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INSTITUTIONAL DETERMINANTS OF WORKER FLOWS: A CROSS-COUNTRY/CROSS-INDUSTRY APPROACH

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SUMMARY

There is little cross-country comparative evidence on the way labour market institutions shape gross job and worker flows, by and large because comparable data for many countries are scarce. By using a unique harmonised dataset on hirings and separations at the industry-level for a large majority of OECD countries, we fill this gap, by analysing the role of a number of labour and product market institutions in shaping cross-country differences in gross worker flows. In order to identify the effect of policies and institutions we consider an industry-level difference-in-difference approach. The basic premise of this approach is that the effect of a particular policy on gross job flows is greater in industries where the policy is more likely to constrain firm behaviour. We check, however, the robustness of our results using more standard cross-country/time-series estimates. The richness of the data available to us allows estimating the impact of the institutions also on the transitions from job to job, the transitions from job to non-employment and the transitions from non-employment to jobs. We find that cross-country differences in job protection for open-ended contracts and unemployment benefits can explain a large share of cross-country variation in gross worker flows. However, the effect of the former is essentially limited to job-to-job flows.

Keywords: worker flows; job-to-job transitions; labour market institutions; cross-country data

RÉSUMÉ

Il y a peu de résultats comparatifs à travers les pays sur la manière dont les institutions du marché du travail façonnent les flux bruts d'emplois et de main d'oeuvre, en raison d'un manque de données comparables pour un certain nombre de pays. Cet article comble cet écart, en s'appuyant sur une base de données harmonisées sur les embauches et les cessations d'emplois au niveau des secteurs d'activité pour un grand nombre de pays de l'OCDE, et en analysant comment un certain nombre d'institutions nationales des marchés du travail et des produits façonnent les écarts de réallocation de main d'œuvre entre les pays. Afin d'identifier l'effet des politiques et des institutions nationales, nous considérons un modèle de différence en différences au niveau des secteurs d'activité. Le principe de base de cette méthode est que l'effet d'une certaine politique sur le flux brut d'emplois est supérieur dans les industries où cette politique est de nature à imposer une contrainte sur le comportement des entreprises. Nous vérifions, cependant, la robustesse des résultats en utilisant des estimations plus standards en coupe transversale et série temporelle. La richesse des données disponibles permet également l'estimation de l'impact des institutions sur les transitions d'un emploi à l'autre, les transitions d'un emploi au non-emploi et les transitions du non-emploi vers l'emploi. Nous trouvons que les différences inter-pays dans la protection de l'emploi pour les contrats à durée indéterminée et les prestations de chômage peuvent expliquer une large proportion des variations inter-pays des flux bruts de main d'oeuvre. Cependant, l'effet du premier est essentiellement limité aux flux d'un emploi à l'autre.

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Introduction

Market-based economies are characterised by a continuous reallocation of labour and other productive 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.* Bassanini and Marianna, 2009).

Job and worker flows are remarkably different across countries: in some countries annual job and worker reallocation are as large as 25% and 45%, respectively, of dependent employment. By contrast, in a number of other countries, less than 15% of jobs are created and/or destroyed and about 25% of all workers are hired or separate from their employer in a given year. This suggests that country-specific policies and institutions are likely to play an important role in determining the level of job and worker reallocation. However, there is little cross-country comparative evidence on the way labour market institutions shape these flows, by and large because comparable data for many countries are scarce. The few studies that look at the cross-country impact of institutions on labour reallocation are usually confined to overall employment protection, consider a small number of OECD countries and/or use data that are not thoroughly comparable (see, among others, Micco and Pages, 2006, Haltiwanger *et al.*, 2008, Gomez-Salvador *et al.*, 2004, Boeri and Garibaldi, 2009, Cingano *et al.*, 2010). By using a unique harmonised dataset on hirings and separations at the industry-level for a large number of OECD countries, we fill this gap, by analysing the role of a number of labour and product market institutions in shaping cross-country differences in labour reallocation. As shown in previous work, at the cross-country/cross-industry level job and worker flows are closely correlated (see *e.g.* Bassanini and Marianna, 2009) therefore our conclusions are likely to hold also as regards gross job flows.

We think that looking at determinants of worker flows is important insofar as labour reallocation has been found to be a key driver of productivity growth: less productive firms tend to destroy more jobs and more productive ones to create more jobs (see *e.g.* OECD, 2009, for a survey). More generally, a growing body of evidence suggests that the process of firm birth and death, as well as the reallocation of resources from declining to expanding businesses, contribute significantly to productivity and output growth (*e.g.* Griliches and Regev, 1995; Foster *et al.*, 2001; and Bartelsman *et al.*, 2009). In addition, existing evidence suggests that the positive relationship between reallocation and productivity is not due to firm heterogeneity (Bassanini and Marianna, 2009). In this paper, therefore, we focus on the relationship between worker flows and a number of labour and product market institutions (including employment protection regulations, unemployment benefits, minimum wages and anti-competitive product market regulation) that have been found to have a significant impact on productivity (see Nicoletti and Scarpetta, 2003, Bassanini and Venn, 2008, and Bassanini *et al.*, 2009, and the literature cited therein). Our findings can indeed contribute to shed some light on the channels through which these institutions affect productivity performance. In particular, to anticipate our results, we find that cross-country differences in job protection for open-ended contracts can explain a large share of cross-country variation in worker (and possibly job) flows, which allows speculating that the reason why dismissal regulation tend to impair productivity growth is because of the barriers it imposes to efficient labour reallocation. We also find a significant impact of unemployment benefits and product market regulation on worker flows. By contrast we cannot detect any effect of minimum wages.

In order to identify the effect of policies and institutions, we follow Micco and Pages (2006) and Haltiwanger et al. (2008) and consider an industry-level difference-in-difference approach. The basic premise of this approach is that the effect of particular policies on gross job flows is greater in industries where the policy is more likely to constrain firm behaviour – hereafter called "policy-binding industries". For example, employment protection is more likely to be binding in industries where the propensity to make staff adjustments on the external labour market is high. 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. The advantage of this approach is that it allows controlling for all factors that are unlikely to affect labour flows differently in policy-binding and other industries. In addition, through this approach we can better address endogeneity issues.

The richness of the data available to us allows estimating the impact of the same institutions on the transitions from job to job, the transitions from job to non-employment and the transitions from non-employment to jobs. A dynamic labour market could indeed represent both an opportunity and a cost for workers. Some workers quit their jobs, because they have decided to search for jobs that better match their skills and needs and are hired to fill new positions or to replace previous employees. In the process, these workers typically progress in their career and pay (*e.g.* Postel-Vinay and Robin, 2002, Connolly and Gottschalk, 2004, and Contini and Villosio, 2007). But other workers are dismissed, either because of post suppressions or because their employers decide to replace them with other workers. For those who are dismissed or have been asked to leave, it might take time to find another job and such job might not offer comparable pay (*e.g.* OECD, 2004). For this reason, it is important to look at these type of transitions in order to trace out key distributional implications of labour market reforms. In the same vein, the paper also considers how institutions affect the wage premium/penalty associated to these transitions.

The paper is organised as follows. Section 1 present the main definitions of gross job and worker flows we refer to in this paper. Section 2 provides a brief survey on the effects of the selected policies and institutions on gross job and worker flows. Section 3 discusses empirical specifications. Data are presented in Section 4 and results in Section 5. Concluding remarks follows.

1. Definitions

At the level of an individual production unit (the firm in this paper), *gross job reallocation* (also commonly called *gross job turnover*, see for example Davis and Haltiwanger, 1992, 1999, Davis *et al.*, 1996, and OECD, 1996), is simply the absolute value of the net change in employment between two points in time. In this terminology, *job creation*, at the level of the individual firm, is equal to the net employment change, if the latter is positive, and zero otherwise. Conversely, *job destruction*, is equal to the absolute value of the net change, if the latter is negative, and zero otherwise. Job reallocation, job creation and job destruction are commonly called *gross* job flows, in order to differentiate them from the more familiar measures of *net* employment growth. Net and gross job flows coincide at the level of a single firm, but that is no longer the case when groups of firms are considered. For brevity, we often omit the qualifier "gross" when the context makes it clear that the flows being discussed are gross flows. Gross job flows are defined so as to be non-negative. They are also defined so as to exclude job vacancies which remain unfilled or jobs that begin and end within the interval of observation.

Gross worker flows reflect movements of workers into jobs (*hirings*) and out of jobs (*separations*) over a specified period of time. A measure of worker flows over a specified period could be based on: *i)* a full counting of all events during that period (*i.e.* every time a worker is hired or separates during the period); or *ii)* a more limited counting based on comparing two points in time (*i.e.* hirings equal the number of workers who are with the firm at time *t*, but were not with that employer at time *t*-1, and separations equal the number of workers who were with the firm at *t*-1, but not at *t*). Davis and Haltiwanger (1999) refer to the first definition as *worker turnover* and the second definition as *worker reallocation*. A number of intermediate definitions are also possible (see *e.g.* Davis *et al.*, 2006). Different definitions, however, result in entirely different estimates of worker flows, as can be illustrated with the following hypothetical example. Suppose a given firm had ninety-five employees at year t-1 and has 105 at t. During this period, ten people were hired to fill newly created posts. Suppose also that five other workers left the firm and were replaced by new recruits, another five workers were temporarily laid-off but re-called during the period and yet another five people were hired on fixed-term contracts that expired during the period and were not renewed. Job reallocation at the level of this firm (*i.e.* the absolute value of the net change in employment, as defined above) is equal to ten. By contrast, worker reallocation would be equal to forty or twenty according to definitions *(i)* or *(ii)*, respectively (intermediate definitions would lead to intermediate numbers, see Davis *et al.*, 2006). Because of data availability, we adopt the second definition, which is not uncommon in the literature (*e.g.* Abowd *et al.*, 1999, and Golan *et al.*, 2006). Whatever the definition, however, the following identity holds for each firm *i* at each time *t*:

$$\Delta E_{it} = JC_{it} - JD_{it} = H_{it} - S_{it}$$

where *E*, *JC*, *JD*, *H* and *S* stand for employment, job creation, job destruction, hirings and separations and Δ for differences between time *t*-1 and *t*.

In this paper we use one year as reference period. This implies that firm-level gross job flows refer to employment changes over a one year time span. Similarly hirings and separations are defined as one-year transitions across different employers and/or employment statuses. As firm-level employment is subject to short-term fluctuations (due for example to seasonal activity, temporary fluctuations in product demand or difficulties in filling vacancies after quits) and workers can change many jobs during a given time period, it is important to keep in mind that the period of time over which these flows are measured is key. For example, annual rates of hirings and separations – computed using the definition adopted in this paper – tend to be smaller than the sum of flows that can be calculated at a higher frequency during the same year (*e.g.* the sum of quarterly flows for all the four quarters of a given year).

Worker flows are aggregated by simply adding up their values over all firms in the group being considered, that is, by summing hirings and separations over all members of the specified group, where the group can be defined in terms of either groups of firms (*e.g.* all firms in an industry) or all workers sharing a particular demographic characteristic (*e.g.* belonging to a given age class or gender). According to the definition of hirings and separations adopted for this paper (*i.e.* one-year transitions), group-level hirings (separations) will be simply the number of workers with the given characteristics who were with one employer in year t but not with the same employer in t-1 (workers with given characteristics who were with one employer at time t-1, but not at t). For any group of job matches involving individuals with the same characteristics (*e.g.* a particular age or employed in a particular industry), we can define *excess worker reallocation* as the difference between total worker reallocation and the group's absolute net change in employment – that is for, say, industry *j* at time *t*:

$$EXCW_{jt} = REALW_{jt} - \left|\Delta E_{jt}\right| = H_{jt} + S_{jt} - \left|H_{jt} - S_{jt}\right|$$

where *EXCW* and *REALW* stand for excess and total gross worker reallocation, respectively. Excess worker reallocation provides a useful measure of the number of job matches that are created and destroyed, over and above the minimum necessary to accommodate net employment growth. In other words, it reflects the reallocation of job matches (the reshuffling of jobs and workers) within the same group.

Consistent with the literature (see *e.g.* Davis and Haltiwanger, 1999), all labour market flow measures from *t*-1 to *t* are expressed in this paper as rates by dividing flow totals by average employment in *t*-1 and *t*. In the hypothetical example above, the job reallocation rate is 10%, while the worker reallocation rate is 20%, in the definition adopted for this paper (one-year transitions).

Industry-level data constructed for this paper (see Section 3 below) allow distinguishing between job-to-job transitions and transitions from, and to, non-employment. We define *job-to-job transitions* as the count of workers that are in employment at both *t* and *t*-1 but who changed employer between these two dates. By contrast, *job-to-jobless transitions* occur when a worker is in employment at *t*-1 but not at *t*, and vice versa for *jobless-to-job* ones. As a consequence, for each industry and country, the hiring rate can be decomposed into *job-to-job* and *jobless-to-job hiring rates* – that is, the percentage ratios of the number of job-to-job and jobless-to-job transitions, respectively (concerning workers with an employer in that industry and country at time *t*) to the average of employment in *t*-1 and *t* for the same industry and country. In the same way, it is possible to decompose the separation rate into *job-to-job* and *job-to-jobless separation rates*, except that information on the industry of the employer at *t*-1 will be used. Job-to-job separations can be further decomposed into *same-industry* and *other-industry separations*, depending if industries at time *t* and *t*-1 are the same or different, while job-to-jobless separations can be decomposed into *employment-quitting* and *employment-losing separations*, depending on whether they were voluntary or involuntary.[1]

At the firm, industry or economy-wide level, it is also possible to compare job and worker flows. Following Burgess *et al.* (2000), we can define *churning flows* as the difference between worker reallocation and job reallocation – that is for, say, industry *j* at time *t*:

$$CH_{jt} = REALW_{jt} - REALJ_{jt} = H_{jt} + S_{jt} - JC_{jt} - JD_{jt}$$

where *CH* stands for churning flows, that is labour reallocation arising from firms churning workers through continuing jobs or employees quitting and being replaced on those jobs. Given these definitions, gross job flows are usually interpreted as reflecting essentially the dynamics of labour demand while churning flows are usually viewed as reflecting the pure match component determined by both supply and demand factors (see *e.g.* Davis and Haltiwanger, 1999; Pries and Rogerson; 2005; Haltiwanger *et al.*, 2008). However, Bassanini and Marianna (2009) shows that there is little cross-country variation of industry-level churning rates and that a simple regression of total worker reallocation on total job reallocation (including a constant) gives a coefficient of 0.98, insignificantly different from unity. In other words, a one-percentage-point increase in job reallocation is associated with an equal increase of worker reallocation, with no increase in worker churning. All this implies that, to a large extent, cross-country variation in gross worker flows is essentially due to demand factors and measures of worker or job flows can be used as substitutes in cross-country analysis: any conclusion drawn on the basis of one type of data is likely to be valid for the other. This is key for our analysis, insofar as cross-country comparable worker flow data are available for many more OECD countries.

1. Unfortunately, available data do not allow distinguishing between voluntary and involuntary job-to-job separations.

2. Selected institutions and worker flows: Theoretical literature and previous evidence

Employment protection

There is a large theoretical literature that looks at the impact of firing restrictions on labour flows with, by and large, consensual predictions. The rationale of dismissal regulations is that financial market imperfections might limit the ability of risk-averse workers to get insurance against dismissal (see *e.g.* Pissarides, 2010). However, by imposing implicit or explicit costs on the firm's ability to adjust its workforce to optimal levels, employment protection (EP hereafter) may inhibit efficient job separations and, indirectly, reduce efficient job creation (*e.g.* Mortensen and Pissarides, 1994). In principle, inefficiencies implied by firing regulations can be offset by private payments, wage adjustments or the design of efficient contracts (Lazear, 1990). However, wage rigidities, financial market imperfections or uncertainty about the future of the firm may prevent the effective operation of these channels. Standard equilibrium models of the labour market such as those of Nickell (1978), Bentolila and Bertola (1990) and Bertola (1990) describe firms' optimal behaviour in the presence of positive firing costs and show that the best strategy for firms is to reduce both job creation and destruction, with an ambiguous effect on average employment levels. Nevertheless, stricter EP implies a slower adjustment towards equilibrium employment levels. Search and matching models such as those of Garibaldi (1998) and Mortensen and Pissarides (1999) come to similar conclusions about job mobility being negatively affected by EP.

The theoretical analysis of the effect of regulation on temporary contracts is more straightforward. If the use of temporary contracts is liberalised while maintaining strict EP regulations for open-ended contracts, firms will react by substituting temporary for regular workers, with no long-run effect on employment, due to the smaller cost involved with the termination of the employment relationship at the end of a temporary contract (see *e.g.* Boeri and Garibaldi, 2007; Bentolila *et al.*, 2008). This also implies that the effect of regulation on temporary contracts cannot be seen in isolation, but it is conditional to the degree of stringency of EP for regular contracts. In the presence of protected insiders, covered by job security provisions, those under temporary contracts will bear the main burden of employment adjustment (Saint Paul, 1996).

There are a large number of country-specific studies that investigate the impact of EP legislation and jurisprudence on job flows on the basis of micro data. 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), using an Italian matched employer-employee dataset, and by Kugler and Pica (2008), who exploit an Italian reform that in 1990 increased firing restrictions for small firms. 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. She finds that the firing hazard for these employees decreased by 26% with respect to that of workers with 2–4 years of tenure. Moreover, the risk of job loss of new hires with less than one year of tenure also decreased by 19%, which is consistent with more selective recruitment practices. Kugler *et al.* (2010) 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. Finally, Venn (2010) analyses the impact on hirings of a recent Turkish reform of dismissal costs that applies differently to small and large firms, and reports large negative effects, especially for workers in the formal sector.

In contrast with these findings, a few micro studies find no impact of dismissal regulations on job or worker flows. Insignificant effects are found by Bauer *et al.* (2007), who look at changes of small-firm exemption thresholds on worker turnover using German matched employer-employee data. Similarly, Venn (2010) looks at the effect of a recent threshold increase for small firms in Australia and finds no impact on hiring, firing or working hours, possibly because employment protection rules in Australia were already among the least strict in the OECD prior to the reform. The small economic significance of certain specific exemptions perhaps could also explain why exemptions from procedural requirements for dismissal have not been found to have a significant effect on hiring or firing in exempted firms in Portugal (Martins, 2009) and Sweden (von Below and Thoursie, 2008).

Micro-studies can be complemented by cross-country studies, in particular to the extent that differences in the type of reforms limit the comparability of the findings of micro-econometric studies. Few studies look at the impact of employment protection on labour reallocation from a multi-country perspective and they mainly focus on gross job flows. Boeri and Garibaldi (2009) estimate an aggregate cross-country/time-series regression model on a small aggregate panel for 13 European countries covering the 1990s and find a negative impact of employment protection for temporary contracts on job-to-job transitions but no impact of provisions for regular workers. 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. The negative impact of employment protection on job reallocation, job creation and job destruction is found to be larger in industries where total employment is contracting and where firms cannot achieve substantial reductions in employment levels purely by relying on voluntary quits.

Standard cross-country/time-series studies that try to identify the effect of aggregate policies on labour reallocation through over-time variation are likely to suffer, however, from endogeneity and omitted-variable biases, insofar as it is impossible to write down the full list of institutional variables that are likely to be correlated with EP and affect worker flows. More relevant for this paper, Micco and Pages (2006) and Haltiwanger *et al.* (2008) use a difference-in-differences estimator on a cross-section of industry-level data for more than 15 countries. They find that the negative relationship between layoff costs and job flows is more negative in industries where US reallocation rates are larger, that is where it can be expected that EP effects are, if any, stronger. However, their samples include few OECD countries and their data come from different national sources;[2] therefore, it is difficult to generalise their result to the OECD as a whole. Equivalent results are obtained by Cingano *et al.* (2010), who apply a similar difference-in-differences methodology on firm-level data for 14 European countries, except that they use an estimate of the predicted job turnover that would occur in the absence of employment protection in order to classify industries.[3] Yet, their data exclude job reallocation due to entry and exit of firms.[4] Besides, their results become insignificant if France is excluded from the sample or if UK reallocation rates rather than predicted values are used to classify industries.

2. Data in Haltiwanger *et al.* (2008) are, however, harmonised ex-post using the same definitions and extraction procedure, which makes them in principle comparable.

3. As suggested by Ciccone and Papaioannou (2007).

4 . An additional issue concerning Gomez-Salvador *et al.* (2004), Messina and Vallanti (2007) and Cingano *et al.* (2010) is that none of these studies reports information on the data-cleaning treatment, despite using firm-level data from the Bureau van Dijk's Amadeus database where small businesses are severely under-represented and employment data are often inconsistent (see *e.g.* OECD, 2009).

There is less – albeit more consensual – evidence on the effects of regulation for temporary contracts, perhaps because its effects are more straightforward. Kahn (2010) uses longitudinal microdata for nine European countries and finds that recent policy reforms making it easier to create temporary jobs on average raised the probability that a worker will be on a fixed-term contract. However, he finds no evidence that such reforms increased employment: they rather appear to have encouraged substitution of temporary for permanent work. In a similar vein, several studies focus on major Spanish reforms in the early 1980s that liberalised temporary contracts without changing dismissal costs for regular contracts and find, in general, that this led to a very large increase of fixed-term contracts and a reduction in employment on permanent contracts (see *e.g.* Bentolila *et al.*, 2008; Aguirregabiria and Alonso-Borrego, 2009). Finally, several papers find that the difference in the cost of adjusting the stock of workers on different types of contract explains both the share of temporary workers and their relative volatility (see, for example, Goux *et al.*, 2001). This suggests that, ceteris paribus, stringent regulation on regular contracts should encourage the use of temporary contracts, a prediction which is confirmed by the literature (see *e.g.* OECD, 2004; Pierre and Scarpetta, 2004; Boockmann and Hagen, 2001).

Unemployment benefits

There are a number of channels through which unemployment benefits (UBs hereafter) could affect labour reallocation. First, generous UBs, by reducing search efforts, can increase the duration of unemployment spells and the overall level of unemployment (see OECD, 2006, for a survey of recent literature). This will tend to slow the transitions from unemployment to employment and therefore gross worker flows. Moreover, generous UBs (in terms of either duration, replacement rate or both) may provide a buffer of time and resources to allow the unemployed to find a job that better matches their skills and experience, resulting in higher quality matches between the unemployed and available job vacancies (Marimon and Zilibotti, 1999). In turn, higher quality job matches are likely to last longer, thereby depressing worker flows. However, the impact on reallocation rates is ambiguous: they could even increase if the effect on employment levels is larger than the effect on flows.

Second, in a standard equilibrium matching model of the labour market (*e.g.* Mortensen and Pissarides, 1994; 1999), more generous UBs, by increasing the reservation wage, will increase the sensitivity of job-matches to productivity shocks, thereby increasing job destruction in the short-run. If raising benefit entitlements does not affect the productivity of newly-created matches, job destruction will increase also in the long-run and greater unemployment, by increasing the number of applicants per vacancy, will progressively reduce recruitment costs, thereby raising hirings. However, if greater reservation wages increase the productivity threshold at which new job matches are created, thereby increasing the number of low-productivity potential matches that are turned down, the overall long-run effect on gross job and worker flows is a priori ambiguous.

Third, UB generosity might affect firm recruitment behaviour (Pries and Rogerson, 2005). Due to asymmetric information, firms might be unaware of the productivity potential of prospective job applicants. If wages are low with respect to the expected worker performance, the employer can afford to hire and discover on the job the worker's productive abilities. Whenever the newly hired worker turns out to be not suitable for the position, the match is destroyed and the firm issues a new vacancy. By contrast, to the extent that higher replacement rates raise reservation and bargained wages, firms might become choosier in selecting successful candidates. This in turn will reduce experimentation and mismatch, with consequent reduction in hirings, separations and short job spells, without necessarily reducing job creation and destruction.

Fourth, it is also possible that the provision of generous UBs encourages the creation of higher productivity jobs that are located in more volatile, innovative activities, or require workers with more specific skills and, therefore, carry greater risk of job mismatch (Acemoglu and Shimer, 1999, 2000). Job-matches created in this way would be exposed to greater destruction hazards, thereby increasing the pace of labour reallocation. For example, there is some evidence that there are higher rates of involuntary turnover in high-technology industries (as proxied by technology use, R&D investment or use of skilled labour – Givord and Maurin, 2004; Zavodny, 2004). If this is the case, in the absence of UBs, the unemployed will have an incentive to apply for low-skill jobs with a corresponding low risk of future displacement and firms will find it more difficult to fill high-technology/high-skill positions. In this context, generous UBs could allow the unemployed to risk future displacement by taking a high-skill job, knowing that, if they were laid off in the future, they would be supported by a safety net. Firms might therefore be more willing to offer such jobs, as the corresponding vacancies will be more easily filled, thereby increasing the share of high-skill jobs but also the overall risk of involuntary separations.

There is little empirical literature that looks directly at the effect of UBs on gross job or worker reallocation rates, in particular from a cross-country perspective. Boeri and Garibaldi (2009) estimate the impact on worker flows using aggregate cross-country/time-series data for 13 European countries and find a negative association of average gross replacement rates with employment-unemployment transitions but little association with job-to-job transitions. Gomez-Salvador *et al.* (2004) find a negative relationship between job creation and benefit duration – but no impact on job destruction – using a classical linear regression analysis based on European firm-level data and controlling for the effect of other labour market institutions, even though not for the level of the replacement rate. By contrast, Sjöberg (2007) finds a positive association between UB generosity and worker flows, by using a cross-section of individual data on job-to-job transitions drawn from Eurobarometer that are, however, simply regressed on aggregate average net replacement rates, with few other institutional controls. Finally, Boeri and Macis (2010) study the effect of reforms that introduced for the first time UB schemes in countries that previously did not have any such scheme. Using a large number of countries that had UBs throughout the period as a control group, they find that the introduction of benefits significantly increases between-industry job reallocation, although the estimated effect fades over time. Nevertheless, the relevance of this result remains limited since between-industry reallocation accounts for only a small fraction of total reallocation (see Bassanini and Marianna, 2009).

Minimum wages

Only few theoretical papers discuss directly the impact of minimum wages on gross worker flows. Burdett and Mortensen (1998) argue that in the presence of employer monopsony power, the distribution of wages can be inefficiently dispersed and separations rates excessively large. In such a case, minimum wages, by compressing the distribution of wage offers, could reduce voluntary separations and improve tenure. By contrast, Pries and Rogerson (2005) argue that high minimum wages, by increasing hiring wages, raise the productivity threshold at which job matches are created and make firm more selective in their recruitment practices. This will inefficiently reduce both hirings and separations. By running different simulations with their model, the authors predict a much greater effect of changes in the minimum wage than of changes in EP.

By contrast, the theoretical literature on the effects of wage rigidity on gross job and worker flows typically predicts a positive correlation between rigidity and labour adjustments. For example, Bertola and Rogerson (1997) argue that in the presence of downward wage rigidity, firms hit by negative shocks, being unable to adjust labour costs, will increase labour shedding, implying greater separations and subsequent re-hiring when their prospects improve. To the extent that binding minimum wages do not adjust as a function of economic conditions and firm performance, this argument can easily be applied to minimum wages as well.

There is a large empirical literature on the impact of statutory minimum wages on worker flows based on individual data from the United States. While early studies tend to find negative impact of minimum wages on job retention for individuals at, or close to, the minimum wage, more recent studies, by improving the sources of identification, have generally found no significant impact (Zavodny, 2000; Abowd *et al.*, 2005). Evidence for other countries is scarcer. Abowd *et al.* (2005) find no impact of real minimum wages on entry into employment in France, but a strong positive impact on exit from employment. By contrast, Portugal and Cardoso (2006), exploiting a specific Portuguese reform that in 1987 lifted dramatically minimum wages for very young workers, find that raising minimum wages had a significant negative effect on both separations and hirings. Finally Draca *et al.* (2008), using a difference-in-difference methodology similar to that adopted in this paper but on firm-level data, find that the introduction of a minimum wage in the United Kingdom in 1999 lead to insignificant changes in firm entry and exit patterns. Anyway, the degree to which this empirical evidence simply reflects short-time adjustment to a new equilibrium with different employment levels is unclear.

Product market regulation

There is a large consensus in the economic literature that regulations increasing the cost for firms of establishing new businesses in a specific market reduce both entry and exit of firms. If entry costs are lowered by a regulatory reform, ex-ante expected benefits from entry will be higher, thereby lowering the expected-productivity threshold at which a firm decides to set up its business. However, if the same regulatory reform does not affect each firm's potential operating costs, net of starting costs, productivity shocks will more frequently force low-productivity newly entered firms out of the market (*e.g.* Hopenhayn and Rogerson, 1993). Given that entry and exit account for about one third of gross job flows (see OECD, 2009), barriers to entry are likely to have an important impact on labour reallocation. Moreover, entering firms might be more efficient than incumbents, thereby forcing the latter to downsize and, possibly, exit the market (*e.g.* Aghion and Howitt, 1998). Finally, entering firms are likely to progressively expand, as they learn-by-doing how to run their business efficiently (*e.g.* Bahk and Gort, 1993).

Other types of regulation, such as price controls and public authorisation of strategic decisions, by potentially affecting normal operating costs of firms, have theoretically ambiguous effects on gross job reallocation. In fact, changes in these costs can increase or decrease the reactivity of firms to productivity shocks. On the one hand, an increase in fixed costs also makes entry less attractive, which by reducing the number of firms increases equilibrium prices. On the other hand, each firm has to spend more on fixed costs, which reduces net profits. In equilibrium, the net effect on profits is likely to be less negative/more positive for the most efficient firms, which gain more from higher prices. This might imply that, in order to survive, firms need to be more efficient in markets with higher fixed costs, which would imply a greater sensitivity to productivity shocks (Asplund and Nocke, 2006; Koeniger and Prat, 2007). Finally, the increase in trade competition due to globalisation and trade liberalisation is generally considered to increase restructuring at least in the short-run, thereby increasing job destruction but also job creation (see Melitz, 2003; and OECD, 2007 for a survey).

There is extensive cross-country empirical evidence on the negative association between product market regulation and firm entry and exit (see Schiantarelli, 2008, for a survey). This evidence is supported by the microeconometric literature, which typically tries to identify the impact of deregulation by evaluating the effects of specific reforms (see *e.g.* Aghion *et al.*, 2008). However, while there is abundant research on deregulation and employment and earnings (see *e.g.* Hirsch and Macpherson, 2000; Black and Strahan, 2001; Wozniak, 2007), there are fewer studies that look directly at the effect of deregulation on gross job and worker flows, and most of this literature focuses on the impact of trade with mixed results, particularly on job-to-job transitions (see *e.g.* OECD, 2007; Bloom *et al.*, 2010). Using a difference-in-differences estimator on a cross-section of industry-level data for several OECD and non-OECD countries, Haltiwanger *et al.* (2008) find a weakly-positive relationship between overall product market regulation and job turnover.

3. Estimation strategy

Difference-in-difference estimates

While we use more standard identifying assumptions in the case of product market regulation due to the nature of the data (see below), in order to identify the effect of our selected labour market policies on labour reallocation, this paper mainly looks at *within-country* worker-flow differences between industries. If a policy *POL* has a direct demand impact on worker reallocation – that is if worker reallocation is affected by the partial-equilibrium firms' direct response to changes in *POL*, this effect (be it positive or negative) is likely to be larger in industries where this type of policy tends to be more frequently binding. These industries will be called *policy-binding industries*, hereafter. For example, in the case of dismissal regulations, these are likely to be those industries that have a relatively high "natural" propensity to adjust their human resources through layoffs, due to industry-idiosyncratic technological and market-driven factors. In contrast, in industries where firms can restructure through internal adjustments, 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 adopting a difference-in differences approach. This difference-in-difference estimation 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 policy-binding industries as in other industries, in particular labour supply factors, can be controlled for by country dummies. This is particularly important as it is almost impossible to include in the empirical analysis a full list of all aggregate policies and institutions that are likely to affect gross worker flows so that standard cross-country/time-series estimates are likely to suffer severely from omitted-variable bias (see Bassanini *et al*., 2009, for an extensive discussion).

In practice, however, it is unlikely that a policy *POL* is either always binding or always not binding in a particular industry. Rather, whether and to what extent policies are binding depends on the costs (or opportunities) they impose on firms, which will vary from one industry to another in an almost continuous way. In the spirit of Rajan and Zingales (1998), a slightly more sophisticated identification assumption can therefore be considered: it is posited that, on average, the difference in gross worker flows between any two industries in any country can be expressed as a function of *POL* that is greater, the greater the difference in the likelihood that *POL* be binding for the average firm in those industries. To continue the example with dismissal regulations, it seems natural to assume that the impact of these regulations depends on the frequency at which firms in each industry would adjust human resources on the external labour market in the absence of regulations. In turn, this implies that the difference in the impact of these regulations can be assumed to be a function of the difference between the external-reallocation propensities of the two industries. More precisely, it is assumed that:

$$E[REAL_{cj}] = (\alpha + g(\Lambda_j))f(POL_c) + o_{cj} \qquad [1]$$

where *REAL* stands for the average gross flow rate of interest in country c and industry j in a specific period of time (2000-2007 in most of this paper), g is a non-negative and non-decreasing function of Λ, that is the likelihood that a particular policy *POL* be binding in industry j, α is a non-negative parameter, f is a generic 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. As a consequence, the difference in *REAL* between any pair of industries (indexed by k and h) can be written as:

$$E[REAL_{ck} - REAL_{ch}] = (g(\Lambda_k) - g(\Lambda_h))f(POL_c)$$

that is solely as a function of inter-industry differences in the likelihood that a policy be binding and the level of the policy itself.

The key issue in this procedure is how to measure Λ in practice. Let us denote with B the selected measure (called benchmark measure hereafter). In order to avoid endogeneity biases, B must not be affected by the level of the policy POL in each country. B must therefore be country-invariant and, to be meaningful, capture the likelihood that a given policy be binding if implemented in a country where there is no such policy. For instance, to continue with the EP example used above, one could argue that, given the permissive EP legislation in the United States, the distribution of US reallocation or dismissal rates by industries well captures the natural propensity to adjust on the external market that firms in each industry would have in the absence of regulations. The second key choice is the selection of the functional form g. The simplest possible functional form that can be assumed for g is the identity function ($g(x) = x$), in the spirit of Rajan and Zingales (1998). This implies that the model [1] becomes:

$$REAL_{cj} = X_{cj}\beta + \delta B_j POL_c + \eta_c + \eta_j + \varepsilon_{cj} \qquad [2]$$

where X stands for a vector of additional controls (which can include other policies and institutions interacted with B), the ηs represent country and industry fixed effects (estimated by including the corresponding one-dimensional dummies in the specification), ε is the standard error term and β and δ are parameters to be estimated. The parameter of interest is δ. The sign of δ provides an indication of the direction of direct demand effects. For the average industry, it is then possible to derive a quantitative estimate of the direct demand effect of the policy by simply multiplying δ by the average value of B, if it is further assumed that there are no direct effects in a hypothetical industry whose benchmark measure B would be equal to 0. Quantitative estimates presented in this paper (including the tables below) are based on this assumption. The advantage of this presentation style is that estimates with different benchmarks can be directly compared.

The choice of the appropriate benchmark measure obviously depends on the policy variable of interest. In the case of dismissal regulations, a standard choice in the literature is to US job turnover or US worker reallocation rates (see Micco and Pages, 2006, Haltiwanger *et al.*, 2008, and Cingano *et al.*, 2010). The idea is that worker reallocation rates measures the propensity of an industry to adjust on the external labour market and, given the low level of employment protection in the United States, US rates will be informative on what this propensity would be in the absence of regulation. As argued by Bassanini *et al.* (2009), however, in the case of regulation for individual and collective dismissals, the US distribution of dismissals might provide an alternative and more appropriate benchmark measure for the intensity of the constraint imposed by regulation on staffing changes. Firing restrictions are in fact unlikely to be binding in industries where voluntary quits are frequent and dismissals rare (such as hotels and restaurants). Indeed, in these industries natural attrition of staff might suffice to make the required staff adjustments. However, not all employer-induced separations result in dismissals, particularly in countries where employment is at-will and wages can be renegotiated downwards by the employer, such as in the United States, which suggests that a benchmark based on gross reallocation rates might be preferable. Another potential problem that concerns both the US dismissal and reallocation rates is that the composition of industries in terms of more disaggregate sub-industries may differ between the United States and the other countries in the sample. In addition, US rates might be affected by specific institutional features of the US economy. 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. This suggests checking results by using benchmarks based on both worker reallocation and dismissal rates.

The theoretical discussion of the previous section suggests that UBs should have stronger direct demand-side effects on gross worker flows, be they positive or negative, in industries that are more naturally exposed to productivity shocks requiring workforce adjustments and/or have a greater tendency to experiment with new recruits. It can be argued that the cross-industry distribution of gross worker flows is closely associated with the frequency of idiosyncratic productivity shocks on businesses and the need of experimenting with new recruits. Therefore, worker reallocation rates by industry in the United States – that is the country with the lowest benefit generosity (see the appendix) – appear to be a reliable measure of workforce adjustment needs in the absence of UBs. As an alternative benchmark, following Bassanini and Venn (2007), we will consider firm turnover rates – that are likely to capture the riskiness of business activities in each industry – from a country with low entry barriers such as the UK (see the next section for more details about the data).

In the case of the minimum wage, we consider two alternative identifying assumptions, derived from the theoretical arguments underlined above. On the one hand, minimum wages are particularly likely to prevent downward adjustment of wages for workers that are paid the minimum wage or only slightly more. As a consequence, industries that, because of their technological characteristics, are more heavily reliant on low-wage labour are likely to be more affected by any change in the minimum wage. Following Bassanini and Venn (2007), in order to reduce bias due to the possible relationship between minimum wages and the distribution of low-wage employment, the incidence of low-wage workers by industry in the United Kingdom prior to the introduction of statutory minimum wages in 1999 – when there was virtually no floor on wages, except for constraints imposed by collective bargaining – is used as an indicator of the propensity of industries to employ low-wage labour. Alternatively, as done for UBs, it can be argued that the effects of minimum wages, be it positive or negative, is likely to be larger in industries where gross worker flows tend to be larger, since greater flows are related to the frequency of idiosyncratic productivity shocks on businesses and the selectivity of firm recruitment policies.

Even though the choice of the benchmark intuitively depends on the policy variable, Micco and Pages (2006) provide a formal (S,s)-type model with constant adjustment hazards à la Calvo (1983) showing that adjustment costs have a greater impact on labour reallocation in industries with a greater natural propensity to reallocate labour. The reason being that the greater the need to adjust employment, the greater the constraint imposed by adjustment costs. The same holds also under the alternative assumption of quadratic adjustment costs, due the equivalence between constant-hazard models and models with quadratic adjustment costs (see e.g. Caballero and Engel, 1993, for a discussion). In Micco and Pages' model the presence of institutions can only increase adjustment costs, an assumption that does not appear to be consensual in the literature as regards certain policies such as unemployment benefits. Nevertheless, under that assumption, one can use an estimate of the reallocation rates that would occur in the absence of adjustment costs as benchmark for any policy *POL* which is likely to have a significant impact on worker flows through its impact on labour demand (see Garnero, 2010, for a formal proof). In turn, this would justify the use of reallocation rates in countries that can be thought to be characterised by relatively lax institutions and therefore low adjustment costs, such as the United States or the United Kingdom, for all the three labour market institutions discussed above.[5]

The standard way of choosing *B* on the basis of the distribution of a suitable variable across industries in a given benchmark country, where *POL* is assumed to be close to zero, can, however, be problematic. First, the composition of industries in terms of more disaggregate sub-industries may differ between the benchmark country and other countries in the sample. Second, the chosen variable might be affected by

5 . The advantage of using the same benchmark for several policies is that the demand effect of these policies can be simultaneously estimated. The disadvantage is, of course, that the effect of omitted institutions affecting adjustment costs would not be controlled for in the regressions and would bias coefficient estimates. For this reason, comparing results with alternative benchmarks is key.

specific institutional features of the benchmark country other than *POL*. This suggests that it is always desirable to experiment with different benchmark measures based on different benchmark countries, as done in this paper. However, Ciccone and Papaioannou (2007) 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 low-*POL* countries than in high-*POL* countries for reasons unrelated to the policy variable, then one might incorrectly attribute the cross-country differences in the inter-industry distribution of reallocation rates to an effect of *POL* on the absolute magnitude of gross flows. To circumvent the problem, Ciccone and Papaioannou (2007) suggest constructing the predicted notional benchmark in the absence of POL on the basis of available data for *all* countries. In practice, this implies two steps. In the first one, the following specification is fitted:

$$B_{cj} = \lambda_j POL_c + \eta_c + \eta_j + u_{cj}$$

where u is a standard error term and λ is an industry-specific parameter to be estimated that captures the way in which B changes as the result of changes in *POL*. In the second step, estimated industry fixed effects from this specification are used to replace B in [2]. Cingano *et al.* (2010) apply this methodology as baseline in their paper on EP and job flows. This methodology is used here as a sensitivity check for the analysis of the impact of both employment protection and unemployment benefits.

We use a cross-section of average data in these difference-in-difference exercises because annual data on worker flows appear to suffer from a large noise component due to the fact that the industry dimension is not included in the sampling design of labour force surveys, and many institutional variables need to be replaced by a surrogate measure in order to obtain long time series (see next section). This is the case, for instance, for both EP and UB generosity: in the former case, additional restrictions for collective dismissals are unavailable prior to 1998 while in the latter gross replacement rates must be used instead of net replacement rates for time-series starting before 2001. Nevertheless, as a sensitivity analysis, interactions between policy and benchmark measures are estimated using also time-series variation on annual data for the period 1995-2007. The estimated specification is in this case:

$$REAL_{cjt} = X_{cjt}\beta + \delta B_j POL_{ct} + \eta_{ct} + \eta_{jt} + \varepsilon_{cjt}$$

where the ηs represent now country-by-time and industry-by-time fixed effects.

Time-series regressions

The disadvantage of the difference-in-difference approach outlined above is that it might be difficult to derive the aggregate effect of those policies that are likely to impact worker flows by affecting both demand and supply simultaneously, and can be assumed to be homogeneous across industries, or where general-equilibrium effects can offset direct (partial-equilibrium) demand effects (such as in the case of unemployment benefits). To visualize this problem, let us denote the sum of hirings and separations with Y and average employment with N. The aggregate reallocation rate *AREAL* in country c can be written as the cross-industry weighted average of reallocation rates *REAL*, with the employment share n of each industry j as weight:

$$AREAL_c = \frac{Y_{c.}}{N_{c.}} = \sum_j \frac{Y_{cj}}{N_{cj}} \frac{N_{cj}}{N_{c.}} = \sum_j REAL_{cj} n_{cj}$$

where the dot denotes aggregation over the industry dimension for Y and N. Let us now suppose that industries can be divided in two groups, binding b and non-binding nb, and that the effect of the policy POL differs between the two groups. It follows that the impact of a policy variation can be written as:

$$\frac{dAREAL}{dPOL} = n_b \frac{\partial REAL_b}{\partial POL} + n_{nb} \frac{\partial REAL_{nb}}{\partial POL} + REAL_b \frac{\partial n_b}{\partial POL} + REAL_{nb} \frac{\partial n_{nb}}{\partial POL}$$

which, taking into account that $n_{nb} = 1 - n_b$, yields

$$\frac{dAREAL}{dPOL} = n_b \left(\frac{\partial REAL_b}{\partial POL} - \frac{\partial REAL_{nb}}{\partial POL} \right) + \frac{\partial REAL_{nb}}{\partial POL} + (REAL_b - REAL_{nb}) \frac{\partial n_b}{\partial POL}. \qquad [3]$$

The aggregate, general-equilibrium, effect of a policy variation depends on: *i*) the difference between the impacts on binding and non-binding industries; *ii*) the average impact in non-binding industries; and *iii*) the effect of the policy in reallocating labour towards (or away from) binding industries. Strictly speaking, the difference-in-difference procedure described above allows only the estimation of the former effect (represented by the first term on the right-hand side of [3]). While, in the case of the direct, partial-equilibrium demand effect, the two other terms can be assumed to be zero, this is not true in general equilibrium. The latter effect can be estimated directly by fitting the same specification as [2] with the logarithm of employment as dependent variable. In fact, due to the presence of country dummies, this is equivalent to estimating an equation with the logarithm of the employment share of each industry in aggregate employment as dependent variable. It turns out that none of the policies that are studied in this paper has any effect on the employment share of binding industries (see below); therefore we can approximate the last term on the right-hand side with zero even in general equilibrium. The middle term is, however, more problematic. For this reason, the analysis is complemented by a more standard cross-country/time-series analysis on annual data. More precisely, the following general specification is estimated:

$$REAL_{cjt} = X_{cjt}\beta + \gamma POL_{ct} + \delta(B_j - \overline{B})POL_{ct} + \eta_c + \eta_{jt} + \varepsilon_{cjt} \qquad [4]$$

where B has been demeaned so that γ captures the general-equilibrium effect of the policy POL for the average industry (a bar over a variable indicates its global sample mean). This cross-country/time-series analysis, however, has the disadvantage of being based on more noisy data and short time series (see above). Nevertheless one can draw relatively robust conclusions from the consistency of results from difference-in-difference and cross-country/time-series experiments. In fact, if general-equilibrium effects, over and above direct, partial-equilibrium effects, are minor, one would expect the estimate of γ to be close to that of δB obtained by estimating equation [2].

In the case of product market regulation, however, the relevant provisions are also industry-specific (see next section). A standard cross-country/cross-industry/time-series regression approach, in which an homogenous coefficient to product market regulation and including country-by-time dummies to control for aggregate institutions as equation [4] above, appears the preferable alternative to us.

Individual wage regressions

In this paper, we also estimate average wage premia to job changes in certain European countries, using individual data, with the purpose, then, to assess distributional consequences of specific labour market reforms affecting worker flows. We fit the following specification to the data:

$$\log w_{icjt} = X_{icjt}\beta + \gamma m_{icjt} + \eta_{ct} + \eta_{cj} + \eta_i + \varepsilon_{icjt}$$

where w is the gross hourly wage of worker i in country c and industry j at time t, m is a variable (hereafter called "counter") that increases by 1 each time a worker changes employer, X stands for a vector of additional controls, the ηs represent individual, country-by-time and country-by-industry fixed effects (estimated by including the corresponding one or two-dimensional dummies in the specification), ε is the standard error term and β and γ are parameters to be estimated. The latter represents the wage premium to job change. Wage premia are also estimated for voluntary and involuntary separations, as well as for job-to-job and job-to-jobless transitions. In these cases, two counters m, one for each type of transition, are simultaneously included in the same specification. The estimated penalty to job-to-jobless transitions refers to foregone earnings at re-employment but does not include foregone earnings during job-search.

In order to avoid that changes in educational attainment confound the estimate of the wage premium, when an individual increases his/her education level, a new individual fixed effect is included. In such a way comparisons are made only within-educational attainment levels. The same treatment applies to individuals with missing observations, for whom a new fixed effect is generated for all years above the one with missing values. As the main interest is on the effect of different types of separations, industry affiliation is based on that of the previous employer. Other controls are age classes, a public sector dummy and a temporary contract dummy. A sensitivity analysis including tenure classes, a dummy for change of industry and previous unemployment experience yields qualitatively similar results.

Finally, the same difference-in-difference strategy as above is applied to assess how labour market institutions affect wage premia/penalties to job change. In this case, the estimated specification is:

$$\log w_{icjt} = X_{icjt}\beta + \gamma_i m_{icjt} + \gamma_j m_{icjt} + \delta B_j POL_c m_{icjt} + \eta_{ct} + \eta_{cj} + \eta_i + \varepsilon_{icjt}$$

Where γ_i and γ_j are now country-specific and industry-specific parameters, respectively (estimated by including interactions between job-change counters and relevant dummies). The parameter of interest is again δ. As above, assuming that the direct demand-side impact of *POL* on the wage premium to job change is larger in policy-binding industries than in other industries, the sign of δ provides an indication of the direction of this effect.

4. The data

We have data 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). We aggregate worker flow data at the industry level from individual microdata drawn from European and national labour force surveys (LFS hereafter), harmonised using large cross-country comparable national-account-based industry databases such as the OECD STAN database and EU KLEMS. In practice, hiring rates at the industry level are obtained from job tenure data in labour force surveys, while separation rates are obtained by subtracting net employment growth rates from hiring rates, the former derived from STAN and KLEMS. More precisely, the ratio of annual hirings to employment is computed from job tenure data available in LFS. Workers with tenure shorter than one year are unambiguously new hires according to the definition adopted for this paper (see Section 1 above). Separations are then obtained as the difference between hirings and employment changes between two years. As different waves of labour force surveys are hard to compare at disaggregate industry level because the industry dimension is not taken into account in the LFS sampling design, employment level and growth data at the industry-level from EUKLEMS or STAN are used for all countries where they are available (all countries except Iceland and Slovenia).[6] Hirings and separations are therefore re-scaled on the basis of the discrepancies between LFS and national

6 . All results presented in this paper are robust to the elimination of these countries.

accounts. Then final reallocation rates are obtained by dividing hirings or separations for the average of employment levels of the two consecutive years, which transitions refer to.[7]

For each industry, rates for other types of transitions are obtained by multiplying the hiring or separation rate of that industry, as appropriate, by the corresponding share of each type of transition in total hirings or separations. An additional consistency rule, requiring that job-to-job hirings and separations be equal at the level of the whole economy, is also imposed. As available data allow also a more disaggregate analysis by gender, education and age classes, the same re-scaling method is used to compute hiring and separation rates by education, gender and age classes.

Worker flow data constructed in this way are in principle available at the annual level between 1995 and 2007, even though the window in which data are available might be shorter for certain countries. However, industry-level annual data constructed from LFS are often very imprecise because of the sampling-design issue explained above. For this reason, the main difference-in-difference estimation sample is limited to 2000-2007 averaged data. 2000-2007 average rates by country, adjusted for industry composition, are presented in the Appendix. The Appendix also reports main descriptive statistics for the 1995-2007 sample of time-varying annual data.

Labour and product market institutions come from OECD sources and are detailed in the Appendix. We consider mainly two indicators of EP stringency: employment protection for regular workers, including collective dismissals (EPRC) and employment protection for temporary workers (EPT). The former is obtained as a weighted average of the indicator of regulation for individual dismissals (EPR) and additional regulation for collective dismissals (EPC), with weights 5/7 and 2/7, respectively. The latter indicator is available only since 1998, therefore we will use EPR only 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.[8]

We prefer to measure UB generosity by using the UB *net* replacement rate, excluding social assistance and expressed in percentage of the wage in the previous job. Average net benefit replacement rates are a synthetic measure of both the level and duration of take-home benefits. In fact, replacement rates are averaged across different earnings level, family situations and unemployment durations up to 5 years. The reason for excluding social assistance is that it is means-tested and usually not conditional on searching for jobs, thereby representing the ultimate safety net and being unlikely to have a labour demand effect on flows. Net replacement rates are available only since 2001. Therefore, we will use gross rates only as a surrogate of net rates in the time-series analysis on the 1995-2007 sample.

Minimum wages are measured as the ratio of statutory minimum wage to median wage, in percent. Other covariates concerning labour market institutions are detailed in the Appendix.

As regards product market regulation we use two alternative variables. On the one hand, we use the OECD overall economy-wide indicator of anti-competive product market regulation. This indicator captures, in principle all aspects of anti-competitive economic and administrative regulation. It is however available only in 1998, 2003 and 2008. For this reason, we use it only in the analysis based on the cross-sectional 2000-2007 s ample, using 2003 indicators.[9] Nevertheless, insofar as its subcomponents are usually aggregated on the basis of industry-level information (see Woelfl *et al.*, 2009), care must be taken in interpreting this variable, which is mainly included only as a control. On the other hand, OECD

7 . See Bassanini and Marianna (2009) for more details.

8. In the case of Slovenia, only EP data for 1998 are available.

9. 2008 for Slovenia, due to data availability.

industry-specific indicators of the degree of stringency of anti-competitive regulation are available for five non-manufacturing industries at our level of aggregation (energy, retail trade, transports, communications, and professional services) for all countries. One option would be restricting the attention to these industries only and perform a standard time-series analysis using data varying by countries, industries and years. However, this would result in an excessively small sample, given the short available time-series for the worker reallocation data. By contrast, after the implementation of the European Single-Market Programme (SMP) in the early 1990s, before-enlargement European Union countries share essentially the same regulations in manufacturing, including the same trade barriers, except for economy-wide provisions applying to all industries (such as administrative barriers to start-ups). As suggested by Bassanini and Brunello (2010), it is therefore possible to enlarge the sample to manufacturing industries for these countries, by setting regulation equal to an arbitrary value in manufacturing, provided that industry-by-time and country-by-time dummies are included, the former to control for industry-specific regulations applying to all countries in the sample (such as trade barriers) and the latter for country-specific regulation applying to all industries, such as main administrative barriers to start-ups.

All individual data are from the European Community Household Panel. Wages are gross hourly wages including overtime pay and hours. The only exception are the data used to construct the UK share of low-wage workers, which is the share of wage and salary employees working at least 30 hours per week with gross monthly wages less than two-thirds of the median wage of all employees, averaged over 1994-99.

5. Results

Difference-in-difference estimates

Table 1 presents baseline results from estimating equation [2] using average US reallocation rates as benchmark measure to classify industries. As discussed above, this benchmark can be considered suitable for both EP and UB generosity. All aggregate variables are interacted with benchmark measures and reported coefficients are the product of the average value of the benchmark and the estimated coefficient of the interaction – that is δB obtained in equation [2]. In other words, the table presents the estimated average impact of selected policies and institutions, obtained under the assumption that these policies would have no direct effect in an hypothetical industry whose benchmark measure – the US worker reallocation rate in this case – would be equal to zero. For example, under this assumption, a one point increase in the index of EP stringency for regular workers – roughly corresponding to two-thirds of the difference between the OECD average and the country with the lowest value of the EP index (United States)[10] – appears to reduce, on average, both total and excess worker reallocation by between 5.2 and 6.7 percentage points, depending on which confounding factors are included in the specification. Similarly, the same variation in EP stringency is estimated to reduce separation rates by between 3.1 and 3.6 percentage points and hiring rates by between 2.1 and 3 percentage points. And a ten-percentage-point increase in average net replacement rate – a large reform from an historical perspective, roughly corresponding to two standard deviations of the time-series variation of the indicator observed over the period (that is obtained netting out cross-sectional variation) or a 25% change from the OECD average – appears to increase, on average, both total and excess worker reallocation by about 1 percentage point.

EP for regular contracts and UB generosity appear to be strongly associated with inter-industry differences of gross reallocation rates, no matter the measure of reallocation retained (except for hirings in

10. One point corresponds also to 1.5 standard deviations in the cross-country distribution of the EP index for regular contracts (including additional restrictions on collective dismissals), as well as to one third of the difference between Portugal (the country with most stringent 2000-2007 average of the index) and the United States.

the case of the replacement rate) and no matter the other institutions included. All specifications control for the economy-wide index of anti-competitive product market regulation, which is important because this index is closely correlated to EP indexes. Nevertheless, as already mentioned, despite the fact that it attracts a negative and significant coefficient, which might point to a negative effect of product market regulation on reallocation,[11] caution is required in interpreting this variable, since it is partially based on aggregation of industry-specific aspects of regulation. By contrast, all the estimated coefficients of other policy variables appear by and large insignificant (except in few specifications with several controls, where significant estimates might simply be the result of multicollinearity). Moreover, this is true also for variables capturing collective bargaining institutions. This is reassuring insofar as it implies that the effect of other institutional variables, if any, is orthogonal to the natural propensity to reallocation of each industry, as measured by the US rates, consistent with the identification assumptions outlined in the previous subsection.[12]

In all specifications of Table 1, we include the share of workers on temporary contracts and the share of workers aged 35 years or less, derived from LFS data. The presented specification results from sequential elimination of other covariates from an extended model. These covariates include dummies for years included in the sample, the share of self-employment, the share of workers aged 55 years or more, the share of women, the share of workers with less than secondary education and the share of workers with secondary education. All excluded variables were insignificant and did not affect estimates of the main variables of interest. Controlling for demographic characteristics appears desirable insofar as EP could simply shift employment of high-reallocation worker groups (such as youth) from one industry to another without any net increase in the aggregate employment rate of these groups. Suppose for the sake of the exposition that an EP reform increases employment of youth in high reallocation industries and reduces employment of youth in low-reallocation industries, without changing individual employment and mobility hazards. Without controlling for the share of each group in each industry, one would attribute the difference between high and low-reallocation industries to the effect of EP on average reallocation rates, while in fact this would simply be due to offsetting changes in the demographic composition of the two industries (in a sense, the treated and the control group would be affected by the policy reform in opposite directions). In addition, we want to control for the share of temporary contracts in order to obtain estimates that are close to the effect of EP on the reallocation of workers on open-ended contracts. This is key from a policy perspective: there is in fact large evidence in the literature that high rates of reallocation due to extensive use of temporary contracts yield inefficient outcomes in terms of productivity growth (see *e.g.* Bentolila *et al*., 2008, Bassanini *et al*., 2009).

Estimated coefficients of EP and UB generosity also appear reasonably robust to elimination of countries one-by-one from the sample, as shown in Figure 1, which reports results for preferred specifications regarding total and excess reallocation (Columns 1 and 5 of Panel A in Table 1). The only partial exception is the estimated coefficient of net replacement rates in the equation for total reallocation, which becomes almost twice as large as the baseline estimate upon exclusion of the United States but become insignificant upon exclusion of either Hungary or the Slovak republic. However, dropping all these three outliers simultaneously make estimated coefficients significant again and close to baseline estimates.

11. Note that, to the extent that product market regulation affects entry and exit of firms, and job creation and destruction by entry and exit account for an important share of total gross worker flows, US reallocation might represent a good benchmark also for this variable.

12. Importantly, note that this does not imply that these other institutions have no effect on worker reallocation.

Table 1. **Institutions and gross worker reallocation: baseline benchmark**

Panel A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benchmark:	US real	US real	US real	US real	US real	US real	US real	US real
Dep. Variable:	Real	Real	Real	Real	Excess	Excess	Excess	Excess
EPRC	-5.71***	-5.53***	-5.27***	-6.56***	-6.19***	-5.95***	-5.67***	-6.69***
	(1.14)	(1.18)	(1.18)	(1.15)	(1.20)	(1.20)	(1.22)	(1.08)
PMR	-5.50***	-5.17**	-6.35***	-5.22**	-5.52**	-5.07**	-6.31***	-5.51***
	(2.11)	(2.22)	(2.17)	(2.03)	(2.14)	(2.29)	(2.11)	(2.05)
Av. net repl. rate (%)	0.07**	0.07**	0.09**	0.07*	0.09***	0.10***	0.11***	0.11***
	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)
Tax wedge		-0.05		0.02		-0.06		0.05
		(0.09)		(0.09)		(0.11)		(0.10)
ALMP/U / GDP/POP			-7.33	-13.03*			-6.95	-11.63*
			(5.97)	(6.98)			(6.25)	(6.75)
Coll. barg. cov.				-0.03				-0.09*
				(0.05)				(0.05)
Temporary (%)	0.71***	0.71***	0.72***	0.68***	0.66***	0.66***	0.67***	0.63***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.07)
Age: 15-24 (%)	0.39***	0.39***	0.38***	0.43***	0.42***	0.41***	0.41***	0.46***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
Age: 25-34 (%)	0.24***	0.25***	0.25***	0.24***	0.24***	0.25***	0.26***	0.25***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
Observations	521	521	486	486	521	521	486	486
R-squared	0.927	0.927	0.921	0.923	0.918	0.919	0.918	0.920
Cou / ind dums	yes	yes	yes	yes	yes	yes	yes	yes
Corporatism dummies	no	no	no	yes	no	no	no	yes

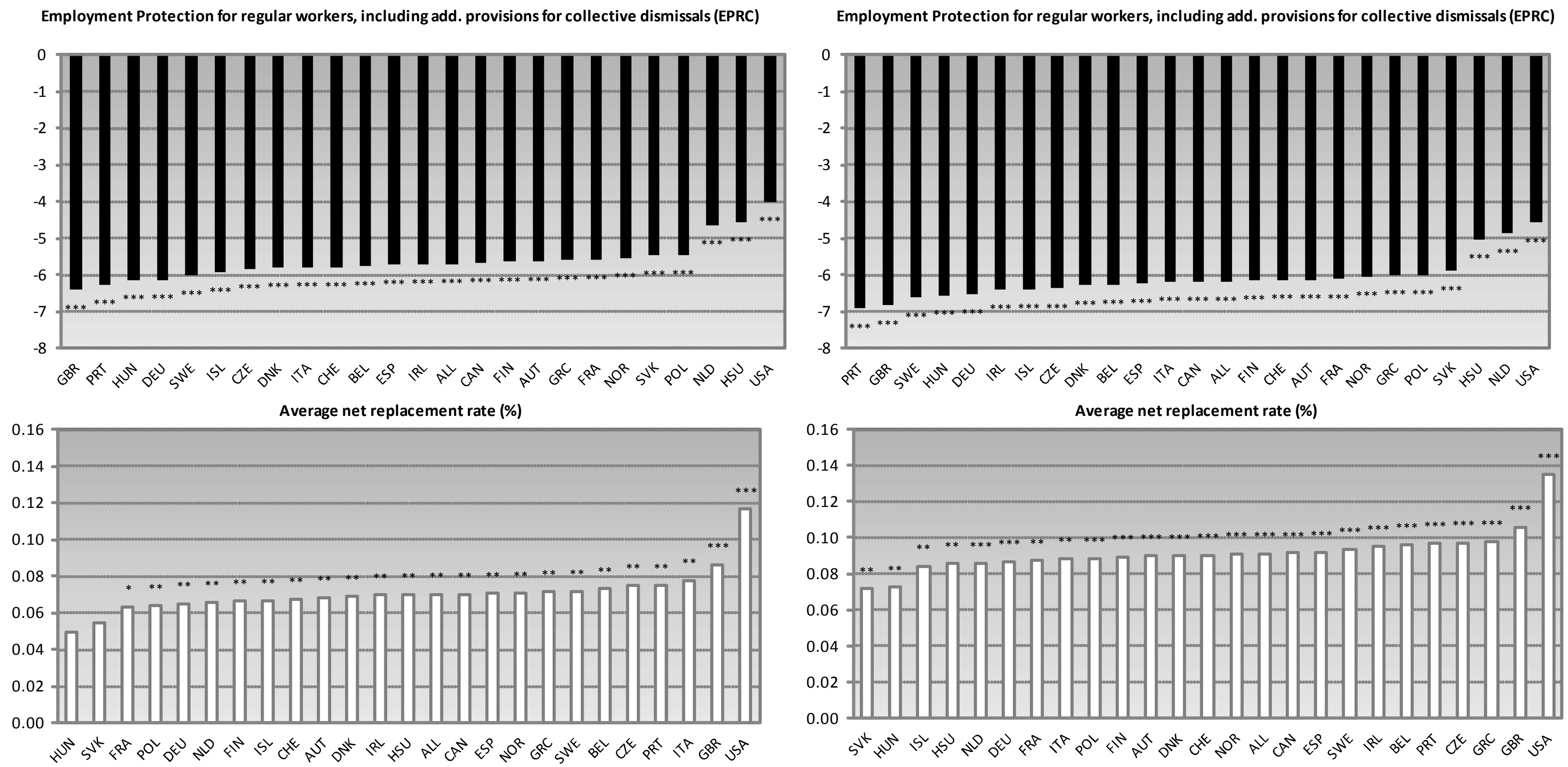
Panel B.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benchmark:	US real	US real	US real	US real	US real	US real	US real	US real
Dep. Variable:	SR	SR	SR	SR	HR	HR	HR	HR
EPRC	-3.10***	-2.98***	-3.11***	-3.63***	-2.61***	-2.55***	-2.16***	-2.94***
	(0.68)	(0.69)	(0.71)	(0.69)	(0.55)	(0.57)	(0.57)	(0.60)
PMR	-2.02	-1.79	-2.52*	-1.98	-3.49***	-3.38***	-3.83***	-3.24***
	(1.37)	(1.45)	(1.34)	(1.29)	(0.86)	(0.90)	(0.94)	(0.91)
Av. net repl. rate (%)	0.04**	0.04**	0.05**	0.05**	0.03*	0.03*	0.04**	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Tax wedge		-0.03		0.01		-0.01		0.01
		(0.06)		(0.06)		(0.04)		(0.04)
ALMP/U / GDP/POP			-2.06	-4.70			-5.26*	-8.33**
			(3.18)	(3.60)			(3.13)	(3.84)
Coll. barg. cov.				-0.04				0.01
				(0.03)				(0.03)
Temporary (%)	0.31***	0.31***	0.34***	0.32***	0.39***	0.39***	0.39***	0.37***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Age: 15-24 (%)	0.18***	0.18***	0.15***	0.18***	0.21***	0.21***	0.23***	0.26***
	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)
Age: 25-34 (%)	0.07	0.07*	0.07	0.07	0.17***	0.17***	0.18***	0.18***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.03)	(0.03)	(0.04)	(0.03)
Observations	521	521	486	486	521	521	486	486
R-squared	0.875	0.875	0.869	0.872	0.937	0.937	0.931	0.933
Cou / ind dums	yes	yes	yes	yes	yes	yes	yes	yes
Corporatism dummies	no	no	no	yes	no	no	no	yes

Notes: OLS estimates. Real: total reallocation rate. Excess: excess reallocation rate. SR: separation rate. HR: hiring rate. US Real: US reallocation rate, used as benchmark. EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). PMR: economy-wide index of anti-competitive product market regulation. Av. net repl. rate (5y): average net replacement rate over a 5-year unemployment period (in percentage). Tax wedge: average labour tax wedge for couples (in percentage). ALMP/U / GDP/Pop: ALMP spending per unemployed as a fraction of GDP per capita. Coll. Barg. Cov.: percentage of employees covered by collective agreements (including administrative extension). Temporary: share of temporary workers. Age: 15-24 and Age: 25-34: share of workers in the specified age class. Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Corporatism dummies: dummies for high and intermediate corporatism, interacted with the benchmark. Cou/ind dums: industry dummies and country dummies. Robust standard errors in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

Figure 1. **Sensitivity analysis when countries are excluded one-by-one from the sample, US reallocation as benchmark**

Panel A. **Worker reallocation rates** | Panel B. **Excess worker reallocation rates**



Notes: Coefficient estimates obtained by excluding indicated countries one-by-one. ALL: baseline estimate, excluding no country (cf. Table 3.A1.3, Panel A, Columns 1 and 5). HSU: excluding Hungary, the Slovak Republic and the United States. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

As argued above, benchmarks based on one specific country might be misleading, insofar as they might be affected by idiosyncratic country factors. As a sensitivity analysis, in Table 2, UK reallocation rates are used as an alternative benchmark. This appears particularly suitable in the case of EP, given that the United Kingdom is the country with the second lowest value of the EP index. Alternatively Table 2 presents estimates obtained with the methodology of Ciccone and Papaioannou (2007; see Section 3 above), that is based on the projected level of reallocation at, alternatively, zero EP for regular contract and zero replacement rate. No major differences emerge with respect to baseline estimates.

Table 2. **Institutions and gross worker reallocation: other benchmarks based on worker reallocation**

Panel A.

	(1)	(2)	(3)	(4)	(5)	(6)
Benchmark:	UK real	Ciccone eprc	Ciccone nrr	UK real	Ciccone eprc	Ciccone nrr
Dep. Variable:	Real	Real	Real	Excess	Excess	Excess
EPRC	-6.37***	-6.67***	-5.72***	-6.95***	-7.07***	-6.04***
	(1.37)	(1.20)	(1.28)	(1.53)	(1.28)	(1.33)
PMR	-7.30***	-6.98***	-6.19**	-6.55***	-6.77***	-5.69**
	(2.08)	(2.07)	(2.55)	(2.40)	(2.16)	(2.63)
Av. net repl. rate (%)	0.10***	0.07**	0.07*	0.12***	0.09***	0.08**
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Temporary (%)	0.71***	0.73***	0.72***	0.66***	0.68***	0.67***
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)
Age: 15-24 (%)	0.39***	0.37***	0.42***	0.43***	0.41***	0.46***
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)
Age: 25-34 (%)	0.24***	0.25***	0.24***	0.23***	0.24***	0.23***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)
Observations	521	521	521	521	521	521
R-squared	0.927	0.928	0.926	0.917	0.919	0.916
Cou / ind dums	yes	yes	yes	yes	yes	yes

Panel B.

	(1)	(2)	(3)	(4)	(5)	(6)
Benchmark:	UK real	Ciccone eprc	Ciccone nrr	UK real	Ciccone eprc	Ciccone nrr
Dep. Variable:	SR	SR	SR	HR	HR	HR
EPRC	-3.80***	-3.80***	-3.34***	-2.57***	-2.87***	-2.39***
	(0.80)	(0.71)	(0.77)	(0.69)	(0.60)	(0.61)
PMR	-3.17**	-2.98**	-2.56	-4.13***	-4.00***	-3.63***
	(1.38)	(1.32)	(1.62)	(0.89)	(0.89)	(1.04)
Av. net repl. rate (%)	0.06***	0.05**	0.05**	0.04**	0.03	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Temporary (%)	0.32***	0.33***	0.33***	0.38***	0.40***	0.39***
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Age: 15-24 (%)	0.17***	0.16***	0.19***	0.22***	0.21***	0.23***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Age: 25-34 (%)	0.07*	0.07*	0.07	0.17***	0.17***	0.17***
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Observations	521	521	521	521	521	521
R-squared	0.877	0.878	0.876	0.935	0.936	0.934
Cou / ind dums	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. Real: total reallocation rate. Excess: excess reallocation rate. SR: separation rate. HR: hiring rate. EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). PMR: economy-wide index of anti-competitive product market regulation. Av. net repl. rate (5y): average net replacement rate over a 5-year unemployment period (in percentage). Temporary: share of temporary workers. Age: 15-24 and Age: 25-34: share of workers in the specified age class. UK Real: UK reallocation rate, used as benchmark. Ciccone eprc and Ciccone nrr are predicted benchmarks at zero EPRC and zero average replacement rate, respectively, obtained using the methodology of Ciccone and Papaioannou (2007). Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Cou/ind dums: industry dummies and country dummies. Robust standard errors in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

As discussed in Section 3 above, in the case of regulation for individual and collective dismissals, the US distribution of dismissals might provide a more appropriate benchmark measure for the intensity of the constraint imposed by regulation on staffing changes. Firing restrictions are in fact unlikely to be binding in industries where voluntary quits are frequent and dismissals rare (such as hotels and restaurants). Indeed, in these industries natural attrition of staff might suffice to make the required staff adjustments. For this reason, the US dismissal rate is used as alternative benchmark in Table 3. Interestingly, only the coefficient of EP appears significant – albeit somewhat lower, and insignificant in the case of excess reallocation, consistent with the a-priori identifying assumption that the impact of other institutions is likely to be the same in high and low dismissal industries. Similarly, theory suggests that UB generosity could have a greater effect in industries where business activities are riskier. Insofar as firm-turnover rates are likely to capture business volatility, we use UK firm turnover rates from Hijzen *et al.* (2007) as an alternative benchmark. Indeed, even if the United Kingdom is not the country with the lowest benefits, it is likely to provide the most adequate firm-turnover benchmark measure since firm turnover is mainly determined by entry regulations, and the United Kingdom is the OECD country where these regulations are less stringent (see Woelfl *et al.*, 2009). As before, one would expect that this benchmark would be relevant only for UB generosity and that, when this variable is used as a benchmark, EP would turn out insignificant. Reassuringly, this is indeed what happens. By contrast, baseline estimates for the average replacement rate appears, by and large, confirmed.

Statutory minimum wages exist in a subset of OECD countries only. To preserve sample size, minimum wages were not included in Table 1, even though, as discussed in Section 3, US reallocation rates can be used as benchmark even in the case of the minimum wage. Table 4 fills this gap by estimating the baseline specification (Columns 1 and 5 in Table 1) augmented for average minimum wages for the countries for which statutory minima are available.[13] The latter are measured as the percentage ratio of the gross statutory minimum wage to median wage. As shown in the table, estimated coefficients are always insignificantly different from zero. By contrast, estimates for the other institutional variables (not shown in the table) remain, by and large, close to those presented above and significant, despite the smaller sample, which provides another reassuring confirmation of previous findings.

Alternatively, one could use the incidence of low-wage workers by industry in the United Kingdom prior to the introduction of the minimum wage in that country in 1999, when there was virtually no floor on wages, except for constraints imposed by collective bargaining. In fact, this variable could proxy the propensity of industries to employ low-wage workers in the absence of a minimum wage, and it seems natural to assume that any effect of the minimum wage will be greater in industries that employ more low-wage workers. Data on the share of low-wage labour are obtained from the British Household Panel Survey (BHPS) component of the European Community Household Panel (ECHP; see above), and are available for a coarser partition of industries, which leads to a reduction in sample size by about one half. Nevertheless, estimates obtained by using this benchmark confirm, by and large, results obtained by using the US worker reallocation rate as benchmark. Moreover, elimination of countries one-by-one (not shown in the table) never makes coefficients significant at the 5% level.

13. The sample includes Belgium, Canada, Czech republic, France, Greece, Hungary, Ireland, the Netherlands, Poland, Portugal, Slovak republic, Spain, the United Kingdom and the United States.

Table 3. **Institutions and gross worker reallocation: other benchmarks**

Panel A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benchmark:	US layoff	US layoff	UK firm-t	UK firm-t	US layoff	US layoff	UK firm-t	UK firm-t
Dep. Variable:	Real	Real	Real	Real	Excess	Excess	Excess	Excess
EPRC	-2.77**	-2.63***	-2.18*		-1.61	-1.31	-2.21	
	(1.19)	(0.95)	(1.31)		(1.14)	(0.92)	(1.39)	
PMR	1.02		-3.39		1.89		-3.54	
	(2.47)		(2.31)		(2.25)		(2.36)	
Av. net repl. rate (%)	0.00		0.06*	0.06*	-0.02		0.07*	0.07*
	(0.04)		(0.04)	(0.04)	(0.03)		(0.04)	(0.04)
Temporary (%)	0.51***	0.51***	0.57***	0.53***	0.45***	0.47***	0.52***	0.48***
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.06)	(0.07)
Age: 15-24 (%)	0.61***	0.62***	0.56***	0.60***	0.64***	0.65***	0.60***	0.64***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
Age: 25-34 (%)	0.20***	0.21***	0.20***	0.22***	0.19**	0.19**	0.20**	0.21***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)
Observations	521	541	521	521	521	541	521	521
R-squared	0.917	0.916	0.920	0.917	0.907	0.907	0.910	0.907
Cou / ind dums	yes	yes	yes	yes	yes	yes	yes	yes

Panel B.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benchmark	US layoff	US layoff	UK firm-t	UK firm-t	US layoff	US layoff	UK firm-t	UK firm-t
Dep. Variable	SR	SR	SR	SR	HR	HR	HR	HR
EPRC	-2.04**	-1.79***	-1.18		-0.74	-0.84*	-1.01	
	(0.81)	(0.62)	(0.80)		(0.53)	(0.45)	(0.63)	
PMR	0.68		-1.17		0.34		-2.21**	
	(1.66)		(1.50)		(0.94)		(0.97)	
Av. net repl. rate (%)	0.02		0.04*	0.04*	-0.01		0.02	0.03
	(0.02)		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)
Temporary (%)	0.22***	0.22***	0.25***	0.23***	0.29***	0.29***	0.32***	0.30***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Age: 15-24 (%)	0.29***	0.29***	0.26***	0.28***	0.32***	0.33***	0.30***	0.32***
	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Age: 25-34 (%)	0.05	0.06	0.06	0.06	0.15***	0.14***	0.15***	0.16***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)
Observations	521	541	521	521	521	541	521	521
R-squared	0.867	0.864	0.868	0.865	0.927	0.926	0.930	0.927
Cou / ind dums	yes	yes	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. Real: total reallocation rate. Excess: excess reallocation rate. SR: separation rate. HR: hiring rate. EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). PMR: economy-wide index of anti-competitive product market regulation. Av. net repl. rate (5y): average net replacement rate over a 5-year unemployment period (in percentage). Temporary: share of temporary workers. Age: 15-24 and Age: 25-34: share of workers in the specified age class. US layoff: US dismissal rate, used as benchmark. UK firm-t: UK employment-weighted firm-turnover rate, used as a benchmark. Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Cou/ind dums: industry dummies and country dummies. Robust standard errors in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

Table 4. **Minimum wage and gross worker reallocation**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benchmark:	US real	US real	US real	US real	UK low-wage share	UK low-wage share	UK low-wage share	UK low-wage share
Dep. variable:	Real	Excess	SR	HR	Real	Excess	SR	HR
Ratio min to med wage (%)	-0.15	-0.16	-0.13	-0.01	-0.02	-0.02	-0.03	0.01
	(0.17)	(0.17)	(0.10)	(0.08)	(0.06)	(0.05)	(0.03)	(0.03)
Observations	325	325	325	325	164	164	164	164
R-squared	0.923	0.919	0.862	0.937	0.937	0.936	0.877	0.957
Cou / ind dums	yes	yes	yes	yes	yes	yes	yes	yes
Share temporary	yes	yes	yes	yes	yes	yes	yes	yes
Age controls	yes	yes	yes	yes	yes	yes	yes	yes
Other institutions	yes	yes	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. Real: total reallocation rate. Excess: excess reallocation rate. SR: separation rate. HR: hiring rate. Ratio of min to med wage: ratio of statutory minimum wage to median wage (in percentage). Share temporary: share of temporary workers. Age controls: shares of workers aged 15-24 years and 25-34 years. US Real: US reallocation rate, used as benchmark. UK low-wage share: UK share of low-wage workers 1994-1999. Cou/ind dums: industry dummies and country dummies. Other institutions: index of employment protection for regular workers (including additional provisions for collective dismissals), economy-wide index of anti-competitive product market regulation, average net replacement rate over a 5-year unemployment period. Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Robust standard errors in parentheses.

One potential problem concerning estimates presented so far is that it has implicitly being assumed that the impact of any variable on worker reallocation is linear. 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, Boeri and Macis, 2010), it is correct only if the microeconomic process generating hirings and separations can be approximated by a linear probability model. However, this is not necessarily true, insofar as worker reallocation can vary by a factor of three across industries. In these conditions a logit or a probit model would be a more credible approximation of the probability of making a transition. For low transition hazards (as in most of the observations in the sample), this implies that the impact of any factor that has an effect on reallocation is approximately proportional to the level of reallocation prevailing when that factor is zero. This, however, has potentially serious consequences on the identification strategy adopted so far. To see why consider this example: suppose that equations [1] and subsequent equations in Section 3 are still correct except that the left-hand side term represents a latent variable, normally or logistically distributed, that would determine individual transitions when it takes value above a given threshold (as in a standard probit or logit model, see Wooldridge, 2002). Suppose also that one important aggregate variable, correlated with the institutions studied above, has been omitted in equation [2] that was estimated in previous tables using observed reallocation rates as dependent variable. Even if there is no reason to believe that the effect of this variable on the latent variable is different across industries, it cannot be safely assumed that the effect of this variable is controlled for by interacting aggregate institutions with the industry propensity to reallocate. In fact, it can easily be shown that this variable will have an impact that is approximately proportional to average reallocation rates in each industry. As a consequence, given the high cross-country correlation of industry distributions of reallocation rates, results in Table 1 might simply be due to the correlation of EP and UBs with the omitted factor and not to an impact of these institutions on worker reallocation.

The use of alternative benchmarks, such as US dismissal or UK firm-turnover rates, which are only mildly correlated with US or UK worker reallocation rates, provides some reassuring evidence that this potential problem is perhaps not very important in the analysis of this paper. However, to rule it out in a more rigorous way, one can estimate a generalised linear model (GLM), resulting from the aggregation of equation [1] when the latter is interpreted as referring to the individual latent propensity to make a transition. More precisely, the following analogous of equation [2] is fitted 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):[14]

$$E(REAL_{cj}) = G(X_{cj}\beta + \delta B_j POL_c + \eta_c + \eta_j) \qquad [5]$$

where G is the inverse-logit or probit function and *REAL* stands for either hiring or separation rates.[15] Table 5 shows estimated results. They appear globally consistent with linear estimates for UBs and EP, taking into account the relative inefficiency of GLM estimates. Indeed, the estimated effect of both EP and UB tend to be larger for separations, where it is significant or close to significance for both variables. In the case of EP, it is also significant or close to significance in the case of hirings. By contrast, the effect of product market regulation appears insignificant in all specifications and differently-signed in the case of hirings, consistent with the caution expressed above about the interpretation of this variable.

Table 5. **GLM estimates**

	(1)	(2)	(3)	(4)
Benchmark:	US real	US real	US real	US real
Function:	logit	probit	logit	probit
Dep. Variable:	SR	SR	HR	HR
EPRC	-10.78**	-6.64***	-6.29	-4.21*
	(4.34)	(2.49)	(4.09)	(2.25)
PMR	2.12	0.26	-5.77	-4.29
	(10.24)	(5.72)	(7.55)	(4.14)
Av. net repl. rate (%)	0.19	0.14**	0.09	0.07
	(0.13)	(0.07)	(0.12)	(0.07)
Temporary (%)	0.02***	0.01***	0.02***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
Age: 15-24 (%)	0.01***	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
Age: 25-34 (%)	0.01**	0.00**	0.02***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
Observations	497	497	497	497
Cou / ind dums	yes	yes	yes	yes

Notes: Quasi-maximum-likelihood generalised-linear estimates. Logit: inverse-logit model. Probit: inverse-probit model. SR: separation rate. HR: hiring rate. US Real: US reallocation rate, used as benchmark (benchmark country excluded). EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). PMR: economy-wide index of anti-competitive product market regulation. Av. net repl. rate (5y): average net replacement rate over a 5-year unemployment period (in percentage). Temporary: share of temporary workers. Age: 15-24 and Age: 25-34: share of workers in the specified age class. Aggregate variables multiplied by the benchmark. Cou/ind dums: industry dummies and country dummies. Robust standard errors in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

The next table looks at other dependent variables, and notably the share of temporary workers and the share of workers aged less than 35 years, whose effects were controlled for in Table 1. Specifications in Table 1 control for the share of temporary workers since the objective is to estimate reallocation for regular workers, which is likely to be the most efficient part of labour reallocation (see above). The additional advantage is that a model controlling for temporary contracts is simpler, since specific determinants of the latter need not be included. This is obviously not the case when the share of temporary contracts is the

14. Papke and Wooldridge (1996) show that QMLE estimators of this kind yield consistent estimates of equation [5] 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 also Wooldridge, 2002).

15. The benchmark country is also omitted to reduce further the risk of bias.

dependent variable. An obvious co-variate that must be included in this case is the stringency of regulation concerning fixed-term contracts and temporary help agencies. This is done in Column 1 of Table 6. As discussed in Section 2, these regulations cannot be seen in isolation but their impact is likely to be conditional on the stringency of regulation for regular contracts. For this reason, in Column 2, the ratio of the indexes for the two types of regulation is included rather than both regulations separately. Quite surprisingly, however, in both specifications, the share of temporary contracts appear to be larger in high-reallocation industries in countries where regulations for temporary contracts are more restrictive, which runs against theoretical intuitions presented in Section 2. Several reasons could perhaps explain this pattern. First, the share of temporary workers includes also employees under other types of contracts beyond those considered in the indicator; in particular, seasonal workers are included in this category and they represent the largest share of temporary workers in certain high-reallocation industries such as hotels and restaurants and food processing. Second, a different tax treatment applies to fixed-term contracts in some countries (Italy, for example), but no comparable indicators are available to be included as control. Third, and perhaps more important, the degree of enforcement might be particularly heterogeneous across countries as regards regulation for temporary contracts. In fact, enforcement of EP 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 to legislation on temporary contracts (particularly in the case of violations of hiring restrictions under such contract) are a much vaguer group. Summing up, these considerations suggest that the index of regulatory restrictions concerning temporary contracts is endogenous – that is, correlated to other factors that affect the dependent variable independently. Reassuringly, omitting this variable does not appear to affect estimated coefficients for the other variables of interest.

Table 6. **Other dependent variables**

	(1)	(2)	(3)	(4)	(5)	(6)
Benchmark:	US real	US real	US real	US real	US real	US real
Dep. variable:	Temp	Temp	Temp	Temp	Age < 35	ln(Empl)
EPRC	3.17***	3.98***	4.05***	4.12***	-4.27**	-0.00
	(1.19)	(1.14)	(1.14)	(0.98)	(1.77)	(0.25)
EPT	1.70**					
	(0.79)					
EPT/EPRC		3.76*				
		(1.99)				
Av. net repl. rate (%)	-0.03	-0.03	-0.01		-0.01	-0.01
	(0.03)	(0.03)	(0.03)		(0.04)	(0.01)
Temporary (%)						0.39
						(0.75)
Age: 15-24 (%)	0.42***	0.42***	0.40***	0.40***		0.97
	(0.05)	(0.05)	(0.05)	(0.05)		(0.61)
Age: 25-34 (%)	0.03	0.03	0.04	0.05		-0.18
	(0.07)	(0.07)	(0.06)	(0.06)		(0.78)
Observations	521	521	521	541	521	521
R-squared	0.852	0.851	0.848	0.848	0.694	0.934
Cou / ind dums	yes	yes	yes	yes	yes	yes
Control for PMR	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. Temp: percentage share of temporary workers. Age<35: percentage share of workers aged less than 35 years. Ln(Empl): logarithm of employment (head count). EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). EPT: index of employment protection for temporary contracts. Av. net repl. rate (5y): average net replacement rate over a 5-year unemployment period (in percentage). Temporary: share of temporary workers. Age: 15-24 and Age: 25-34: share of workers in the specified age class. US Real: US reallocation rate, used as benchmark. Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Control for PMR: the specification includes the economy-wide index of anti-competitive product market regulation, interacted with the benchmark. Cou/ind dums: industry dummies and country dummies. Robust standard errors in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

Using data on enforcement of legislation from the Fraser Institute (Gwartney *et al.*, 2008), it is possible to provide a rough test of whether the degree of enforcement is the key omitted factor in Table 6 and of whether this matters particularly for regulatory provisions for temporary contracts. As no available enforcement indicator refers explicitly to enforcement of labour legislation, two indicators are retained here. One is the indicator on "integrity of the judicial system", which is available for the whole period of analysis, except for 2007. The other is the indicator on the "enforcement of contracts", which is however available only for 2004 and 2005. Column 1 of Table 7 shows results obtained by restricting the sample to countries where the average of these two indicators is above the median of the sample.[16] Strikingly, in this subsample, the index of regulation for temporary contracts attracts a negative and significant coefficient, while all other coefficients appear only marginally affected. The indicator on "enforcement of contracts" seems to be provided by the Fraser Institute on an experimental basis only, as shown by its relative volatility over time. Therefore it might be more cautious to use only the "integrity" indicator in this analysis, which has been already used as measure of the degree of enforcement in the EP literature (see *e.g.* Haltiwanger *et al*., 2008). When the sample is restricted to those countries that have maximum score in this indicator in at least one year in the period considered,[17] the evidence confirms that the degree of enforcement significantly affects the coefficient of regulation for temporary contracts only (Column 2). As an additional sensitivity exercise, the estimation is repeated on the whole sample, by allowing indexes of regulation to interact with the "integrity" indicator (Columns 3 to 6). Interestingly, only the interaction between enforcement and regulation for temporary contracts appears significant (and its coefficient is negative, as expected). By contrast, coefficients for all other variables appear unaffected. Overall this confirms the importance of enforcement issues in the case of regulation for temporary contracts.

By simultaneous estimation of specifications from Table 6 or 7 and from Table 1 it is possible to derive an estimate of the overall direct partial-equilibrium impact of our variables of interest on labour reallocation, including the effect of these variables that occurs through inducing a greater or smaller use of fixed-term contracts. Insofar as the coefficient of UBs are insignificant in Tables 6 and 7, this computation is relevant only for EP (for UBs, coefficients in Table 1 represent already the overall impact). Under the same assumption as above concerning the direct impact in an hypothetical industry with US reallocation rates equal to zero, a one-point increase in the index of EP stringency for regular workers appears to reduce, on average, total worker reallocation – including that due to start and end of temporary contracts – by between 2.9 and 3.6 percentage points, depending on the specification. All these estimates are significant at conventional statistical levels.

Specifications considered so far include controls for the share of youth in the industry/country cells used as unit of observation. However, institutions are unlikely to affect employment rates of different demographic groups in the same way. In particular stringent EP for regular workers might depress employment rates for outsiders, including youth (see e.g. OECD, 2006). As this group tends to have high reallocation rates, estimates presented in Table 1 could underestimate the true effect of EP. Indeed, EP for regular workers appears to reduce the share of workers in high-reallocation industries (Table 6, Column 5). However, as noted above, this finding must be interpreted with caution since lower EP, by creating more opportunities in high-reallocation industries might simply shift young workers from low-reallocation to high-reallocation industries, without increasing overall youth employment and therefore without any extra bust to worker reallocation.

16. The sample is thereby restricted to Austria, Belgium, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, the Netherlands, Norway, Sweden, Switzerland, the United Kingdom and the United States.

17. The sample is thereby restricted to Austria, Canada, Denmark, Finland, Iceland, Ireland, Italy, the Netherlands, Norway, Sweden, Switzerland and the United Kingdom.

Table 7. Determinants of the share of temporary contracts, including indicators of enforcement

	(1)	(2)	(3)	(4)	(5)	(6)
Benchmark:	US real	US real	US real	US real	US real	US real
Dep.var:	Temp	Temp	Temp	Temp	Temp	Temp
Sample:	Above median integrity and enforcement of contracts	Integrity equal to max score	Full sample; EP and PMR interacted with integrity	Full sample; EP interacted with integrity	Full sample; EP interacted with integrity	Full sample; EP interacted with integrity
EPRC	5.21***	6.24***	3.87***	3.87***	4.00***	4.17***
	(1.96)	(2.37)	(1.04)	(1.01)	(1.12)	(1.03)
EPT	-1.39***	-1.98**	0.88	0.86	0.95	0.75
	(0.52)	(0.79)	(0.66)	(0.67)	(0.66)	(0.64)
Enforcement			0.07	0.05	0.18	-0.29
			(0.62)	(0.58)	(0.62)	(0.49)
Av. net repl. rate (%)	-0.03	-0.06*	-0.03	-0.03	-0.03	
	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)	
EPT x enforcement			-1.10***	-1.11***	-1.05***	-1.05***
			(0.41)	(0.38)	(0.35)	(0.35)
EPRC x enforcement			0.80	0.79		
			(1.52)	(1.48)		
PMR x enforcement			-0.13			
			(1.51)			
Observations	334	263	521	521	521	541
R-squared	0.833	0.821	0.856	0.856	0.856	0.856
Cou / ind dums	yes	yes	yes	yes	yes	yes
Other controls	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. Temp: percentage share of temporary workers. EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). EPT: index of employment protection for temporary contracts. Av. net repl. rate (5y): average net replacement rate over a 5-year unemployment period (in percentage). PMR: economy-wide index of anti-competitive product market regulation. Av. net repl. rate (5y): average net replacement rate over a 5-year unemployment period (in percentage). Enforcement: Fraser's institute index of integrity of the judicial system. US Real: US reallocation rate, used as benchmark. Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Aggregate variables are demeaned when they are interacted among themselves. Other controls: the specification includes PMR, interacted with the benchmark, and the shares of workers aged 15-24 and 25-34 years. Cou/ind dums: industry dummies and country dummies. Robust standard errors in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

It is also quite unlikely that the direct partial-equilibrium impact of institutions on gross flows be the same for all demographic or skill group. In the case of the main gross flow measures, available data can be disaggregated further by gender, age classes and educational attainment for a number of countries, even if at the price of greater measurement error. The analysis of Table 1 can therefore be replicated by controlling more directly for these characteristics through a series of dummies and by checking cross-group differences in the impact of those institutions that appear to be significant in Table 1. Average estimated effects of EP and UBs remain consistent with those reported in Table 1. Reallocation patterns concerning high-skilled workers appear to be somewhat less robustly affected by EP, particularly in the case of total reallocation (Table 8). This result might reflect the fact that, in all countries, expanding industries tend to have a large, growing demand for skilled labour (see Bassanini and Marianna, 2009) and suggest that EP provisions have a smaller effect on these industries. Similarly, it appears that stringent regulations have a particularly depressing impact on gross worker flows involving youth and, to a more limited extent, women. This finding appear consistent with the idea that EP for regular workers has a particularly negative effect on the job perspectives of outsiders, a group in which women and youth tend to be over-represented (see *e.g*. OECD, 2006).

Table 8. Institutions and worker reallocation, cells by country, industry, gender, age, education

Panel A: **Total worker reallocation**

	(1)	(2)	(3)	(4)	(5)	(6)
		Controlling for PMR			No other institutional control	
Benchmark:		US worker reallocation rate			US worker reallocation rate	
Het. Impact by:	Gender	Age	Education	Gender	Age	Education
EPR x men	-1.755			-3.315***		
	(0.032)			(0.022)		
EPR x women	-3.192**			-3.433***		
	(0.031)			(0.022)		
EPR x 15-24		-7.038***			-8.674***	
		(0.038)			(0.027)	
EPR x 25-34		-2.701**			-2.666***	
		(0.032)			(0.024)	
EPR x 35-44		-1.228			-1.881*	
		(0.03)			(0.023)	
EPR x 45-54		0.070			-1.274	
		(0.033)			(0.024)	
EPR x 55-64		-0.414			-0.598	
		(0.037)			(0.026)	
EPR x Low			-3.407***			-3.159***
			(0.027)			(0.02)
EPR x Medium			-3.041**			-3.945***
			(0.028)			(0.02)
EPR x High			-0.35			-2.648***
			(0.031)			(0.021)
NRR x men	0.081***			0.110***		
	(0.001)			(0.001)		
NRR x women	0.068**			0.086***		
	(0.001)			(0.001)		
NRR x 15-24		0.305***			0.332***	
		(0.001)			(0.001)	
NRR x 25-34		0.148***			0.162***	
		(0.001)			(0.001)	
NRR x 35-44		0.064*			0.084**	
		(0.001)			(0.001)	
NRR x 45-54		-0.020			0.006	
		(0.001)			(0.001)	
NRR x 55-64		-0.167***			-0.152***	
		(0.001)			(0.001)	
NRR x Low			0.046			0.061*
			(0.001)			(0.001)
NRR x Medium			0.111***			0.134***
			(0.001)			(0.001)
NRR x High			0.065**			0.100***
			(0.001)			(0.001)
Observations	10838	10838	10838	10838	10838	10838
R-squared	0.74	0.75	0.74	0.74	0.75	0.74
PMR	yes	yes	yes	No	No	No
Oth. controls	yes	yes	yes	yes	yes	yes
Country dums	yes	yes	yes	yes	yes	yes
Industry dums	yes	yes	yes	yes	yes	yes
Gender dums	yes	yes	yes	yes	yes	yes
Age dums	yes	yes	yes	yes	yes	yes
Education dums	yes	yes	yes	yes	yes	yes

Table 8. **Institutions and worker reallocation, cells by country, industry, gender, age, education** *(Cont.)*

Panel B: **Excess worker reallocation**

	(1)	(2)	(3)	(4)	(5)	(6)
	Controlling for PMR			No other institutional control		
Benchmark:	US worker reallocation rate			US worker reallocation rate		
Het. Impact by:	Gender	Age	Education	Gender	Age	Education
EPR x men	-3.621***			-5.255***		
	(0.029)			(0.021)		
EPR x women	-5.195***			-4.948***		
	(0.03)			(0.021)		
EPR x 15-24		-9.371***			-8.828***	
		(0.04)			(0.03)	
EPR x 25-34		-4.917***			-3.958***	
		(0.029)			(0.026)	
EPR x 35-44		-3.133**			-3.684***	
		(0.029)			(0.026)	
EPR x 45-54		-1.438			-3.235***	
		(0.03)			(0.026)	
EPR x 55-64		-0.750			-2.977***	
		(0.032)			(0.027)	
EPR x Low			-4.426***			-4.437***
			(0.03)			(0.022)
EPR x Medium			-5.584***			-5.468***
			(0.027)			(0.021)
EPR x High			-3.279**			-5.345***
			(0.031)			(0.022)
NRR x men	0.051*			0.076**		
	(0.001)			(0.001)		
NRR x women	0.039			0.049		
	(0.001)			(0.001)		
NRR x 15-24		0.260***			0.265***	
		(0.001)			(0.001)	
NRR x 25-34		0.115***			0.115***	
		(0.001)			(0.001)	
NRR x 35-44		0.026			0.038	
		(0.001)			(0.001)	
NRR x 45-54		-0.061*			-0.037	
		(0.001)			(0.001)	
NRR x 55-64		-0.141***			-0.113***	
		(0.001)			(0.001)	
NRR x Low			0.022			0.032
			(0.001)			(0.001)
NRR x Medium			0.083***			0.092***
			(0.001)			(0.001)
NRR x High			0.050*			0.077**
			(0.001)			(0.001)
Observations	10838	10838	10838	10838	10838	10838
R-squared	0.68	0.70	0.68	0.68	0.70	0.68
PMR	yes	yes	yes	No	No	No
Oth. controls	yes	yes	yes	yes	yes	yes
Country dums	yes	yes	yes	yes	yes	yes
Industry dums	yes	yes	yes	yes	yes	yes
Gender dums	yes	yes	yes	yes	yes	yes
Age dums	yes	yes	yes	yes	yes	yes
Education dums	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. US reallocation rate, used as benchmark. EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). NRR: average net replacement rate (computed for different earnings levels, family situations and unemployment durations up to 5 years). PMR: economy-wide index of anti-competitive product market regulation. Other controls: share of temporary workers. Country dums: country dummies. Industry dums: industry dummies. Gender dums: gender dummies. Age dums: age dummies. Education dums are education dummies for 3 levels of highest completed educational attainment used as a proxy for skills: low - less than upper secondary -, medium - upper secondary and some post-secondary -, high - tertiary level of education. Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Robust standard errors, adjusted for clustering on countries and years, in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

Looking at differences across groups in the association between cross-industry differences in gross job flows and average net replacement rates sheds additional light on the channels through which UB generosity affects labour reallocation. In fact, the positive relationship between UB generosity and gross worker flows is thoroughly confined to relatively young workers (Table 8). As age increases this relationship becomes progressively negative, so that for workers aged 55 years or more a ten-percentage-point increase in unemployment benefits would reduce gross worker reallocation by more than 2 percentage points. This evidence could reflect the fact that, in the case of older workers, generous benefits might represent a post-displacement route to *de facto* early retirement, which is likely to be larger in industries where separations are more frequent. Nevertheless, it might also suggest that higher reservation and bargained wages induced by generous UBs make firms more selective in their recruitment policies, thereby reducing experimentation with new recruits, as predicted by Pries and Rogerson (2005). In fact, this effect is theoretically predicted to occur only for workers eligible for benefits, thereby excluding most of youth. By contrast, the direct job-destruction effect, predicted by standard equilibrium matching models, applies at any age,[18] and the same occurs for indirect general-equilibrium effects for hirings. All these effects add up, generating the age pattern shown in Table 8. In addition, the Pries-Rogerson's experimentation effect should be as large for hirings as for separations, in contrast with the standard productivity-shock/job-destruction direct effect that is greater for separations. Consistent with this interpretation, cross-age differences in estimated coefficients are similar for both hirings and separations (not shown in the table), although the average effect on the latter appears higher.

Up to here, institutions have also been assumed to be exogenous. As they are defined at the aggregate level, in fact, country dummies control for the fact that in countries (and years) where, say, job destruction is larger, demands for institutional changes might be greater. In other words, the inclusion of country dummies, rules out the most evident source of endogeneity. However, there is a more subtle political economy argument that can be put forward and that is potentially more problematic at least for EP – that is the institution that appears to have the largest impact on gross worker reallocation. Suppose that dismissal regulations do not affect the amount of dismissals but only its costs and therefore profits. This will occur especially in industries were the need to adjust on the external market is more prominent. Given the size of the effects estimated here, it is not inconceivable that firms in these industries will lobby more actively for reducing EP when, because of some shock, worker flows are greater in these industries. As a consequence, due to lobbying pressure only, EP would tend to be lower in countries where high reallocation industries have greater worker flows, and estimated coefficients might simply measure this correlation.

Following Bassanini *et al*. (2009), an instrumental variable strategy is adopted here to examine this issue. Three aggregate variables interacted with benchmark measures are considered: a dummy for common law systems, a dummy for civil law systems with codified civil code and a dummy for countries that experienced dictatorships in the 20th century (excluding during World War II).[19] All these historical and institutional factors pre-date the legislation on employment protection, thereby limiting the risk of

18 . If any, the direct productivity-shock/job-destruction effect occurs mainly for workers that were not eligible for benefits at the time of recruitment but have become eligible as they get seniority on the job. For these workers, in fact, one can assume that UBs do not affect the productivity threshold at which efficient job-matches are created.

19 . One would expect more lenient EP in common law countries and more constraining regulations in countries under civil law with a civil code tradition. In fact, countries with common law systems tend to be attached to the principle of freedom of contracts and have relatively few regulatory provisions concerning labour contracts, while most civil law systems tend to minutely regulate. Moreover, due to their paternalistic view of labour relationships, 20th-century fascist regimes were historically inclined to guarantee workers strong protection against dismissals, albeit within an industrial relation system with no workers' voice. Stringent regulations generally survived the fall of these political regimes (see Bassanini *et al.*, 2009, where data come from, for a more extensive discussion).

reverse causality. The validity of instruments is, however, checked using the robust score test for overidentification, as suggested by Wooldridge (1995). While the legal indicators do not raise major concerns, overidentification tests are sometimes rejected when the dictatorship indicator is included (Table 9), probably due to the relatively recent experience of specific countries with dictatorship regimes. Anyway, endogeneity tests never reject the exogeneity assumption, thereby validating estimates presented in Table 1.[20]

Table 9. **Endogenous employment protection**

	Dep. Var.: **Worker Reallocation**							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benchmarks:	US real	US real	Ciccone	Ciccone	US layoffs	US layoffs	UK real	UK real
Instruments:	com. law civil code	com. law civil code dictator	com. law civil code	com. law civil code dictator	com. law civil code	com. law civil code dictator	com. law civil code	com. law civil code dictator
EPRC	-2.76*	-1.54	-3.55**	-2.38	-3.72***	-3.01**	-2.13	-1.28
	(1.562)	(1.433)	(1.730)	(1.573)	(1.399)	(1.347)	(1.818)	(1.672)
Age: 15-24 (%)	0.584***	0.604***	0.579***	0.596***	0.645***	0.642***	0.602***	0.613***
	(0.063)	(0.061)	(0.062)	(0.060)	(0.057)	(0.056)	(0.062)	(0.060)
Age: 25-34 (%)	0.243***	0.242***	0.246***	0.244***	0.245***	0.244***	0.244***	0.242***
	(0.084)	(0.084)	(0.084)	(0.084)	(0.083)	(0.083)	(0.084)	(0.084)
Cou x ind dums	yes	yes	yes	yes	yes	yes	yes	yes
Observations	427	427	427	427	427	427	427	427
R-squared	0.922	0.922	0.922	0.922	0.921	0.922	0.921	0.921
Tests (P-values):								
Overidentification	0.374	0.026**	0.485	0.055*	0.160	0.131	0.515	0.257
Endogeneity (score)	0.965	0.225	0.891	0.332	0.189	0.504	0.897	0.404
Endogeneity (Hausman)	0.967	0.236	0.898	0.346	0.205	0.529	0.902	0.412
Fisher F of instruments	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***

Notes: 2SLS estimates. US Real: US reallocation rate, used as benchmark. UK Real: UK reallocation rate, used as benchmark. US layoffs: US dismissal rate. Ciccone is the predicted benchmark at zero EPRC, obtained using the methodology of Ciccone and Papaioannou (2007). EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals), treated as endogenous. Instruments, interacted with the benchmark, indicated in columns' titles. Age: 15-24 and Age: 25-34: share of workers in the specified age class. Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Cou/ind dums: industry dummies and country dummies. Score tests are Wooldridge's (1995) robust score tests. The Hausman test for endogeneity is the t-test on the estimated coefficient of the 1st-stage residual in augmented OLS specifications. Robust standard errors in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

20. Due to the potential endogeneity of the share of temporary workers and of other policy co-variates, these variables are excluded in these exercises. This implies that results when using US dismissals as benchmark can be considered more conclusive, as no other co-variate appears significant in Table 3 when this benchmark is used.

Table 10. **Decomposing the effect of employment protection for regular workers into that of different components**

Panel A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Benchmark:	US real	US real	US real	US real	US real	US real	US real	US real	US real
Dep. Variable:	Real	Real	Real	Real	Real	Excess	SR	HR	Temp
Reg. on coll. dismiss.	-1.35*	-1.06	-1.01						2.19***
	(0.80)	(0.85)	(0.76)						(0.78)
Reg. on ind. Dismiss.	-4.11***								
	(0.81)								
Procedur. Inconven.		-0.15							
		(1.24)							
Notice / Severance pay		-1.54***	-1.55***	-1.50***	-1.47**	-1.40**	-0.91***	-0.59**	1.19**
		(0.57)	(0.55)	(0.54)	(0.64)	(0.57)	(0.32)	(0.28)	(0.60)
Difficulty of dismissal		-2.29**	-2.39***	-2.30***	-2.65***	-2.59***	-1.19***	-1.11***	1.39**
		(1.00)	(0.67)	(0.66)	(0.75)	(0.70)	(0.40)	(0.32)	(0.69)
Observations	521	521	521	521	541	521	521	521	521
R-squared	0.927	0.927	0.927	0.927	0.921	0.918	0.875	0.937	0.849
Cou / ind dums	yes	yes	yes	yes	yes	yes	yes	yes	yes
Share temporary	yes	yes	yes	yes	yes	yes	yes	yes	no
Age controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Other institutions	yes	yes	yes	yes	no	yes	yes	yes	yes

Panel B.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Benchmark:	US real	US real	US real	US real	US real	US real	US real
Dep. Variable:	Real	Real	Real	Real	Excess	SR	HR
Notice / Severance pay	-1.72***	-1.60***	-1.55***	-1.50**	-1.52***	-0.92***	-0.63**
	(0.60)	(0.61)	(0.59)	(0.65)	(0.59)	(0.33)	(0.31)
Defin. unfair dismissal	0.49	0.13					
	(0.37)	(0.34)					
Length of trial period	-1.93***	-1.52***	-1.47**	-1.69***	-1.60***	-0.72**	-0.76***
	(0.58)	(0.56)	(0.57)	(0.54)	(0.61)	(0.33)	(0.28)
Compens. unfair dism.	-0.52						
	(0.49)						
Extent of reinstatement	-1.03***	-1.11***	-1.09***	-1.41***	-1.08***	-0.65***	-0.44**
	(0.36)	(0.37)	(0.35)	(0.34)	(0.37)	(0.19)	(0.19)
Observations	487	521	521	541	521	521	521
R-squared	0.921	0.929	0.928	0.926	0.919	0.878	0.937
Cou / ind dums	yes	yes	yes	yes	yes	yes	yes
Share temporary	yes	yes	yes	yes	yes	yes	yes
Age controls	yes	yes	yes	yes	yes	yes	yes
Other institutions	yes	yes	yes	no	yes	yes	yes

Notes: OLS estimates. Real: total reallocation rate. Excess: excess reallocation rate. SR: separation rate. HR: hiring rate. US Real: US reallocation rate, used as benchmark. Share temporary: share of temporary workers. Age controls: shares of workers aged 15-24 and 25-34 years. Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Other institutions include the economy-wide index of anti-competitive product market regulation and average net replacement rate over a 5-year unemployment period, interacted with the benchmark. Cou/ind dums: industry dummies and country dummies. Robust standard errors in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

The overall index of EP for regular workers has been considered so far. However, available indicators allow exploring the relevance of different components. The first obvious distinction is between regulations for individual dismissals and additional provisions for collective dismissals. Estimating models distinguishing between these two components show that most of the correlation between regulation and reallocation regarding workers in open-ended contracts is due to provisions for individual dismissals, while additional provisions for collective dismissals play a bigger role in determining the use of temporary contracts (Table 10). Decomposing further regulation for individual dismissals, after subsequent elimination of insignificant institutions, it appears that significant components are notice and severance payments and difficulty of dismissals, and within the latter the length of the trial period and easiness with which reinstatement is ordered by courts in the case of conviction for unfair dismissal. By contrast, procedural inconveniences appear to have no significant effect, in contrast to what often claimed in many theoretical and policy analysis (see *e.g.* Cahuc, 2003, L'Haridon and Malherbet, 2009, and the literature cited therein). These results might be the outcome of the greater difficulty of scoring notification procedures and delays and, therefore, the greater measurement error associated to it – because of the cross-country heterogeneity of the procedures that are requested in the case of dismissals. However, they appears also consistent with micro studies for Portugal, Sweden and the United Kingdom that find no significant impact of exemptions from procedural requirements for dismissals (Martins, 2009 and von Below and Thoursie, 2010) and significant effect of the length of trial period (Marinescu, 2009).

Next, the impact of institutions on other types of transitions is considered, using the preferred specification. Most noteworthy is the fact that the only type of separation rate that is significantly affected by EP regulations is the rate of job-to-job separations, which are less likely to generate important wage losses at re-employment (Table 11). This cautiously suggests that those workers, who end up being displaced in the aftermath of a reform reducing EP for regular workers but would have not been displaced without the reform, are likely to find another job within a relatively short period of time.[21] Moreover, flexibility-enhancing EP reforms appear to be entirely associated to more frequent same-sector transitions, which are typically associated to greater wage premia in the case of voluntary job changes and lower wage penalties in the case of displacement (see *e.g.* Neal, 1995).

Table 11. **Other types of transitions**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benchmark	US real	US real	US real	US real	US real	US real	US real	US real
Dep. Variable:	J2J(HR)	Jobless2J	J2J(SR)	J2Jobless	SameSR	OtherSR	Empl-losingR	Empl-quittingR
EPRC	-1.49**	-1.02*	-1.92**	-0.60	-1.93**	0.20	-0.24	0.10
	(0.67)	(0.53)	(0.95)	(0.65)	(0.80)	(0.44)	(0.45)	(0.37)
Av. net repl. rate (%)	0.01	0.04**	0.03	0.05***	0.02	0.01	0.03***	0.01**
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Temporary (%)	0.22***	0.16***	0.18***	0.11***	0.18***	-0.00	0.12***	0.02
	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.02)	(0.03)	(0.01)
Age: 15-24 (%)	-0.04	0.22***	0.13***	0.05	0.03	0.14***	0.01	0.08***
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Age: 25-34 (%)	0.11***	0.07***	0.06*	0.03	0.00	0.02	0.00	-0.01
	(0.03)	(0.02)	(0.04)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)
Observations	421	421	405	405	371	371	368	368
R-squared	0.880	0.900	0.869	0.739	0.858	0.826	0.761	0.655
Cou / ind dums	yes	yes	yes	yes	yes	yes	yes	yes
Control for PMR	yes	yes	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. J2J(HR): job-to-job hiring rate. Jobless2J: jobless-to-job hiring rate. J2J(SR): job-to-job separation rate. J2Jobless: job-to-jobless separation rate. SameSR: Same-sector separation rate. OtherSR: Other-sector separation rate. Empl-losingR: employment-losing separation rate. Empl-quittingR: employment-quitting separation rate. US Real: US reallocation rate, used as benchmark. EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). Av. net repl. rate (5y): average net replacement rate over a 5-year unemployment period (in percentage). Temporary: share of temporary workers. Age: 15-24 and Age: 25-34: share of workers in the specified age class. Aggregate variables multiplied by the benchmark, with reported estimates referring to estimated coefficients of the interaction terms multiplied by the average benchmark. Control for PMR: the specification includes the economy-wide index of anti-competitive product market regulation, interacted with the benchmark. Cou/ind dums: industry dummies and country dummies. Robust standard errors in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

Time-series estimates

Rigorously speaking, estimates up to here refer only to partial equilibrium labour demand effects. In principle, general equilibrium mechanisms can enhance or offset these effects. For example, less stringent EP provisions might make downsizing easier for firms in high-reallocation industries, if these industries

21 . Notice, however, that, given the definition of job-to-job transitions adopted here (see Section 1), this finding does not imply that EP reforms would not increase the number of displaced workers that experience short unemployment spells after the separation.

are in a contraction stage of their lifecycle – that is if they are on a steadily-shrinking trend. In turn, this could decrease the share of high-reallocation industries in aggregate employment. If this were the case, the aggregate impact of a liberalisation of EP provisions would be the addition of partially offsetting within-industry and between-industry effects. However, neither dismissal regulations nor net replacement rates seem to affect employment shares in such a systematic way. Results presented in Column 6 of Table 6 indeed confirm that this holds true. In fact, interactions between relevant institutions and the benchmark do not appear to have any significant impact on the logarithm of employment.[22]

Alternatively, the increase in the number of vacancies in high-reallocation industries, induced by less stringent EP regulations, could provide better job opportunities for workers in low-reallocation industries and therefore increase voluntary separations in these industries (Zweimüller, 2009). If this were the case, cross-industry differences would provide only a lower bound estimate of the true aggregate effect of EP provisions. In order to shed some light on these issues, equation [4] is estimated 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. The main disadvantage of this approach is that many institutional variables need to be replaced by a surrogate measure in order to obtain longer time series. As discussed in section 3, this is the case, for instance, for both EP and unemployment benefit generosity: in the former case, additional restrictions for collective dismissals are unavailable prior to 1998 while in the latter gross replacement rates must be used instead of net replacement rates for time-series starting before 2001. The index of EP for regular workers excluding additional provisions for collective dismissals appear to be a good proxy for the overall degree of stringency of EP for regular workers, as the two EP indexes appear to be closely correlated in the subsample in which both are available. By contrast, this is not the case for net and gross replacement rates that appear to be only weakly correlated, which suggests that the latter can be used only as a control variable and its coefficient should not be over-interpreted.

The analysis, presented in Table 12, starts with the simplest specification of equation [4], without interactions with the benchmark measure. The effect of aggregate variables in these specifications is therefore identified through over-time variation only, as in a standard aggregate cross-country/time-series regression model. Then, interactions with the benchmark are considered.[23] Finally, as a sensitivity analysis for the difference-in-difference estimates presented above, which are obtained from averaged data, country-by-time dummies are included to control for all aggregate effects. Table 13 replicates the analysis using the share of temporary contracts as dependent variable and including enforcement indicators as additional covariates in Panel B.[24] As labour reallocation rates are well known to increase in downturns (see *e.g.* Davis *et al.*, 2006), all specifications control for the difference between the current and average growth rate of employment (the latter computed over the period 1990-2007 for each industry and country). Estimated general equilibrium effects are strikingly close to difference-in-difference estimates on averaged data presented above. More precisely, it appears that, if any, difference-in-difference estimates underestimate general-equilibrium effects of institutions of interest.

22. Similar findings are reported by Bassanini *et al*. (2009) on the basis of a different sample.

23. US reallocation rates are divided by their overall average to preserve comparability with previous tables.

24. In these specifications, the direct effect of enforcement cannot be identified (although controlled for) since it is collinear with country dummies.

Table 12. **Determinants of gross worker flows: time-series estimates**

Panel A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Benchmark:	Nothing	US real	US real	Nothing	US real	US real	US real	US real	Nothing	US real	US real	Nothing	US real	US real	US real	US real
Dep. Variable:	Real	Real	Real	Real	Real	Real	Real	Real	Excess	Excess	Excess	Excess	Excess	Excess	Excess	Excess
EPR	-6.12***	-5.98***							-6.33***	-6.17***						
	(1.87)	(1.91)							(1.78)	(1.81)						
EPRC				-9.87***	-10.14***							-9.55***	-9.85***			
				(3.14)	(3.15)							(3.16)	(3.17)			
Av. gr. repl. rate (%)	-0.04	-0.04							-0.02	-0.02						
	(0.05)	(0.05)							(0.05)	(0.05)						
Av. net repl. rate (%				0.09**	0.09**							0.08*	0.08			
				(0.04)	(0.04)							(0.05)	(0.05)			
EPR x benchmark		-2.74***	-3.08***				-2.80***			-2.88***	-3.13***				-2.20**	
		(0.49)	(0.34)				(0.97)			(0.49)	(0.37)				(1.03)	
EPRC x benchmark					-5.27***	-5.61***		-2.71**					-5.55***	-5.77***		-2.44*
					(0.99)	(0.61)		(1.33)					(1.02)	(0.67)		(1.46)
AGRR x benchmark		0.10***	0.10***				0.03			0.11***	0.11***				-0.01	
		(0.03)	(0.02)				(0.09)			(0.03)	(0.03)				(0.09)	
ANRR x benchmark					0.08***	0.09***		0.14***					0.11***	0.11***		0.08*
					(0.02)	(0.02)		(0.04)					(0.02)	(0.02)		(0.05)
Temporary (%)	0.55***	0.59***	0.63***	0.66***	0.75***	0.76***	0.77***	0.93***	0.50***	0.54***	0.58***	0.60***	0.70***	0.70***	0.71***	0.87***
	(0.04)	(0.04)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.06)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)
PMR (sectoral)							-0.63***	-0.58*							-0.82***	-1.00***
							(0.22)	(0.30)							(0.26)	(0.34)
Observations	3973	3973	4083	2458	2458	2568	1683	999	3973	3973	4083	2458	2458	2568	1683	999
R-squared	0.844	0.847	0.874	0.840	0.846	0.872	0.803	0.810	0.826	0.829	0.854	0.821	0.828	0.853	0.765	0.772
Oth. controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country dums	yes	yes	no	yes	yes	no	no	no	yes	yes	no	yes	yes	no	no	no
Cou x time dums	no	no	yes	no	no	yes	yes	yes	no	no	yes	no	no	yes	yes	yes
Ind x time dums	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Table 12. **Determinants of gross worker flows: time-series estimates** *(Cont.)*

Panel B.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Benchmark:	Nothing	US real	US real	Nothing	US real	US real	US real	US real	Nothing	US real	US real	Nothing	US real	US real	US real	US real
Dep. Variable:	SR	SR	SR	SR	SR	SR	SR	SR	HR	HR	HR	HR	HR	HR	HR	HR
EPR	-3.11***	-3.04***							-3.01***	-2.94***						
	(0.93)	(0.94)							(0.95)	(0.97)						
EPRC				-4.96***	-5.10***							-4.91***	-5.04***			
				(1.59)	(1.58)							(1.57)	(1.58)			
Av. gr. repl. rate (%)	-0.02	-0.02							-0.02	-0.02						
	(0.03)	(0.03)							(0.03)	(0.03)						
Av. net repl. rate (%				0.04**	0.04**							0.05**	0.04**			
				(0.02)	(0.02)							(0.02)	(0.02)			
EPR x benchmark		-1.43***	-1.61***				-1.60***			-1.31***	-1.47***				-1.20***	
		(0.26)	(0.18)				(0.54)			(0.26)	(0.17)				(0.46)	
EPRC x benchmark					-2.90***	-3.10***		-1.21					-2.37***	-2.51***		-1.50**
					(0.52)	(0.32)		(0.79)					(0.51)	(0.31)		(0.63)
AGRR x benchmark		0.06***	0.06***				0.05			0.04***	0.05***				-0.03	
		(0.02)	(0.01)				(0.05)			(0.01)	(0.01)				(0.04)	
ANRR x benchmark					0.04***	0.04***		0.10***					0.04***	0.04***		0.04*
					(0.01)	(0.01)		(0.02)					(0.01)	(0.01)		(0.02)
Temporary (%)	0.28***	0.30***	0.32***	0.33***	0.38***	0.38***	0.42***	0.51***	0.27***	0.29***	0.31***	0.33***	0.37***	0.37***	0.35***	0.42***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
PMR (sectoral)							-0.32***	-0.30*							-0.31***	-0.28**
							(0.12)	(0.17)							(0.11)	(0.14)
Observations	3973	3973	4083	2458	2458	2568	1683	999	3973	3973	4083	2458	2458	2568	1683	999
R-squared	0.832	0.835	0.862	0.830	0.836	0.861	0.821	0.831	0.865	0.867	0.891	0.859	0.863	0.885	0.821	0.823
Oth. controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country dums	yes	yes	no	yes	yes	no	no	no	yes	yes	no	yes	yes	no	no	no
Cou x time dums	no	no	yes	no	no	yes	yes	yes	no	no	yes	no	no	yes	yes	yes
Ind x time dums	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. Real: total reallocation rate. Excess: excess reallocation rate. SR: separation rate. HR: hiring rate. US Real: US reallocation rate, used as benchmark, divided by its average value. Nothing: no benchmark interacted with aggregate variables. EPR: index of employment protection for regular workers (excluding additional provisions for collective dismissals). EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). Av. gr. repl. rate (5y) (AGRR): average gross replacement rate over a 5-year unemployment period (in percentage). Av. net repl. rate (5y) (ANRR): average net replacement rate over a 5-year unemployment period (in percentage). PMR (sectoral): industry-specific index of anti-competitive product market regulation (set to zero in manufacturing). Temporary: share of temporary workers. Other controls: sector-specific difference between current and average employment growth, shares of workers aged 15-24 and 25-34 years and trade-union density. Country dums: country dummies. IndXtime dums: industry-by-time dummies. CouXtime dums: country-by-time dummies. Robust standard errors, adjusted for clustering on countries and years, in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

Table 13. Determinants of the share of temporary workers: time-series estimates

Panel A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benchmark:	Nothing	US real	US real	Nothing	US real	US real	US real	US real
Dep. Variable:	Temp	Temp	Temp	Temp	Temp	Temp	Temp	Temp
EPR	0.15	-0.02						
	(0.93)	(0.90)						
EPRC				1.03	1.22			
				(0.87)	(0.85)			
Av. gr. repl. rate (%)	0.11***	0.11***						
	(0.02)	(0.02)						
Av. net repl. rate (%)				-0.02**	-0.02			
				(0.01)	(0.01)			
EPR x benchmark		3.16***	3.07***				0.91	
		(0.44)	(0.26)				(0.70)	
EPRC x benchmark					6.44***	6.17***		-0.22
					(0.72)	(0.46)		(1.43)
AGRR x benchmark		0.01	0.01				-0.06	
		(0.03)	(0.02)				(0.04)	
ANRR x benchmark					-0.05***	-0.04***		-0.10***
					(0.02)	(0.01)		(0.03)
PMR (sectoral)							-0.17	0.01
							(0.16)	(0.23)
Observations	4134	4134	4244	2550	2550	2660	1683	999
R-squared	0.770	0.786	0.800	0.782	0.807	0.810	0.828	0.844
Oth. controls	yes	yes	yes	yes	yes	yes	yes	yes
Country dums	yes	yes	no	yes	yes	no	no	no
Cou x time dums	no	no	yes	no	no	yes	yes	yes
Ind x time dums	yes	yes	yes	yes	yes	yes	yes	yes

Table 13. **Determinants of the share of temporary workers: time-series estimates** *(Cont.)*

Panel B.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Benchmark:	Nothing	US real	US real	Nothing	US real	US real	US real	US real
Dep. Variable:	Temp	Temp	Temp	Temp	Temp	Temp	Temp	Temp
EPR	-0.30	-0.58						
	(0.84)	(0.81)						
EPRC				1.04	1.53			
				(0.92)	(0.96)			
EPT	-1.29***	-1.17***		-1.62**	-1.26*			
	(0.23)	(0.23)		(0.68)	(0.68)			
EPT x Enforcement	-0.96***	-0.70***		-0.84***	-0.48			
	(0.14)	(0.14)		(0.30)	(0.30)			
Av. gr. repl. rate (%)	0.08***	0.07***						
	(0.02)	(0.02)						
Av. net repl. rate (%)				-0.01	-0.00			
				(0.01)	(0.01)			
EPR x benchmark		3.15***	3.03***				-0.17	
		(0.43)	(0.26)				(0.76)	
EPRC x benchmark					5.86***	5.54***		-1.39
					(0.82)	(0.53)		(1.66)
EPT x benchmark		0.04	0.13		1.21***	1.23***	2.70***	2.84***
		(0.31)	(0.22)		(0.46)	(0.29)	(0.56)	(0.79)
Enforcement x Bench		-1.69***	-1.74***		-1.21**	-1.26***	0.96	1.44*
		(0.35)	(0.26)		(0.53)	(0.35)	(0.60)	(0.84)
EPT x Enf. x Bench.		-1.44***	-1.39***		-1.37***	-1.33***	0.23	-0.37
		(0.14)	(0.12)		(0.25)	(0.17)	(0.33)	(0.64)
AGRR x benchmark		0.08***	0.08***				-0.12***	
		(0.03)	(0.02)				(0.04)	
ANRR x benchmark					-0.04*	-0.04**		-0.13***
					(0.02)	(0.02)		(0.03)
PMR (sectoral)							-0.11	0.12
							(0.15)	(0.21)
Observations	4134	4134	4244	2550	2550	2660	1683	999
R-squared	0.772	0.807	0.818	0.783	0.823	0.825	0.831	0.847
Oth. controls	yes	yes	yes	yes	yes	yes	yes	yes
Country dums	yes	yes	no	yes	yes	no	no	no
Cou x time dums	no	no	yes	no	no	yes	yes	yes
Ind x time dums	yes	yes	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. Temp: percentage share of temporary workers. US Real: US reallocation rate, used as benchmark, divided by its average value. Nothing: no benchmark interacted with aggregate variables. EPR: index of employment protection for regular workers (excluding additional provisions for collective dismissals). EPT: index of employment protection for temporary contracts. EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). Av. gr. repl. rate (5y) (AGRR): average gross replacement rate over a 5-year unemployment period (in percentage). Av. net repl. rate (5y) (ANRR): average net replacement rate over a 5-year unemployment period (in percentage). Enforcement: Fraser's institute index of integrity of the judicial system. PMR (sectoral): industry-specific index of anti-competitive product market regulation (set to zero in manufacturing). Other controls: sector-specific difference between current and average employment growth, shares of workers aged 15-24 and 25-34 years and trade-union density. Aggregate variables are demeaned when they are interacted among themselves. Country dums: country dummies. IndXtime dums: industry-by-time dummies. CouXtime dums: country-by-time dummies. Robust standard errors, adjusted for clustering on countries and years, in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

As discussed in the data section, on a restricted sample of before-enlargement European Union countries for the years 1996-2007 only, we re-estimate the time-series models that include industry-by-time and country-by-time dummies including the indicator of industry-specific product market regulation for the five non-manufacturing industries for which it is available and setting it equal to an arbitrary constant in manufacturing (Columns 7-8 and 15-16 of Tables 12 and 13). The reason why this is possible is that, in countries belonging to the Single Market Programme, regulatory barriers are essentially composed by trade barriers, which are industry-specific but equal across countries, and administrative barriers are country-specific but equal for all industries within a country. All these barriers will be controlled for by our dummies, no matter what value is attributed to their indicator.

With these caveats in mind, product market deregulation in typically regulated industries appears to have increased worker reallocation in affected industries but not the share of temporary workers. Nevertheless, given the short time-period in which these regressions are estimated, it cannot be excluded that these coefficients reflect mainly short-time adjustments rather than the long-run equilibrium.

Wage estimates

There is quite a lot of evidence that gross job reallocation and productivity growth are positively correlated. In particular, several single-country studies based on dynamic accounting decompositions have shown that jobs are reallocated from firms with lower labour productivity to firms with higher labour productivity (see *e.g.* Griliches and Regev, 1995; Haltiwanger, 1997; Foster *et al.*, 2001; 2006; Disney *et al.*, 2003; Baldwin and Gu, 2006, Bottazzi et al., 2010). This result has been confirmed by multi-country studies (*e.g.* Bartelsman *et al.*, 2009), and appears to be even stronger when efficiency levels are measured through multi-factor productivity – MFP hereafter (*e.g.* Brown and Earle, 2008). In addition, the observed association between efficiency levels and labour reallocation does not appear to be due to firm heterogeneity (Bassanini and Marianna, 2009). As a result, aggregate productivity growth tends to be greater, the greater the labour reallocation. Therefore, through this simple accounting mechanism, labour reallocation appears to enhance aggregate productivity growth.

EP for regular workers and product market regulation have been found to have a negative impact on productivity growth in previous work, while UB generosity appear to enhance productivity growth (see *e.g.* OECD, 2007). Overall, the results presented here suggest that enhancing or impairing labour reallocation could be one of the channels through which these institutions enhance or impair productivity growth.[25]

One could argue that workers are likely to benefit in the long-run from the faster productivity growth that is enabled by greater reallocation, to the extent that productivity gains are shared with workers through higher wages. There is indeed some, albeit limited, empirical evidence suggesting that job flows and wage growth are correlated. For example Faberman (2002) shows that US metropolitan areas with larger job flows tend to have greater growth rates of average wages, while Belzil (2000) finds a positive impact of job creation on wages using Danish matched employer-employee data, although this effect is weaker at longer tenure. More generally, if productivity gains brought about by greater labour reallocation were not reflected into higher wages one would expect the wage share to decline with labour reallocation. In order to explore whether any increase in productivity brought about by changes in these institutions is reflected into higher wages, in Table 14, we estimate a few rough dynamic models of the wage share computed from EU KLEMS and STAN, derived by a standard growth model but consistent with previous equations.[26] Only UB generosity, as measured by net replacement rate, appears to have a significant effect on the wage

25 . By contrast, no such channel appears to have any relevance for the minimum wage.

26. The largest sample include Australia, Austria, Belgium, Canada, Czech republic, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Korea, Japan, the Netherlands, Norway, Poland, Portugal, Slovak republic, Spain, Sweden, Switzerland, the United Kingdom and the United States, for 1985-2007 (certain years unavailable for certain countries).

share.[27] Overall this cautiously suggests that productivity-enhancing reforms of EP, UBs and product market regulation would bring about some benefits for the average worker.

Table 14. Determinants of the wage share: dynamic time-series models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Benchmark:	Nothing	Nothing	US real	US real	Nothing	Nothing	US real	US real	US real	US real
Dep. Variable:	WS	WS	WS	WS	WS	WS	WS	WS	WS	WS
EPR	0.67	1.06	1.04							
	(0.65)	(0.73)	(0.73)							
EPRC					1.05	1.38	1.38			
					(2.77)	(3.04)	(3.04)			
Av. gr. repl. rate (%)	-0.00	-0.01	-0.01							
	(0.02)	(0.04)	(0.04)							
Av. net repl. rate (%)					0.07*	0.10**	0.10**			
					(0.04)	(0.04)	(0.04)			
EPR x benchmark			-0.21	-0.22					0.42	
			(0.26)	(0.22)					(0.94)	
EPRC x benchmark							0.54	0.58		2.81
							(0.60)	(0.55)		(1.75)
AGRR x benchmark			-0.00	0.00					0.06	
			(0.02)	(0.02)					(0.07)	
ANRR x benchmark							-0.01	-0.01		0.04
							(0.02)	(0.02)		(0.06)
PMR (sectoral)									0.06	-0.71
									(0.38)	(0.61)
Observations	9715	7517	7517	9715	3373	3229	3229	3373	2083	1172
R-squared	0.978	0.980	0.980	0.980	0.980	0.980	0.980	0.981	0.966	0.968
Lagged dep. var.	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Oth. aggr. controls	no	yes	yes	no	no	yes	yes	no	no	no
Country dums	yes	yes	yes	no	yes	yes	yes	no	no	no
Cou x time dums	no	no	no	yes	no	no	no	yes	yes	yes
Ind x time dums	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: OLS estimates. WS: wage share in value added. US Real: US reallocation rate, used as benchmark, divided by its average value. Nothing: no benchmark interacted with aggregate variables. EPR: index of employment protection for regular workers (excluding additional provisions for collective dismissals). EPRC: index of employment protection for regular workers (including additional provisions for collective dismissals). Av. gr. repl. rate (5y) (AGRR): average gross replacement rate over a 5-year unemployment period (in percentage). Av. net repl. rate (5y) (ANRR): average net replacement rate over a 5-year unemployment period (in percentage). PMR (sectoral): industry-specific index of anti-competitive product market regulation (set to zero in manufacturing). Other aggregate controls: tax wedge, corporatism dummies and trade-union density. Country dums: country dummies. IndXtime dums: industry-by-time dummies. CouXtime dums: country-by-time dummies. Robust standard errors, adjusted for clustering on countries and years, in parentheses. *, **, ***: statistically significant at the 10%, 5% and 1% level, respectively.

Source: OECD estimates.

Not all workers benefit from the dynamism of the labour market in the same way, however. Workers who separate from their employer against their will are likely to experience difficulties in finding a job with comparable pay and working conditions. Comparative data on dismissals are scarce. Yet, looking at the five countries for which they are available, it appears that, on average, about 5% of dependent workers are dismissed each year in high reallocation countries – such as the United States – against about 3% in middle-to-low reallocation countries – such as Germany (see OECD, 2009). Displaced workers typically suffer from substantive losses in terms of post-displacement earnings and working conditions. Several US

27. Specifications whose results are presented in Table 14 do not include controls for demographic characteristics. This is done to increase sample size. Nevertheless, specifications augmented by these co-variates are also estimated as a sensitivity exercise, from which it emerges that including those variables has no impact on the coefficients of institutions of interest.

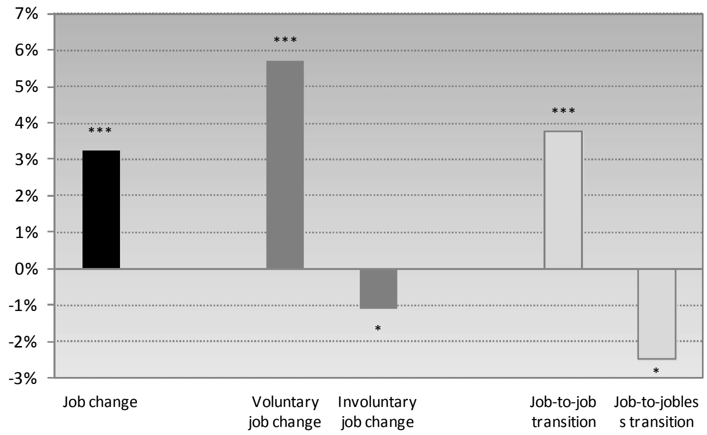
studies argue that displaced workers are more likely to end up in precarious jobs and, in general, tend to have much smaller earnings, once re-employed (see *e.g.* Podgursky and Swaim, 1987, Farber, 1999, 2003). Moreover, Kletzer and Fairlie (2003) show that significant wage losses persist for up to 5 years after displacement. In particular, immediate wage losses are greater in the case of older workers with long pre-displacement tenure, but young workers suffer from displacement in terms of wage growth prospects. Post-displacement wage and consumption losses are also observed for many European countries and Canada (e.g. Burda and Maertens, 2001, OECD, 2003, Houle and van Audenrode, 1995, Browning and Crossley, 2008). These effects persist even when sorting and selective mobility are taken into account (von Wachter and Bender, 2006).[28] The negative impact of job loss appears to be particularly large if it leads to protracted unemployment spells (Ruhm, 1991, Gregory and Jukes, 2001) and in the case of white collars (Schwerdt et al., 2010).

Overall, the empirical literature suggests that those workers who are dismissed or forced to leave suffer from significant wage and welfare losses. Figure 2 graphically highlights this conclusion for 13 EU countries (before-enlargement countries except Luxembourg and Sweden), using data from the European Community Household Panel for the period 1995-2001 (see Section 3 for specification and controls). Assuming that premia are the same for all countries, the chart shows that, while the average wage premium is almost 6 percentage points in the case of a voluntary job change, in the case of an involuntary separation wages after re-employment were, on average, about 1 percentage point smaller than what they would have been if the job match had not been destroyed.[29] In addition, and consistent with the "scarring" effect of unemployment, the wage loss at re-employment was about twice as large in the case of job-to-jobless transitions, no matter whether voluntary or involuntary.

28. Von Wachter and Bender (2006) find, however, that when sorting and negative selection are taken into account, young displaced workers experience significant wage losses only in the first five year after displacement.

29. Note that the fact of controlling for type of contract strongly reduce the estimated loss due to involuntary separation.

Figure 2 **Average wage premia to job change, 1995-2001**



Notes: Percentage-point estimated average differences between wages at the new and previous jobs, based on wage and salary employees only. Voluntary job changes occur when the reason to stop the previous job is that the worker obtained a better / more suitable job. An involuntary job change occurs when the reason why the worker stopped the previous job was: obliged to stop by employer or end of temporary contract. *, ***: statistically significant at the 10% and 1% level, respectively.

What are the implications of institutional reforms for these wage premia? Using the same difference-in-difference identification strategy used in most of this paper and the micro-data underlying Figure 2, it is possible to estimate the impact of EP for regular workers and UB generosity on the wage premium to job changes. However, in the case of individual wages, general-equilibrium effects might be more important[30] and caution must be exerted in interpreting the results.[31] Table 15 shows that EP for regular workers and average net replacement rates appear to have no significant effect on the difference in wage premia between industries with different US reallocation rates. However, EP appears to have substantially larger negative effects on inter-industry differences concerning both the wage premium to voluntary separations and the wage penalty at re-employment to involuntary separations, although significant (at the 10% level) only in the case of the former. In particular, a one-point increase in EP for regular workers appears to entail a one percentage point reduction in the difference in the wage premium to a voluntary separation between two industries whose reallocation rates, in the United States, differ by ten percentage points, which points to a substantial negative effect of EP on wage premia to voluntary separations.[32] Overall, this cautiously suggests that greater flexibility induced by EP reforms, by creating more job opportunities, improves career tracks for those in employment who wish to search for better jobs, and, conditional on displacement, does not worsen, and possibly improves, job perspectives for displaced workers.

By contrast an increase in UB generosity is associated with a lower wage penalty at re-employment, although the estimated effect is significant at the 10% level only, suggesting that greater UB generosity might improve match quality. Taking together this evidence supports a flexicurity model in which low EP is coupled with generous UBs. In such a system, workers are compensated for the greater risk of job loss through better hiring opportunities and a social protection system providing good insurance against the risk of a (temporary) unemployment spell and subsequent wage loss at re-employment.

30. For example, because of collective bargaining, wage increases in one industry are likely to boost wages in other industries.

31 . For this reason, aggregate effects are not derived.

32. Ten percentage points corresponds to the mean absolute deviation from the average reallocation rate.

Table 15. **Institutions and individual wage premia to job change**

	(1)	(2)	(3)
Dep. Variable		Log gross hourly wage (%)	
Benchmark:		US worker reallocation rate	
EPRC x number of job changes	-0.407 (0.433)		
NRR x number of job changes	0.013 (0.018)		
EPRC x number of voluntary job changes		-1.050* (0.607)	
NRR x number of voluntary job changes		-0.032 (0.030)	
EPRC x number of involuntary job changes		-0.829 (0.679)	
NRR x number of involuntary job changes		0.019 (0.022)	
EPRC x number of job-to-job changes			-0.504 (0.450)
NRR x number of job-to-job changes			0.008 (0.020)
EPRC x number of job-to-jobless changes			-1.300 (1.083)
NRR x number of job-to-jobless changes			0.057* (0.030)
Observations	142974	142974	142974
Country x year dummies	yes	yes	yes
Country dummies x number of job changes (by type as appropriate)	yes	yes	yes
Industry x country dummies	yes	yes	yes
Industry dummies x number of job changes (by type as appropriate)	yes	yes	yes
Age dummies	yes	yes	yes
Individual x education dummies	yes	yes	yes
Other controls	yes	yes	yes

Notes: Difference-in-difference OLS estimates on individual data. All covariates are multiplied by the benchmark divided by 10. For example the coefficient in the third row of the second column suggests that a one-point increase from the OECD average in the index of EP for regular workers (including additional restrictions on collective dismissals) compresses by 1.05 percentage points the difference of wage premia to a voluntary job change between two industries whose worker reallocation rates, in the United States, differ by 10 percentage points. Estimates are obtained by assuming that, in each industry, the impact of employment protection and unemployment benefits is greater, the greater the US reallocation rate for that industry. Estimates are based on 13 business-sector industries for pre-enlargement EU countries, excluding Sweden and Luxembourg. Other controls include type of contract and public/private sector. Robust standard errors in parentheses. *: statistically significant at the 10% level..

5. Concluding remarks

We have analysed the impact of specific policies and institutions on labour reallocation by using harmonised industry-level data for several OECD countries. This is of great relevance, insofar as previous empirical evidence suggests that labour reallocation is one of the main drivers of productivity growth. At the same time, previous research has shown that several labour and product market policies and institutions have a significant impact on productivity growth. The evidence we have presented in this paper sheds therefore some further light on the mechanisms through which labour reallocation shapes the relationship between these policies and institutions and productivity growth. In this respect, one of our main findings is that employment protection for regular workers (including additional restrictions on collective dismissals) significantly depresses gross worker flows, and its cross-country variation can explain up to 30% of the cross-country variation in total flows. By contrast, we find that generous unemployment benefits promote labour reallocation. However, the effect of employment protection is essentially limited to job-to-job flows, while the effect of unemployment benefits appears to vary greatly across ages. As a standard search-and-matching model would predict, in fact, unemployment benefits appear in fact to increase reallocation rates of youth, who are not eligible for benefits at the time of hiring, but reduce reallocation rates of the elderly.

Appendix: 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. More details on the procedure of harmonisation are available in OECD (2009).

Other benchmark variables, not based on reallocation data.

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.

The UK firm turnover rate is defined as the ratio of job creation by entry plus job destruction by exit to average employment. Data are from Hijzen *et al.* (2007).

The UK share of low-wage workers is the share of wage and salary employees working at least 30 hours per week with gross monthly wages less than two-thirds of the median wage in total workers, averaged over 1994-98. The source is the British Household Panel Survey module of the European Community Household Panel.

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. When data are also disaggregated by gender, age class and educational attainment classes, the share of temporary workers is obtained as the ratio of employees on temporary contracts divided by total employees in the same country, industry, age class, educational-attainment class, gender and year, excluding individuals with missing observations.

The wage share in value added is defined as the ratio of gross labour compensation in value added. It is from EU KLEMS except for Canada, Switzerland and Norway, for which it is from OECD STAN. For recent years, EUKLEMS data are extrapolated on the basis of predicted wage-share growth rates from OECD STAN.

Institutional variables

EP 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 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 EP stringency; see Venn, 2009). All indicators vary from 0 to 6 from the least to the most stringent.

UB generosity is measured on the basis of average 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). Net benefits are net of taxes and transfers, but exclude means-tested social assistance. The source is the OECD Benefits and Wages database.

The aggregate and industry-specific 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. See Woelfl *et al.* (2009) for more details on subcomponents.

Minimum wages are measured as the ratio of the statutory minimum wage to median wage of full-time workers, in percent. 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. The source of all these variables is the OECD Employment Database (*www.oecd.org/els/employment*).

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

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. Dummies are then constructed for each of these values of corporatism. The source of all these variables is Bassanini and Duval (2009).

Legal enforcement indexes come from Gwartney *et al.* (2008). The two retained indexes are subcomponents of the area “Legal Structure and Security of Property Rights” of the EFW index of the Fraser Institute and concern the “Integrity of the Judicial System” and “Enforcement of Contracts”. They vary from 0 to 10 from the lowest to the greatest degree of enforcement.

Individual data

All individual data are from the European Community Household Panel. Wages are gross hourly wages obtained as gross monthly earnings in the main job divided by 52/12 and then by usual weekly hours of work for employees working for at least 15 hours a week and not in education. Overtime pay and hours are included.

Table A.1. Gross worker flows by country and other non-institutional covariates, cross-section sample, 2000-07, percentages

	Total worker reallocation rate	Excess worker reallocation rate	Hiring rate	Separation rate	Jobless-to-job hiring rate	Job-to-job hiring rate	Job-to-job separation rate	Job-to-jobless separation rate	Same-sector separation rate	Other-sector separation rate	Employment-losing separation rate	Employment-quitting separation rate	Share of temporary workers	Share of workers aged 15-34 years
Austria	33.7	31.7	17.2	16.5	6.1	10.8	11.0	5.3	8.0	3.0	2.5	2.8	7.9	40.4
Belgium	33.2	30.8	17.0	16.1	6.8	10.3	11.1	5.2	6.3	4.8	2.6	2.5	7.0	38.3
Canada	48.7	45.8	25.2	23.4	. .	. .	. .	. .	. .	. .	. .	. .	11.1	42.6
Czech Rep.	30.6	27.4	15.9	14.8	7.3	8.6	8.7	6.2	4.9	3.7	2.7	3.5	8.4	38.5
Denmark	51.5	48.7	25.9	25.6	11.0	14.9	14.6	10.4	9.6	5.0	4.8	5.6	7.4	40.3
Finland	46.4	43.7	23.9	22.5	10.4	13.5	14.2	8.1	8.9	5.3	5.0	3.1	13.0	36.8
France	37.7	35.6	19.3	18.4	9.4	11.3	11.6	7.5	6.2	5.4	4.7	2.8	12.1	38.3
Germany	34.4	32.0	17.0	17.4	7.7	9.3	9.6	7.8	7.6	2.0	4.6	3.2	11.8	34.1
Greece	26.8	22.6	14.2	12.6	7.1	7.1	7.3	5.3	5.3	2.0	3.2	2.2	11.2	40.9
Hungary	28.7	23.3	15.4	13.4	7.2	8.2	7.5	5.9	4.4	3.1	3.0	2.9	6.6	41.1
Iceland	56.5	45.3	29.4	27.0	9.2	20.3	23.7	5.5	12.4	12.0	1.3	4.3	6.5	42.1
Ireland	42.0	37.0	22.4	19.6	. .	. .	. .	. .	. .	. .	. .	. .	3.8	49.5
Italy	28.6	25.6	15.5	13.1	7.5	8.0	8.3	4.9	5.7	2.6	2.4	2.5	10.2	38.8
Netherlands	40.9	38.5	21.4	19.5	6.4	12.5	13.4	3.8	. .	. .	. .	. .	15.5	43.6
Norway	34.7	32.0	17.0	17.7	6.3	10.8	13.4	4.6	5.5	7.9	1.6	2.7	7.2	40.7
Poland	40.1	37.4	20.7	19.4	12.3	8.4	8.6	10.7	5.9	2.8	7.0	3.7	29.1	43.7
Portugal	33.3	30.6	17.2	16.1	8.4	8.9	9.0	7.1	5.2	3.9	3.9	3.2	20.9	43.3
Slovak Rep.	28.8	23.1	15.6	13.2	8.6	7.1	6.7	6.5	4.2	2.5	4.0	2.5	4.9	42.2
Slovenia	30.0	22.8	15.7	14.3	6.5	9.2	9.5	4.8	7.1	2.0	2.8	2.0	16.8	39.1
Spain	47.1	42.8	25.5	21.7	13.4	12.0	12.1	9.4	8.2	3.9	6.2	3.2	29.8	44.7
Sweden	38.0	35.5	19.4	18.6	8.9	9.2	9.3	8.2	4.8	4.6	7.6	4.3	14.3	36.8
Switzerland	35.9	33.6	18.4	17.6	. .	. .	. .	. .	. .	. .	. .	. .	11.0	37.7
United Kingdom	45.1	42.2	22.4	22.6	7.7	14.7	. .	. .	. .	. .	. .	. .	4.5	39.3
United States	49.5	46.9	24.8	24.7	. .	. .	. .	. .	. .	. .	. .	. .	1.4	39.6

Table A.2. Institutional covariates, main cross-section sample, 2000-2007

	EP indiv. and collective dismissals	EP temporary contracts	UB net replacement rate (%)	Product Market Regulation	Tax wedge (%)	Collective bargaining coverage (%)	Corporatism	ALMP spending per unempl. as % of GDP per headd	Ratio of minimum to median wage (%)
Austria	2.7	1.5	59.1	1.8	39.7	95	3	18.8	. .
Belgium	2.4	2.6	63.6	1.6	45.8	90	3	21.9	51.2
Canada	1.6	0.3	21.2	1.1	26.6	32	1	6.6	40.3
Czech Rep.	2.9	0.7	22.5	2.0	34.6	25	1	4.0	37.5
Denmark	2.2	1.4	67.4	1.2	35.5	80	3	49.7	. .
Finland	2.3	1.9	64.5	1.3	39.4	90	3	13.4	. .
France	2.4	3.6	58.1	1.7	42.6	90	2	16.7	60.6
Germany	3.1	1.5	53.6	1.6	43.4	68	3	16.7	. .
Greece	2.6	3.9	19.2	2.1	39.4	. .	. .	. .	46.6
Hungary	2.2	0.9	16.7	2.0	44.5	30	1	7.6	48.3
Iceland	2.2	0.6	56.0	1.2	19.6	. .	. .	. .	. .
Ireland	1.8	0.4	52.8	1.3	15.0	85	3	25.2	54.8
Italy	2.7	2.2	6.2	1.8	40.2	80	3	12.0	. .
Netherlands	3.0	1.2	48.3	1.4	34.6	80	3	58.3	45.4
Norway	2.4	2.9	46.1	1.4	32.7	70	3	23.2	. .
Poland	2.5	1.8	46.7	3.0	42.1	40	1	3.7	42.9
Portugal	3.9	2.9	43.4	1.6	30.6	80	2	14.9	47.8
Slovak Rep.	2.8	0.5	18.3	1.6	33.5	50	1	2.8	43.7
Slovenia	3.1	1.9	. .	1.5	. .	. .	. .	. .	51.3
Spain	2.7	3.5	37.2	1.7	34.9	80	2	10.4	42.6
Sweden	3.1	1.6	44.5	1.5	44.3	90	2	35.7	. .
Switzerland	1.9	1.1	26.1	1.7	22.8	40	3	21.8	. .
United Kingdom	1.6	0.3	58.4	0.8	27.8	30	1	10.1	43.2
United States	0.9	0.3	5.7	1.0	23.6	14	1	4.8	33.1

Table A.3. **Main benchmarks, in percentage**

Isic Rev.1 code	Industry label	US worker reallocation	UK worker reallocation	Ciccone benchmark (EPRC)	Ciccone benchmark (UB)	US dismissals	UK firm turnover
15-16	Food , beverages and tobacco	39.3	38.0	49.2	46.5	3.6	8.8
17-19	Textiles, leather and footwear	45.6	46.0	57.0	51.2	9.0	14.0
20	Wood and manufacturing of wood and cork	43.7	40.2	51.2	49.8	6.1	9.6
21-22	Pulp, paper, printing and publishing	36.6	35.0	42.9	40.7	5.2	11.3
23	Coke, refined petroleum and nuclear fuel	40.1	24.4	44.2	44.7	5.0	5.0
24	Chemicals and chemical products	30.4	30.9	38.0	36.4	4.5	9.5
25	Rubber and plastics	35.8	39.8	45.4	41.9	5.0	7.8
26	Other non-metallic mineral products	38.7	35.0	46.2	40.1	4.8	9.2
27-28	Basic metals and fabricated metal	35.5	36.2	43.5	36.8	5.2	9.3
29	Machinery, not elsewhere classified	33.6	35.2	45.0	39.2	7.1	8.7
30-33	Electrical and optical equipment	37.0	38.9	48.7	46.8	7.6	8.2
34-35	Transport equipment	30.3	28.7	36.0	38.0	3.9	8.1
36-37	Other manufacturing; Recycling	43.5	40.9	54.2	47.1	6.4	15.5
40-41	Electricity, gas and water supply	18.3	33.2	24.6	27.3	2.2	11.6
45	Construction	58.6	44.8	63.7	60.6	8.6	16.1
50	Motor vehicles: sales and repair	59.5	43.6	65.2	46.4	3.9	13.8
51	Wholesale trade, except of motor vehicles	42.1	39.1	46.1	44.7	5.0	10.2
52	Retail Trade, except of motor vehicles	65.6	56.2	77.1	54.8	3.8	9.0
55	Hotels and restaurants	88.4	79.0	97.9	72.9	4.5	18.0
60-63	Transport and storage	42.6	39.8	46.4	40.3	4.6	9.2
64	Post and telecommunications	31.3	31.0	36.6	34.2	4.4	7.9
65-67	Financial intermediation	42.2	36.6	44.6	36.6	3.3	13.3
70	Real estate activities	49.3	38.1	51.8	45.6	3.4	18.6
71-74	Other business services	48.5	48.9	58.1	49.5	5.4	16.7

Table A.4. **Descriptive statistics, main time-series sample**

	Mean	Standard deviation
Total worker reallocation rate (%)	33.5	13.6
Excess worker reallocation rate (%)	30.4	13.7
Hiring rate (%)	16.9	7.3
Separation rate (%)	16.6	6.9
Share of temporary contracts (%)	9.2	7.0
Wage share in value added (%)	56.9	17.1
Share of workers aged 15 to 24 years (%)	12.5	6.8
Share of workers aged 25 to 34 years (%)	26.7	5.6
Employment growth gap (p.p.)	0.0	4.0
Regulation for individual dismissals	2.1	0.9
Regulation for temporary contracts	1.9	1.5
Regulation for indiv. And collective dismissals	2.4	0.6
average UB gross replacement rate (%)	28.8	12.7
average UB net repalcement rate (%)	39.7	20.4
Trade union density (%)	37.3	22.3
Tax wedge (%)	32.7	9.0

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