22/10/2012

Dismissal Protection and Worker Flows in OECD Countries*

Evidence from Cross-country/Cross-industry Data

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Abstract

Exploiting a unique dataset including cross-country comparable hiring and separation rates by type of transition for 24 OECD countries, 23 business-sector industries and 13 years, we study the effect of dismissal regulations on different types of gross worker flows, defined as one-year transitions. We use both a difference-in-difference approach – in which the impact of regulations is identified by exploiting likely cross-industry differences in their impact – and standard time-series analysis – in which the effect of regulations is identified through regulatory changes over time. We find that the more restrictive the regulation, the smaller is the rate of within-industry job-to-job transitions, in particular towards permanent jobs. By contrast, we find no significant effect as regards separations involving an industry change or leading to non-employment. The extent of reinstatement in the case of unfair dismissal appears to be the most important regulatory determinant of gross worker flows. We also present a large battery of robustness checks that suggest that our findings are robust.

Keywords: gross worker flows; industry-specific human capital; job-to-job transitions; EPL; reinstatement; cross-country data

JEL codes: J23; J24; J62, J63

*The data used for this paper are available at https://sites.google.com/site/bassaxsite/home/files/BGdata.zip , with the exception of ECHP microdata. We are grateful to Philippe Askenazy, Luc Behaghel, Giuseppe Bertola, Eve Caroli, Antonio Ciccone, Marc Gurgand, Adriana Kugler, John Haltiwanger, Stefano Scarpetta, Alessandro Sembenelli and one anonymous referee as well as audiences in Louvain-la-Neuve, Nuremberg, Paris and Rome for helpful comments and discussions. Sebastien Martin provided excellent research assistance. All errors are, obviously, ours. We gratefully acknowledge financial support from CEPREMAP. The opinions expressed in this paper are those of the authors and cannot be attributed to the OECD or its Member Countries.

Introduction

Market-based economies are characterised by a continuous reallocation of labour resources. New firms are created; existing firms expand, contract or shut down. A number of firms do not survive their first few years in the market, while other successful young businesses develop rapidly. In the process, large numbers of jobs are created and destroyed. At the same time many individuals enter the market and fill new job vacancies, while others change jobs or leave employment. Each year, more than 20% of jobs, on average, are created and/or destroyed, and around one-third of all workers are hired and/or separate from their employer (see *e.g.* OECD, 2009).

A large body of theoretical and empirical literature suggests that employment protection legislation (EPL hereafter), and especially dismissal regulation, is a key determinant of labour reallocation. From a theoretical viewpoint, standard equilibrium models of the labour market (e.g. Bentolila and Bertola, 1990, and Bertola, 1990) describe firms' optimal behaviour in the presence of positive firing costs – as well as wage rigidities, financial market imperfections and/or uncertainty about the future of the firm – and show that the best strategy for firms is to reduce both job creation and destruction, with an ambiguous effect on average employment levels.[1] These predictions are by and large confirmed by the empirical literature: both microeconometric evaluations of policy reforms and cross-country macroeconometric studies tend to find, with few exceptions, that restrictive dismissal regulations hinder job creation and hiring while simultaneously compressing job destruction and separations.[2] In other words, stringent dismissal regulations dampen the reallocation of labour resources across firms.

In this paper we ask whether dismissal regulations affect also where labour resources are reallocated. Put it another way, in economies with less stringent regulations, do separations result more often in job-to-job transitions within the same industry as opposed to job-to-job transitions across industries or transitions from employment to non-employment? Job-to-job transitions are defined here as situations in which an individual is with one employer at one year and with another one at the subsequent year.[3] In order to investigate this issue, we build and exploit a unique dataset including cross-country comparable hiring and separation rates

[1] Search and matching models, such as those of Garibaldi (1998) or Mortensen and Pissarides (1999), also come to the conclusion that job mobility is negatively affected by the stringency of dismissal regulations.
[2] See among others Autor et al. (2007), Boeri and Jimeno (2005), Marinescu (2009), Gomez-Salvador et al. (2004), Messina and Vallanti (2007), Haltiwanger et al. (2008), Cingano et al. (2010), and, for less conclusive findings, Bauer et al. (2007), Martins (2009) and von Below and Thoursie (2010).
[3] Obviously, workers might experience short spells of unemployment between the two dates. By contrast, employment to non-employment transitions imply that individuals are not in employment the subsequent year.

by type of transition for 24 OECD countries and 23 business-sector industries. To anticipate our results, we find that the more restrictive the regulations, the smaller is the rate of job-to-job transitions within the same industry– and in particular of transitions towards permanent jobs – while no significant effect is detected as regards other types of separations. Moreover, as we have very detailed data in terms of regulatory provisions, we can assess the different importance of each of them as regards these transitions. In particular, we find that the possibility of reinstatement in the case of unfair dismissal is key in shaping gross worker flows.

We think that tracing where labour resources are reallocated and assessing the impact of employment protection on different types of transitions is interesting because structural reforms that relax the stringency of regulations might decrease the efficiency of the reallocation process while increasing overall reallocation. For example, the Spanish experience of the past thirty years suggests that reforms that increase the use of temporary contracts have opposite effects on reallocation and productivity (see e.g. Dolado and Stucchi, 2010). A key concern about reforms of dismissal regulations is that if they induce excessive turnover they might enhance inefficient destruction of industry-specific human capital, thereby impairing productivity growth in the long-run. In fact, the literature on job displacement has shown that dismissals leading to protracted unemployment spells and/or industry changes induce long-lasting wage penalties that are interpreted as due to destruction of (usually industry-specific) human capital.[4] Therefore, by increasing displacement, reforms relaxing firing restrictions might reduce the efficiency of the reallocation process. However, to the extent that laxer firing restrictions prompt firms to do more experimentation with new recruits and more hirings, more productive matches might also be realised, resulting in greater efficiency. Although in our dataset we cannot distinguish dismissals from voluntary quits, by distinguishing separations leading to either unemployment spells or a job in the same industry or a job in another industry, our analysis sheds some light on the likelihood that the increase in reallocation associated with the relaxation of firing restrictions could induce excessive destruction of (industry-specific) human capital.

One key problem in the cross-country analysis of the impact of regulations is that it is difficult to control for an exhaustive list of confounding factors. In addition, regulatory changes might be endogenous to worker flows, in particular insofar as they might be

[4] See e.g. Neal (1995), Gregory and Jukes (2001), Kletzer and Fairlie (2003), von Wachter and Bender (2006), Schmieder et al. (2012).

prompted by a sudden rise in dismissals and job destruction. Theory however predicts that, under standard assumptions on adjustment costs, dismissal regulations have a greater impact on job and worker flows in industries with greater natural propensity to make staff adjustments on the external labour market, in the absence of adjustment costs (see e.g. Micco and Pages, 2006). For example, if firms need to lay off workers to restructure their operations in response to changes in technologies or product demand, high firing costs are likely to slow the pace of reallocation of resources. By contrast, in industries where firms restructure through internal adjustments, changes in employment protection can be expected to have little impact on adjustment costs and, therefore, on labour reallocation. As done in a few recent cross-country studies on EPL and labour reallocation (e.g. Haltiwanger et al., 2008, and Cingano et al., 2010), we identify the effect of dismissal regulations by exploiting this theoretical property and using a difference-in-difference approach à la Rajan and Zingales (1998), where low-reallocation industries are used as a sort of control group for high-reallocation industries. The advantage of this approach is that it allows controlling for all factors that are unlikely to affect labour flows in a different way in high- and low-reallocation industries. In addition, through this approach we can better address endogeneity issues. In contrast with cross-country studies on labour reallocation, however, we explicitly acknowledge possible cross-industry general-equilibrium effects, which would not be identified through industry comparisons, and check that our results also hold when we estimate a standard cross-country/time-series model in which the effect of EPL is identified through regulatory changes over time.

Our paper complements existing micro and macro studies on EPL and labour reallocation. Autor *et al.* (2007) study the impact of the adoption of wrongful-discharge protection norms by state courts in the United States on several performance variables constructed using establishment-level data. By using cross-state differences in the timing of adopting stricter job security provisions, they find a negative effect of these provisions on job flows and firm entry. Using Italian firm-level data, Boeri and Jimeno (2005) exploit exemption clauses exonerating small firms from job security provisions within a difference-in-differences approach. Their estimates confirm a significant effect of employment protection on job turnover and job destruction in particular. Similar findings are obtained by Schivardi and Torrini (2008) and Kugler and Pica (2008). Marinescu (2009) exploits a 1999 British reform that reduced the trial period for new hires from 24 to 12 months of tenure, thereby directly affecting only employees within this window, and finds that the firing hazard for these employees significantly decreased with respect to that of workers with longer job tenure. Kugler *et al.* (2003) study the effects of a 1997 Spanish reform, which lowered dismissal costs for older and younger workers, and find that it was associated with a relative increase in worker flows for these groups. By contrast, insignificant effects are found by Bauer *et al.* (2007), Martins (2009) and von Below and Thoursie (2010) – who look at the impact of small-firm exemptions on worker turnover in Germany, Portugal and Sweden, respectively – possibly because of the small economic significance of the exemptions, typically concerning only procedural requirements. The fact that significant changes to labour legislation are rare makes it difficult to evaluate the impact of large differences in regulations through microeconometric studies concerning specific reforms in single countries. This is why a relative large cross-country empirical literature has emerged on this issue. Gomez-Salvador *et al.* (2004) estimate the effect of different degrees of stringency of employment protection legislation using a classical cross-country/time-series regression analysis based on European firm-level data and find a negative effect on job reallocation controlling for the effect of other labour market institutions. On the same data, Messina and Vallanti (2007) find that strict employment protection significantly dampens job destruction over the cycle with mild effects on job creation. In order to avoid omitted variable and endogeneity problems, Micco and Pages (2006), Haltiwanger *et al.* (2008) and Cingano et al. (2010) use a difference-in-differences estimator similar to that used in this paper on a cross-section of industry-level data for more than a dozen countries. They find that the negative relationship between layoff costs and job flows is more negative in industries where reallocation rates are larger, that is where it can be expected that EPL effects are, if any, stronger. We complement these papers, by looking at the impact of dismissal regulations on different types of transitions. In addition, as far as we know, our paper is the first cross-country study using harmonised data covering all firms and workers for a large number of OECD countries.[5] We believe that we are also the first who, on the basis of cross-country evidence, simultaneously compare the effect on gross flows of different types of regulations concerning dismissals of regular workers.

Our paper is also related to the literature on EPL and productivity. Recent studies have pointed out that dismissal regulations tend to reduce multi-factor productivity growth (see e.g. Autor et al., 2007, Bassanini et al., 2009, van Schaik and van de Klundert, 2013). These

[5] The samples of Micco and Pages (2006) and Haltiwanger et al. (2008) include few OECD countries and their data come from different national sources. Gomez-Salvador *et al.* (2004), Messina and Vallanti (2007) and Cingano *et al.* (2010) use firm-level data from the Bureau van Dijk's Amadeus, which are in principle comparable but exclude firm entry and exit.

findings have been linked to a growing body of evidence suggesting that the reallocation of resources from declining and less efficient businesses to expanding and more efficient companies, contribute significantly to productivity and output growth (*e.g.* Griliches and Regev, 1995; Foster *et al.*, 2001; Bassanini, 2010; and OECD, 2009, for a survey). Although given these two bodies of evidence it seems natural to argue that EPL slows down productivity growth by impairing efficient labour reallocation,[6] this conclusion would not be warranted if laxer EPL reduced the efficiency of the reallocation process. We are not aware of any paper providing evidence on this. We contribute to this debate by showing that the effect of dismissal regulations on separations is essentially confined to those leading to rapid job finding within the same industry, suggesting that it is unlikely that laxer regulations lead to inefficient destruction of industry-specific human capital.

Finally, this paper can be of interest to scholars and policy-makers who worry about distributional consequences of structural reforms and, more generally, the political economy of reforms. There is no doubt that a liberalisation reform negatively affects those workers that are displaced after the policy change and would not have been displaced in the absence of the reform. While the trajectories of displaced workers have been intensively researched, often comparing different countries (see e.g. Bender et al., 2002, for one of these cross-country comparisons), there are only few studies that follow these trajectories in the aftermath of structural reforms (see Eslava et al., 2011, and Menezes-Filho and Muendler, 2011, for two examples concerning trade reforms in developing countries) and we are aware of no such cross-country study. By showing that dismissal regulations affect mainly within-industry job-to-job transitions, our results provide suggestive evidence that those displaced workers that would not have been displaced in the absence of deregulation tend to find relatively quickly another job that is likely to fit their previously accumulated competences.

The layout of the rest of the paper is as follows. Section 1 presents the empirical strategy. Section 2 describes the dataset and presents summary statistics. Section 3 reports our empirical results. Section 4 concludes.

[6] For theories suggesting alternative channels through which stricter dismissal regulations negatively affect productivity growth, see Saint-Paul (2002) and Bartelsman et al. (2010).

1. Identification strategy and empirical specification

The goal of this paper is to relate different types of gross worker flows to dismissal regulations using comparable data from several countries. As dismissal regulations are typically defined at the country level, a standard approach (see e.g. Gomez-Salvador et al., 2004) would be to use aggregate data and estimate a linear specification such as:

$$WF_{ct} = X_{ct}\beta + \gamma EPL_{ct} + \eta_c + \eta_t + \varepsilon_{ct} \qquad [1]$$

where *WF* stands for a measure of average gross worker flows (that is, for example, the sum of hirings and separations or the number of job-to-job transitions divided by average employment, see the next section) in country *c* at time *t*, *EPL* is an indicator capturing dismissal regulations, *X* is a vector of control, including other institutions, ε is a standard error term and the ηs represent country and time fixed effects, and other Greek letters are parameters to be estimated.

The use of aggregate cross-country/time-series data makes it possible to exploit the large variation in policies across countries and over time and examine general equilibrium effects. But, a key problem with aggregate models such as [1] is that it is difficult to control for an exhaustive list of confounding factors. This is particularly the case of omitted institutions since they are often correlated with the variable of interest, due to institutional consistency. In addition dismissal regulations are likely to be endogenous. For example, during the first stage of a recession, there might be strong political pressure to make regulations more restrictive. By contrast, very bad recessions could induce political support for radical relaxation of restrictions (see e.g. Drazen and Easterly, 2001).

In order to circumvent these problems, we exploit the fact that our dataset include cross-country comparable data on worker flows at the industry level, and that, while EPL is defined at the aggregate level, its impact is likely to differ across industries. Within this context we use a difference-in-difference strategy in the spirit of Rajan and Zingales (1998). In fact, if dismissal regulations have a direct impact on worker flows – that is if worker flows are affected by the firms' response to changes in firing costs – this effect is likely to be larger in industries where dismissal regulations are more binding. We call these industries EPL-binding industries, which in turn are likely to be those industries that have a relatively high "natural" propensity to adjust their human resources through dismissals, due to idiosyncratic technological and demand factors. In contrast, in industries where firms can restructure through internal adjustments, product demand is less volatile and/or restructuring tends to be less frequent, dismissal regulations can be expected to have little impact on worker reallocation. As a consequence, the impact of dismissal regulations on gross worker flows is likely to differ across industries and can be investigated by comparing differences in worker flows between EPL-binding and other industries in countries with different degrees of EPL.[7] This strategy has the advantage that it controls for policies or institutions that influence gross worker flows in the same way in all industries. More precisely, all factors and policies that can be assumed to have, on average, the same effect on gross worker flows in EPL-binding industries as in other industries (in particular, labour supply factors) can be controlled for by country-level dummies.[8] Moreover, the availability of an additional dimension (the industry) allows us to suppress the time dimension by taking averages over a specified period of time (corresponding approximately to an entire business cycle), thereby reducing endogeneity problems due to business-cycle fluctuations.

In practice, however, it is unlikely that firing restrictions are either always binding or always not binding in a particular industry. Rather, whether and to what extent they are binding depends on the adjustment costs they impose on firms. These costs will be higher, the larger the firm's natural propensity to adjust its workforce in the absence of adjustment costs. In other words, we assume that

$$E[WF_{cj}] = (a + \Lambda_j) f(EPL_c) + o_{cj} \qquad [2]$$

where *WF* stands for the average gross flow rate of interest in country *c* and industry *j* in a specific period of time, Λ is the natural propensity to adjust labour in industry *j* in the absence of adjustment costs, *a* is a non-negative parameter, *f* is a monotonic function, *E* stands for the expectation operator and *o* stands for the contribution of other factors, including possibly other policies whose effects are assumed to be independent of Λ (see the Appendix for a more formal derivation of eq. [2] on the basis of a simple model with quadratic adjustment costs, adapted from Micco and Pages, 2006).

The key issue is how to proxy Λ in practice. Previous studies (see for example Haltiwanger et al., 2008 and Bassanini et al., 2009) have argued that, in each industry, average US job or

[7] More precisely, the sign of the difference in the effect of dismissal regulations between EPL-binding and other industries will provide an indication on the sign of the average effect, subject to the identifying assumption that in non-binding industries this effect must be either zero or smaller and of the same sign than in EPL-binding industries.

[8] Consistent with this observation, in the empirical section we also show that the impact of other measurable institutions does not vary across binding and other industries.

worker reallocation rates[9] could well proxy the firms' natural propensity to adjust labour in the absence of regulations. This argument appears justified because dismissal regulations in the United States are unrestrictive, in comparison with other countries (see Appendix), and cross-industry distributions of reallocation rates in different countries are relatively similar (see OECD, 2009). Assuming *f* linear, and taking into account other confounding factors, yields the following specification that can be estimated excluding the United States from the sample (to avoid circularity):

$$WF_{cj} = X_{cj}\beta + \delta B_j EPL_c + \eta_c + \eta_j + \varepsilon_{cj} \qquad [3]$$

where B is the benchmark measure of Λ (the US worker reallocation rate in this case), and X and the ηs represent additional covariates, country and industry fixed effects, respectively, that are included to capture additional confounding factors. The parameter of interest is δ. The greater this parameter, the greater is the relative effect of *EPL* in binding industries with respect to non-binding industries. The sign of δ also provides an indication of the direction of the average effect of *EPL* subject to the identifying assumption that in non-binding industries this effect is either of the same sign and smaller or zero (cf. equation [2] and the Appendix). We will estimate [3] using alternatively different types of gross worker flow rates as dependent variable, including hirings and different types separations, i.e. leading to a job within the same industry, a change in industry, or prolonged joblessness.

The standard way of choosing the United States, where the indicator *EPL* is close to zero, to construct the benchmark measure of Λ might however be problematic. First, the composition of industries in terms of more disaggregate sub-industries may differ between the United States and other countries in the sample. Second, US reallocation rates might be affected by specific US institutional features. For instance, unemployment insurance premia in the United States are, in part, dependent on past layoffs (experience-rating). It cannot be excluded that, despite very weak dismissal regulations, experience-rating imposes significant additional costs on firms firing workers, which might differ across industries, thereby acting like endogenous additional firing restrictions.

We address this issue in two ways in a sensitivity analysis. First, we experiment with UK reallocation rates instead of US rates. The argument supporting this choice is that the United Kingdom is the country with the second laxest dismissal regulations, according to OECD

[9] Job reallocation is defined as the sum of gross job creation and job destruction. Worker reallocation is defined as the sum of hirings and separations.

indicators. However, Ciccone and Papaioannou (2010) have shown that measurement error originating from country-benchmarking can bias the estimates of δ if the benchmark reflects, among other factors, idiosyncratic shocks. For instance, if patterns of worker reallocation across industries in the benchmark country correlate more closely to reallocation patterns in countries with lax regulations than in countries with strict regulations for reasons unrelated to regulation itself, then one might incorrectly attribute the cross-country differences in the inter-industry distribution of reallocation rates to an effect of *EPL* on gross flows. To circumvent the problem, as a second robustness exercise, we follow the procedure suggested by Ciccone and Papaioannou (2010), which involves instrumenting $B_j EPL_c$ through a two-step procedure. In a first step we obtain predicted industry slopes $\hat{\kappa}_j$ of *EPL* from the estimation of the following regression:

$$WF_{cj} = \kappa_j EPL_c + \eta_c + \eta_j + \varepsilon_{cj}. \qquad [4]$$

Then the interaction of EPL and predicted industry-specific slopes ($\hat{\kappa}_j EPL_c$) is used as an instrument for $B_j EPL_c$ and [3] is estimated through standard two-stage least squares.

Rigorously speaking, the approach adopted here allows us identifying only differential effects between binding and other industries. This provides us with some indication on the direction of the average effects of EPL across all industries, subject to the identification assumption that the effect of EPL in non-binding industries is of the same sign and smaller than that in EPL-binding industries (or zero; see equation [2] and the Appendix). For comparison purposes, it is also possible to derive a rough quantitative estimate of the direct effect of regulations for the average industry by simply multiplying δ as obtained from [3], by the average value of *B*. This is equivalent to assuming further that dismissal regulations would have no effect in a hypothetical industry whose benchmark measure *B* would be equal to 0. However, our estimate might underestimate the true average effect of dismissal regulations. In fact, general equilibrium effects, and in particular those related to labour supply, might be similar across industries and be swept away by aggregate dummies. For example, high EPL, by reducing opportunities for outsiders, could discourage youth to participate in the labour market, thereby depressing hirings and separations in all industries, since young workers have typically high mobility.[10] In order to check that these homogenous effects play a minor role

[10] By contrast, one could expect that older workers are more likely to search for jobs in industries in which they have more work experience. If this is the case, a more flexible labour market, brought about by laxer EPL, would create more opportunities for firms in those industries to fill their vacancies. This would still represent a

and our estimates still provide useful quantitative measures of the average magnitude of the effect of dismissal regulations, we also complement our analysis by estimating more standard cross-country/time-series specifications on annual data. More precisely, we estimate the following general specification:

$$WF_{cjt} = X_{cjt}\beta + \gamma EPL_{ct} + \eta_c + \eta_j + \eta_t + \varepsilon_{cjt} \qquad [5]$$

where γ captures the overall effect of *EPL* for the average industry.[11] Obviously, this cross-country/time-series specification is likely to suffer from the standard problems of endogeneity and omitted variables mentioned above. Nevertheless we can use it as a useful benchmark to assess the extent to which δB represent an underestimate of the true effect of EPL. In fact, if additional, homogenous general equilibrium effects of EPL were essentially minor, one would expect the estimate of γ to be close to that of δB obtained by estimating equation [3].

2. The Data

We construct harmonised data on gross worker flows for 24 OECD countries and 24 business-sector industries at, approximately, the 2-digit level of the ISIC rev. 3 classification (see the Appendix for the list of countries and industries).[12] The period covered by our data is 1995-2007. However, only few countries are available for the whole period. Due to data limitations (see below), we define worker flows in term of one-year transitions. In other words, hirings equal the number of workers who are with one employer at time *t*, but were not with that employer one year before (that is at *t*-1), and separations equal the number of workers who were with the firm at *t*-1, but not at *t*.[13]

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general equilibrium effect, but it would be industry-specific and therefore captured by our identification strategy.

[11] We cluster errors at the country-by-time level.

[12] For issues of data reliability, agriculture, mining and fuel are excluded. Our data are available at https://sites.google.com/site/bassaxsite/home/files/BGdata.zip , with the exception of ECHP microdata (see below).

[13] In one alternative, frequently-used definition, gross worker flows are computed over a specified period based on a full counting of all events during that period – *i.e.* every time a worker is hired or separates during the period. However, our definition is not uncommon in the literature (see *e.g.* Abowd *et al.*, 1999, Golan *et al.*, 2006, and Davis et al., 2006). However, the choice of definition is not entirely neutral: as shown by Hall (1995), a large fraction of job spells last no more than few days, and hiring and separations associated with these spells are by and large excluded by the definition retained here, therefore the reader must bear in mind that our results do not necessarily generalise to any definition. Nevertheless, one-year transitions are typically used in the analysis of gross job flows and in the literature on reallocation and efficiency (see the references in the introduction), so that our results can be directly compared with those in that literature.

Our main sources of data are labour force surveys (LFS hereafter) of various OECD countries.[14] LFS data contain information on industry, employment, job tenure, type of contract plus standard individual characteristics such as gender, age and education. These variables are comparable across countries or can easily be made comparable – such as in the case of education, if attainment is grouped into three categories. Since workers with less than one year of job tenure are clearly new hires according to the above definition, we can reconstruct separations at the industry level by exploiting the following standard identity.

$$S_{cjt} = H_{cjt} - \Delta E_{cjt} \qquad [6]$$

where *S*, *H* and *E* are separations, hirings and employment, respectively, in country *c*, industry *j* and time *t* and Δ represents one-year differences. In words, in each industry, separations can be derived as the difference between new hires and employment changes. The problem is that the industry dimension is not taken into account in the LFS sampling design, so that industry employment levels obtained by aggregating individual LFS data might exhibit spurious fluctuations from one year to another. Therefore, following the procedure suggested by OECD (2009), we draw industry-level employment levels and changes from EU KLEMS and OECD STAN, which are derived from national accounts and are the most reliable cross-country comparable sources for industry-level data. We then write hirings as

$$H_{cjt} = H^{L}_{cjt}(E_{cjt} / E^{L}_{cjt}) = h^{L}_{cjt} E_{cjt} \qquad [7]$$

where the superscript *L* indicates LFS variables, *E* without superscript stands for employment from EU KLEMS or STAN and $h^{L}_{cjt} = H^{L}_{cjt} / E^{L}_{cjt}$ is the share of workers with less than one year of tenure as drawn from LFS. We then use this definition for hirings to compute separations using [6]. Total gross worker reallocation will then be defined as the sum of *H* ans *S* as standard.

LFS contain also some information on employment and job characteristics one year before, based on retrospective questions. In particular, respondents are asked whether they were in employment one year before and, in the case of a positive answer, which was the industry and whether their employer was the same. If the employer one year before the survey was not the same as at the time of the survey we have a separation according to our definition. Therefore, we could have used this information to aggregate directly separations at the industry-level.

[14] More precisely, we use the European Labour Force Survey for European Union countries, Iceland, Norway, Switzerland and Turkey, the bi-annual January Displaced workers/Job tenure supplement of the Current Population Survey, for the United States (even years only), and the Canadian Labour Force Survey for Canada.

However, non-respondents to this question are likely to be much less frequent if the worker has not changed employer. Therefore, separation rates would be underestimated and the accounting identity [6] would not hold (see OECD, 2009, for a more extensive discussion). By contrast, we can use this information to construct rates of different types of transitions using rescaling rules similar to [7]. Job-to-job separations *JJS* – that is, the number of employees at time *t* that changed employer between *t*-1 and *t*, classified according to their industry in *t*-1 – will be obtained as $JJS_{cjt} = JJS^{L}_{cjt}(S_{cjt} / S^{L}_{cjt})$ where, again, the superscript *L* indicates LFS variables, *j* stands for the industry of origin and *S* is defined from [5].[15] Job-to-jobless separations *J2JL* will then be defined as the difference between *S* and *JJS*. Using a similar re-scaling rule we then derive same-sector job-to-job separations *SS* – that is, the number of employees at time *t* that changed employer between *t*-1 and *t* but remained in the same industry – as $SS_{cjt} = SS^{L}_{cjt}(JJS_{cjt} / JJS^{L}_{cjt})$ and other sector separations *OS* as the difference between *JJS* and *SS*. *SS* and *OS* are key variables of interest in our analysis, insofar as we want to know whether dismissal regulations have a stronger effect on the reallocation of workers within industries or across industries. As all these definitions are based on one-year transitions, job-to-job separations include a certain amount of transitions leading to short jobless spells between *t*-1 and *t*.

Consistent with the literature (see *e.g.* Davis et al., 1997), we then construct rates for all these flow variables by dividing flow totals (that is hirings, separations, or other type of transitions) by average industry employment in *t*-1 and *t*. Tables A1 and A2 in the Appendix present average worker flow rates by country and industry for the period 2000-2007, which approximately corresponds to a full business cycle and where we have a similar number of observations in all countries and industries, making statistics more comparable. This is also the sample we will use for the cross-sectional difference-in-difference analysis (see the previous section). Hiring and separation rates in the country with the greatest rates (Turkey) are almost three times larger than in the country with the lowest rates (Greece). Interestingly, an even larger variation is observed across industries. The same pattern emerges for job-to-job transitions and, in particular, same-sector transitions, while job-to-jobless separations are less variable across both countries and industries.[16] Two other interesting facts emerge from

[15] JJS^L is the number of employees that in the LFS wave of time *t* reported that they changed employer between *t*-1 and *t*, classified according to the industry they declared they were in at time *t*-1. SS^L (see below) is the number of these respondents that declared to have remained in the same industry.

[16] For example, the standard deviation of job-to-job separations is twice as large as that of job-to-jobless separations.

the table: first, job-to-job transitions are more frequent than job-to-jobless transitions, except in textile, leather and footwear manufacturing, which however contracted massively in the period of interest, and electricity, gas and water supply; second, the majority of job-to-job transitions occur within industries even at this relatively fine-grained disaggregation of the business sector,[17] suggesting industry segmentation of the labour markets, possibly due to the fact that industry-specific human capital is accumulated with job experience (see e.g. Neal, 1995).

Labour and product market institutions come from OECD sources. Descriptive statistics of the main variables as well as precise definitions and sources are reported in the Appendix. In particular, we consider two main indicators of stringency of dismissal regulations: employment protection for regular workers, excluding collective dismissals (EPR) and including collective dismissals (EPRC). The latter is obtained as a weighted average of EPR and additional regulation for collective dismissals (EPC), with weights 5/7 and 2/7. EPRC better captures all aspects of dismissal regulations but is available only since 1998; therefore we will use EPR as a surrogate of EPRC in the time-series analysis on the 1995-2007 sample. A further breakdown of components of EPR is also used. All indicators vary from 0 to 6 from the least to the most stringent. To grasp a quantitative perception of what these numbers imply, 1 point of the EPRC indicator corresponds to slightly more than the difference between the values for the United Kingdom and the United States, the countries with the lowest indicators, and almost half of the difference between the United States and the OECD average. By contrast, in this paper we do not consider regulations concerning temporary contracts, whose effects, in some specifications, are simply controlled for by including the share of employees under those contracts. The main reason is that the degree of enforcement might be particularly heterogeneous across countries as regards regulation for temporary contracts. In fact, enforcement of employment protection legislation is mainly dependent on individuals who consider themselves as victims and lodge a complaint. While potential plaintiffs are well identified and able to react in the case of dismissals, victims of breaches of legislation on temporary contracts (particularly in the case of violations of hiring restrictions under such contract typology) are a much vaguer group. As a result, indicators of legal restrictions concerning hiring of temporary workers appear to be a bad predictor of their share in total employment (see e.g. OECD, 2010).

[17] Belgium, France, Norway and Sweden are exceptions to this pattern.

3. Empirical Results

3.1 Cross-sectional results

Baseline results

We start our analysis by estimating the impact of the stringency of dismissal regulations, as measured by EPRC, on various types of worker flows averaged across 2000-2007, using our difference-in-difference strategy à la Rajan and Zingales. In Table 1, we consider the simplest possible specifications of [3], that is: i) without controls except for country and time dummies; and ii) including standard worker characteristics such as gender, age classes, educational attainment and the share of self-employed and temporary workers,[18] all expressed in percentage of total employment. In Panel A of Table 1 we look at standard measures of worker flows (total reallocation, hirings and separations). In all specifications the interaction between EPRC and US worker reallocation is negative and significant, consistent with a negative impact of dismissal regulations on flows. Remarkably, point estimates are almost unaffected by the presence of standard controls, which is reassuring taking into account that some of these confounding factors are potentially endogenous. Taking these estimates at face value, considering that US worker reallocation is 43.2% in the average industry (see Table A1 in the Appendix), one would predict a one-point reduction of EPRC from the OECD average – that would correspond to a significant reform in historical terms –[19] to be associated with an increase in both hirings and separations of 2.2-2.7 percentage points in the average industry, that is an increase of about 15%.[20]

[18] The inclusion of the share of temporary workers deserves particular attention. Indeed, one would expect that dismissal regulations affect particularly the separation rate of workers with open-ended contracts. Ideally, therefore, one would like to restrict the sample by excluding temporary workers. However, the type of contract before the transition is not available in EULFS data. As a second best, we include the share of temporary workers as a control. However, we worry that the relationship between worker flows and temporary employment might be non-linear (see, for example, Costain et al., 2010) so that including a simple linear control might not be sufficient. We therefore experiment with the inclusion of a polynomial in the share of temporary employment up to the fifth degree and, reassuringly, we find that all terms except the linear one are always insignificant, while our main estimates remain stable. As a further sensitivity analysis, in the Appendix we replicate our analysis for a coarser partition of industries and a smaller number of countries using microdata from the European Community Household Panel (ECHP), where the information on contract status before the transition is available. As expected, our findings suggest that the impact of dismissal regulations on separations is more significant for permanent workers.

[19] For example, the 2003 reform of severance payments in Austria, which is often cited as an example of significant reform, entailed a reduction of only 0.55 points in the indicator (see for example Bassanini et al., 2009).

[20] This prediction is valid if we assume that the effect of EPRC is zero at zero US worker reallocation (see Section 1). More rigorously, our estimates suggest that, in a country with EPRC one-point below the average, inter-industry differences in terms of hirings and separations are larger by about 15% than at the OECD average.

We look at other types of transition in Panel B. There is no evidence that EPRC has any impact on job-to-jobless transitions or other-sector job-to-job transitions. By contrast, stricter regulations for regular workers appear to reduce considerably the rate of job-to-job transitions within the same industry.[21] Comparing estimates across panels, it appears that about 80% of the effect of EPRC on separations is accounted for by the negative relationship between EPRC and same-sector job-to-job separations. This result can be viewed as consistent with our finding on hirings: in countries with lighter legislation, not only do workers separate more often in binding industries than in other industries, but also firm hiring incentives are stronger and hiring rates higher in these industries. This suggests that, in these industries, separating workers have more opportunities to find another job in the same industry when regulations are less strict.

In addition, our data allows us to decompose same-sector separations by type of contract in the new job.[22] Re-estimating the specifications of Panel B separately for same-sector transitions to permanent and to temporary jobs, we obtain coefficients of -0.042 and 0.011, respectively, in the specification with controls, and -0.059 and 0.011, respectively, in the specification without controls.[23] In other words, the whole effect on same-sector job-to-job separations is due to transitions to open-ended jobs. To the extent that stricter EPRC is expected to discourage only hiring on open-ended contracts, this finding can be explained as a reflection of the effect of EPRC on hiring behaviour in the same way as before.

Overall, these findings suggest that countries with laxer legislation regulating permanent contracts are likely to have larger gross flows, probably including more dismissals.[24] But the additional separations brought about by laxer regulations essentially lead to rapid re-employment within the same industry in jobs characterized by open-ended contracts. Indeed, countries with fewer dismissal restrictions are not characterized by more transitions (including job losses) leading to job-to-jobless transitions and/or situations in which

[21] Moreover, if equations for different dependent variables are simultaneously estimated, cross-equation statistical tests suggest that the coefficients of EPRC are significantly different across equations. More precisely, chi-square test statistics of the difference between the coefficients of EPRC in the regressions for SSR and OSR are 4.33 and 6.93 for specifications without and with controls, respectively. In the case of the difference between coefficients for J2JLR and SSR, chi-square test statistics are 3.91 and 4.57 for specifications without and with controls, respectively. All these statistics are significant at the 5% level.

[22] Unfortunately, as mentioned before, information on the type of contract in the previous job is not available.

[23] Standard errors are 0.013, 0.013, 0.014 and 0.010, respectively.

[24] For evidence concerning dismissals based on five countries, see OECD (2009).

separating workers have to accept precarious jobs or jobs in different industries, with the consequent likely loss of human capital.[25]

Robustness checks

We argued that one of the key advantages of our difference-in-differences approach is that it allows us controlling for other aggregate confounding factors, including other institutions and policies, some of which are not easy to quantify. This claim is correct provided that the impact of aggregate institutions on gross worker flows does not vary, on average, between EPL-binding and other industries. In order to provide evidence in support of our identification assumption, we augment our preferred specification with interactions between US reallocation rates and several aggregate indicators of labour market institutions and product market regulations that are typically used in aggregate unemployment equations (see e.g. Bassanini and Duval, 2009).[26] Table 2 shows results from the estimation of various specifications with, alternatively, total reallocation and same-sector separations as dependent variables (in Panels A and B, respectively). As institutional covariates are not always available for the countries for which we have gross worker flow data, we start with the simplest specifications including only indicators that are available for the largest number of countries, and progressively include additional covariates, available for an increasingly smaller sample. Consistent with our identification assumptions, we find no robust association between other institutions and differences in worker flows between EPL-binding and other industries (as shown by the lack of significant coefficients on the interactions between

[25] As mentioned before, there is an extensive literature showing that industry-changes following displacement bring about a significant loss of valuable industry-specific human capital (see references mentioned in the introduction). However, displacement account for a small fraction of separations (see e.g. OECD, 2009). As our data concern total separations, we might worry that there might be a significant amount of job-to-job transitions across industries that would not entail losses of human capital. In particular, this would be the case for moves across industries of workers that remain in the same occupation. In order to explore this question, we have access to 5-quarter rotating panels for UK LFS from 2005 to 2008. Defining transitions in the same way as in our main dataset and using a 4-digit classification of occupations and our same partition of industries, we find that 40% of same-sector transitions maintain the same 4-digit occupation, while this is the case in only 7% of other-sector transitions. This appears consistent with the idea that other-sector transitions often involve the loss of specific skills.

[26] These are: the average labour tax wedge, the average unemployment benefit replacement rates (averaged across different durations and family situations), the level of corporatism in collective bargaining, the share of workers covered by collective agreements (including administrative extension), the rate of home-ownership and the ratio of spending in active labour market programmes per unemployed to GDP per capita. Following the literature (e.g. Bassanini and Duval, 2009), we also add an indicator of the degree of stringency of anti-competitive product market regulation. All indicators are drawn from OECD databases (see the Appendix).

institutions and US reallocation rates).[27] By contrast, and reassuringly, estimated effects of EPRC do not appear to be sensitive to the specification.[28]

As noted above, US reallocation rates could be affected by specificities of US institutions and industrial structure and this might bias our estimates. As a first robustness check we replace US with UK reallocation rates and re-estimate our specifications by excluding UK worker flows from the sample (to avoid circularity). Results obtained this way are remarkably similar (Table 3, Columns 1 and 2 of Panels A and B), in particular if account is taken for the fact the mean and variance of UK reallocation rates are smaller.[29] Alternatively, as proposed by Ciccone and Papaioannou (2010), we instrument the interaction between EPRC and the US reallocation rate with the product of EPRC and predicted industry-specific slopes, the latter obtained by fitting equation [4] with total reallocation rates as dependent variable and excluding the United States from the sample (see Section 1 above). [30] Re-assuringly, results are stronger but qualitatively similar to those obtained with our baseline models (Table 3, Columns 3 and 4 of Panels A and B).[31]

Breaking down dismissal regulations

So far we have considered only the overall index of employment protection for individual and collective dismissals. However, our data allow us to dig further into the relationship between worker flows and different types of dismissal restrictions, thereby shedding light on the effect of specific regulations on worker flows. Looking at the separate impact of each kind of

[27] The coefficients of product market regulation (in Panel A) as well as of the tax wedge (in Panel B) are partial exceptions. However, these exceptions occur only in specifications with several covariates. Given the high correlation across different institutional indicators (see e.g. Bassanini and Duval, 2009), this result is likely due to multicollinearity. As a matter of fact, when these institutions are included one-by-one in the specifications of Table 2, they turn out insignificant.

[28] We also run a sensitivity analysis to check that our results are robust to the choice of the estimation sample. We verify that the estimation of the effect of EPRC on worker flows is not driven by a single country or industry, excluding them one-by-one. Results are available from authors upon request.

[29] Taking these estimates at face value, a 1-point increase in EPL would entail an increase of reallocation rates of 20% and same sector separations of 40%, against 15% and 40%, respectively, as obtained when US reallocation rates are used as benchmark.

[30] Bassanini et al. (2009) use the US distribution of dismissal rates to proxy the propensity of industries to adjust on the external labour market in the absence of adjustment costs. The justification behind that choice is that dismissal restrictions are likely to be particularly binding in industries that cannot rely on the natural attrition of staff to make the required workforce adjustments. Our results are also robust to the replacement of our benchmarks with this alternative one.

[31] The fact that point estimates are not smaller when 2SLS estimators are used instead of OLS suggests that, in countries with laxer EPL, the distribution of employment across industries is no closer to that of the United States than in countries with strict EPL. This is consistent with the results of Bassanini et al. (2009) who find that EPL has no impact on the distribution of employment across industries.

provision can better inform policy-makers on the likely consequences of reforming specific regulations.[32]

We first disentangle regulations for individual dismissals from the additional provisions applying to collective dismissals (Column 1 in Table 4). Both indicators attract a negative and significant coefficient. Additional provisions for collective dismissals play a particularly important role in the case of same-sector job-to-job transitions. Taking estimates at face value, a 1-point reduction in both indicators – in both cases almost one half of the difference between the United States and the OECD average – is estimated to be associated with an increase in same-sector separations almost twice as large as what would occur if only regulations for individual dismissals were reformed.

When the effect of regulations for individual dismissals is further decomposed, neither procedural inconveniences, including notification delays and procedures, nor notice periods and severance payments appear to have any significant impact (cf. Columns 2 and 3 in both panels of Table 4). These results appear consistent with micro studies for Portugal and Sweden that find no significant impact of exemptions from procedural requirements for dismissals (see Martins, 2009; von Below and Thoursie, 2010). By contrast, the difficulty of dismissals, including the stringency of the definition of unfair dismissal and its consequences, appears negatively and significantly associated with both total worker reallocation and same-sector job-to-job separations, at least when insignificant indicators are excluded from the specification (Columns 2 to 4 in both panels of Table 4). More precisely, the indicator of difficulty of dismissals is the average of four components: the definition of unfair dismissal; the length of trial period under which a worker can be fired "at will"; the compensation due in the case of conviction for unfair dismissal; and the extent of reinstatement following unfair dismissals. Disentangling further among these provisions we find that the frequency at which reinstatement is ordered by courts (when dismissals are judged unfair) is the only component that is significantly associated with total flows and same-sector job-to-job transitions (Columns 5 in both panels of Table 4). This might explain why employment protection is perceived to be extremely rigid in a country like Italy (*e.g.* Ichino et al., 2003), despite a relatively low score as regards overall EPL concerning individual dismissals. Italy appears, in fact, to score the highest as regards the extent of reinstatement according to OECD indicators. Finally, we also find that the length of the trial period is negatively associated with total

[32] Nonetheless, in drawing conclusions from the results, it must be kept in mind that the greater the disaggregation of EPL indexes, the greater the measurement error. Furthermore, different provisions might be complementary or substitutable to each other. This issue is, however, beyond the scope of this paper.

flows, at a level of significance close to 10%, although this variable appears unrelated with same-sector separations. Indeed, and perhaps not surprising, repeating the specifications of Table 4 for hirings and total separations, we find this variable to be significantly correlated with hiring but not with separations.[33]

3.2 Time-series results

The approach we followed up to now cannot capture general equilibrium effects if they do not differ, on average, between EPL-binding and other industries. If these effects are large, using coefficients in Tables 1 to 4 to predict the impact of reforms of dismissal regulations would likely underestimate the true effect. In order to check whether this is the case, we estimate equation [5] on annual cross-country/cross-industry/time-series data for the period 1995-2007. By identifying the effect of institutions through over-time variations only, it is possible, in principle, to capture their overall impact resulting from both general and partial equilibrium effects.[34] However, additional restrictions for collective dismissals are unavailable prior to 1998. We use therefore the index of EPL for regular workers excluding additional provisions for collective dismissals (EPR), which appears to be a good proxy for the overall degree of stringency of EPL for regular workers, as the two indexes are closely correlated in the subsample in which both are available.[35]

As labour reallocation rates are well known to increase in downturns (see e.g. Davis et al., 2006), we control for the difference between the current and average growth rates of employment (the latter computed over the period 1990-2007 for each industry and country). Consistent with the literature we find that bad economic conditions are associated with fewer hirings and greater separations (Table 5). As one would expect, downturns are particularly correlated with an upsurge of job-to-jobless transitions (Column 7). Anti-competitive product market regulations appear to be associated with smaller worker flows of any type as theory would suggest (e.g. Hopenhayn and Rogerson, 1993). We also find that union density is associated with a greater share of job-to-job transitions, in particular those leading to open-ended contracts. Finally, and more important, our time-series estimates confirm that stringent

[33] Results available from authors upon request.
[34] Nevertheless, as discussed in Section 1, the main disadvantage of this approach is that omitted institutions and policy endogeneity might bias our estimates.
[35] Similarly, collective bargaining coverage is not available in time series, for this reason we substitute union density for that variable. By contrast, no change in corporatism is observable in our indicators over the sample period. Therefore, this variable is collinear to country fixed effects.

dismissal regulations depress both hiring and separations (Table 5, Columns 1 to 3). Estimated effects appear somewhat larger – point estimates of time-series coefficients are about 20% higher than those derived from coefficients of Table 1 – but differences are not large enough to claim that they are significantly different. Overall, these results suggest that additional general equilibrium effects, not captured by difference-in-difference estimates, are probably minor.

Time-series estimates also confirm that the effect on dismissal regulations on same-sector job-to-job transitions accounts for most of their effect on separations (Column 4). Moreover, within these transitions, those leading to an open-ended contract are the most affected by the stringency of regulations (Column 5). By contrast job protection regulations appear to have no significant effect on other types of separations (Columns 6 and 7). In contrast with cross-sectional estimates, however, the coefficient of EPR in the regression for job-to-jobless separations is imprecisely estimated so that, rigorously speaking, we cannot claim, on the basis of the results presented in Table 5, that the impact of EPR on job-to-jobless separations is significantly smaller than that on same-sector job-to-job transitions.

We perform two types of robustness checks on these data.[36] First, one could argue that different stages in the industry life-cycle might be associated with different rates of gross job and worker flows. Moreover, in different countries, industries are composed of different sub-industries that might be characterized by heterogeneous rates of transitions. In order to check that these types of composition effects do not affect our results, we re-estimate Table 5 by including country-by-industry and industry-by-time dummies, and obtain virtually the same results.

Second, we have implicitly assumed so far that the impact of EPL on worker reallocation is linear (see also the model in the Appendix). Although this is a standard and never-tested assumption in the literature (see *e.g.* Gomez-Salvador *et al*., 2004, Messina and Vallanti, 2007, Haltiwanger *et al*., 2008, Cingano *et al*., 2010), it is correct only if the microeconomic process generating individual hirings and separations can be approximated by a linear probability model. However, this is not necessarily true, and this approximation could be particularly bad in our case taking into account that worker reallocation can vary by a factor of three across industries and countries (see Tables A1 and A2 in the Appendix). In these conditions a probit model for individual hirings and separations would be a more credible

[36] Detailed results are available from authors upon request.

approximation of the probability of making an individual transition. Therefore, we also estimate a generalised linear model (GLM), issued by the aggregation of a probit model for individual transitions. We do so by fitting the following analogous of equation [2] to the data using a quasi-maximum likelihood estimator (QMLE), where the quasi-likelihood function is the binary choice log likelihood, as suggested by Papke and Wooldridge (1996):[37]

$$E(WF_{cjt}) = G(X_{cjt}\beta + \gamma EPL_{ct} + \eta_c + \eta_j + \eta_t) \quad [8]$$

where G is the inverse-probit function and WF stands for either hiring or separation rates (also disentangled by type). Re-assuringly, no significant difference from Table 5 appears (Table 6). If any, the effect on EPR on same-sector job-to-job separations appears stronger.

4. Conclusions

In this paper we look at the impact of dismissal regulations on different type of gross worker flows, defined as one-year transitions, using both a difference-in-difference approach à la Rajan and Zingales – in which the impact of regulations is identified by exploiting likely cross-industry differences in the impact of firing restrictions – and standard time-series analysis – in which the effect of regulations is identified through regulatory changes over time. In order to do so we construct a unique dataset including cross-country comparable hiring and separation rates by type of transition for 24 OECD countries and 23 business-sector industries. We find that the more restrictive the regulations, the smaller the rate of job-to-job transitions, while no significant effect is detected as regards job-to-job transitions involving an industry change and/or job-to-jobless transitions – that is, situations in which a worker is with one employer at *t*-1 and jobless at *t*. Estimated effects appear significant from an economic point of view: taking our estimates at face value implies that reducing the indicator of employment protection for regular contracts from the OECD average to the level of the United States entails an increase in the rate of same-sector job-to-job transitions by about 60%. We also assess the importance of different regulatory provisions and find that the

[37] Papke and Wooldridge (1996) show that QMLE estimators of this kind yield consistent estimates of equation [8] independently of any assumption on the error term, for which a robust variance estimator can be easily devised. In addition, in contrast to the more classical weighted-least-square (WLS) estimation of a linear model with log-odd transformation of the dependent variable, the GLM specification does not require adjustment for boundary values (such as zeros) and can be estimated when fractional data are obtained by sample averages in samples of unknown size that cannot therefore be used to construct weights, as is the case for the data used in this paper (see Bassanini and Brunello, 2011, for an application of a similar model to cross-country LFS data at the industry level).

practice of reinstatement in the case of unfair dismissal plays a crucial role in shaping gross worker flows: the more frequent this practice and the smaller are the flows.

Our results do not necessarily imply that relaxing dismissal regulations brings about an increase in dismissals. Indeed our data do not allow distinguishing dismissals from voluntary quits and there is some evidence that stricter employment protection depresses the latter (see e.g. Gielen and Tatsiramos, 2012). However, our results are consistent with the idea that if reforms liberalising dismissal regulations yield an increase in dismissals, they also increase the job finding rate following displacement. Thus, our results cautiously suggest that those displaced workers that would not have been displaced in the absence of deregulation tend to find relatively quickly another job. What is more, our evidence indicates that most of the additional transitions induced by regulatory changes will occur across jobs within the same industry, with therefore limited destruction of industry-specific human capital and likely no negative effects on reallocation efficiency. Yet, assessing more directly the impact of dismissal regulations on the efficiency of the reallocation process appears a much needed and promising avenue for future research. Moreover, as many countries have significantly reformed employment protection for open-ended contracts in recent years, individual longitudinal data should be mobilised to explore more directly the trajectories of displaced workers in the aftermath of regulatory reforms.

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Tables

Table 1 Baseline difference-in-difference results

Panel A: Standard worker flows (total, hirings and separations)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable	REAL	REAL	HR	HR	SR	SR
EPRC x US REAL	-0.123***	-0.108***	-0.061***	-0.052***	-0.063***	-0.056***
	(0.039)	(0.026)	(0.020)	(0.014)	(0.021)	(0.015)
Temporary (%)		0.611***		0.327***		0.284***
		(0.041)		(0.019)		(0.027)
Age: 15-24 (%)		0.493***		0.271***		0.223***
		(0.067)		(0.034)		(0.039)
Age: 25-34 (%)		0.245**		0.148***		0.097
		(0.109)		(0.054)		(0.065)
Age: >54 (%)		-0.116		-0.091		-0.024
		(0.119)		(0.061)		(0.073)
Low educated (%)		0.137**		0.070**		0.067*
		(0.062)		(0.031)		(0.036)
Med. Educated (%)		0.056		0.033		0.023
		(0.049)		(0.025)		(0.031)
Self employed (%)		0.039		0.036*		0.003
		(0.040)		(0.020)		(0.026)
Women (%)		-0.029		-0.020		-0.009
		(0.040)		(0.019)		(0.026)
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	528	528	528	528	528	528
R-squared	0.843	0.921	0.856	0.935	0.789	0.859

Table 1 Baseline difference-in-difference results (*cont.*)
Panel B: Other type of separations

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable	J2JLR	J2JLR	SSR	SSR	OSR	OSR
EPRC x US REAL	-0.003	0.007	-0.048***	-0.043***	-0.015	0.002
	(0.018)	(0.015)	(0.017)	(0.016)	(0.014)	(0.010)
Temporary (%)		0.118***		0.154***		0.005
		(0.022)		(0.023)		(0.012)
Age: 15-24 (%)		0.086**		0.063*		0.136***
		(0.036)		(0.034)		(0.028)
Age: 25-34 (%)		0.032		0.023		0.018
		(0.040)		(0.038)		(0.028)
Age: >54 (%)		-0.005		-0.055		-0.014
		(0.048)		(0.049)		(0.037)
Low educated (%)		0.042**		0.063***		-0.013
		(0.021)		(0.020)		(0.015)
Med. Educated (%)		0.050***		0.021		-0.026*
		(0.019)		(0.017)		(0.015)
Self employed (%)		0.005		0.014		0.008
		(0.016)		(0.015)		(0.014)
Women (%)		-0.002		-0.020		0.003
		(0.016)		(0.018)		(0.012)
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	415	415	415	415	415	415
R-squared	0.674	0.736	0.784	0.844	0.768	0.810

Notes: Robust standard errors in parentheses. REAL: Total worker reallocation rate. HR: Hiring rate. SR: Separation rate. J2JLR: job-to-jobless separation rate. SSR: same-sector separation rate. OSR: other-sector separation rate. EPRC: Indicator of employment protection legislation for regular contracts, including provisions for collective dismissals. Data are averaged over the 2000-2007 period. Average US REAL is 43.2%. *** p<0.01, ** p<0.05, * p<0.1.

Table 2 Including institutional controls

Panel A: Total worker reallocation rates

Dep. variable	(1) REAL	(2) REAL	(3) REAL	(4) REAL	(5) REAL
EPRC x US REAL	-0.103***	-0.112***	-0.110***	-0.129***	-0.111***
	(0.027)	(0.027)	(0.028)	(0.031)	(0.031)
PMR x US REAL	-0.052	-0.030	-0.026	-0.122	-0.105**
	(0.055)	(0.056)	(0.058)	(0.095)	(0.050)
ARR x US REAL		0.002	0.002	-0.001	-0.001
		(0.001)	(0.001)	(0.003)	(0.004)
Tax wedge x US REAL			-0.001	0.003	-0.001
			(0.002)	(0.004)	(0.005)
Corporatism x US REAL				-0.029	0.003
				(0.026)	(0.029)
Coll. Barg. Cov. x US REAL				0.002	0.003
				(0.002)	(0.002)
Home Ownership x US REAL				-0.002*	
				(0.001)	
ALMP Intensity x US REAL					-0.001
					(0.001)
Country dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
Workers' characteristics	Yes	Yes	Yes	Yes	Yes
Observations	528	508	508	341	409
R-squared	0.921	0.923	0.923	0.940	0.924

Table 2 Including institutional controls (*cont.*)
Panel B: Same-sector job-to-job separations

	(1)	(2)	(3)	(4)	(5)
Dep. variable	SSR	SSR	SSR	SSR	SSR
EPRC x US REAL	-0.047***	-0.044***	-0.049***	-0.056***	-0.038**
	(0.016)	(0.016)	(0.017)	(0.016)	(0.017)
PMR x US REAL	-0.021	-0.019	-0.009	-0.008	-0.035
	(0.027)	(0.035)	(0.037)	(0.050)	(0.026)
ARR x US REAL		0.000	0.000	-0.002*	-0.002
		(0.001)	(0.001)	(0.001)	(0.002)
Tax wedge x US REAL			-0.002	-0.004**	-0.002
			(0.001)	(0.002)	(0.002)
Corporatism x US REAL				0.017	0.022*
				(0.014)	(0.012)
Coll. Barg. Cov. x US REAL				0.001	0.001
				(0.001)	(0.001)
Home Ownership x US REAL				-0.001	
				(0.001)	
ALMP Intensity x US REAL					-0.000
					(0.001)
Country dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
Workers' characteristics	Yes	Yes	Yes	Yes	Yes
Observations	415	395	395	250	318
R-squared	0.845	0.853	0.855	0.898	0.884

Notes: Robust standard errors in parentheses. REAL: Total worker reallocation rate. SSR: same-sector separation rate. EPRC: Indicator of employment protection legislation for regular contracts, including provisions for collective dismissals. PMR: Product market regulation. ARR: Average replacement rate. Data are averaged over the 2000-2007 period. Average US REAL is 43.2%, and its standard deviation is 14.4%. Workers' characteristics are those indicated in Table 1. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Alternative proxies for the industry's reallocation propensity

Panel A: Total worker flows

	(1)	(2)	(3)	(4)
Benchmark and Method	UK REAL, OLS	UK REAL, OLS	US REAL, 2SLS	US REAL, 2SLS
Dep. variable	REAL	REAL	REAL	REAL
EPRC x US REAL	-0.174***	-0.167***	-0.211***	-0.198***
	(0.065)	(0.043)	(0.044)	(0.033)
Temporary (%)		0.617***		0.641***
		(0.041)		(0.040)
Age: 15-24 (%)		0.486***		0.453***
		(0.067)		(0.063)
Age: 25-34 (%)		0.243**		0.256**
		(0.112)		(0.104)
Age: >54 (%)		-0.162		-0.112
		(0.122)		(0.116)
Low educated (%)		0.146**		0.131**
		(0.063)		(0.059)
Med. Educated (%)		0.063		0.045
		(0.050)		(0.047)
Self employed (%)		0.053		0.049
		(0.041)		(0.039)
Women (%)		-0.027		-0.030
		(0.041)		(0.038)
Country dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
F-test on instrument			120.8***	136.1***
Observations	505	505	528	528
R-squared	0.840	0.921	0.841	0.919

Table 3: Alternative proxies for the industry's reallocation propensity (*cont.*)
Panel B: Same sector separations

	(1)	(2)	(3)	(4)
Benchmark and Method	UK REAL, OLS	UK REAL, OLS	US REAL, 2SLS	US REAL, 2SLS
Dep. variable	SSR	SSR	SSR	SSR
EPRC x US REAL	-0.063**	-0.063***	-0.064***	-0.054***
	(0.025)	(0.023)	(0.017)	(0.016)
Temporary (%)		0.156***		0.156***
		(0.023)		(0.021)
Age: 15-24 (%)		0.061*		0.060*
		(0.034)		(0.031)
Age: 25-34 (%)		0.022		0.023
		(0.038)		(0.036)
Age: >54 (%)		-0.060		0.015
		(0.048)		(0.014)
Low educated (%)		0.065***		0.062***
		(0.020)		(0.019)
Med. Educated (%)		0.023		0.020
		(0.017)		(0.016)
Self employed (%)		0.015		-0.053
		(0.015)		(0.046)
Women (%)		-0.018		-0.019
		(0.018)		(0.017)
Country dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
F-test on instrument			72.8***	79.3***
Observations	415	415	415	415
R-squared	0.784	0.845	0.783	0.844

Notes: Robust standard errors in parentheses. REAL: Total worker reallocation rate. SSR: same-sector separation rate. EPRC: Indicator of employment protection legislation for regular contracts, including provisions for collective dismissals. 2SLS estimates are obtained by instrumenting EPRC x US REAL by the interaction of EPRC and industry-specific slopes in an equation where REAL is regressed on EPRC and country and industry dummies. Data are averaged over the 2000-2007 period. Average US and UK REAL are 43.2% and 40.4%, respectively, with standard deviation 14.4% and 10.4%, respectively. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Detailed dismissal regulations

Panel A: Total worker flows

	(1)	(2)	(3)	(4)	(5)
Dep. variable	REAL	REAL	REAL	REAL	REAL
Reg. on individual dismissal	-0.076***				
	(0.019)				
Of which					
Procedural Inconvenience		-0.016			
		(0.031)			
Notice/Severance pay		-0.011	-0.012		
		(0.015)	(0.014)		
Difficulty of dismissal		-0.048*	-0.059***	-0.061***	
		(0.029)	(0.018)	(0.017)	
Of which					
Definition of unfair dismissal					-0.008
					(0.009)
Length of trial period					-0.025
					(0.016)
Compensation for unfair dism.					0.009
					(0.013)
Possibility of reinstatement					-0.032***
					(0.008)
Reg. on collective dismissal	-0.047**	-0.043*	-0.038*	-0.035*	-0.020
	(0.021)	(0.024)	(0.020)	(0.020)	(0.022)
Country dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
Workers' characteristics	Yes	Yes	Yes	Yes	Yes
Observations	528	528	528	528	528
R-squared	0.920	0.921	0.921	0.921	0.924

Table 4: Difference-in-difference results with detailed EP (*cont.*)

Panel B: Same sector separations

	(1)	(2)	(3)	(4)	(5)
Dep. variable	SSR	SSR	SSR	SSR	SSR
Reg. on individual dismissal	-0.033***				
	(0.012)				
Of which					
Procedural Inconvenience		-0.012			
		(0.015)			
Notice/Severance pay		-0.005	-0.006		
		(0.006)	(0.006)		
Difficulty of dismissal		-0.015	-0.021**	-0.021**	
		(0.014)	(0.010)	(0.010)	
Of which					
Definition of unfair dismissal					-0.004
					(0.004)
Length of trial period					-0.000
					(0.008)
Compensation for unfair dism.					-0.005
					(0.009)
Possibility of reinstatement					-0.008*
					(0.004)
Reg. on collective dismissal	-0.027***	-0.026***	-0.022***	-0.019***	-0.017*
	(0.009)	(0.010)	(0.007)	(0.006)	(0.010)
Country dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
Workers' characteristics	Yes	Yes	Yes	Yes	Yes
Observations	415	415	415	415	415
R-squared	0.843	0.844	0.843	0.842	0.843

Notes: Robust standard errors in parentheses. REAL: Total worker reallocation rate. SSR: same-sector separation rate. All regulation variables are multiplied by US REAL. Data are averaged over the 2000-2007 period. Average US REAL is 43.2%. Workers' characteristics are those workers' covariates that are significant in specifications of Table 1. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Time series results: linear model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	REAL	HR	SR	SSR	SSR (open-ended)	OSR	J2JLR
EPR	-6.06***	-2.96***	-3.10***	-1.79**	-1.34***	0.34	-1.40
	(1.96)	(0.98)	(0.98)	(0.70)	(0.49)	(0.83)	(0.86)
ARR	-0.07	-0.04	-0.03	-0.05*	-0.03	-0.02	0.04
	(0.06)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.05)
Union density	-0.13	-0.06	-0.06	0.10*	0.07*	0.01	-0.07
	(0.17)	(0.09)	(0.08)	(0.05)	(0.04)	(0.05)	(0.06)
Tax wedge	-0.10	-0.05	-0.05	-0.08**	-0.04	0.00	0.04
	(0.11)	(0.06)	(0.06)	(0.04)	(0.02)	(0.04)	(0.05)
PMR	-0.70***	-0.29***	-0.41***	-0.22**	-0.30***	-0.16**	-0.23***
	(0.19)	(0.10)	(0.10)	(0.09)	(0.08)	(0.07)	(0.07)
Δ employment gap	-0.63***	0.18***	-0.81***	-0.19***	-0.13***	-0.20***	-0.39***
	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Workers' characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,966	2,966	2,966	1,940	1,905	1,940	1,986
R-squared	0.772	0.806	0.776	0.535	0.559	0.582	0.627

Notes: Clustered standard errors at country-by-time level in parentheses. REAL: Total worker reallocation rate. HR: Hiring rate. SR: Separation rate. SSR: same-sector separation rate. OSR: other-sector separation rate. J2JLR: job-to-jobless separation rate. EPR: Indicator of employment protection legislation for regular contracts, excluding provisions for collective dismissals. ARR: average unemployment benefit replacement rate. PMR: Product market regulation. Δ employment gap is the difference between the current and average growth rates of employment (the latter computed over the period 1990-2007). Workers' characteristics are those indicated in Table 1. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Time series results: GLM

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	HR	SR	SSR	SSR (open-ended)	OSR	J2JLR
EPR	-0.136***	-0.128***	-0.179***	-0.154**	0.028	-0.124
	(0.044)	(0.044)	(0.066)	(0.066)	(0.120)	(0.077)
ARR	-0.001	-0.001	-0.006*	-0.004	-0.003	0.002
	(0.001)	(0.001)	(0.003)	(0.003)	(0.004)	(0.004)
Union density	-0.003	-0.003	-0.010**	-0.005	-0.002	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Tax wedge	-0.002	-0.001	-0.023**	-0.040***	-0.030***	-0.021***
	(0.002)	(0.002)	(0.009)	(0.011)	(0.010)	(0.006)
PMR	-0.013***	-0.020***	0.012**	0.010**	0.003	-0.002
	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.005)
Δ employment gap	0.008***	-0.034***	-0.021***	-0.019***	-0.026***	-0.032***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Workers' characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,966	2,966	1,940	1,905	1,940	1,986

Notes: generalized (inverted-probit) linear model, estimated by quasi-maximum likelihood. Reported coefficients refer to β parameters of equation [8]. Clustered standard errors at country-by-time level in parentheses. HR: Hiring rate. SR: Separation rate. SSR: same-sector separation rate. OSR: other-sector separation rate. J2JLR: job-to-jobless separation rate. EPR: Indicator of employment protection legislation for regular contracts, excluding provisions for collective dismissals. ARR: average unemployment benefit replacement rate. PMR: Product market regulation. Δ employment gap is the difference between the current and average growth rate of employment (the latter computed over the period 1990-2007). Workers' characteristics are those indicated in Table 1. *** p<0.01, ** p<0.05.

Appendix

A.1 Theoretical framework

Micco and Pages (2006) suggest that in a simple model of quadratic adjustment costs with idiosyncratic firm shocks, dismissal regulations have a greater impact on employment adjustments at the firm level in industries where the optimal employment level for the firm would be more volatile, for technological or demand reasons, in the absence of adjustment cost.[38] To set ideas, we briefly review their model here. Consider a closed economy with no entry. Each firm *j* uses labour *L* as only input to produce a quantity *Q* of a good or service with constant returns to scale. Product markets are imperfectly competitive, but the firm is small with respect to its industry so that it is price-taker in both the product and labour markets and faces a linear product demand $p = a_j - c_j Q_j$, where *p* is the price and *a* and *c* are parameters. Profits π can be written as:

$$\pi_j = \left(a_j - c_j Q_j\right) Q_j - wL_j \qquad \text{[A1]}$$

where *w* stands for wages. Substituting the production function $Q_j = b_j L_j$ in [A1] and maximizing with respect to *L*, one obtain that the optimal level of employment is $L_j^* = A_j$, where $A_j = (a_j b_j - w)/2c_j b_j^2$. Now, suppose that A_j is subject to stationary idiosyncratic shocks that have the same distribution within industries but different distribution across industries.[39] Following Davis et al. (1997), total job reallocation for industry *i* can be defined as the sum of the absolute value of net employment growth of each firm in the industry divided by average employment, that is:

$$JF_i = \frac{2\sum_{j \in i} \left| L_{jt} - L_{jt-1} \right|}{\sum_{j \in i} L_{jt} + \sum_{j \in i} L_{jt-1}} = \frac{E_i\left(\left| L_{jt} - L_{jt-1} \right|\right)}{E_i(L_{jt})} \qquad \text{[A2]}$$

[38] This result is also valid for (S,s) adjustment models if the probability of adjustment is constant (see Rotemberg, 1987, and Caballero and Engel, 1993, for the equivalence between (S,s) models and models with quadratic adjustment costs).

[39] Focusing on idiosyncratic shocks is important insofar the literature on gross job and worker flows (e.g. Davis et al. 1997, 2006) has shown that even in narrowly defined industries, each period expanding firm coexists with contracting firms and most of the resource reallocation occurs within the industry. Assuming that the distribution of shocks differ across industries appears plausible insofar as there is evidence that output volatility differs across industries (see e.g. di Giovanni and Levchenko, 2009, and Bodman, 2009) and that this results in larger differences in gross job flows across industries than across countries (see e.g. OECD, 2009).

where expectations are taken over all firms of industry *i* and the second equality follows from the law of large numbers and the fact that $E_i(L_t) = E_i(L_{t-1})$. In the absence of adjustment costs, the firm sets employment at the optimal level each period, which yields:

$$JF_i^* = \frac{E_i\left(\left|A_{jt} - A_{jt-1}\right|\right)}{E_i(A_{jt})}. \qquad [A3]$$

Adjusting labour is, however, costly. In particular, in countries where dismissal regulations are restrictive, downsizing is particularly costly (see e.g. Abowd and Kramarz, 2003, Kramarz and Michaud, 2010). Assuming that adjustment costs are quadratic, the optimal employment level of the firm is a weighted average of the employment level that would be set in the absence of adjustment costs and its long-run expected value, that is $L_{jt} = \lambda A_{jt} + (1-\lambda)E_j(A_{jt})$, where λ is a decreasing function of adjustment costs. Inserting this expression in [A2] and rearranging taking into account [A3] one obtains $JF_i = \lambda E_i(\left|A_{jt} - A_{jt-1}\right|)/E_i(A_{jt}) = \lambda JF_i^*$. In words, the actual job reallocation rate in industry *i* is given by the product of the rate that would be realized in the absence of adjustment costs and a decreasing function of adjustment costs. If dismissal regulations are the main determinants of adjustment costs, this yields

$$JF_i = \lambda(EPL)JF_i^* \qquad [A4]$$

where *EPL* stands for an indicator of stringency of dismissal regulations. In other words, we can expect that the effect of dismissal regulations on job reallocation is greater, the greater the natural propensity of an industry to reallocate labour.

In this paper, we have access to cross-country comparable data on worker flows. However, OECD (2009) shows that, in cross-country/cross-industry data defined in the same way as ours, job and worker reallocation rates are closely linked to one another and that regressing worker reallocation rates on job reallocation rates plus a constant yields a regression coefficient insignificantly different from 1. This suggests that we can approximate job flows as $JF_i \cong WF_i - \alpha$, where *WF* is the worker flow rate and α is a non-negative constant. Substituting this expression into [A4] yields:

$$WF_i \cong \alpha - \alpha\lambda(EPL) + \lambda(EPL)WF_i^* \qquad [A5]$$

which corresponds to [2] in the main text if one assumes $\Lambda = WF^*$ and $f = \lambda$.

A.2 Data construction, sources and descriptive statistics

Worker reallocation

In order to estimate gross worker flows among dependent employees, data from different Labour Force Surveys (LFS hereafter) for 25 countries are used. These data include the European Labour Force Surveys, the bi-annual Displaced workers/Job tenure supplement of the US Current Population Surveys, and the Canadian Labour Force Survey. These data are complemented with national accounts data at the industry level (drawn from EU KLEMS and OECD STAN), as described in the text.

Dismissal rates.

The US dismissal rate is from OECD (2009) and it is based on various waves of the CPS Displaced Workers Supplement (2000-2006, even years). An individual is considered to have been dismissed if he/she lost his/her job in the most recent year covered by each survey, because of plant closing or moved, insufficient work, or position or shift abolished. Only wage and salary employees in the private-for-profit sector are considered.

Other industry-level data

Several industry level variables are derived directly from LFS. These are the shares of temporary workers, self-employed workers, specific age classes, women and specific educational-attainment classes. In all cases they are obtained as the ratio of the specified group of employees divided by total employees in the same country, industry and year, excluding individuals with missing observations.

Institutional variables

EPL indicators come from the OECD Indicators of Employment Protection (www.oecd.org/employment/protection). The index of employment protection for regular workers including additional provisions for collective dismissals (EPRC) is obtained as the weighted average of the indexes for individual and collective dismissals (with weights equal to 5/7 and 2/7, consistent with the overall indicator of EPL stringency). In principle, all indicators vary from 0 to 6 from the least to the most stringent. In practice, however, cross-country variation is smaller: for example, EPRC varies between 0.94 in the United States and 3.87 in Portugal. Missing values for components at the most disaggregate levels are replaced with the value of the corresponding upper-level component.

UB generosity is measured on the basis of average gross replacement rates, defined as average unemployment benefit replacement rate across two income situations (100% and 67% of average worker earnings), three family situations (single, with dependent spouse, with spouse in work) and three different unemployment durations (first year, second and third years, and fourth and fifth years of unemployment). The source is the OECD Benefits and Wages database. Even years are interpolated.

Indexes of anti-competitive product market regulation come from the OECD Regulatory Database. They vary from 0 to 6 from the least to the most restrictive. The time-series is based only on the aggregation of regulatory changes for few detailed industries. See Wölfl et al. (2009) for more details on subcomponents.

Trade union density is defined as the percentage of employees who are members of a trade-union. ALMP expenditures are defined as public expenditures on active labour market programmes per unemployed worker as a share of GDP per capita. In order to minimise the effect of the cycle on this variable, raw data are regressed on the output gap (drawn from the OECD EO database) and only the residual is included in estimated specifications. The source of these variables is the OECD Employment Database.

The tax wedge considered in this paper is the wedge between the labour cost for the employer and the corresponding net take-home pay of the employee for couples with two children and averaged across four income situations. It is expressed as the sum of personal income tax and all social security contributions as a percentage of total labour cost. The time series refers only to a single-earner couple with two children earning 100% of average worker earnings. The source is the OECD Taxing Wages Database.

Home ownership is defined as the ratio of home-owners in the adult population. Collective bargaining coverage is the share of workers covered by a collective agreement, in percentage. The degree of corporatism takes values 1 for decentralised and uncoordinated wage-bargaining processes, and 2 and 3 for intermediate and high degrees of centralisation/co-ordination, respectively. The source of these variables is Bassanini and Duval (2009).

Table A1: Gross worker flows by country, 2000-2007 (percentages)

	Hiring rate	Separation rate	Job-to-job separation rate	Job-to-jobless separation rate	Same-sector separation rate	Same-sector sep. rate (open-ended)	Other-sector separation rate
Austria	14.90	15.03	9.87	4.94	6.84	5.70	3.03
Belgium	14.84	14.95	10.21	4.73	5.04	4.20	5.17
Canada	21.24	20.18					
Czech Republic	14.37	13.78	8.00	5.77	4.21	2.93	3.79
Denmark	22.15	23.30	13.36	9.76	8.09	7.08	5.27
Finland	20.08	19.75	12.19	7.53	7.15	4.31	5.04
France	16.28	16.50	10.11	6.97	4.90	3.13	5.21
Germany	14.44	15.47	8.47	7.01	6.58	4.67	1.89
Greece	11.89	11.74	6.52	5.22	4.24	2.74	2.28
Hungary	13.80	13.29	7.23	6.06	3.72	3.00	3.51
Iceland	28.54	26.92	23.18	3.27	11.66	10.67	11.52
Ireland	18.79	17.56					
Italy	12.97	12.04	7.73	4.32	4.87	3.41	2.86
Netherlands	18.73	17.65					
Norway	14.77	16.47	12.34	4.19	4.53	3.58	7.82
Poland	18.12	16.61	7.26	9.38	4.53	1.52	2.73
Portugal	14.44	14.64	8.12	6.52	4.21	1.97	3.90
Slovakia	13.54	12.28	6.24	5.94	3.53	2.93	2.71
Slovenia	13.45	13.20	8.55	4.81	6.37	3.57	2.18
Spain	22.29	19.38	10.75	8.50	6.69	1.78	4.06
Sweden	15.90	16.12	7.96	7.07	3.64	2.52	4.33
Switzerland	16.17	15.82					
Turkey	30.12	25.79	16.32	9.47	9.57	7.83	6.75
United Kingdom	19.50	21.16					
United States	21.21	22.11					

Table A2: Gross worker flows by industry, 2000-2007 (percentages)

Isic Rev.1 code	Industry label	Hiring rate	Separation rate	Job-to-job separation rate	Job-to-jobless separation rate	Same-sector separation rate	Other-sector separation rate	US total worker reallocation
15-16	Food , beverages and tobacco	17.85	18.84	11.18	7.60	5.85	5.33	39.34
17-19	Textiles, leather and footwear	13.80	19.82	8.51	10.24	4.80	3.70	45.59
20	Wood and manuf. of wood and cork	17.74	17.82	10.76	6.89	5.55	5.20	43.66
21-22	Pulp, paper, printing and publishing	14.84	16.54	9.22	6.96	4.80	4.42	36.57
24	Chemicals and chemical products	12.29	13.28	7.17	5.84	3.37	3.80	30.44
25	Rubber and plastics	15.76	15.12	8.38	5.94	3.84	4.53	35.85
26	Other non-metallic mineral products	14.39	14.71	7.91	6.15	3.98	3.93	38.65
27-28	Basic metals and fabricated metal	15.22	14.03	7.75	5.32	4.40	3.34	35.48
29	Machinery, not elsewhere classified	13.97	13.92	7.79	5.27	3.80	3.99	33.64
30-33	Electrical and optical equipment	15.90	16.65	9.16	6.85	4.96	4.20	36.97
34-35	Transport equipment	13.92	13.72	7.27	5.83	3.77	3.50	30.34
36-37	Other manufacturing; Recycling	16.77	17.34	9.80	6.58	4.88	4.93	43.52
40-41	Electricity, gas and water supply	8.45	9.74	4.77	4.80	2.50	2.26	18.29
45	Construction	24.47	21.90	14.52	7.41	11.06	3.46	58.56
50	Motor vehicles: sales and repair	19.61	17.91	11.23	5.32	6.55	4.68	59.49
51	Wholesale trade, excl. motor vehicles	18.32	16.45	10.47	5.50	5.65	4.82	42.13
52	Retail Trade, except of motor vehicles	25.36	23.20	13.38	8.11	7.83	5.55	65.59
55	Hotels and restaurants	34.86	32.49	20.65	10.31	13.65	7.01	88.41
60-63	Transport and storage	16.14	15.04	9.43	4.93	6.17	3.26	42.64
64	Post and telecommunications	14.21	14.76	7.70	6.28	3.66	4.04	31.28
65-67	Financial intermediation	13.30	12.32	7.00	4.38	4.62	2.38	42.18
70	Real estate activities	18.97	16.12	8.63	6.55	4.90	3.73	49.29
71-74	Other business services	23.54	19.08	12.05	5.80	7.54	4.51	48.46

Table A3: Explanatory variables used in cross-sectional regressions

	Mean	Std. Dev.		Mean	Std. Dev.
Employment protection legislation					
EPRC	2.47	0.59	Difficulty of dismissal	2.62	0.97
EPR	2.14	0.88	Definition of unfair dismissal	1.68	1.86
EPC	3.19	0.69	Lenght of trial period	3.97	1.36
Procedural incoveniences	2.14	1.05	Compensation for unfair dism.	2.42	1.37
Notice/Severance pay	1.79	1.01	Possibility of reinstatement	2.39	1.91
Other control variables					
	Mean	Std. Dev.		Mean	Std. Dev.
% temporary workers	9.94	8.08	ARR	27.62	13.41
% self employed	12.20	10.39	PMR	1.62	0.46
% Low education	28.29	19.45	Coll. bargaining coverage	62.78	26.20
% Middle education	53.48	18.27	Corporatism	2.13	0.89
% Age 15-24	12.37	6.70	Tax wedge	35.10	8.12
% Age 25-34	26.76	5.84	Home ownership rate	62.88	14.04
% Age 55+	10.49	4.55	ALMP intensity	31.54	25.09
% Women	32.45	16.94	Output gap	0.44	0.59

Notes: EPRC: Indicator of employment protection legislation for regular contracts, including provisions for collective dismissals. EPR: Indicator of employment protection legislation for regular contracts, excluding provisions for collective dismissals. EPC: Indicator of additional employment protection provisions for collective dismissals. PMR: Product market regulation. ARR: Average replacement rate. Data are averaged over the 2000-2007 period.

Table A4: Descriptive statistics (time-series sample)

	Mean	Std.Dev.
Total reallocation rate (%)	33.79	13.52
Separation rate (%)	16.69	6.90
Hiring rate (%)	17.10	7.30
Same-sector job-to-job separation rate (%)	5.32	3.39
Other-sector separation rate (%)	3.77	2.38
Job-to-jobless separation rate (%)	6.17	3.04
Same-sector separations, leading to open-ended contract (%)	3.63	2.46
Same-sector separations, leading to temporary contract (%)	1.76	2.02
EPR	2.08	0.87
ARR (%)	28.61	12.99
Tax wedge (%)	32.88	9.27
Corporatism	2.10	0.89
Union density (%)	34.88	21.10
PMR	0.84	1.47
Temporary workers (%)	9.07	7.15
Self employed (%)	12.84	11.38
Women (%)	32.66	16.58
Low educated (%)	30.63	19.67
Med. Educated (%)	50.93	17.49
age: 15-24 (%)	12.38	6.74
age: 25-34 (%)	26.58	5.59
age: 35-54 (%)	50.16	7.42
age: >55 (%)	10.87	4.54
Δ employment gap (%)	0.377	3.665

Notes: EPR: Indicator of employment protection legislation for regular contracts, excluding provisions for collective dismissals. ARR: average unemployment benefit replacement rate. PMR: Product market regulation. Δ employment gap is the difference between the current and average growth rates of employment (the latter computed over the period 1990-2007).

A.3 Separations of workers on open-ended contracts

In this appendix we use data from the European Community Household Panel (ECHP) to replicate our baseline estimates by excluding employees on temporary contracts before the separation. However, due to data availability, this can be done only for a much coarser partition of industries and smaller number of countries. The ECHP is a longitudinal survey modelled on the British Household Panel Survey (BHPS). This survey provides a wealth of information on individual income and socio-economic characteristics for a number of EU countries. Due to the common questionnaire, the information contained in the ECHP is, in principle, comparable across countries and it is meant to be representative both in cross-sections and longitudinally. However, given the limited number of observations, some caution is required when drawing cross-country comparisons. For each worker we identify that a separation has taken place by exploiting the information on the date of start of the current job and the date of the interview. In this way we are able to identify separations between t and $t + 1$ year for each wave. This information is also cross-checked across waves as well as using data on the end date of the previous job, and we drop the few individuals with inconsistent responses. Our panel covers 11 countries between 1995 and 2001.[40] Given that the industry information is less detailed than in LFS data, the business-sector is disaggregated in only 13 industries. We therefore aggregate US reallocation rates at this level of aggregation and match them with our database and with data on employment protection. Then we estimate the following simple linear probability model:[41]

$$T_{cjit} = X_{it}\beta + \delta B_j EPL_c + \eta_{ct} + \eta_j + \varepsilon_{cj} \qquad \text{[A6]}$$

where T is an indicator variable taking value 1 if a transition of a given type occurred between time t and t+1 for an individual i who was in country c and industry j at time t. Other variables are as in Sections 1 and 3, except the ηs that represent country-by-time and industry fixed effects. As errors are likely to be correlated within countries and industries and over time, we cluster errors at the country-by-industry level. Finally, in order to obtain estimates that can be compared with those in Section 3, we limit additional controls to those that are included in the specifications of that section and multiply estimated coefficients by 100.

Table A5 presents results obtained by estimating equation [A6] on the full sample of employees, including both temporary and permanent workers. When occurrence of any type

[40] Austria, Belgium, Denmark, Finland, France, Greece, Ireland, Italy, the Netherlands, Portugal and Spain.
[41] We use a linear probability model here to maintain comparability with estimates in Section 3.

of separation is used as dependent variable, the estimated coefficient of the interaction between EPRC and the US reallocation rate is close to those reported in Table 1 if no additional control is included, while it is slightly smaller but more significant if controls, including contract type, are included. In the case of same-sector separations, estimates appear close to those reported in Table 1. Overall, Table A5 suggests that we can meaningfully compare estimates obtained from the ECHP with those obtained with our main dataset.

Excluding employees on fixed-term contracts yields slightly smaller but more significant estimates for both all separations and same-sector separations (Table A6). This can be explained by the fact that, as expected, EPRC is not a good predictor of the separation hazard for temporary workers. By contrast, the estimated coefficient of the interaction between EPRC and the US reallocation rate is small and insignificant in the case of both other-sector transitions and job-to-jobless transitions. Statistical tests (available from authors upon request) also show that the effect on same-sector separations is significantly different from that of other type of separations. Overall, these results confirm that our findings on the association between EPRC and separations is most likely due to its impact on separation hazards for employees on open-ended contracts.

Table A5: Baseline difference-in-difference results estimated on the ECHP

	(1)	(2)	(3)	(4)
Dep. Variable	S	S	SS	SS
EPRC x US REAL	-0.061*	-0.037**	-0.041*	-0.035**
	(0.031)	(0.015)	(0.023)	(0.015)
Country-by-year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Other controls	No	Yes	No	Yes
Observations	91,339	90,256	91,339	90,256
R-squared	0.030	0.137	0.025	0.066

Notes: The sample includes only wage and salary employees. Estimated coefficients multiplied by 100. Robust standard errors, clustered on country and industries, in parentheses. REAL: Total worker reallocation rate. S: dummy variable equal to 1 in the case of a separation. SS: dummy variable equal to 1 in the case of a same-sector separation. EPRC: Indicator of employment protection legislation for regular contracts, including provisions for collective dismissals. Other controls include: gender, 6 age classes, 3 educational-attainment classes and temporary contract status. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Baseline difference-in-difference results, excluding temporary contracts

Panel A: No controls, except for country-by-year and industry dummies

	(1)	(2)	(3)	(4)
Dep. variable	S	SS	OS	J2JL
EPRC x US REAL	-0.048**	-0.030**	-0.012	-0.006
	(0.020)	(0.013)	(0.010)	(0.004)
Country-by-year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Other controls	No	No	No	No
Observations	81,316	81,316	81,316	81,316
R-squared	0.016	0.011	0.011	0.003

Panel B: Other controls

	(1)	(2)	(3)	(4)
Dep. variable	S	SS	OS	J2JL
EPRC x US REAL	-0.028***	-0.025**	0.001	-0.004
	(0.011)	(0.010)	(0.008)	(0.004)
Country-by-year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Observations	80,456	80,456	80,456	80,456
R-squared	0.047	0.022	0.030	0.005

Notes: The sample includes only permanent employees. Estimated coefficients multiplied by 100. Robust standard errors, clustered on country and industries, in parentheses. REAL: Total worker reallocation rate. S, SS, OS and J2JL are dummy variables equal to 1 in the case of a separation, a same-sector separation, an other-sector separation and a job-to-jobless separation, respectively. EPRC: Indicator of employment protection legislation for regular contracts, including provisions for collective dismissals. Other controls include: gender, 6 age classes and 3 educational-attainment classes. *** p<0.01, ** p<0.05, * p<0.1.