F., Winter-Ebmer, R. and Zweimüller, J. (2002). 'Job reallocation and job destruction in a regulated labour market: The case of Austria', Working Paper No. 78, Österreichische Nationalbank.

Vereck, S. (2004). 'Threshold effects of dismissal protection legislation in Germany', IZA Discussion paper no. 991.