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ABSTRACT

Job Flow Dynamics and Firing Restrictions: Evidence from Europe*

We exploit homogeneous firm level data of manufacturing and non-manufacturing sectors to study the impact of firing restrictions on job flow dynamics across 14 European countries. We find that more stringent firing laws dampen the response of job destruction to the cycle, thus making job turnover less counter-cyclical. Moreover, the impact of firing costs on job creation and job destruction varies across sectors, depending on sector-specific trend growth. Our findings clearly suggest that such costs are more important in contracting than in growing sectors.

JEL Classification: J23, J63, J68

Keywords: gross job flows, Europe, business cycle, firing costs

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1 Introduction

Following Davis and Haltiwanger's (1990, 1992) seminal work, a large empirical literature has looked at the stylized facts of job creation and job destruction using firm or establishment level data. A controversial stylized fact relates to the relationship between job turnover and the business cycle. While all studies report a pro-cyclical movement of job creation and a counter-cyclical movement of job destruction, the volatility of these two flows over the business cycle differs across countries. Estimates for the US, Canada and the UK typically show that the increase in job destruction during economic downturns tends to be stronger than the increase in job creation during upturns, resulting in counter-cyclical movements of job reallocation (the sum of job creation and job destruction).[1] By contrast, estimates for continental European countries present a less clear picture, with job reallocation tending to be a-cyclical or slightly pro-cyclical.[2]

In spite of this contrast, a number of models have been developed to capture the apparent counter-cyclicality of job 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 adjustment costs. As a consequence, job creation will tend to be smoothed over the business cycle and job destruction will be concentrated in recessions, implying a counter-cyclical pattern in job reallocation. In Mortensen and Pissarides (1994), counter-cyclical movements of job reallocation are generated by the 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. Hence job turnover is counter-cyclical.

Garibaldi (1998) takes stock of the cross-country differences in job flow dynamics and shows that extending the Mortensen and Pissarides (1994) framework to allow for the presence of fixed adjustment costs associated with dismissals can result in a-cyclical or

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

[2]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).

even pro-cyclical movements of job reallocation. 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 patterns relies on differences in 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 asymmetric behavior of job creation and job destruction in US data appears to be a peculiarity of the manufacturing sector. In service industries, the positive trend of employment growth implies a higher variability of job creation over the business cycle, resulting in a pro-cyclical movement of job turnover.

This paper contributes to the understanding of the role of EPL on labour market dynamics. 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. Moreover, it presents a difference-in-difference identification strategy that avoids the problems of lack of degrees of freedom typically encountered in the empirical macro literature when identifying the impact of labour market institutions. Our findings indicate that firing restrictions play a significant role in shaping the response of job flows to fluctuations in the business cycle, while sectoral characteristics are less important. As suggested by Garibaldi (1998), we find that firms facing tight firing restrictions smooth job destruction over the business cycle. Hence, countries where EPL is more stringent present more pro-cyclical job turnover in all productive sectors.

A closely related result relates to the impact of EPL on the level of job turnover. From a theoretical perspective, EPL should reduce both job creation and job destruction and therefore labour turnover.[3] In spite of this unambiguous theoretical prediction, the

[3] See Bertola (1999) and the references therein.

empirical cross-country evidence on the effects of EPL on aggregate job flows presents mixed results.[4] Bertola and Rogerson (1997) argue that other institutions, notably wage compression, are also present in tight EPL countries, thus counter-balancing the effects of firing restrictions on job flows. Blanchard and Portugal (2001) and Wolfers (2005) argue instead that firing restrictions should affect mostly short term employment fluctuations having little impact on annual estimates of job flows. Indeed, they provide evidence suggesting noticeable cross-country differences in labour market dynamics at the quarterly or seasonal frequency, and relate them to firing restrictions. A third possible explanation explored in this paper relates to differences in trend growth. Bentolila and Bertola (1990) show that higher trend growth is expected to dampen the impact of firing restrictions on job flows. Exploiting the sectoral nature of our data, our findings clearly suggest that the expected negative impact of EPL on job turnover is weakened in sectors characterized by an expanding employment trend. Thus, previous studies that failed to control for differences across countries in aggregate trend growth or the phase of the business cycle might have missed an important element at the time of evaluating the impact of firing restrictions on employment dynamics.

The rest of the paper is organized as follows. Next section presents the main characteristics of the data and Section 3 sets out the empirical methodology. The main results of the paper are presented in Section 4. Section 5 performs a series of robustness checks and Section 6 draws some concluding remarks.

2 The data

Our main data source is Amadeus, a firm-level database collected by the Bureau van Dijk (BvD) from balance sheet data in European countries.[5] The information is collected by the national Chambers of Commerce and homogenized by BvD applying uniform formats to allow accurate cross-country comparisons. The period of analysis used for this study spans from 1992 to 2001 depending on the country, and the sample includes all EU-15

[4] See OECD (2004) for a recent survey of the empirical literature.

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

countries plus Norway with the exception of Luxemburg and Ireland.

Amadeus has several important advantages for the study of job flow dynamics 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 one to accurately identify 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 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 it is precisely job turnover of continuing firms the component of total job turnover that is more likely to be affected by firing restrictions (OECD, 1999). A second limitation relates to the sampling of Amadeus, which introduces a bias against very small firms.[6] This is common in firm level data sets, but is potentially important when measuring job flows since a relevant 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.[7] 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 legislation.[8] This sampling bias is unlikely to affect our results, as long as it remains relatively constant over time, since our empirical strategy mostly relies on within country and sector comparisons by exploiting the differential impact of EPL across different phases of the business cycle.

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

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

[8]Evidence suggests that threshold effects are present, although are quantitatively small. See Borgello et al. (2002) and Schivardi and Torrini (2004) for a discussion of the Italian case.

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 most convincingly, employment growth rates from Amadeus follow quite closely the growth rate of employment in the LFS, suggesting that the sample in Amadeus is representative of the total firm's population. Figure 1 shows annual employment growth in 24 different sectors and 14 countries as measured in Amadeus, against employment growth measured in those sectors by STAN, the Structural Analysis Database constructed by the OECD. We do not expect a perfect correlation here, 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.

There are several indices of employment protection in the literature. Our preferred indicator is the most recent index developed by the OECD (2004), which ranges theoretically from 0 to 6, and empirically from 0.5 to 3.5, 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. An alternative measure of employment protection was first developed by Blanchard and Wolfers (2000) and updated by Nickell et al. (2005) and Gomez 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.

Job flow statistics from Amadeus are merged with employment and value added data at the sectoral level from STAN. To this purpose, we construct annual job flow statistics for 24 sectors, which are those covered in STAN. The advantage of STAN is that it contains long time series 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.[9] In Section 5 we report robustness checks using employment growth measured by

[9] The output gap is constructed applying the Hodrick-Prescott filter to value added series in each sector over the period 1970-2002. Regressions using alternative output gap series obtained from a Band-Pass filter over a window of 2 to 8 years yielded qualitatively similar results.

STAN rather than the output gap as a measure of the cycle. Although often used in the literature, the disadvantage of employment growth over the output gap indicator is that the former is likely to be affected by employment protection. Thus, our main focus will be on the output gap as a measure of the business cycle.

3 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 as 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 $JR_{ijt} = JC_{ijt} + JD_{ijt}$.

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

$$\begin{aligned} JF_{ijt} &= \alpha + N_{ijt}\gamma \left(1 + F_{jt}\beta + G_{ij}\phi + (F_{jt} \times G_{ij})\,\delta\right) + \\ &\quad + (F_{jt} \times G_{ij})\,\varphi + G_{ij}\theta + D\beta + \mu_j + \varepsilon_{ijt} \\ \text{for } i &= 1, ..., 24 \text{ and } j = 1, ..., 14 \end{aligned} \tag{1}$$

, where JF_{ijt} denotes job flows (job reallocation, job creation or job destruction depending on the specification), N_{ijt} is a business cycle indicator, D is a set of sectoral and time dummies, F_{jt} denotes for the index of employment protection legislation, G_{ij} is the sectoral trend employment growth (measured as the average net employment growth in each sector over the sample period) and μ_j stands for a country fixed effect.

The coefficients of primary interest are $\gamma\beta$ and $\gamma\phi$, which correspond to the interaction terms between the business cycle indicator and the EPL index on the one hand, and sectoral trend growth on the other. When the dependent variable is JR, a positive sign on $\gamma\beta$ would support Garibaldi (1998) empirical hypothesis suggesting that more stringent EPL increases the cyclicality of job turnover. Similarly, Foote (1998) result for the US would be confirmed by our sample of European countries if $\gamma\phi > 0$, suggesting more pro-cyclical turnover in sectors experiencing higher trend growth. Note that we further interact the EPL index with trend employment growth and allow for a triple interaction of this term with the indicator of the cycle. Thus, we assess whether firing costs may have a different impact (on the cyclicality as well as on the level of job flows) depending on the average trend growth in each sector.

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.[10]

4 Empirical results

4.1 The cyclical properties of job reallocation

We start the analysis by illustrating the cyclical patterns of job turnover. Following most of the literature, Table 1 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).[11] As noted before, the period of observation spans at most between 1992-2001, and differs across countries and sectors. Overall job reallocation is in most cases a-cyclical with the clear exception of the United Kingdom and (perhaps more surprisingly) Spain, where the correlation between job reallocation and the indicator of

[10] 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 virtually unaffected by the weights used.

[11] 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, defense and education; Health and social work; Other community, social and personal services

the cycle is negative and statistically significant.[12] 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.

4.2 Job dynamics and firing restrictions

Can firing restrictions account for the cross-country differences in the cyclicality of job turnover? Table 2 presents OLS estimates following equation 1 for JR. Column 1 includes year and sectoral dummies, but excludes country dummies. According to this specification, EPL has a direct negative impact on the level of job turnover, which is significant at the 1% level. In line with Garibaldi (1998) theoretical predictions, the interaction term $Cycle * EPL$ is positive and statistically significant at the 5 percent level, suggesting that JR is less counter-cyclical in the presence of firing restrictions. However, we do not find significant differences in the cyclical patterns of JR in different sectors depending on their trend growth. The coefficient of the interaction term $Cycle * TrendG$ is positive, in line with Foote (1998) hypothesis, but far from standard levels of statistical significance. Column 2 introduces country dummies into the regression. The main effect of introduc-

[12]Spain is characterized 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), whose incidence is the highest in Europe, resulting in higher job turnover (Gomez-Salvador et al, 2004). We examine this issue further below.

ing country dummies is observed on the coefficient of EPL, which retains its negative sign but is now only significant at the 5% level. This is not surprising, given the little information of the EPL index in the within variation of our short panel of countries. In half of the countries considered there has been no changes in EPL legislation according to our index, and only Italy presents more than one change in legislation during the sample period. More importantly, the interaction term $Cycle * EPL$ is virtually unaffected by the inclusion of country dummies, retaining its positive and statistically significant sign. Columns 3 and 4 add to the list of controls a full set of $Sector * Year$ dummies, which should account for any sector specific trends that might by confounded with the effects we want to capture here. Neither the EPL coefficient nor its interaction with the cycle are affected by the inclusion of this set of dummies.

An interesting additional result refers to the different role of employment protection across sectors, depending on their trend growth. In all specifications presented in Table 2, $EPL * TrendG$ presents a positive and statistically significant effect. The interpretation is quite intuitive, and requires to keep in mind the negative sign of EPL in the regressions. Accordingly, the negative role of EPL on JR is less important in fast growing sectors. This finding provides 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 cost on firm's hiring and firing decisions. Finally, note that the coefficient of the triple interaction $Cycle * EPL * TrendG$ is never statistically different from zero.

Tables 3 and 4 show estimates of eq. 1 for JC and JD respectively. As before, columns (2) and (4) add country dummies to the baseline specification, while columns (3) and (4) additionally include a full set of interactions between sectoral and year dummies. We find a negative impact of EPL on the levels of JC and JD in all specifications, but the coefficients are only statistically significant when country dummies are excluded from the regression. As regards the role of EPL on the cyclicality of these two flows, the interaction term $Cycle * EPL$ is always positive in JC and JD regressions, but only statistically significant when JD is the dependent variable. This yields further support to Garibaldi (1998) theoretical predictions, suggesting that the rate at which firms destroy obsolete jobs is less responsive to the cycle in countries with more stringent employment protection legislation. Tables 3 and 4 yield further light into the role of trend growth on the determination of job flows, and its interaction with employment protection. Note first that, quite trivially, JC (JD) is higher (lower) in faster growing sectors as suggested by the positive (negative) and statistically significant sign of $TrendG$ in the regressions. More interestingly, and consistently across all specifications, we find that the negative impact of EPL in JC and JD is dampened in faster growing sectors, as the interaction term $EPL{*}TrendG$ is always positive and statistically significant. Further, note that the triple interaction $Cycle * EPL * TrendG$ is not significant in JC, but presents a negative and significant sign (only when country dummies are included) in the JD specifications. The latter suggests that the stabilizing effect of EPL on the cyclicality of JD is more important in contracting than in growing sectors. Putting it differently, the negative impact of EPL on JD is more important in contracting sectors (as suggested by the positive coeff. $EPL * TrendG$), and this effect is reinforced during economic downturns.

In order to illustrate the magnitudes in the response of labour market flows to the cycle for varying degrees of employment protection Figure 2 simulates the estimates presented in the second column (including country dummies) of Table 2 for a sector with the sample average growth. The thick line represents the actual response of the cyclical behavior of JR to changes in EPL, and the light lines stand for 95 % confidence intervals. According to this graph, JR would be counter-cyclical in a country like the UK (cycle = 0.5) but a-cyclical in most continental European countries (whose EPL values are typically larger than 2). Figures 3 and 5 replicate the analysis for JC and JD respectively, thus simulating the estimates in column 2 of tables 3 and 4. They clearly show that JC would be basically a-cyclical in the UK, while JD would by strongly counter-cyclical, explaining the counter-cyclical pattern observed in JR. At the other extreme, JC would be pro-cyclical and JD a-cyclical in the country with the most stringent EPL laws (e.g. in Portugal where $EPL = 3.7$).

Our next set of graphs concentrates on the effects of EPL on the levels of job flows, rather than on their response to the cycle. As discussed above, the lack of variation of EPL over time difficults the interpretation of the main effect of EPL when country dummies are included in the regression. However, our estimates of EPL are very similar in magnitude in specifications with and without country dummies. The main effect of including country dummies lies on the efficiency of the estimates, as standard errors almost double. Figure 5 presents simulations of the effects of EPL on JR, JC and JD as a function of the business cycle following the specification in column 1 of tables 2, 3 and 4, thus, excluding country dummies. A distinction is made between growing (right hand side graphs) and contracting sectors (in the left hand side).[13] As expected, in contracting sectors EPL reduces job turnover, more significantly when the sector experiences a recession than when the sector experiences an expansion. In contrast, the impact of EPL on JR for expanding sectors is never significantly different from zero. A similar pattern is observed regarding JC and JD, with the only difference that the negative impact of EPL on JC is virtually not affected by the phase of the business cycle. Our estimates thus suggest that the negative association between EPL and job flows is stronger in sectors experiencing negative trend growth, but might not be visible in expanding sectors or during expansionary periods. Figure 4 present the graphs of the specifications including country dummies (column 2 in tables 2, 3 and 4). Confidence bands grow larger here, and the negative effect of EPL on JC is never statistically significant. However, we still find a negative and significant effect of EPL on JD and JR flows within contracting sectors, which weakens as we move from a recession into an expansion.

5 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. In columns 1 and 2 of Table 5 we present our baseline estimates (Columns 1 and 2 of Table 2) for the EPL measure developed by Blanchard and Wolfers (2000) and extended by Gómez-Salvador et al. (2004). The new measure yields a negative

[13] In the two simulations the effect of EPL on job flows has been evaluated at the mean employment growth rate of contracting ($trendG \leq 0$) and expanding sectors ($trendG > 0$).

coefficient on the EPL level that is negative and statistically significant at the 1 percent level even in the presence of country dummies. Similarly, the interaction terms $Cycle * EPL$ and $EPL * TrendG$ are positive and statistically significant at standard levels in both specifications. Columns 3 and 4 show that our results are not much affected by the measure of the cycle. We return here to our preferred measure of EPL, but use employment growth instead of the output gap as a cycle measure. The interaction term $Cycle*EPL$ is positive and statistically significant at the 1 percent level. Similarly, EPL and $EPL*TrendG$ retain their expected signs, but are only statistically significant when country dummies are excluded from the regression. Finally, columns 5 and 6 shows that our main results are unaffected when we combine the new EPL variable with employment growth as a cycle measure.

We explore next the sensitivity 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 2 in Table 1). 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) 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 CDF as the average of the individual cumulative distributive functions.

The first part of Table 6 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 percent level in the two cases of primary interest: $Cycle*EPL$ and $EPL*TrendG$. Even EPL maintains its negative sign and significance at the 5 percent level, quite a surprising result taking into account that country dummies are present in the regression. A similar result is obtained in the second part of the table, which clearly shows that the number of sectors included in the regressions does not alter the main message of our baseline specification.

Our final set of robustness checks concentrates on the possible role of competing factors. Gómez-Salvador et al. (2004) find a negative impact of unemployment benefits, union coordination 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 reasons to expect any of these institutions to have a role on the determination of the cyclical behavior of job flows, with the possible exception of 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 behavior of JR.

Columns 1 and 2 in Table 7 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.[14] Hence, it adds 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

[14] The index of the duration of unemployment benefits (OECD, 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) between the real (monetary) labour cost faced by the firms and the consumption wage received by the employees is calculated as the sum of employment tax rate, the direct tax rate and inderect tax rate normalized 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).

and statistically significant in both specifications, thus confirming our previous results. Contrary to our expectations $Cycle * Temp$ is not statistically different 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 the somewhat puzzling negative and significant (at the 10% level when country dummies are present) effect of employment subsidies. Gómez-Salvador et al. (2004) find a negative effect of employment subsidies on JD, which is partially compensated by a positive (although non significant) impact on JC. Our results seem to indicate that the reduction of JD due to employment subsidies is more important during economic upturns. Columns 3 and 4 add the interactions between institutional variables and trend growth.[15] Note that their inclusion does not alter the positive and significant coefficient of $Cycle * EPL$. The interaction of $EPL * TrendG$ 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 again of employment subsidies) on JR does not seem to be affected by sectoral trend growth.

6 Conclusions

This paper evaluates the impact of employment protection legislation (EPL) on the cyclicality of job turnover using comparable sectoral firm level data for 14 European countries. Our data set 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 novel empirical strategy does not suffer from the small sample problems typically encountered in cross-country studies, since we focus on the impact EPL has on the employment adjustment in different sectors and phases of the business cycle.

We find that EPL induces a positive co-movement of job turnover with different

[15] The triple interactions $Institution * Cycle * TrendG$ were never found significantly different from zero, and thus are excluded from the regression.

indicators of the cycle. This positive co-movement is mainly driven by the behavior of job destruction. In line with 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. Moreover, the simulations presented in the paper show that the effects of firing restrictions on employment dynamics are large in magnitude, being able to account for observed cross-country patterns in the cyclicality of job flows.

Our estimates further suggest that the negative impact of EPL on job turnover is closely related to trend growth in the sector. Accordingly, firms in fast growing sectors appear less affected by firing costs, displaying higher job creation and job destruction.

Our results have potentially important policy implications. Understanding the behavior of gross job flows over the cycle and its determinants is fundamental for the assessment of the extent and need for stabilization policies. In line with an abundant theoretical literature, our findings strongly suggest a role for EPL in stabilizing employment fluctuations, which in the absence of better insurance mechanisms against labour income risk might be taken into account when evaluating alternative structural reforms.

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Tables and Figures

Table 1: 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 percent level. The table shows the response of job reallocation to 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 the period (depends on the country) 1992-2001. For a definition of the sectors see Footnote 11. Growing (contracting) sectors are those whose average growth rate is above (below) the country average.

Table 2: Employment protection and the cyclical behavior of job reallocation

	(1) JR	**(2)** JR	**(3)** JR	**(4)** JR
Cycle	-0.169	-0.176	-0.183	-0.191
	(2.55)*	(2.74)**	(2.53)*	(2.73)**
EPL	-0.619	-0.806	-0.661	-0.871
	(3.78)**	(1.96)*	(3.84)**	(2.06)*
$Cycle * EPL$	0.060	0.061	0.066	0.068
	(2.03)*	(2.05)*	(2.14)*	(2.12)*
$Cycle * TrendG$	0.001	0.021	-0.002	0.020
	(0.08)	(1.29)	(0.09)	(1.08)
$Cycle * EPL * TrendG$	-0.001	-0.006	0.000	-0.005
	(0.14)	(0.85)	(0.01)	(0.69)
$EPL * TrendG$	0.173	0.112	0.188	0.122
	(4.17)**	(2.79)**	(4.25)**	(2.84)**
TrendG	0.153	0.014	0.114	-0.016
	(1.37)	(0.13)	(0.97)	(0.14)
Intercept	11.958	10.098	9.275	8.039
	(12.13)**	(7.13)**	(12.18)**	(4.57)**
Country Dummy	No	Yes	No	Yes
$Sector * year$ *Dummy*	No	No	Yes	Yes
Observations	2080	2080	2080	2080
R^2	0.40	0.46	0.44	0.50

Note: Robust standard errors. t-statistics in parenthesis. * and ** denote significant at the 5 and 1 percent level respectively. All the specifications include time dummies and industry dummies.

Table 3: Employment protection and the cyclical behavior of job creation

	(1) JC	**(2)** JC	**(3)** JC	**(4)** JC
Cycle	-0.025	-0.032	-0.047	-0.054
	(0.58)	(0.76)	(1.06)	(1.23)
EPL	-0.350	-0.361	-0.391	-0.459
	(3.50)**	(1.21)	(3.77)**	(1.56)
$Cycle * EPL$	0.014	0.017	0.021	0.023
	(0.77)	(0.87)	(1.07)	(1.15)
$Cycle * TrendG$	-0.022	-0.009	-0.022	-0.009
	(1.81)	(0.80)	(1.57)	(0.67)
$Cycle * EPL * TrendG$	0.008	0.005	0.008	0.005
	(1.56)	(1.04)	(1.38)	(0.93)
$EPL * TrendG$	0.107	0.059	0.122	0.071
	(3.81)**	(2.03)*	(3.99)**	(2.25)*
TrendG	0.500	0.497	0.453	0.451
	(6.28)**	(5.94)**	(5.38)**	(5.06)**
Intercept	5.319	3.899	4.085	3.038
	(8.43)**	(3.97)**	(3.99)**	(2.02)*
Country Dummy	No	Yes	No	Yes
$Sector * year$ *Dummy*	No	No	Yes	Yes
Observations	2080	2080	2080	2080
R^2	0.56	0.59	0.60	0.63

Note: Robust standard errors. t-statistics in parenthesis. * and ** denote significant at the 5 and 1 percent level respectively. All the specifications include time dummies and industry dummies.

Table 4: Employment protection and the cyclical behavior of job destruction

	(1) JD	(2) JD	(3) JD	(4) JD
Cycle	-0.179	-0.172	-0.159	-0.157
	(4.45)**	(4.28)**	(3.73)**	(3.66)**
EPL	-0.330	-0.244	-0.316	-0.200
	(3.59)**	(0.58)	(3.32)**	(0.45)
$Cycle * EPL$	0.055	0.054	0.051	0.051
	(3.22)**	(3.10)**	(2.88)**	(2.83)**
$Cycle * TrendG$	0.022	0.035	0.017	0.033
	(2.04)*	(3.30)**	(1.40)	(2.69)**
$Cycle * EPL * TrendG$	-0.007	-0.012	-0.006	-0.012
	(1.66)	(2.87)**	(1.27)	(2.50)*
$EPL * TrendG$	0.088	0.061	0.083	0.053
	(3.72)**	(2.66)**	(3.32)**	(2.14)*
TrendG	-0.356	-0.493	-0.339	-0.475
	(5.59)**	(8.22)**	(5.08)**	(7.36)**
Intercept	6.753	5.777	4.880	4.612
	(12.71)**	(4.41)**	(10.56)**	(3.69)**
Country Dummy	No	Yes	No	Yes
$Sector * year$ *Dummy*	No	No	Yes	Yes
Observations	2080	2080	2080	2080
R^2	0.21	0.29	0.26	0.34

Note: Robust standard errors. t-statistics in parenthesis. * and ** denote significant at the 5 and 1 percent level respectively. All the specifications include time dummies and industry dummies.

Table 5: Robustness check. Alternative EPL and cycle measures

Cycle Indicator:	Output Gap		Employment Growth		Employment Growth	
EPL variable:	GS (2004)		OECD 2004		GS (2004)	
	(1)	(2)	(3)	(4)	(5)	(6)
	JR	JR	JR	JR	JR	JR
Cycle	-0.169	-0.168	-0.308	-0.319	-0.246	-0.268
	(2.80)**	(2.89)**	(3.22)**	(3.54)**	(2.85)**	(3.31)**
EPL	-0.607	-1.996	-0.591	-0.657	-0.572	-1.795
	(4.04)**	(3.24)**	(3.73)**	(1.56)	(3.92)**	(3.01)**
$Cycle * EPL$	0.059	0.058	0.117	0.117	0.092	0.095
	(2.20)*	(2.14)*	(2.57)*	(2.68)**	(2.18)*	(2.36)*
$Cycle * TrendG$	0.012	0.028	0.061	0.070	0.065	0.081
	(0.81)	(1.80)	(1.86)	(2.27)*	(2.16)*	(2.92)**
$Cycle * EPL * TrendG$	0.127	0.059	0.173	0.067	0.109	0.072
	(1.24)	(0.58)	(1.40)	(0.53)	(1.00)	(0.63)
$EPL * TrendG$	-0.005	-0.009	-0.014	-0.015	-0.015	-0.019
	(0.82)	(1.30)	(1.07)	(1.25)	(1.30)	(1.68)
TrendG	0.177	0.092	0.142	0.070	0.162	0.068
	(4.75)**	(2.45)*	(3.12)**	(1.42)	(4.03)**	(1.50)
Intercept	11.871	13.684	12.240	10.155	12.105	13.518
	(12.34)**	(7.13)**	(11.96)**	(7.07)**	(12.06)**	(7.14)**
Country Dummy	No	Yes	No	Yes	No	Yes
$Sector * year$ *Dummy*	No	No	No	No	No	No
Observations	2080	2080	2098	2098	2098	2098
R^2	0.40	0.46	0.41	0.47	0.41	0.47

Note: Robust standard errors. t-statistics in parenthesis. * and ** denote significant at the 5 and 1 percent level respectively. All the specifications include time dummies and industry dummies. GS (2004) refers to Gomez-Salvador et al. (2004)

Table 6: Robustness check. Sensitivity with respect to the number of countries and sectors included in the regression

Combining Countries (560 regressions)				
	$Mean$	$s.d$	CDF_N	CDF_{NN}
$Cycle$	-0.185	0.077	0.99	0.99
EPL	-0.790	0.491	0.95	0.91
$Cycle * EPL$	0.063	0.034	0.97	0.96
$Cycle * TrendG$	0.023	0.021	0.86	0.85
$TrendG$	0.033	0.133	0.60	0.66
$Cycle * EPL * TrendG$	-0.006	0.008	0.78	0.76
$EPL * TrendG$	0.105	0.049	0.98	0.95
Combining Sectors (2600 regressions)				
$Cycle$	-0.177	0.068	1.00	0.99
EPL	-0.800	0.440	0.97	0.96
$Cycle * EPL$	0.062	0.032	0.97	0.97
$Cycle * TrendG$	0.021	0.018	0.88	0.87
$TrendG$	0.019	0.116	0.56	0.65
$Cycle * EPL * TrendG$	-0.006	0.007	0.78	0.77
$EPL * TrendG$	0.110	0.043	0.99	0.99

Note: Robust standard errors. t-statistics in parenthesis. All the specifications include time dummies, industry and country dummies. The results refer to all the regressions resulting from droping any combinations of 3 countries (first part of the table) or 3 sectors (second part of the table) in the specification presented in Table 2, Column 2. CDFN : cumulative distributive function under normality assumption. CDFNN: cumulative distributive function under non-normality assumption

Table 7: The role of other labor market institutions in the determination of job turnover

	(1) JR	(2) JR	(3) JR	(4) JR
Cycle	-0.138	-0.129	-0.153	-0.151
	(0.85)	(0.81)	(0.94)	(0.94)
$Cycle * TrendG$	0.006	0.009	0.008	0.011
	(0.73)	(1.16)	(0.99)	(1.34)
$TrendG$	0.571	0.576	0.396	0.392
	(8.17)**	(7.48)**	(1.24)	(1.28)
$Cycle * EPL$	0.148	0.111	0.149	0.115
	(2.92)**	(2.34)*	(3.04)**	(2.47)*
EPL	-0.296	-0.569	-0.782	-0.917
	(1.09)	(0.90)	(2.17)*	(1.36)
$Cycle * Temp$	0.002	0.002	0.002	0.002
	(0.71)	(0.62)	(0.85)	(0.77)
$Temp$	0.031	0.077	0.016	0.056
	(1.23)	(1.95)	(0.26)	(0.83)
$Cycle * Benefits$	0.130	0.093	0.145	0.110
	(1.23)	(0.94)	(1.40)	(1.10)
$Unemployment\ Benefits$	-2.047	-10.257	-2.438	-10.055
	(3.90)**	(4.94)**	(2.80)**	(4.62)**
$Cycle * TaxWedge$	-0.003	-0.002	-0.003	-0.002
	(0.93)	(0.69)	(1.00)	(0.74)
$TaxWedge$	-0.006	-0.244	-0.007	-0.254
	(0.34)	(4.43)**	(0.29)	(4.33)**
$Subsidies * Cycle$	-0.231	-0.175	-0.217	-0.163
	(2.13)*	(1.78)	(2.13)*	(1.72)
$Employment\ Subsidies$	-0.660	-0.572	0.530	0.169
	(1.14)	(0.75)	(0.71)	(0.20)
$EPL * TrendG$			0.166	0.111
			(2.05)*	(1.43)
$Temp * TrendG$			0.005	0.007
			(0.61)	(0.90)
$Benefits * TrendG$			0.150	0.109
			(0.74)	(0.53)
$TaxWedge * TrendG$			0.000	0.002
			(0.05)	(0.30)
$Subsidies * TrendG$			-0.419	-0.326
			(2.43)*	(1.86)
Intercept	12.044	26.215	12.689	26.962
	(9.75)**	(6.35)**	(6.80)**	(6.35)**
$Time * Sector\ Dummy$	No	No	No	No
Country Dummy	No	Yes	No	Yes
Observations	1972	1972	1972	1972
R^2	0.42	0.46	0.42	0.46

Note: Robust standard errors. t-statistics in parenthesis. * and ** denote significant at the 5 and 1 percent level respectively. All the specifications include time dummies and industry dummies.

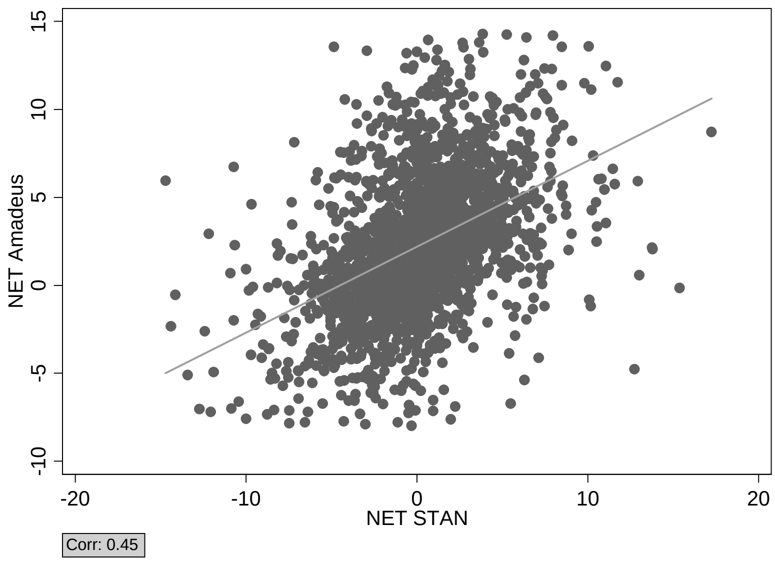

Figure 1: Sectoral Employment Growth. STAN vs. AMADEUS

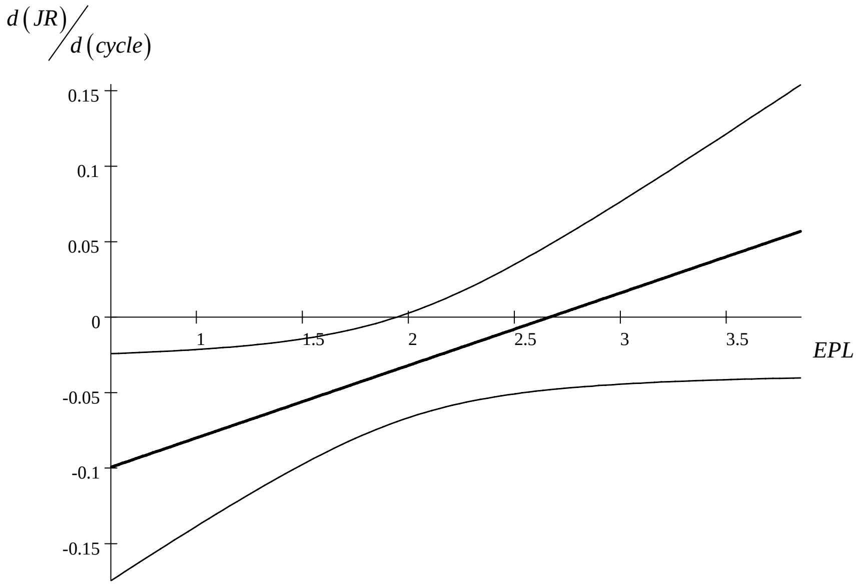

Figure 2: The cyclicality of Job Reallocation and EPL

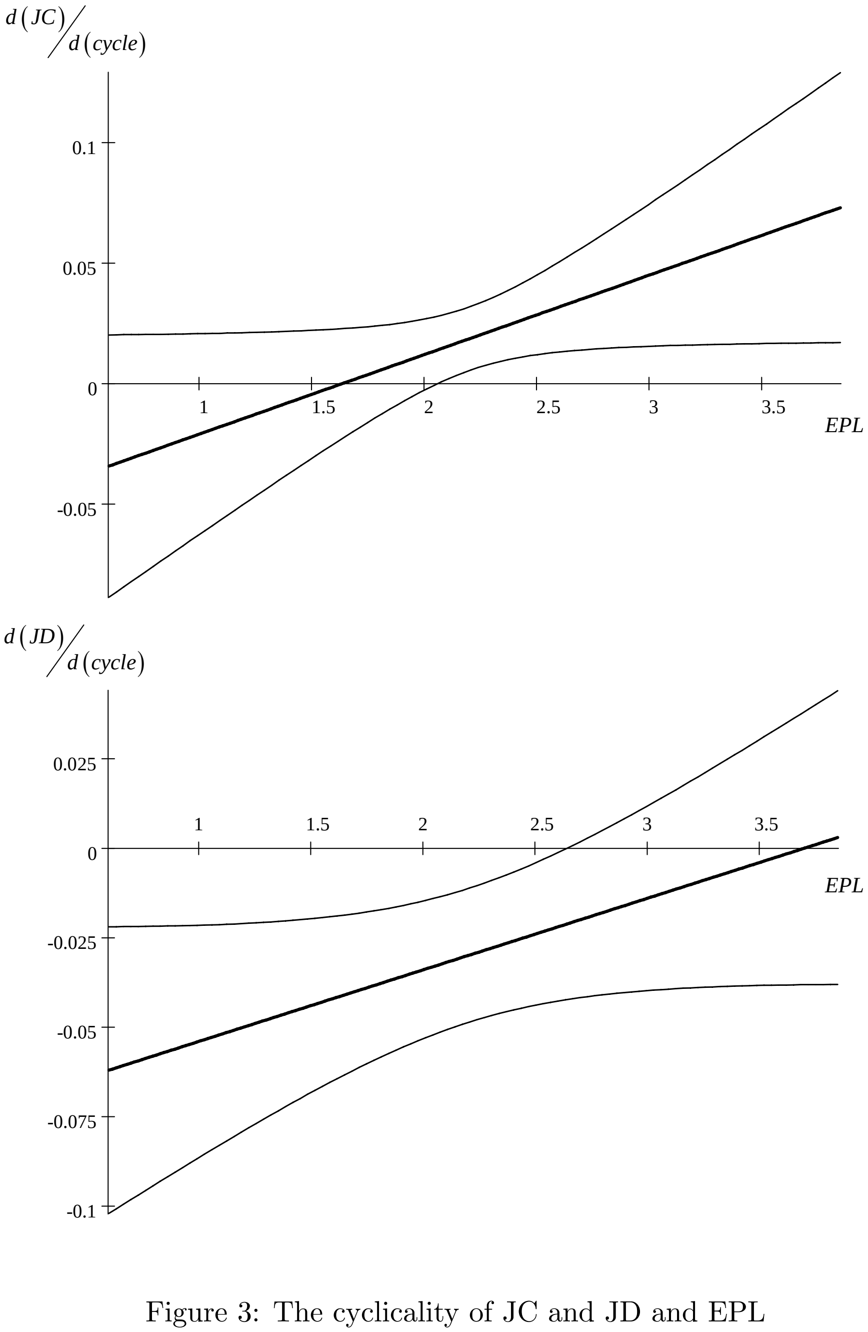

Figure 3: The cyclicality of JC and JD and EPL

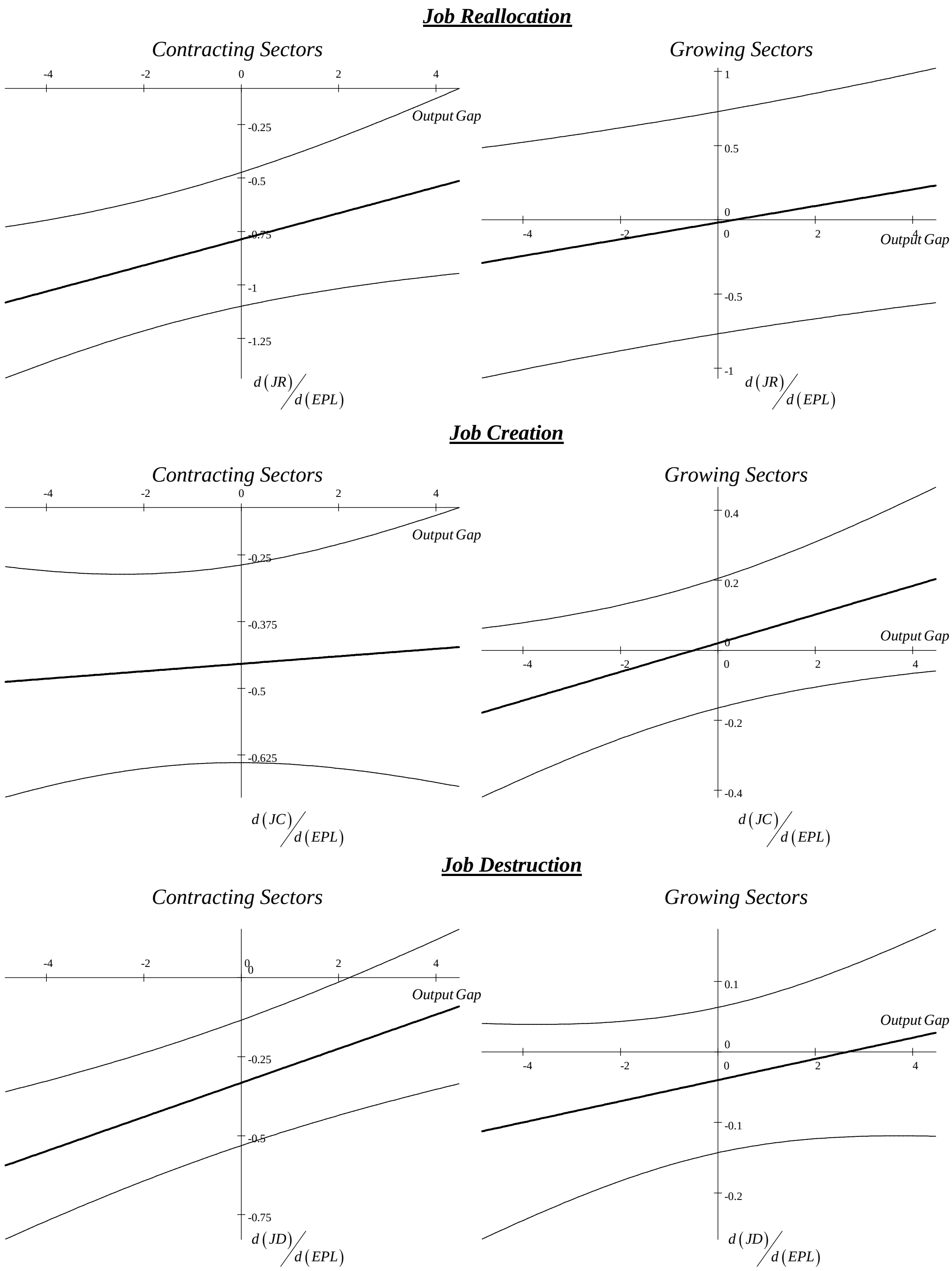

Figure 4: The response of job flows to changes in EPL as a function of the business cycle when country dummies are excluded

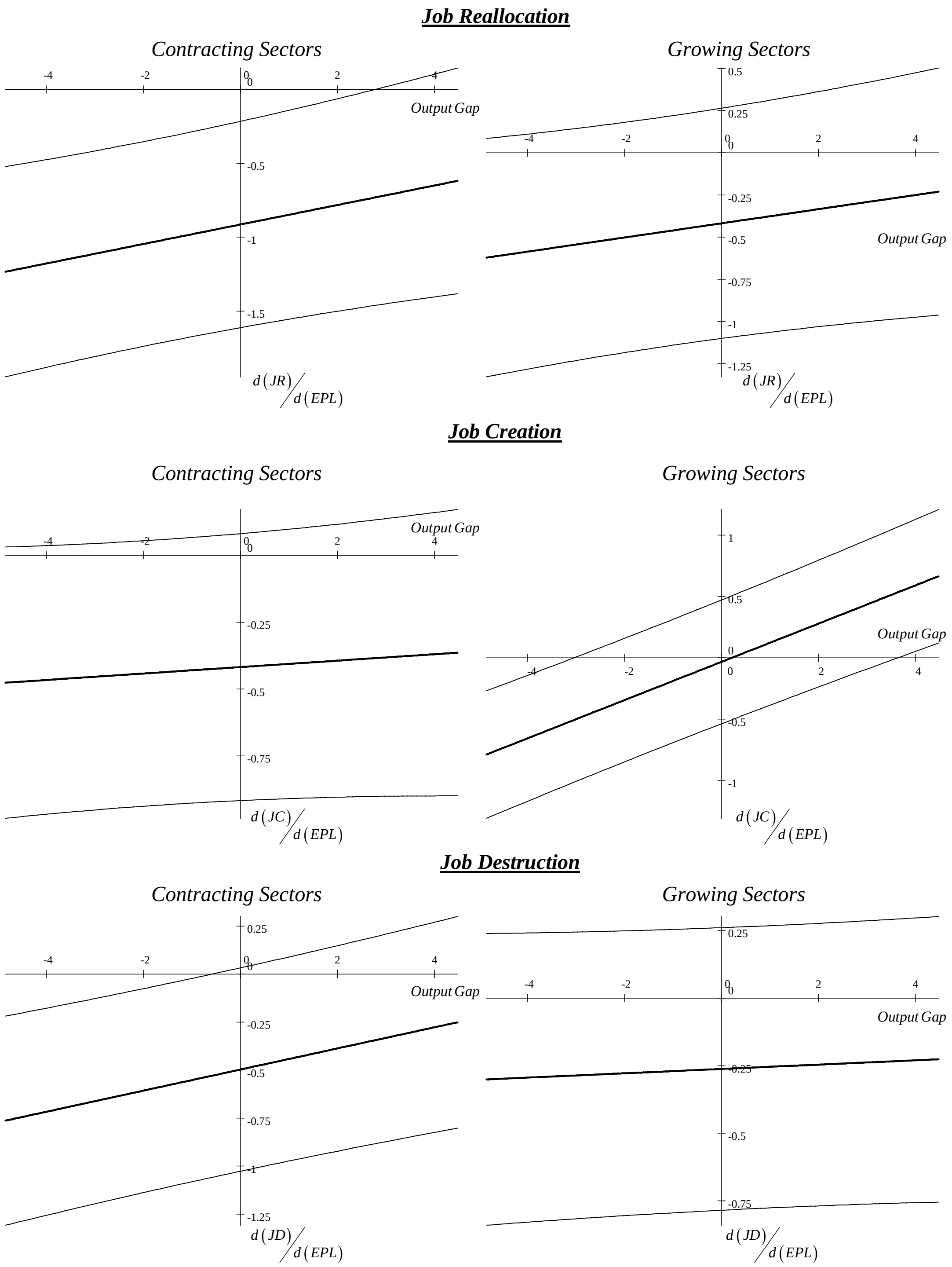

Figure 5: The response of job flows to changes in EPL as a function of the business cycle controlling for country effects