Job protection legislation and productivity growth in OECD countries

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1. INTRODUCTION AND OVERVIEW

During the past 15 years, labour productivity growth accounted for at least half of GDP per capita growth in most OECD countries, and a considerably higher proportion in many of them. As the populations of OECD countries age and the proportion of the population of working age falls, continued growth in productivity, along with enhanced participation by demographic groups currently under-represented in the labour market, will be crucial to maintain and improve living standards. As such, the role of policy in promoting or impeding productivity growth is likely to be of increasing importance in the decades to come.

The impact on productivity of structural reforms, such as tax reductions or product market deregulation, has been widely analysed from a theoretical perspective

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(e.g. Zagler and Durnecker, 2003; Aghion *et al.*, 2001) and has been the subject of a number of recent empirical investigations (e.g. Fölster and Henrekson, 2001; Nicoletti and Scarpetta, 2003). An increasing theoretical interest in the relationship between labour market institutions and productivity or productivity growth has recently manifested in the literature (e.g. Lagos, 2006; Wasmer, 2006). However, the empirical evidence on the impact of labour market policies and institutions on productivity is limited. As a result, structural labour market reforms are typically advocated on the grounds of fostering employment rates (e.g. OECD, 2006).

In the case of employment protection legislation (that is, the set of mandatory restrictions governing the recruitment and dismissal[1] of employees – EPL hereafter), however, there is little evidence of an aggregate employment impact (e.g. Nickell *et al.*, 2005, Bassanini and Duval, 2006). This could explain the burgeoning interest in other effects of EPL, including those on job turnover, firm dynamics and productivity, as a means of justifying reforms in this area on efficiency grounds. Yet, empirically, little is known about the productivity effects of EPL (see, for example, the June 2007 issue of *The Economic Journal Features*).

This paper makes a contribution to filling this gap by providing industry-level cross-country/time-series evidence on the impact of EPL on productivity in order to better inform policy action. Most of the existing evidence for OECD countries uses aggregate or semi-aggregate[2] regression analysis to examine the relationship between EPL and productivity, with inconclusive results (e.g. Nickell and Layard, 1999). Using aggregate cross-country/time-series data makes it possible to exploit the large variation in policies across countries and over time and examine general equilibrium effects. Yet, a key problem with aggregate analysis is that it is difficult to control for an exhaustive list of confounding factors. We circumvent this problem by exploiting the fact that cross-country comparable time-series data on productivity are available at the industry level and that, while EPL is defined at the aggregate level, its impact is likely to differ across industries. Within this context we use a difference-in-difference strategy in the spirit of Rajan and Zingales (1998).

The basic premise of our analysis is that EPL is more likely to be binding in some industries than others. Consider the partial equilibrium productivity effect of any change in EPL due to the behavioural response in reaction to the change in firing costs the employer expects to pay in the event of future layoffs ('direct impact on productivity' hereafter). If reforms of dismissal regulations have an impact on productivity, it will be greater in industries where, in the absence of regulations, firms rely on layoffs to make staffing changes, rather than in industries where internal labour markets or voluntary turnover are more important. By comparing cross-

[1] In this paper the terms firing, dismissal and layoff are used as synonyms to mean 'termination of a contract at the initiative of the employer'.

[2] By semi-aggregate analyses we refer to studies such as Autor *et al.* (2007), where, despite the use of firm-level data, the source of policy variation remains aggregate and the effect of policies is identified through cross-country (cross-state) and time-series variation only.

industry productivity differences in countries with different dismissal regulations we can draw substantial insights on the effects of EPL on productivity. In following this strategy, we control for all aggregate factors that are, on average, unlikely to have a different effect on productivity growth between EPL-binding and other industries and, in practice, use the latter as a control group for the former.

The paper is structured as follows. Section 2 briefly describes the diversity and evolution of EPL across OECD countries, highlights recent cross-country productivity growth patterns and discusses previous literature on the link between EPL and productivity growth. Section 3 presents the data and discusses the empirical set-up. Section 4 presents the main results, focusing mainly on the impact of dismissal regulations on cross-industry productivity differences, along with a battery of robustness checks, including dealing with endogeneity issues. Section 5 discusses the extent to which the results can be used to make inferences on the aggregate impact of EPL and considers several extensions, including the investigation of the effect of hiring regulations. Finally, a set of policy implications are presented in Section 6.

2. BASIC FACTS AND PREVIOUS LITERATURE

2.1. Cross-country trends in job protection and productivity growth

2.1.1. Employment protection legislation

Employment or job protection usually refers to the rules governing hiring and firing employees. In general, regular employment contracts do not specify the duration of the employment relationship. Employment protection regulations for regular contracts typically define conditions for termination of employment. In particular, they set conditions under which it is possible to lay off an employee (fair dismissal) and the sanctions in the case of breach of these provisions (unfair dismissal).[3] These regulations also detail the procedures that should be followed in the case of individual dismissal, which might include provisions for notice periods, involvement of third parties (such as courts, labour inspectorates, works' councils, etc.) as well as procedures for the employee to challenge the layoff decision. Finally, these regulations specify monetary compensations employees are entitled to, once dismissed (severance payments). Additional provisions exist in all OECD countries in the case of collective dismissals and typically include additional procedural inconveniences for the employer. Employment protection regulations also outline conditions under which workers can be hired on fixed-term or other types of contracts (such as seasonal contracts or project-related contracts). These rules usually concern the type of jobs and activities in which these

[3] For instance, in the US private sector, the 'employment-at-will' principle implies that it is usually fair to terminate an open-ended employment relationship without justification or explanation, unless in the case of discriminatory dismissal, explicit restrictions on terminations specified in the employment contract, or implicit long-term relationship implied by the nature of the job (such as a job related to a specific construction project like a bridge, a road, etc.). By contrast, in many continental European countries, dismissals for economic reasons are unfair if the employee could have been retained in another capacity.

contracts are allowed, their maximum duration, conditions for their renewal or termination of the employment relationship and possible employee compensation in the case of termination (see OECD, 2004, for a detailed description of employment protection regulations in OECD countries).

Employment protection regulations may be specified in legislation, collective agreements or individual employment contracts. Their operation in practice depends also on the interpretation of rules by courts or tribunals and the effectiveness of enforcement, which might vary over time and be influenced by external conditions such as the state of the economy (see e.g. Ichino *et al.*, 2003). However, there is little systematic information on average provisions specified in individual contracts and collective agreements in many OECD countries. With few exceptions, information on enforcement is similarly scattered. Therefore, cross-country comparable quantitative measures of the degree of stringency of employment protection that are available in the literature are essentially limited to mandatory legislative restrictions governing recruitments and dismissals – that is, to employment protection legislation (EPL).

In this paper, we quantify the degree of stringency of EPL by using three OECD indicators (OECD, 2004). The index for regular employment (referred to herein as EPLR) refers to individual dismissals and incorporates notification procedures, delays before the notice period can start, the length of the notice period and size of severance payments (both by duration of employment), the circumstances in which a dismissal is considered unfair, and compensation and extent of reinstatement following unfair dismissal. The index for temporary contracts (referred to herein as EPLT) incorporates restrictions on the number of contract renewals and maximum cumulated duration of fixed-term and temporary work agency contracts, as well as the circumstances under which temporary contracts can be used. The index on additional legislation concerning collective dismissals (referred to herein as EPLC) incorporates the definition of, and additional notification requirements for, collective dismissals, delays before the notice period for collective dismissal can start and other costs to employers, such as additional severance payments, retraining or redeployment of redundant workers. The scale of all indicators is 0–6 from least to most restrictive. Table A1 in Appendix 1 provides the scoring procedure and aggregation weights used to construct each index. Similar to other measures available in the literature, these indices generally measure legislative requirements, rather than their operation in practice, although judicial interpretation is incorporated to a limited extent (e.g. components measuring compensation and extent of reinstatement in the event of unfair dismissal take into account courts' decisions where this information is available). Dismissal regulations operating through collective agreements or individual contracts are not incorporated into the indices.

There is considerable variation in the stringency of EPL across OECD countries (Figure 1). Countries where EPL is particularly strict, such as France and Spain, generally have stringent regulations both on dismissals and on the use of temporary forms of employment. In contrast, in the United Kingdom and the United States there is

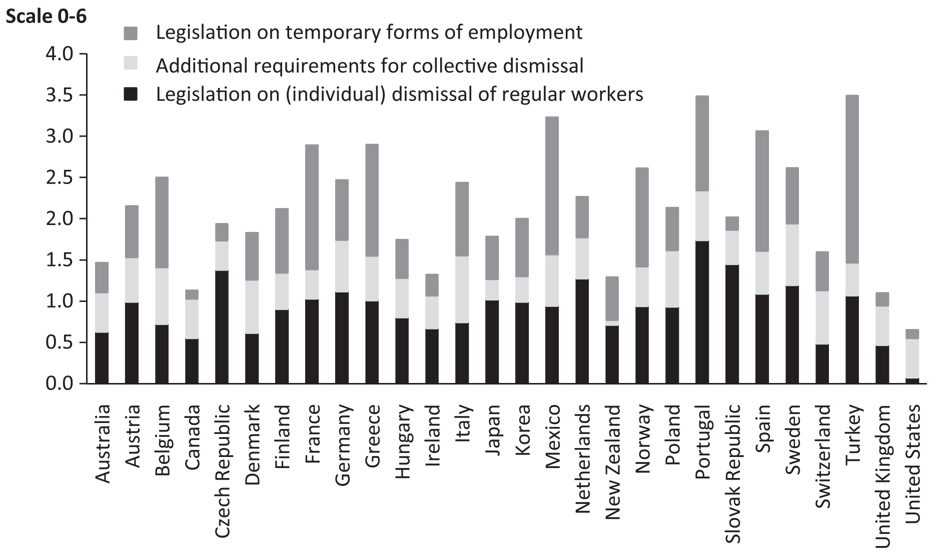

Figure 1. Summary index of EPL strictness and its components, including special provision for collective dismissals, 2003

very little regulation on either individual dismissal of regular workers or the use of temporary contracts. This does not mean, however, that the two types of regulations tend to have the same degree of stringency in all countries. In a number of Eastern European countries and the Netherlands, for example, a degree of flexibility close to the OECD average is obtained by allowing a relatively free use of temporary contracts in a legislative framework where dismissals are relatively difficult. There is also considerably less cross-country variation in the stringency of regulation on collective dismissals, and the inclusion of these additional provisions does not alter significantly the ranking of countries as regards the strictness of dismissal legislation.

Since the early 1980s, many countries have enacted legislation to reform their labour markets, including relaxation of employment protection provisions (Figure 2). Only a handful of countries have implemented reforms increasing job protection, and in most cases starting from relatively lax regulations. However, countries have chosen different routes to reform. Few countries have concentrated on regular employment contracts, while most of the reform action has fallen on rules for temporary contracts, whose liberalization typically raises less political opposition. There is no systematic information on rules for collective dismissals prior to 1998, so, following common practice, these are excluded from the time-series presented in Figure 2. Yet, scattered available information suggests that they have probably changed even less, on average, than regulations for individual dismissals (OECD, 2004).

2.1.2. Productivity growth

With a standard deviation as high as 0.9 percentage points in the past two decades, the cross-country variation of annual GDP per capita growth in the OECD area is

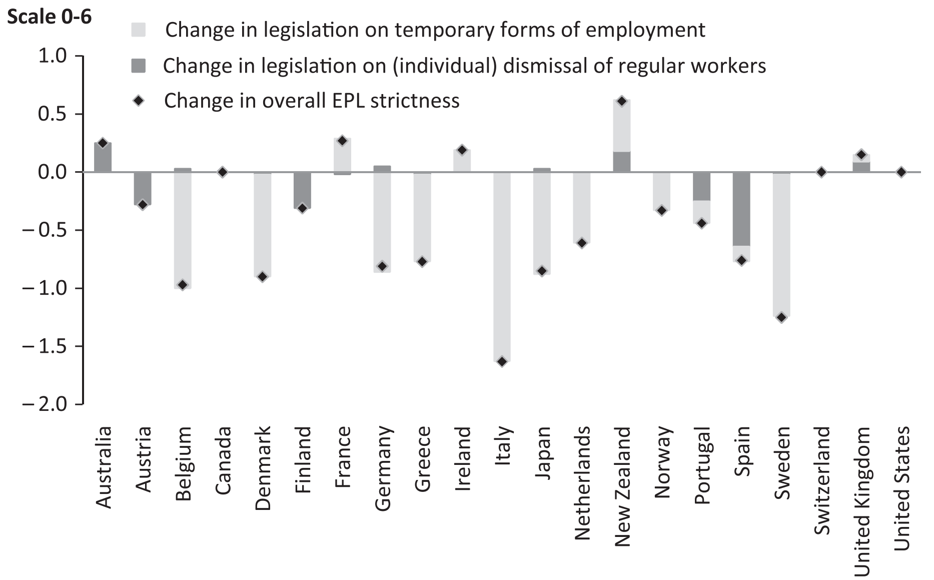

Figure 2. Changes in index of EPL strictness and contributions of its components, excluding special provisions for collective dismissals 1982–2003

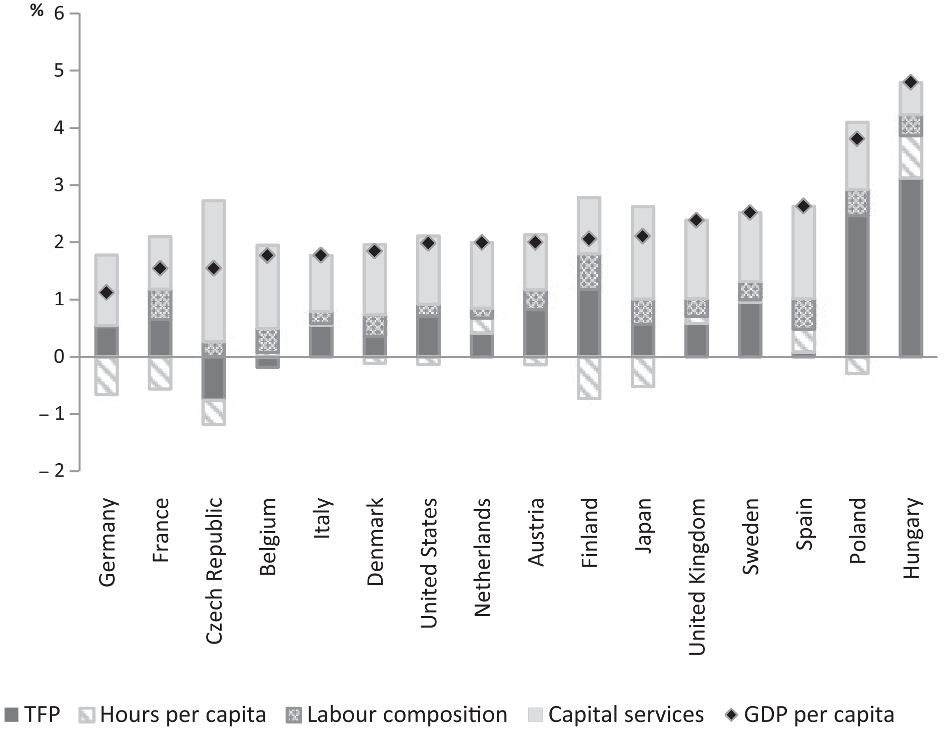

Figure 3. Annual average GDP per capita growth and contribution of its components, 1982–2003 (1985–2003 for Belgium, 1991–2003 for Germany, 1993–2003 for Sweden, 1995–2003 for the Czech Republic and Poland, 1995–2001 for Hungary)

remarkable (see Figure 3). GDP per capita growth can be decomposed into changes in hours worked per capita – that is, the contribution of total employment and demographic factors – and the growth of GDP per hour worked – commonly referred to as labour productivity. In a standard growth accounting framework, the latter can be decomposed further into the contributions of (1) changes in the quality and composition of labour; (2) capital accumulation; and (3) an unexplained residual. The residual of this decomposition is commonly called aggregate total factor productivity (TFP) growth. TFP growth, in principle, captures all efficiency improvements (notably technological change) that increase output for a given amount of labour and capital inputs. Long-lasting differences in TFP growth across countries will be reflected, in the long run, in differences in living standards.

In this paper, we will focus on TFP growth. Although on average capital service growth is the greatest contributor to GDP per capita (and labour productivity), Figure 3 shows that most of the cross-country variation in GDP per capita growth can be attributed to the variation of TFP growth across countries. The cross-country standard deviation of the latter is, in fact, twice as large as that of the contribution of capital services to GDP per capita growth. In other words, the cross-country variation in growth performance can mainly be attributed to cross-country differences in TFP growth. This basic fact motivates our interest in the role of country-specific institutions, and more specifically EPL, in determining cross-country differences in TFP growth.

As TFP growth is defined as the residual portion of output growth after accounting for growth in capital and labour, it will have a different meaning depending on the measure of capital inputs used. One method consists of deflating capital assets using quality-adjusted price indices and aggregating them using the user costs of each asset as weights, obtaining what is commonly called 'aggregate capital services'. This is the method used in Figure 3. In this case, the corresponding TFP growth measure captures disembodied technological and organizational improvements (innovations) that increase output for a given quantity and quality of inputs. Jorgenson (1966) argues that this is the only identifiable component of technological progress. We will call this measure 'fully-adjusted' TFP growth. Alternatively, a common method, often chosen in the literature for feasibility reasons (e.g. Nicoletti and Scarpetta, 2003; Griffith *et al.*, 2004), is to equate capital inputs to productive capital stocks, deflated at real acquisition prices and aggregated using nominal asset shares. Under certain restrictive assumptions, TFP growth computed with this method also captures technical progress embodied in newly adopted, higher-quality technologies, being therefore a proxy for total (embodied and disembodied) technological change (see Bassanini and Scarpetta, 2002, for a more detailed discussion). We will call this measure 'broadly-defined' TFP growth. To the extent that 'fully-adjusted' TFP growth can be more precisely identified and interpreted under more general assumptions, most of the analysis of this paper will be based on 'fully-adjusted' TFP. Anyway, we will show that our results are independent of the choice of the TFP measure.

2.2. EPL and productivity: theory and previous empirical evidence

How does EPL affect economic performance, in general, and productivity, in particular? From a historical point of view, EPL was typically designed to protect jobs and increase job stability, by reducing job destructions. As suggested by Pissarides (2001) among others, firing restrictions may be rationalized in the presence of financial market imperfections which limit the ability of risk-averse workers to get insurance against dismissal. However, by imposing implicit and explicit costs on the firm's ability to adjust its workforce to optimal levels, inefficient statutory dismissal protection may inhibit efficient job separations and, indirectly, reduce efficient job creation (e.g. Mortensen and Pissarides, 1994). In principle, inefficiencies implied by job security provisions could be offset by wage adjustments, private payments or the design of efficient contracts (Lazear, 1990). However, wage rigidities, financial market imperfections or uncertainty about the future of the firm may prevent these channels from operating. Nickell (1978), Bentolila and Bertola (1990) and Bertola (1990) describe firms' dynamic behaviour in the presence of positive firing costs, showing that the optimal strategy for firms is to reduce both hiring and firing, with an ambiguous effect on average employment over the business cycle. Regardless, stricter employment protection implies a slower speed of adjustment towards equilibrium. Labour market equilibrium models such as Garibaldi (1998) and Mortensen and Pissarides (1999) come to similar conclusions about job mobility being negatively affected by EPL.

The impact of EPL on the technical efficiency of production is less clear cut. The theoretical literature focuses almost exclusively on the role of dismissal restrictions, with little attention given to rules for temporary contracts. Stringent layoff regulations increase the cost of firing workers, thereby reducing the productivity threshold at which firms are willing to lay off workers. In addition, they make firms reluctant to hire new workers if they expect to make significant employment changes in the future. As such, EPL is likely to make it more difficult for firms to react quickly to rapid changes in technology or product demand that require reallocation of staff or downsizing, slowing the flow of labour resources into emerging high productivity firms, industries or activities. Under a general equilibrium framework, Hopenhayn and Rogerson (1993) show how the distortion induced by firing restrictions pushes firms to use resources less efficiently. As a result, employment levels adjust at a lower speed and productivity is reduced. Bertola (1994) presents a growth model where job security provisions decrease returns to investment and capital accumulation. Samaniego (2006) emphasizes the role played by industry composition. In a vintage capital model firms optimally reduce their workforce as they fall behind the technological frontier. As a consequence, firing restrictions are more costly in industries characterized by rapid technological change such as ICT. Countries where regulations are more stringent will therefore tend to specialize in industries where the rate of technical change is sluggish. Poschke (2007) emphasizes the role of firing costs in the selection of the most efficient firms and the exit decision of low productivity firms, if exiting firms cannot avoid paying them. However, if stringent EPL raises reservation wages, average productivity can increase simply because firms become more selective and less productive matches are not realized (Lagos, 2006).

Layoff protection might also affect productivity by reducing worker effort because there is less threat of layoff in response to poor work performance or absenteeism. Ichino and Riphahn (2005) provide an empirical estimate of this effect on a sample of Italian white collar workers, showing that the increase in job security represented by the end of the probation period induces a significant increase in absenteeism. Similar findings are obtained by Riphahn (2004) using German data.

Another channel through which EPL may affect productivity growth is by influencing the risk level that firms are willing to endure. Saint-Paul (2002) argues that high firing costs may induce secondary innovation that improves existing products rather than introducing riskier ones with larger productivity growth potential. Similarly, Bartelsman *et al.* (2004) suggest that stringent layoff regulations might discourage firms from experimenting with new technologies, characterized by higher mean returns but also higher variance, in order to avoid the risk of paying high firing costs. They provide some suggestive evidence consistent with this hypothesis by showing that the dispersion of productivity of young businesses and of businesses that actively change their technology is wider in the United States than in Germany, where firing costs are higher.

On the other hand, as argued by Koeniger (2005), layoff regulations could spur productivity-enhancing investments by incumbent firms in order to avoid downsizing. The net effect on aggregate innovation and productivity growth is however unclear, as strict regulations may also deter entry of innovative firms. Belot *et al.* (2007) propose a framework where, by providing additional job security, protection against dismissal may increase workers' incentives to invest in firm-specific human capital, therefore enhancing productivity growth. However, there is a trade-off between the positive effects induced by this channel and the burden implied by firing costs to be paid upon dismissals. As a consequence, it is possible to identify a strictly positive optimal level of employment protection which may depend on other institutions regulating wage rigidity and redistributive patterns. Under this framework, the gain from labour market deregulation may be larger for stricter levels of EPL. Similar considerations are suggested by Soskice (1997) and Hall and Soskice (2001) when comparing innovation patterns in Germany with those in the United Kingdom and the United States. While Germany mainly specializes in incremental innovation, the United Kingdom and the United States specialize in emerging radically new technologies. These two models require different types of labour market regulations, with stable and cooperative relationships between employers and employees supporting the incremental path. Haucap and Wey (2004) provide analogous policy implications when discussing the effects of wage-bargaining regimes on innovation, suggesting a potential policy trade-off between high employment and productivity growth when designing labour market institutions. Nevertheless, as suggested by Wasmer (2006), by inducing substitution of specific for general skills, firing restrictions may have a negative effect on productivity in the presence of major shocks, when workers need to be reallocated across industries, thereby making industry-specific skills useless.

The effects of changes in EPL on productivity may vary according to the specific dimension targeted by labour reforms. For example, Boeri and Garibaldi (2007) provide a dynamic labour demand model where reforms at the margin, such as those undertaken in many European countries in recent decades, have only a temporary effect on employment and productivity. Dolado *et al.* (2007) show instead how the effect of EPL reforms may vary according to the specific type of worker they are targeted at.

Looking at the empirical literature, the existing evidence on the relationship between EPL and productivity growth is mainly based on aggregate data and is not conclusive. For example, DeFreitas and Marshall (1998) find that strict EPL has a negative impact on labour productivity growth in the manufacturing industries of a sample of Latin American and Asian countries. On the other hand, Nickell and Layard (1999) and Koeniger (2005) find a weak positive relationship between EPL strictness and TFP growth and R&D intensity, respectively, for samples of OECD countries.

As far as we know, only three studies go beyond country-level 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 that capital investment is increased while employment flows, firm entry and TFP are reduced. However, they do not control for other possible institutional factors (state minimum wages, experience rating systems, etc.) that might have had a simultaneous effect on productivity. Similar findings are provided by Cingano *et al.* (2008) using Italian data to examine a 1990 reform that raised dismissal costs for firms with fewer than 15 employees only. In a study on EPL and job flows, Micco and Pages (2006) provide also some weak evidence of a relationship between EPL and productivity, using a difference-in-differences estimator on a cross-section of industry-level data for several OECD and non-OECD countries. They find a negative relationship between layoff costs and the level of labour productivity – albeit dependent on the presence of Nigeria in the sample. However, they cannot control for the effect of previous EPL levels, which might have an impact on productivity levels if dismissal regulations affect long-run productivity growth beside any direct effect on levels, as theory seems to suggest.

While not addressing the issue directly, many studies provide evidence on the channels through which EPL may affect productivity. There is a lot of evidence on the effect of EPL on job turnover. 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 EPL on job turnover and job destruction in particular. Similar findings are obtained by Schivardi and Torrini (2008), using an Italian matched employer-employee dataset, by Haltiwanger *et al.* (2006) and Micco and Pages (2006), on samples of 16 and 18 countries, respectively, and by Kugler and Pica (2008), who exploit the 1990 reform in Italy increasing firing restrictions for small firms. On the contrary, Bauer *et al.* (2007) do not find any significant effect of EPL on turnover using German matched employer-employee data. Finally, Messina and Vallanti (2007) find that EPL significantly dampens job destruction over the cycle with mild effects on job creation. The negative impact of EPL on job turnover, 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.

There is some support for the argument that EPL slows the speed of labour adjustment into new high-productivity firms or activities. Burgess *et al.* (2000) examine the relationship between EPL and the dynamics of output and employment, controlling for industry effects. They find that countries with stricter regulations have slower rates of adjustment of productivity to long-run levels. Similarly, Caballero *et al.* (2004) confirm a significant role of EPL in affecting the adjustment speed of employment in the presence of shocks using a cross-section of industry data for several countries. Using a growth model with constant returns to physical capital and diminishing returns to labour, they compute the implied effect on labour productivity growth, which they find large, especially in countries with strong rule of law. By contrast, they find only a minor effect on TFP growth.

Finally, analysing firm level data collected from 46 developing countries, Pierre and Scarpetta (2006) provide some empirical evidence showing that innovative firms are the most negatively affected by stringent EPL.

3. RESEARCH METHOD AND DATA

3.1. Empirical framework

As discussed in the previous section, the theoretical literature on the potential impact of job protection regulations on efficiency levels and productivity growth focuses mainly on the effects of dismissal regulations. We will therefore focus most of our analysis on these types of regulations, quantified by the index of employment protection legislation for individual dismissal of workers with regular contracts (EPLR). We will extend it to hiring and other regulations for temporary jobs (EPLT) in later sections.

In order to identify the effect of dismissal regulations on productivity we look at within-country growth differences between industries and over time. If EPLR has a direct impact on TFP – that is, if productivity is affected by the firms' response to changes in expected firing costs in the event of future dismissals, this effect (be it positive or negative) is likely to be larger in industries where dismissal regulations are more binding. We call these industries EPL-binding industries, which in turn are likely to be those industries that have a relatively high 'natural' propensity to adjust their human resources through layoffs, due to industry-idiosyncratic technological and market-driven factors.[4] For example, consider industries where firms need to lay off workers in order to restructure their operations in response to changes in technologies or product demand and/or in response to the failure of risky innovative ventures. In this case high firing costs are likely to distort efficient resource reallocation and/or discourage firms from undertaking risky projects. In contrast, in industries where firms can restructure through internal adjustments or by relying on natural attrition of staff, dismissal regulations can be expected to have little impact on the incentive to innovate and/or productivity levels. As a consequence the impact of EPLR on TFP is likely to differ across industries and we can investigate it by adopting a difference-in-differences approach.

In the simplest version of our difference-in-differences approach, we assume that differences in average TFP growth between EPL-binding and non-binding industries in any country at any point in time can be expressed as a function of the level of, and/or changes in, EPLR (see Box 1). The main advantage of this approach is that, in contrast with standard aggregate analysis, we can control for all unobserved factors that are unlikely to have different effects, on average, on productivity levels and growth rates in EPL-binding and other industries, including other institutions that have no direct effect on layoffs.

Box 1. Empirical specifications

In our simplest difference-in-differences approach, we assume that industries can be split into two groups – EPL-binding (b) and other (nb) industries – and that the direct effects of EPLR on TFP growth and levels, if any, are stronger in EPL-binding industries. This can be formalized as:

$$E[\Delta \log TFP_{ijt}] = (\alpha + I_b) f(EPLR_{it-1}, \Delta EPLR_{it}) + o_{ijt} \quad (1)$$

[4] Cross-country comparisons of data on job turnover (Haltiwanger *et al.*, 2006; Micco and Pages, 2006) and layoffs (Tables A3 and A4 in Appendix 1 of this paper) show that there is little cross-country variability in the ranking of industries according to their propensity to adjust on the external labour market, suggesting that country-invariant industry-specific factors shape this propensity. These factors could include technological characteristics of production processes, the type of knowledge management required by innovation and production activities and the dynamics of the global demand for the industry.

where $EPLR$ varies along the country i and the time t dimensions, I_b is the indicator function of the set of industries j where EPL is binding (a function equal to 1 in these industries and 0 elsewhere), 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 that we assume does not differ on average between the two groups. In other words, the expected average difference in TFP growth between the two groups can be modelled as a function f of EPLR and its change:

$$E\left[\overline{\Delta \log TFP_{it}^{b}} - \overline{\Delta \log TFP_{it}^{nb}}\right] = f(EPLR_{it-1}, \Delta EPLR_{it})$$

where the bar indicates an average over groups of industries. If we assume that f is linear in $EPLR$ and $\Delta EPLR$, we can estimate the following linear regression model consistent with the equations above (see Appendix 2 for the derivation):

$$\Delta \log TFP_{ijt} = \beta I_{bj} EPLR_{it-1} + \gamma I_{bj} \Delta EPLR_{it} + D_j + D_{it} + \varepsilon_{ijt}$$

where D stands for industry or country-by-time fixed effects (with respective dimensions indicated by subscripts), β and γ capture the effect of EPLR on between-group differences in TFP growth rate and level, respectively, and εs are standard disturbances. Note that, in Equation (2), country-by-time dummies control for all aggregate effects, including the average effect of $EPLR$ and $\Delta EPLR$, which are not therefore included in the specification. This implies that if indirect, general equilibrium effects of EPLR do not differ, on average, across industries, they too will be controlled for by country-by-time dummies. In that case, estimates of β and γ, reflecting direct effects, could be used to infer the direction of the average direct impact of EPLR on TFP growth and levels, going beyond simple cross-industry differences in the impact of EPLR. On the other hand, if the former condition does not hold, both direct and indirect effects will be reflected in the estimated coefficients, and inference on average impacts will depend on additional assumptions. We will come back to this issue in Section 5, when we will discuss implications of our estimates for the aggregate effect of dismissal regulations.

One may find it more plausible, however, that, rather than being entirely binding or entirely non-binding, the extent to which EPL affects each industry depends on the frequency at which firms in the industry would adjust human resources through layoffs in the absence of regulations. This implies that the impact of EPLR on TFP growth will be greater, the greater the industry layoff propensity. In a more general version of the same model, we can therefore specify Equation (1) as:

$$E[\Delta \log TFP_{ijt}] = (\alpha + g(\Lambda_j))f(EPLR_{it-1}, \Delta EPLR_{it}) + o_{ijt} \qquad (1')$$

where g is a non-negative and non-decreasing function of industry layoff propensity Λ. The difference in TFP growth between any pair of industries (indexed by k and h) can then be written as:

$$E[\Delta \log TFP_{ikt} - \Delta \log TFP_{iht}] = (g(\Lambda_k) - g(\Lambda_h))f(EPLR_{it-1}, \Delta EPLR_{it}). \qquad (2)$$

The simplest possible functional form that we can assume for g is the identity function ($g(x) = x$), in the spirit of Rajan and Zingales (1998). This implies that the linear regression model (2) becomes:

$$\Delta \log TFP_{ijt} = \beta\Lambda_j EPLR_{it-1} + \gamma\Lambda_j \Delta EPLR_{it} + D_j + D_{it} + \varepsilon_{ijt}. \qquad (2')$$

Equations (2) and (2′) can be augmented with specific control variables. In particular, the Schumpeterian growth literature suggests that appropriate models of productivity growth at the industry (or firm) level should include, as explanatory variables, the productivity growth of the industry productivity leader as well as the productivity gap (in level terms) between each observation and the industry productivity leader (Aghion and Howitt, 2006; Griffith *et al.*, 2004). This implies generalizing previous models as:

$$\begin{aligned}\Delta \log TFP_{ijt} = \psi_{ijt}\Delta \log TFP^W_{jt} - \phi \log RTFP_{ijt-1} + \beta\Phi_j EPLR_{it-1} \\ + \gamma\Phi_j \Delta EPLR_{it} + D_j + D_{it} + \varepsilon_{ijt}\end{aligned} \qquad (3)$$

where $RTFP$ denotes the ratio between TFP in industry j, country i and time t and the world productivity frontier for that industry, denoted with W, while Φ is either I_b or Λ – that is, the industry classifier, be it dichotomous or quantitative. The coefficient of frontier TFP growth ψ_{ijt} is assumed to be equal to a constant to be estimated, except for the industry productivity leader (for which it is constrained to be 0, see Appendix 2).

Industries are, however, in different stages of their life-cycle and exposed to different global demand dynamics. For instance, ICT-producing industries have experienced substantially faster-than-average productivity growth in most countries in recent years. In order to control for these developments, we include industry-by-time dummies in our preferred specifications. The general model we estimate can therefore be written as:

$$\begin{aligned}\Delta \log TFP_{ijt} = -\phi \log RTFP_{ijt-1} + \beta\Phi_j EPLR_{it-1} + \gamma\Phi_j \Delta EPLR_{it} \\ + X_{ijt}\delta + D_{jt} + D_{it} + \varepsilon_{ijt}\end{aligned} \qquad (4)$$

where X is a vector of other control variables that may or may not be included in different specifications. It is important to notice here that, in contrast with Equation (3), the growth rate of the industry productivity frontier is not included in Equation (4). In fact, being almost perfectly collinear with industry-by-time dummies, its effect cannot be identified, although it is, by and large, controlled for by these dummies.

The main advantage of our approach is that, in contrast with standard aggregate regression analysis, by including country-by-time dummies, we control for all unobserved aggregate institutions that are unlikely to have different effects, on average, on productivity levels and growth rates in EPL-binding and other industries, or, more precisely, whose effects are unlikely to be greater the greater the industry layoff propensity. To our knowledge, only Micco and Pages (2006) have applied a similar methodology, although to labour productivity data only. However, lacking the time dimension in their data, they identify the effect of EPL using productivity levels rather than growth rates. Yet, if EPL has an impact on TFP growth, beside an effect on efficiency levels – a possibility suggested by a few theoretical papers (see Section 2.2 above) – TFP levels are determined not only by current dismissal regulations, but also by regulations that were prevailing in the past. In a specification in levels, the impact of pre-sample EPL on TFP levels is unlikely to be captured by country dummies, since it is plausibly greater in EPL-binding industries. Therefore, an identification strategy based on TFP growth (that is, on first differences), which we follow, appears more cautious.

Given the limited time series variation in the indicators of EPLR (see Section 2.1 above), one limitation of our approach is that it is difficult to obtain a precise estimate of the effect of $\Delta EPLR$ in Equations (2)–(4), even when long-differences are used, as we do in a sensitivity analysis (see Appendix 3). In other words, one can argue that Equations (2)–(4) are likely to lead to reliable estimates of β only, while little can be said about γ. A key issue is how to interpret a significant estimate of β in (2)–(4) in the light of theory. Following the difference-in-differences logic outlined above, one would be tempted to interpret it as providing evidence that dismissal regulations have a long-run impact on productivity growth differences across countries. However, while this interpretation is possible, there are at least two reasons why the estimated coefficient might reflect an independent impact on productivity levels – that is, an effect that is not simply due to the impact of EPLR on long-run growth. First, level effects might materialize some time after a reform, so that they might not be captured by γ in an equation specified in relatively short differences. Second, as suggested by Equations (3) and (4), post-reform adjustment towards a new equilibrium might be slow. A reform affecting relative efficiency levels with respect to the productivity frontier might have a temporary effect on growth rates for many years without necessarily having a permanent growth effect. The conclusion is that, while a significant estimate of β suggests a significant effect of EPLR on TFP, one needs to look to other evidence to try to disentangle whether this reflects long-term growth or level effects, or both. Other caveats and limitations concerning the interpretation of coefficient estimates are discussed in Appendix 3.

In practice, however, it is unlikely that firing restrictions are either always binding or always not binding in a particular industry. Rather, whether and to what extent they are binding depends on the costs they impose on firms. These costs will be higher, the larger the firms' natural propensity to adjust through layoffs. To put it another way, if dismissal regulations have a direct effect on productivity, it is likely that that effect will be greater, the larger the natural layoff propensity of an industry. In the spirit of Rajan and Zingales (1998), we can therefore consider a slightly more sophisticated identification assumption, which still retains the advantages of the simplest difference-in-differences approach outlined above: we posit that, on average, the difference in TFP growth between any two industries in any country at any point in time can be expressed as a function of EPLR (and/or its change) that is greater, the greater the difference between the layoff propensities of the two industries (see Box 1). Aggregate implications of our cross-industry estimates will be discussed in Section 5.

3.2. Data

We use two closely related sources of data for TFP growth. Our main data source is the dataset used by Inklaar *et al.* (2008), which is derived from the consortium-only version of the March 2007 release of the EUKLEMS database and contains various measures of annual TFP growth and relative TFP levels with respect to the frontier for 11 OECD countries (Austria, Belgium, Denmark, Finland, France, Germany, Italy, the Netherlands, Spain, the United Kingdom, and the United States) over a period of about 25 years. More specifically, this dataset contains measures of both 'fully adjusted' and 'broadly defined' TFP for a number of manufacturing and non-manufacturing industries at a slightly more aggregate level than two-digits of the ISIC Rev. 3 classification. The second source of data is the public version of the March 2007 release of the EUKLEMS database, which contains industry-level data on 'fully adjusted' TFP growth for 16 OECD countries (those listed above, plus the Czech Republic, Hungary, Japan, Poland and Sweden), as well as data on value added, industry-specific purchasing power parities, capital service growth, employment, hours worked and labour composition by skills, age and gender that we use in certain specifications. As no data on TFP level are available in the public release of EUKLEMS, in most of the analysis we use TFP data from Inklaar *et al.* (2008) that contain such information. However, to increase country coverage, we re-estimate all our specifications that do not include a distance-to-frontier term using the public release of EUKLEMS.

In our baseline specifications we use industry-level US layoff rates – defined as the percentage ratio of annual layoffs to total employment – as a proxy for underlying layoff propensity in the absence of EPL. The United States appears a natural benchmark in this regard because dismissal regulations are very light in comparison with other OECD countries (the EPLR index is close to zero in the United States, see Figure 1 above). Industry classifiers based on layoff rates are likely to be more appropriate than those based on gross job turnover rates (sometimes used in the literature, e.g. Micco and Pages, 2006) insofar as we focus on dismissal regulations. This is because gross job turnover rates tend to be larger in expanding industries characterized by a high share of hires in total turnover (such as many service industries) and in industries that usually rely on voluntary quits rather than layoffs to adjust their human resources (such as hotels and restaurants or retail trade). Nevertheless, we use job turnover rates in a sensitivity analysis and to study the effect of regulations for temporary employment.

We compute layoff rates from the 2004 CPS Displaced Workers Supplement (covering layoffs in 2001–3). We use the 2004 CPS because it is the only wave with an industry classification that can be matched, at a sufficiently disaggregated level, to the ISIC classification that we use in our analysis.[5] We develop two baseline measures of industry layoff propensity: (1) a 'quantitative' indicator equal to the average industry layoff rate in the three years for which data are available (2001–3); and (2) a 'qualitative' indicator, in which EPL-binding industries are identified as those with a layoff rate above the average for all industries in each of the three years. One potential problem with this approach is that the composition of industries in terms of more disaggregate sub-industries may differ between the United States and other countries in our sample. In addition, US layoff 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). We cannot exclude the possibility that, despite very weak dismissal regulations, experience rating imposes significant additional costs on firms firing workers, which might differ across industries (depending on the choice of more or less risky development tracks by firms in each industry), thereby acting like endogenous additional firing restrictions.

In order to test the sensitivity of our results to the use of the US-based indicators, we re-estimate our main specifications using two similar measures of layoff propensity based on UK layoff rates and computed from the waves of the Quarterly UK Labour Force Survey in which data on redundancies are available (1997–2003). Dismissal regulations in the United Kingdom are the second laxest in the OECD area, after the United States (see Figure 1), making it an alternative natural bench-

[5] To match CPS data with our classification of industries, we adapt the mapping developed by OECD (2007) between the industry classification available in the CPS and the ISIC classification.

mark. Reassuringly, as shown in Appendix 1, US and UK average layoff rates appear to be closely correlated (see Tables A3 and A4).

Another key issue is whether the distribution of layoff rates is stable over time. Considering the limitations of our data, we check whether this is the case in three ways. First, we perform a simple analysis of variance to determine how much of the variation in the distribution of UK and US layoffs can be attributed to variation across industries rather than over time. We find that the industry dimension explains an overwhelming share of the variance (see Table A6). Second, for pairs of two-year periods between 1981 and 2004, we check the correlation between the corresponding industry distributions of US layoff rates for a less disaggregate classification of industries (see Table A5). It appears that the industry distribution of US layoff rates was reasonably stable over time. Third, we match our layoff data with US average gross job turnover rates from Haltiwanger *et al.* (2006), covering an earlier period (1991–6) for manufacturing and energy.[6] The distribution of US average job turnover rates in this period appears again to be strongly correlated to both the – more recent – distributions of US and UK layoffs (see Table A4). In addition, it appears that job turnover measures perform almost as well as average layoff measures in explaining the variation in layoff rates across countries, across industries and over time (see Table A6). Nevertheless, as a further sensitivity analysis, we replicate our main results using qualitative and quantitative industry classifiers computed from job turnover rates.

The baseline level of industry aggregation is an intermediate level between one and two digits of the ISIC rev. 3 classification. Our focus is on the non-agricultural business sector. For this reason, we exclude industries that typically have sizeable public sector employment, such as health care services or other business services, including research and development (see Table A3 for the list of industries). In addition, in our baseline specifications we exclude other industries where productivity is also likely to be heavily mismeasured ('Financial intermediation' and 'Coke, refined petroleum and nuclear fuel')[7] as well as 'Motor trade and repair' where average layoff rates are more likely to suffer from measurement error.[8] Nevertheless, we check that our main results are robust to the inclusion of these industries in a sensitivity analysis.

Aggregate cross-country comparable data on EPL and other institutions are mainly from OECD databases (Bassanini and Duval, 2006, Conway and Nicoletti,

[6] Although the original dataset covers the whole business sector, we limit the comparison to manufacturing and energy due to differences in the industry classification.

[7] See Crespi *et al.* (2006), Koszerek *et al.* (2007), and Inklaar *et al.* (2008) for a discussion of productivity mismeasurement in these industries.

[8] Given the level of aggregation of industries in the CPS, our CPS-ISIC mapping is approximated, with few of the CPS industries mapping exactly into an ISIC industry. The potential for measurement error concerning layoffs is particularly large in the case of 'Motor trade and repair' (ISIC 50), where potentially misclassified sub-industries make up 25% of the total employment of that industry.

2006, and OECD, 2007). Further details on data construction and sources as well as descriptive statistics are provided in Appendix 1. We exclude observations for Germany prior to and immediately following the reunification (up to 1992). Following a recent trend in the literature on institutions and aggregate unemployment (see Biagi and Lucifora, 2008, and the literature cited therein), in the base sample we also exclude observations for Finland in the year following the collapse of the Soviet Union (1992), which represented an unusually large idiosyncratic trade shock for this country. We check, however, that our results do not depend on the exclusion of these observations.

Our final 11-country base sample is slightly unbalanced and includes 19 industries that we follow for 21 years (1982–2003), for a total of 4,180 observations. The alternative 16-country sample is much more unbalanced but includes 5,139 observations.

4. RESULTS

4.1. The effect of dismissal regulations on 'fully-adjusted' TFP

We start our analysis by using the simplest difference-in-differences specifications (see Equations (2) and (2′) in Box 1 above) to estimate the impact of the degree of stringency of individual dismissal regulations (EPLR) on cross-industry differences in 'fully adjusted' TFP. In these specifications, we do not include additional controls for possible confounding factors and we use industries where EPL is less likely to be binding as a comparison group for EPL-binding industries, using industry classifiers based on US layoff rates. Table 1 presents the results obtained with both the 11-country base sample and the 16-country sample.

The table unambiguously shows that TFP growth tends to be smaller in industries with greater layoff propensity, the more stringent the level of EPLR. By contrast, changes in EPLR do not appear to have a significant effect, which suggests that we are unable to identify any independent short-run effect of dismissal regulations on the level of efficiency – that is, any short-run effect on efficiency levels that is not simply due to the impact of EPLR on long-run growth. However, as discussed in Box 1, we cannot exclude that, insofar as reforms in this area might show their effects only several years later, our estimates might reflect only a temporary impact on growth due to a long-run impact on TFP levels.

The main limitation of the exercise presented in Table 1 is that the role of possible confounding factors that vary across countries, industries and years is not taken into account. In particular, the Schumpeterian growth literature suggests that one should control for the productivity growth of the industry leader as well as the ratio of the TFP level in a specific country and industry to the TFP level of the leader of that industry – relative TFP hereafter (Aghion and Howitt, 2006;

Table 1. EPLR and TFP growth: simple difference-in-differences models

Sample and indicator of layoff propensity	11-country sample				16-country sample			
	(1) Qualitative	(2) Qualitative	(3) Quantitative	(4) Quantitative	(5) Qualitative	(6) Qualitative	(7) Quantitative	(8) Quantitative
Log relative TFP	−0.346**	−0.365**	−0.174***	−0.172***	−0.317*	−0.338*	−0.139**	−0.142**
	(0.170)	(0.167)	(0.055)	(0.054)	(0.181)	(0.178)	(0.060)	(0.059)
EPLR × Layoff	1.318		−0.130		1.693		0.215	
	(1.927)		(0.635)		(1.932)		(0.644)	
R^2	0.188	0.188	0.189	0.189	0.194	0.194	0.194	0.194

Notes: All estimates contain country-by-year dummies and industry dummies; the estimates for the 11-country sample are based on 4,180 observations; the estimates for the 16-country sample are based on 5139 observations. Robust standard errors in parentheses. ***, **, *: significant at the 1%, 5% and 10% level, respectively.

Table 2. EPLR and TFP growth: baseline Schumpeterian models

Panel A. Including the industry productivity frontier

Indicator of layoff propensity	(1a) Qualitative	(2a) Qualitative	(3a) Quantitative	(4a) Quantitative
Log relative TFP	−0.031***	−0.030***	−0.031***	−0.031***
	(0.004)	(0.004)	(0.004)	(0.004)
ΔlogTFP of the industry frontier	0.063***	0.063***	0.063***	0.063***
	(0.016)	(0.016)	(0.016)	(0.016)
EPLR × Layoff	−0.435***	−0.458***	−0.195***	−0.194***
	(0.166)	(0.164)	(0.054)	(0.053)
ΔEPLR × Layoff	1.579		−0.065	
	(1.841)		(0.627)	
R^2	0.208	0.208	0.210	0.210

Panel B. Including controls for differences in industry life-cycles

Indicator of layoff propensity	(1b) Qualitative	(2b) Qualitative	(3b) Quantitative	(4b) Quantitative
Log relative TFP	−0.039***	−0.038***	−0.039***	−0.039***
	(0.004)	(0.004)	(0.004)	(0.004)
EPLR × Layoff	−0.458***	−0.480***	−0.199***	−0.203***
	(0.163)	(0.160)	(0.052)	(0.051)
ΔEPLR × Layoff	1.593		0.290	
	(1.839)		(0.602)	
R^2	0.329	0.329	0.330	0.330

Notes: All estimates contain country-by-year dummies and industry by year dummies; the estimates are based on 4,180 observations. Robust standard errors in parentheses. ***: significant at the 1% level.

Griffith *et al.*, 2004). Results obtained by augmenting the specifications of Table 1 with these variables (see Box 1, Equation (3)) are presented in Panel A of Table 2. Both the growth of the productivity frontier and relative TFP appear to be significantly associated with observed TFP growth. The signs of both variables are as expected and estimated coefficients are within the range of estimates found in the previous literature (see e.g. Nicoletti and Scarpetta, 2003; Griffith *et al.*, 2004; Inklaar *et al.*, 2008).

Different industries are likely to be in very different stages of their life-cycles. For instance, in almost all countries employment grew faster in service and construction industries than in manufacturing and energy in the period under study. Nonetheless, certain manufacturing industries, such as electrical and optical equipment, experienced an impressive output boom, while more traditional low-tech industries, such as the agro-food and textile industries, underwent employment downsizing and productivity stagnation in most countries. It appears appropriate, therefore, to include further controls for industry-specific shocks and trends that are common across countries. This is done in the specifications presented in Panel B of Table 2 (corresponding to Equation (4) in Box 1). The inclusion of these controls has two main effects: on the one hand, it increases the share of sample variation that is explained by the model by about 50%; and on the other hand, it increases the estimate of the speed of convergence by about one-third.[9]

Table 2 confirms that TFP growth tends to be smaller in industries with greater layoff propensity, the more stringent the level of EPLR, while we cannot identify any effect on TFP levels. The estimated effect of EPLR appears greater than that estimated using the simplest difference-in-difference specifications (Table 1). In addition, results presented in Table 2 are quite stable across panels and specifications.

The estimated effect of dismissal regulations on TFP growth also appears to be significant from an economic point of view. For instance, consider a reform entailing a one-point reduction in the EPLR index, which roughly corresponds to (1) half of the difference between the OECD average and the United States; (2) the difference between the United States and the United Kingdom (the two least regulated countries in the OECD); and (3) the largest within-country time-series variation observed in the sample (in Spain, due to two reforms in the mid-1990s). Taking estimates based on the qualitative indicator of layoff propensity (Columns 1 and 2 in both panels) at face value, we can argue that such a reform would raise by 0.43–0.48 percentage points the relative TFP growth rate of EPL-binding industries − with US layoff rates above the average in all years for which our data are available − compared with that of other industries. A similar figure can also be derived using the estimates based on a quantitative indicator of layoff propensity (Columns 3 and 4 in both panels). In order to see this, note that the estimates based on the qualitative indicator reported above refer to industries that differ, on average, by 2.16 percentage points as regards average US layoff rates (see Table A3, Appendix 1). Taken at face value, estimates obtained using the quantitative indicator of layoff propensity, suggest that a one-point reform should increase by about 0.20 percentage points the difference in TFP growth between two industries whose average layoff rates differed by 1 percentage point. This in turn implies an effect of 0.43 percentage points in the case of two groups of industries that differ, on average, by 2.16 percentage points, such as between those that we labelled

[9] The TFP growth of the industry leader is by construction almost perfectly collinear with industry-by-time dummies and its effect is therefore not well identified. For this reason, we exclude this variable from the specifications presented in Panel B. However, re-estimating them including this variable has no consequence on the estimates concerning the other covariates, while yielding an excessively large and difficult to interpret coefficient for the growth of the leader (results not shown but available from authors on request). Another disadvantage of the specifications in Table 2 is that they can be estimated only on the 11-country sample. This is because, as already noticed, the level of TFP is not available in the public version of EUKLEMS. As an alternative, however, we can augment the specifications of Table 1 by including only controls for industry-specific shocks that are common across countries. This exercise is carried out in Table A13 in Appendix 4 and shows no evidence of lack of robustness.

EPL-binding using the qualitative industry classifier and the other industries.[10] Reassuringly, this suggests that the estimates obtained with alternative indicators are consistent.

Overall, the evidence presented in this section supports the idea that, in OECD countries, dismissal regulations disproportionately reduce the TFP growth rate of industries characterized by high layoff propensity with respect to other industries. However, before discussing the possible aggregate growth consequences and policy implications of this finding, we should challenge our results with further robustness checks, notably concerning their sensitivity to the choice of indicator of layoff propensity, the role of other confounding factors and the possible endogeneity of employment protection regulations. We examine these issues in the next subsection. Moreover, Appendix 3 provides additional robustness checks concerning serial correlation, possible longer lags in the effects of EPL, the possibility that the effect of EPLR is not a monotonic function of layoff propensity, the robustness to excluded observations, the sensitivity to the chosen measures of EPLR and TFP and the sensitivity to countries included in the sample. The latter is particularly important in order to show that our results are not driven by specific industry trends in particular countries.

4.2. Sensitivity analysis

4.2.1. Sensitivity to alternative indicators of layoff propensity

We use a number of alternative indicators of layoff propensity and EPL-binding industries to examine the robustness of our preferred specifications (Columns 2b and 4b in Table 2, Panel B). First, we look at the consequences of using relatively aggregate industries that might contain sub-industries with very different layoff propensity. As discussed in Section 3, insofar as their composition may differ across countries, the US distribution of layoff rates might not be representative of the cross-industry differences in layoff propensity in other countries. We question, therefore, how our results depend on the choice of US layoffs as a benchmark by replacing them with UK layoffs and checking the impact on our results (Table 3, Columns 1 and 2). Second, our layoff data are based on relatively few years of observations. Although we already noted that the distribution of layoffs appears to be relatively stable over time, certain industries have experienced specific shocks during the period for which our data are available. For instance, the UK energy

[10] As we highlight above, it is difficult to tell whether these measured effects represent permanent or transitory impacts on growth. One extreme alternative interpretation is to assume that, in the long run, dismissal regulations have only an impact on efficiency levels. In that case, we can view the specifications presented in Table 2 as variations of some sort of error correction model, with the long-run parameter of TFP of the industry leader constrained to one. Then one can compute the long-run relationship between EPLR and TFP levels by dividing the coefficient of EPLR by the coefficient of relative TFP, obtaining that a 1-point reform of individual dismissal regulations raises by about 5 percentage points the long-run difference in TFP levels between two industries whose average layoff rates differ by 1 percentage point. This still sounds significant from an economic point of view.

Table 3. EPLR and TFP growth: sensitivity to alternative indicators of layoff propensity

Indicator of layoff propensity	(1) UK average layoffs, qualitative	(2) UK average layoffs, quantitative	(3) US median layoffs, qualitative	(4) US median layoffs, quantitative	(5) UK median layoffs, qualitative	(6) UK median layoffs, quantitative	(7) US average job turnover, qualitative	(8) US average job turnover, quantitative
Log relative TFP	−0.038***	−0.038***	−0.039***	−0.039***	−0.037***	−0.038***	−0.043***	−0.044***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)
EPLR × Layoff	−0.430***	−0.106***	−0.741***	−0.146***	−0.714***	−0.091***	−0.574***	−0.046**
	(0.166)	(0.038)	(0.181)	(0.046)	(0.201)	(0.035)	(0.197)	(0.022)
R^2	0.328	0.328	0.330	0.329	0.329	0.328	0.369	0.368

Notes: All estimates contain country-by-year dummies and industry by year dummies; the estimates in Columns (1) to (6) are based on 4,180 observations; Columns (7) and (8) are based on 2,860 observations. For each industry, indicators of layoff propensity are as follows (by column): (1) 1 if the UK layoff rate is above the UK average for all industries in each year between 1997 and 2003 and 0 elsewhere; (2) 1997–2003 industry average of UK layoff rates; (3) 1 in industries where the US layoff rate is above the US median of all available industries in the sample for each of the years 2001, 2002 and 2003 and 0 elsewhere; (4) industry median of US layoff rates between 2001, 2002 and 2003; (5) 1 in industries where the UK layoff rate is above the UK median of all available industries in the sample for each of the years between 1997 and 2003 and 0 elsewhere; (6) 1997–2003 industry median of UK layoff rates; (7) 1 in industries where the US gross job turnover rate is above the US average of manufacturing and energy in each of the years between 1991 and 1996 for which data are available and 0 elsewhere; and (8) 1991–1996 industry average of US gross job turnover rates. Robust standard errors in parentheses. ***, **: significant at the 1% and 5% level, respectively.

industry experienced a major restructuring in the second half of the 1990s due to privatizations that brought about several plant closures and significant downsizing. Similarly, significant employment downsizing in the telecommunication and computer hardware industries occurred in the United States in the aftermath of the 2000 explosion of the internet bubble, particularly in 2001. Qualitative indicators of layoff propensity already take this into account, since only industries with layoff rates above the average in every year are labelled EPL-binding. As an alternative, however, we re-estimate our preferred specification using indicators based on medians that are less dependent on outliers than averages (Columns 3 to 6). Finally, we use a different time period. As we do not have data on layoffs by industry at our level of disaggregation for an earlier period, we derive alternative proxies for the industry layoff propensity from the 1991–6 US distribution of job turnover rates for the industries for which we have data (manufacturing and energy, see Section 3.2) and re-estimate our specifications with this set of indicators on the subsample where they are defined (Columns 7 and 8).

All the estimates of the impact of EPLR obtained using these alternative indicators are significant at conventional statistical levels. Estimates with UK quantitative indicators are smaller than the corresponding estimates obtained with US indicators, but no such finding emerges using qualitative indicators. Conversely, the differential impact of EPLR between EPL-binding and other industries is greater when EPL-binding industries are identified on the basis of median layoffs rather than average layoffs. Finally, even taking into account that turnover rates are more than three times larger than layoff rates (see Table A3 in Appendix 1), estimates using indicators based on US job turnover yield smaller estimates in the quantitative case but the opposite holds for the qualitative case. Overall, taking into account that one would expect lower and less significant estimates with industry classifiers based on less appropriate benchmarks (due to loss of information), these estimates appear remarkably consistent with our baseline results.

4.2.2. Sensitivity to inclusion of additional confounding factors

We argued that one of the key advantages of our difference-in-differences approach is that it allows us to control for other aggregate confounding factors, including other institutions and policies, some of which are not easy to quantify. This claim is correct provided that there is no reason to believe that the impact of aggregate institutions on productivity levels and growth varies, on average, between EPL-binding and other industries and/or proportionally to the industry layoff propensity. For institutions that have no direct bearing on layoffs, it is difficult to think of convincing reasons for such a differential impact. Yet, can we provide stronger evidence that this is the case? In order to do so, we augment our preferred specification with interactions between our baseline quantitative indicator of layoff propensity and levels and first-differences of several aggregate indicators of labour market institutions and product market regulations that are typically used in aggregate unemployment equations – the average labour tax wedge, the average unemployment benefit gross replacement rates (averaged across different durations and family situations), two dummies for high and intermediate levels of corporatism in collective bargaining, the share of workers covered by collective agreements (including administrative extension)[11] and a time-varying aggregate indicator of the degree of stringency of anti-competitive product market regulation, all drawn from Bassanini and Duval (2006) and defined more precisely in Appendix 1.

Two policies – tax wedge and unemployment benefits – appear to have an effect that is significantly different across industries with different layoff propensity in some specifications (Table 4). This is the case both in the most general model including the whole set of institutions defined above and in slightly simplified models where we do not simultaneously include all bargaining variables – since they essentially capture the same thing (Columns 1a to 3a). Does this result undermine our claim that these institutions have a similar impact on productivity in EPL-binding and other industries? Such a judgement would be hasty. It is important to remember that our empirical strategy exploits both cross-country and time-series variation in institutions and that cross-country correlations among labour market institutions are high in OECD countries. We therefore suspect that this result emerges because of multicollinearity. Indeed, this appears to be the case: if we implement a simple tournament in which we estimate all possible models with one and two institutions (interacted with our indicator of layoff propensity), the only institution whose coefficient appears to be significant in all models is EPLR, consistent with our priors. As shown in Table 4, the coefficient of unemployment benefits becomes insignificant in these simpler models (Column 4a), and the coefficient of the tax wedge is significant only when EPLR is also included (Columns 5a and 6a). Moreover no institution, except EPLR, turns out to be significant when we use qualitative indicators of layoff propensity (see Table A14 in Appendix 4).

Next we consider other possible covariates that are defined at the industry level. Standard models of TFP growth typically include R&D intensity to capture innovative effort (see e.g. Griffith *et al.*, 2004), particularly in models estimated for the manufacturing and energy industries only, where R&D statistics are widely available and not excessively plagued by measurement error. Some theoretical models also predict

[11] We use collective bargaining coverage rather than union density insofar as the latter is usually not comparable across countries (see OECD, 2004). A true time series for coverage is not available. However, there is evidence that it varies little over time; therefore, we include only its sample average by country. Similarly, the intermediate corporatism dummy does not vary over time in our sample. Conversely, since all other variables are also included in first-differences, we include also a first-differenced term for EPLR. Results obtained by dropping it are, by and large, the same. We also use a measure of the implicit tax to continuing work for workers aged 55 to 64 years as an additional covariate, in order to check for the potential effects of early retirement regulations. The inclusion of this variable reinforces our findings, with the estimated effect of EPL being larger. Our results are not shown in the table, however, since sample size is reduced by more than 40% when the implicit tax is included.

Table 4. EPLR and TFP growth: additional covariates. Quantitative indicators of layoff propensity

Panel A. Aggregate covariates	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
Log relative TFP	−0.040***	−0.040***	−0.039***	−0.039***	−0.039***	−0.038***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
EPLR × Layoff	−0.299***	−0.311***	−0.243***	−0.266***	−0.183***	
	(0.090)	(0.090)	(0.075)	(0.055)	(0.061)	
Tax wedge × Layoff	0.027**	0.027**	0.027**	0.025***		0.011
	(0.012)	(0.012)	(0.012)	(0.008)		(0.008)
Unemp. ben. × Layoff	−0.012*	−0.011*	−0.005		−0.003	
	(0.006)	(0.006)	(0.004)		(0.004)	
PMR × Layoff	−0.012	−0.046	−0.017			
	(0.122)	(0.097)	(0.116)			
High corp. × Layoff	0.511*	0.432				
	(0.291)	(0.268)				
Medium corp. × Layoff	0.458	0.416				
	(0.302)	(0.297)				
Coll. barg. coverage × Layoff	−0.003		0.001			
	(0.005)		(0.004)			
ΔEPLR × Layoff	0.145	0.149	0.172	0.146	0.355	
	(0.610)	(0.611)	(0.606)	(0.603)	(0.605)	
ΔTax wedge × Layoff	0.055	0.051	0.054	0.050		0.026
	(0.041)	(0.041)	(0.040)	(0.039)		(0.040)
ΔUnemp. ben. × Layoff	0.004	0.006	0.007		0.016	
	(0.034)	(0.034)	(0.032)		(0.031)	
ΔPMR × Layoff	0.259	0.242	0.270			
	(0.326)	(0.324)	(0.319)			
ΔHigh corp. × Layoff	−0.080	−0.115				
	(0.636)	(0.634)				
R^2	0.333	0.333	0.333	0.332	0.330	0.327

Panel B: Industry-level covariates	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)
Log relative TFP	−0.032***	−0.032***	−0.039***	−0.039***	−0.034***	−0.035***
	(0.007)	(0.007)	(0.004)	(0.004)	(0.007)	(0.008)
EPLR × Layoff	−0.331***	−0.332***	−0.214***	−0.213***	−0.404***	−0.459***
	(0.075)	(0.075)	(0.053)	(0.053)	(0.076)	(0.092)
ΔEPLR × Layoff	0.290	0.286	0.269	0.274	0.132	−0.267
	(0.736)	(0.736)	(0.600)	(0.599)	(0.736)	(0.984)
Log R&D intensity	0.524**	0.528**			0.636***	0.647**
	(0.208)	(0.212)			(0.210)	(0.258)
Log R&D intensity × Log relative TFP		−0.001				
		(0.005)				
PMR impact			−3.558**	−3.575**	−11.432***	−20.235
			(1.553)	(1.557)	(2.729)	(13.856)
ΔPMR impact			−7.630	−7.703	−3.437	−188.383
			(5.488)	(5.519)	(6.888)	(165.020)
PMR impact ×				−0.006		

Continued

Table 4. Continued

Panel B: Industry-level covariates

	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)
Log relative TFP				(0.018)		
ΔImport-weighted real exchange rate						2.345
						(6.074)
Observations	1904	1904	4180	4180	1904	1737
R^2	0.399	0.399	0.331	0.331	0.404	0.417

Notes: All estimates contain country-by-year dummies and industry by year dummies; the estimates are based on 4,180 observations. When interacted with one another, log R&D intensity, PMR impact and log relative TFP are expressed in deviation from the sample average. Robust standard errors in parentheses. ***, **, *: significant at the 1%, 5% and 10% level, respectively.

that R&D might speed up convergence to the productivity frontier (see Aghion and Howitt, 2006). We therefore re-estimate our baseline specification using data for the manufacturing and energy industries only, augmenting the model by the logarithm of R&D intensity and its interaction with relative TFP (Columns 1b and 2b in Table 4). Data on R&D intensity are drawn from the OECD STAN database.

Theory also suggests that regulation and competition are important determinants of productivity growth (see e.g. Aghion *et al.*, 2001). Ideally, we would like to include also industry-level indicators of product market regulations, since these regulations differ across industries.[12] However, these are available only for a handful of non-manufacturing industries, making their use within our estimation strategy unfeasible. We try to partially capture the contemporaneous impact of the product market regulatory framework in two ways. First, we include the regulation impact indicator of Conway and Nicoletti (2006), which is available for all industries. In each industry, this indicator measures the direct and indirect impact of regulatory barriers to competition in highly regulated industries, under the assumption that industries that buy more of the products of one upstream regulated industry are more affected by the lack of product market competition in that industry (Columns 3b to 5b in Table 4).[13] Second, we also include changes in the industry-specific import-weighted real exchange rate (from OECD, 2007) to capture changes in foreign competition in each industry (Column 6b).[14] All these covariates, except perhaps the latter, appear to attract the expected sign, even though they are not always significant, particularly

[12] As an alternative to regulatory indicators, mark-ups or Lerner indexes are also often used. However, their construction would require at least cross-country comparable capital stocks consistent with our TFP data, which we do not have. In addition, they are strongly endogenous, their level reflects more investment in intangibles than product market competition and even their change might badly capture changes in competitive conditions (see e.g. Boone, 2008).

[13] In low-regulated industries, this indicator quantifies only the likely impact on costs faced by firms that use the output of highly regulated industries as input. It is constructed from total input requirements derived from input-output tables (see Appendix 1) and differs from the aggregate one used in Panel A, where the same average score is applied to the whole economy.

[14] Industry-specific exchange rates can be included in first-differences only, since by construction their level is not comparable across countries (see Appendix 1 for details). In addition, they are available for manufacturing only.

in the case of terms interacted with relative TFP. The positive and insignificant sign for changes in the exchange rate is however consistent with Griffith *et al.* (2004). In all cases, and more relevant for the purpose of this paper, the estimated coefficient of EPLR does not appear to be affected in any noteworthy way.

4.2.3. On possible endogeneity of dismissal regulations

There is evidence that liberalization reforms are easier to implement and occur more frequently in bad economic times (e.g. Drazen and Easterly, 2001), and one can imagine that this argument applies to reforms of dismissal regulations. On the other hand, one can argue that, since dismissal restrictions slow job destruction and reduce unemployment risk for the insiders, political pressure to maintain or increase them will be higher during major downturns. The fact that dismissal regulations were tightened dramatically in the aftermath of the productivity slowdown and economic crisis of the 1970s in some countries (see OECD, 1999) corroborates this alternative view. Up to now, we have treated estimated coefficients of EPLR, interacted with indicators of layoff propensity, as evidence of a causal impact of regulations on cross-industry TFP growth differences. Do these political economy arguments point to the possible endogeneity of dismissal regulations and imply that our causal interpretation is unwarranted? When average TFP growth is lower than usual, EPLR may be more likely to fall or increase, depending on the mechanism that is assumed. Our identification strategy, however, controls for these types of aggregate effects through country-by-time dummies, provided that aggregate downturns do not strike more strongly, on average, in industries with high layoff propensity, which looks *a priori* unlikely.[15] Therefore, it seems fair to conclude that the potential feedbacks outlined above have no bearing on the interpretation of our estimates.

There is, however, a more subtle political economy argument that can be put forward and that is potentially more problematic. Suppose that dismissal regulations do not affect productivity growth but only profits, and that they do so particularly in industries where EPL is binding. It is not inconceivable that industries that are expanding are also more effective in lobbying for their interests. As a consequence, due to lobbying pressure only, EPLR would tend to be lower in countries where EPL-binding industries grow faster, and our estimated coefficients might simply measure this correlation. In order to address this issue of causality, we need to find instruments that can predict the level of EPLR without affecting directly the difference in productivity growth between EPL-binding and other industries.

[15] Simple estimations of specifications including an output gap term provide additional support for this view: if the aggregate business cycle had a differentiated impact on binding and non-binding industries, we should expect to find in our regressions a significant effect of an interaction term between a measure of the output gap and the indicators of layoff propensity. However, when included, this term turns out to be always statistically insignificant suggesting that the correlation between aggregate shocks and industry TFP growth does not differ, on average, between high and low layoff propensity industries.

First, we can look at the characteristics of the legal system. 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. In contrast, most civil law systems, and particularly those with a single codified civil code, tend to minutely regulate (see, for example, House of Lords, 2007). One would therefore expect more lenient dismissal regulations in common law countries and more constraining regulations in countries under civil law with a civil code tradition.[16] Scandinavian countries with no consolidated civil codes and a customary law tradition will be a somewhat intermediate case (see Lando, 2001, and Smits, 2007). In fact, from a historical point of view, in Denmark, Finland and Sweden, employment protection rules were introduced first through collective agreements, with a few of them being reflected in legislation only subsequently (Sigeman, 2002). Next, we can look at countries that experienced dictatorships in the 20th century (excluding during World War II, when most European countries were under puppet pro-Nazi regimes). Due to their paternalistic view of labour relationships, pre-WWII fascist regimes were historically inclined to guarantee workers strong protection against dismissals, albeit within a strict industrial relation system with no voice rights.[17] Stringent regulations generally survived the fall of these political regimes. All these historical and institutional factors pre-date EPL (by more than one century, in the case of legal systems), thereby limiting the risk of reverse causality. True, one can argue that they could also be at the origin of other institutions affecting productivity and/or could have a long-lasting effect on productivity themselves. This is not a problem, however, if we interact these variables with the corresponding indicators of layoff propensity used for EPLR and use the interacted variables as instruments, as we do. In fact, these interacted variables appear to qualify as valid instruments to the extent that we cannot think of any economic mechanism inducing an effect of legal systems or dictatorship spells on productivity that varies across industries as a function of layoff propensity without occurring through their effect on dismissal regulations. Obviously, the validity of our instrumental variable strategy crucially hinges on the validity of this latter statement.[18]

[16] In addition, in common law countries case law might introduce *de facto* restrictions in the absence of legislation. In many US states, for instance, wrongful discharge in violation of public policy, such as because the employee has served on jury duty, is a commonly accepted exception to the employment-at-will doctrine even if it is not always written in a specific statute. This aspect of common law systems, however, represents an important source of measurement error, insofar as it implies that in common law countries EPL understates the effective stringency of regulations. Anyway, our instrumental variable strategy is likely to address this issue. In addition, its importance for our empirical analysis should not be exaggerated: as shown in Table 5 below, EPLR remain significant when instrumented with indicators of legal systems only, which would not occur if the variation of EPLR induced by the variation of legal systems had no effect on productivity.

[17] For example, notice periods were introduced early in Italian legislation by the Mussolini government (Royal Decree 13 November 1924, n. 1825), dismissals required government authorization in the Third Reich (see Shirer, 1960) and were in practice very difficult in Franco's Spain (see Teixeira, 2001).

[18] Anyway, overidentification tests presented in Table 5 provide some empirical support for it.

The disadvantage of these instruments is, however, that they are time invariant. A time-varying instrument can be constructed by looking at the political colour of governments, insofar as one can expect leftwing governments to be more inclined to maintain or increase the stringency of EPL. In certain specifications, therefore, we use the Schmidt index of cabinet composition – drawn from the Comparative Political Data Set (CPDS, see Armingeon *et al.*, 2005), which varies between zero and five from least to most leftwing. As with the other instruments, we interact this variable with indicators of layoff propensity.[19]

Results of instrumental variable regressions are reported in Table 5. Columns 1 to 4 in each panel report results obtained by re-estimating the preferred specification using instruments based on, respectively, the civil law/common law dichotomy; a refinement including information on civil codes; dictatorship spells; and all of the above plus cabinet composition. Two elements stand out from the first four columns of the table. First, the estimated coefficient of EPLR is always significant and not far from baseline estimates (see Table 2). In addition, we find no or little evidence suggesting that EPLR, interacted with indicators of layoff propensity, is endogenous. This downplays the importance of the lobbying argument.[20]

One problem with the estimates presented in the first four columns of Table 5 is that relative TFP, even if predetermined, is likely to be endogenous to productivity growth. In fact, other factors, not included in the model, might simultaneously affect both the productivity gap with the leader and TFP growth. Unfortunately, it is difficult to find variables that affect relative productivity levels without affecting productivity growth directly. As an alternative strategy, we exclude relative TFP from the specification and re-estimate it using our instrumental variables. Point estimates turn out somewhat smaller (Column 5), but they are broadly in line with those presented in Table 1 (and Table A13), confirming previous results. Finally, as most of our instruments are time-invariant, the number of countries in the sample may play a role. As an additional robustness check, using data from the 16-country sample for the countries for which our instruments are available, we re-estimate the specification of Column 5 (the only one that can be estimated on the 16-country sample), which again supports our main findings (Column 6).

[19] To the extent that lobbying activity might influence electoral campaigns, it might be possible that countries tend to elect more frequently rightwing governments where EPL-binding industries grow faster: companies operating mainly in these industries could in fact throw their weight into electoral campaigns in order to support parties that will take a favourable stance as regards EPL. In this case, therefore, this instrument would not qualify as a valid instrument. Although the likelihood of this argument appears limited, we use this instrument only in combination with other instruments, in such a way that we can rule this counter-argument out by means of overidentification tests. See Appendix 4, Table A15 for first-stage estimates.

[20] Since specification tests do not show any sign of endogeneity of EPLR, interacted with indicators of layoff propensity, we can also test the consistency of our instrumental variable strategy in one additional way. We can augment our preferred specifications (Table 2, Columns 2b and 4b) with our instruments and re-estimate them by OLS. If the interacted EPLR variable is not endogenous, OLS estimates will be consistent. Therefore, if our instruments are valid instruments that fulfil the orthogonality condition, as we argued above, we would expect them to have an insignificant impact on TFP growth in these augmented specifications. This indeed turns out to be the case (results not shown but available from authors).

Table 5. EPLR and TFP growth: instrumental variable estimates

Panel A. Qualitative indicator of layoff propensity

Instruments used	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
	Legal systems	Legal systems (refined)	Dictatorship	All	All	All
Log relative TFP	−0.038***	−0.038***	−0.039***	−0.038***		
	(0.004)	(0.004)	(0.004)	(0.004)		
EPLR × Layoff	−0.477**	−0.495***	−0.744**	−0.489***	−0.319*	−0.332**
	(0.191)	(0.180)	(0.318)	(0.174)	(0.179)	(0.169)
Overid. score test (*p*-value)		0.779		0.746	0.675	0.937
Endog. score test (*p*-value)	0.981	0.895	0.333	0.925	0.708	0.986
1st-stage residual test (*p*-value)	0.982	0.903	0.371	0.931	0.729	0.987
F-test on instruments (*p*-value)	0.000	0.000	0.000	0.000	0.000	0.000
Observations	4180	4180	4180	4180	4180	4683
R^2	0.329	0.329	0.328	0.329	0.306	0.306

Panel B. Quantitative indicator of layoff propensity

Instruments used	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)
	Legal systems	Legal systems (refined)	Dictatorship	All	All	All (16-country sample)
Log relative TFP	−0.038***	−0.038***	−0.039***	−0.038***		
	(0.004)	(0.004)	(0.004)	(0.004)		
EPLR × Layoff	−0.129**	−0.158***	−0.319***	−0.157***	−0.120**	−0.107*
	(0.062)	(0.058)	(0.098)	(0.056)	(0.057)	(0.057)
Overid. score test (*p*-value)		0.158		0.195	0.232	0.247
Endog. score test (*p*-value)	0.084	0.195	0.181	0.135	0.090	0.227
1st-stage residual test (*p*-value)	0.109	0.230	0.215	0.167	0.117	0.261
F-test on instruments (*p*-value)	0.000	0.000	0.000	0.000	0.000	0.000
Observations	4180	4180	4180	4180	4180	4683
R^2	0.330	0.330	0.329	0.330	0.308	0.307

Notes: All estimates contain country-by-year dummies and industry-by-year dummies. Score tests are Wooldridge's (1995) robust score tests. The 1st-stage residual test for exogeneity is the t-test on the estimated coefficient of the 1st-stage residual in augmented OLS specifications. Robust standard errors in parentheses. ***, **, *: significant at the 1%, 5% and 10% level, respectively.

5. FURTHER QUESTIONS

5.1. Industry-level labour productivity growth

Does the effect of EPL on cross-industry TFP differentials translate into an impact on cross-industry labour productivity growth gaps? The question is legitimate. On

Table 6. EPLR and labour productivity growth

Indicator of layoff propensity	(1) Qualitative	(2) Quantitative
Log relative productivity	−0.020***	−0.020***
	(0.004)	(0.004)
EPLR × Layoff	−0.559***	−0.174***
	(0.171)	(0.057)
ΔEPLR × Layoff	1.281	0.024
	(1.883)	(0.561)
R^2	0.328	0.328

Notes: All estimates contain country-by-year dummies and industry-by-year dummies; the estimates are based on 4,180 observations. Robust standard errors in parentheses. ***: significant at the 1% level.

the one hand, a few papers in the literature find that EPL tends to increase capital accumulation and has therefore ambiguous effects on labour productivity growth (Autor *et al.*, 2007, Cingano *et al.*, 2008). This might reflect the fact that when firing restrictions are stringent firms have an incentive to substitute capital for labour. On the other hand, a number of theoretical papers predict that strict regulations compress incentives to undertake any type of investment, including physical capital accumulation (see Section 2.2). In order to explore this issue, we replicate our baseline model by replacing TFP with labour productivity (defined as real value added per hour worked) as the dependent variable. Table 6 shows that the estimated impact of EPLR on cross-industry labour productivity growth differences is about the same as on TFP. Taken at face value, estimates obtained with the quantitative indicator of layoff propensity suggest that a one-point EPLR reform should increase by about 0.17 percentage points the difference in labour productivity growth between two industries whose average layoff rates differed by 1 percentage point. Using the qualitative indicators, the estimated effect is somewhat larger (0.56 percentage point greater growth in EPL-binding industries than in other industries).

5.2. Aggregate productivity growth

What do these estimates imply for the aggregate impact of dismissal regulations on aggregate productivity growth? As shown in Appendix 2 (see Equation (A2)), the effect of EPLR on aggregate productivity growth can be approximated as:

$$\frac{d\dot{\Pi}}{dEPLR} \cong \theta_b \frac{\partial(\dot{\pi}_b - \dot{\pi}_{nb})}{\partial EPLR} + \frac{\partial \dot{\pi}_{nb}}{\partial EPLR} + \frac{\partial R}{\partial EPLR}$$

where Π and π are aggregate and industry-level labour productivity, respectively, θ_b represents the value added share of binding industries, the dot indicates growth rates and b and nb stand for binding and non-binding industries, respectively.

Finally, $\partial R/\partial EPLR$ represents the contribution to changes in aggregate productivity of the reallocation of labour and value added across industries induced by changes in EPLR. This reallocation effect might occur, for instance, as a result of the impact of dismissal regulations on the shares of high-productivity industries in total employment. The estimates presented above provide strong evidence on the sign and magnitude of the first term on the right-hand side, which appears to be negative: dismissal regulations depress *within-industry* TFP and labour productivity growth in industries with high layoff propensity *relative* to industries with low layoff propensity. However, in order to derive the impact of EPLR on aggregate labour productivity growth we need to make additional statements on (1) the impact of dismissal regulations on *absolute* productivity growth in non-binding industries and (2) the effect of EPLR on industry composition.

Let us start with the reallocation effect. It can be shown that, in order to evaluate that effect, we need to estimate the impact of dismissal regulations on both employment and real value added shares, where employment is expressed in terms of total worked hours (cf. Equation (A2) in Appendix 2). A thorough analysis of this issue is beyond the scope of this paper. Nevertheless we can obtain a rough estimate of these impacts by adopting for the growth rate of employment (or real value added) the same difference-in-differences strategy we apply for TFP and estimating a specification similar to our baseline model (Equation (4) in Box 1) by substituting employment (or value added) for TFP.[21] Given that common aggregate effects are controlled for in the specification by means of country-by-time dummies and shares add up to one, our estimated coefficients can also be interpreted as estimates of the effect of EPLR on the rate of change of employment (or value added) shares.[22]

Columns 1 and 2 in Table 7 show that EPLR appears to have no significant impact on employment shares. As shown in Appendix 2 (see Equation (A3)), if EPLR does not affect employment shares, its impact on aggregate productivity growth can be further simplified as:

$$\frac{d\dot{\Pi}}{dEPL} \cong \theta_b \frac{\partial(\dot{\pi}_b - \dot{\pi}_{nb})}{\partial EPL} + \frac{\partial \dot{\pi}_{nb}}{\partial EPL} + ((\dot{\pi}_b + \dot{n}_b) - (\dot{\pi}_{nb} + \dot{n}_{nb}))\frac{\partial \theta_b}{\partial EPL} \tag{5}$$

where n stands for the rate of change of employment shares, defined as above. In other words the sign and magnitude of the reallocation effect depend on the impact of EPLR on the value added share of EPL-binding industries and on the relative

[21] In other words, in the qualitative case we assume that differences in average employment or value added growth between EPL-binding and non-binding industries in any country at any point in time can be expressed as a function of the level of and/or changes in EPLR. Conversely, in the quantitative case, we assume that, on average, the difference in growth rates between any two industries in any country at any point in time can be expressed as a function of EPLR (and/or its change) multiplied by the difference between the layoff propensities of the two industries (see Section 3.1).

[22] To see this, note that: (1) as shares add up to 1, looking at cross-industry differences suffices to derive implications for the absolute change of shares; and (2) the logarithm of the industry share of any variable can be written as the difference between the logarithms of industry-level and aggregate values of that variable ($\log(Y/\Sigma Y) = \log Y - \log \Sigma Y$). The latter, being collinear with country-by-time dummies, does not affect the estimated coefficients.

Table 7. Effects of EPLR on employment (hours worked) and value added

Dependent variable and indicator of layoff propensity	Total hours worked		Real value added	
	(1)	(2)	(3)	(4)
	Qualitative	Quantitative	Qualitative	Quantitative
Lagged dependent variable	−0.002	−0.002	−0.004*	−0.004*
	(0.002)	(0.002)	(0.002)	(0.002)
EPLR × Layoff	0.187*	0.033	−0.315*	−0.127**
	(0.104)	(0.033)	(0.165)	(0.054)
ΔEPLR × Layoff	0.193	0.630*	1.327	0.631
	(1.215)	(0.369)	(2.015)	(0.658)
R^2	0.570	0.570	0.416	0.417

Notes: All estimates contain country-by-year dummies and industry-by-year dummies; the estimates are based on 4,180 observations. Robust standard errors in parentheses. **, *: significant at the 5% and 10% level, respectively.

growth of these industries with respect to other industries. As shown in Columns 3 and 4 in Table 7, EPLR appears to have a dampening impact on the growth rate of value added shares of EPL-binding industries (albeit the significance of this impact is sensitive to the choice of the industry classifier). Therefore, in the long-run, value added shares of binding industries are negatively affected by EPLR. Whether this implies a positive or negative contribution to the aggregate effect of EPLR depends on whether in EPL-binding industries employment shares and industry-level productivity grow slower or faster than in other industries. This can be checked in our sample by looking at which type of industry tends to grow faster in the absence of reforms. As shown in Table A8 in Appendix 1, in all our countries except Finland, EPL-binding industries are characterized by a lower growth rate of employment shares and a higher growth rate of productivity. However, the sum of these two rates is larger in EPL-binding industries than in other industries in all countries except Spain. As a result, the contribution of resource reallocation patterns to the long-run aggregate productivity effect of EPLR is likely to be negative in all countries, with perhaps the only exception of Spain.[23]

Since two of the three terms in which we have decomposed the effect of dismissal regulations on aggregate productivity growth appear to be negative, the overall impact will also be negative provided that the effect of these regulations on productivity growth in non-binding industries is not positive and large (cf. Equation (5)).

[23] Spain is the only country where, under these assumptions, the long-run aggregate growth effects would be less negative than the weighted average of industry-level effects. Yet, two considerations are in order. First, the effect of EPLR on the value added share of binding industries is small (0.3 percentage points per year, according to the estimates in Column 3 of Table 7). As a result, given the figures of Table A8, the half-life of the aggregate productivity growth effect of EPL will be about 50 years. Second, Equation (5) is a linear approximation and, strictly speaking, is valid only for a marginal change in EPLR. In the case of a large reform reducing EPLR, labour productivity growth in Spanish EPL-binding industries is likely to sufficiently accelerate to make any gain of value added shares in these industries contribute positively to aggregate productivity growth.

We have already noticed that the direct impact on productivity is likely to be greater in EPL-binding industries. Therefore, if there were only direct effects, the absolute effect in these industries, if any, would be negative. As a result, in order to establish the effect of EPLR on aggregate productivity growth the key issue is to assess indirect general equilibrium effects of EPLR on productivity in non-binding industries. Although we cannot offer the same level of evidence as above, our conjecture is that, in non-binding industries, these effects are either negative or, if positive, small (or, at least, smaller than in EPL-binding industries). In fact, as suggested by Joseph Zweimüller's discussion of this paper, for an increase in EPLR to have a large positive indirect effect on productivity in non-binding industries, one needs to think that the reduction in the number of vacancies in EPL-binding industries, brought about by more stringent regulations, reduces significantly separations in other industries, thereby increasing tenure and firms' incentives to invest in human capital, in these industries. We are unaware of any study testing this mechanism directly. However, there are pieces of evidence that are hard to reconcile with it. First, one would expect this predicted pattern to show up in training statistics. However, employer-sponsored training does not appear to increase with EPLR. On the contrary, it appears to be inversely related to it (see e.g. Bassanini *et al.*, 2007). Second, and related, the aggregate effect of an increase in EPLR on vacancies must be reflected in a sizeable reduction in voluntary quits (and particularly quits motivated by economic reasons and resulting in job-to-job flows) in order to affect employers' behaviour as regards training. If this were the case, we should find evidence that: (1) international differences in turnover rates should be due to international differences in voluntary separations, at least partially, and (2) dismissal regulations should have an impact on job-to-job flows. However, (1) international differences in turnover appear to be essentially due to involuntary separations and not to voluntary quits (Jolivet *et al.*, 2006); and (2) there is no evidence that EPLR has any impact on job-to-job flows (Boeri and Garibaldi, 2009). Third, the mechanism hinges upon the idea that a reduction in the number of vacancies in EPL-binding industries, due to the direct effect of EPLR, significantly affects the quit behaviour in other industries. But, if this inter-industry channel were sizeable in practice, one would expect that job-to-job transitions frequently resulted in industry switches. We can easily look at this issue using Spring quarter data from the UK Quarterly Labour Force Survey, where this information is available for each year since 1992. We find that only between 3% and 6% of all yearly job-to-job transitions originating from non-binding industries are transitions towards an EPL-binding industry, the remainder remaining intra-industry. In other words, intra-industry job-to-job flows appear to be much larger than inter-industry flows, suggesting that changes in the number of vacancies in EPL-binding industries are unlikely to significantly affect quits in other industries.

All in all, the evidence in favour of a large positive impact of EPLR on human capital accumulation in non-binding industries appears to be, at best, very tenuous.

Therefore, if we set to zero the effect of EPLR on productivity growth in non-binding industries, we will either understate the true effect in non-binding industries or make a small error. The same applies also if we assume that dismissal regulations have no impact on industry composition (no reallocation effect) and we approximate the aggregate impact with our estimated differential effect between EPL-binding and other industries only, multiplied by the share of EPL-binding industries. This is even more likely to understate the true impact of EPLR that the qualitative indicator is based on the already cautious assumption of labelling EPL-binding only those industries that appear to have layoff rates above the average in *all* years, so that, in the base sample of nineteen industries, only six of them – accounting, on average, for about 25% of the value added – appear to meet this criteria. As a result, the coefficients in Column 1 of Table 6 translate into an aggregate labour productivity growth effect of 0.14 percentage points for a one-point EPLR reform.

5.3. Regulations for temporary contracts

As discussed in Section 2, the overall level of employment protection depends on a mixture of regulations concerning regular and temporary contracts. In countries with rigid dismissal regulations but lax legislation on the use of temporary contracts, firms can circumvent the constraints imposed by lay-off restrictions by opening fixed-term positions. Countries can therefore 'choose' different combinations of the two types of regulations and achieve similar degrees of 'aggregate flexibility' as regards job flows and employment levels (see Figure 1 above). But do these regulatory choices have the same effect on productivity? In principle, an expansion in temporary work could have opposing effects. On the one hand, in the presence of strict dismissal regulations, temporary contracts allow firms to adapt quickly to changes in technology or product demand and move resources easily into emerging, high-productivity but high-risk activities. Temporary workers might also display greater work effort than other workers if they perceive that good performance could lead to contract renewal or a permanent job offer (Engellandt and Riphahn, 2005). On the other hand, there is some evidence that temporary workers are less likely to participate in job-related training (e.g. OECD, 2002), or even are more prone to workplace accidents (Guadalupe, 2003). Establishing the impact of legislation for temporary contracts is relevant for policy purposes. Indeed, partial EPL reforms – whereby regulations on temporary contracts are weakened while maintaining strict EPL on regular contracts – have been more frequent in OECD countries in the last two decades (see Figure 2 above), often because they are easier to implement and are typically less opposed by insiders (see e.g. OECD, 2004).

We look at this issue in different ways. First, we augment our preferred specifications by including the index of regulation for temporary contracts (EPLT), in such a way that the effect of both types of regulations is simultaneously estimated. This provides also another type of robustness check for our main result, since

Table 8. EPL, EPLR, EPLT and TFP growth

Indicator of layoff propensity	US layoffs, qualitative			US layoffs, quantitative			US job turnover, qualitative			US job turnover, quantitative		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Log relative TFP	−0.038***	−0.038***	−0.038***	−0.038***	−0.038***	−0.038***	−0.044***	−0.044***	−0.043***	−0.043***	−0.043***	−0.043***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
EPLR × Layoff	−0.514***	−0.500**		−0.228***	−0.228***		−0.784***	−0.748***		−0.074***	−0.084***	
	(0.175)	(0.194)		(0.055)	(0.060)		(0.215)	(0.248)		(0.024)	(0.028)	
EPLT × Layoff	0.057	0.070		0.041	0.041		0.354***	0.384**		0.047***	0.039**	
	(0.106)	(0.126)		(0.033)	(0.039)		(0.128)	(0.154)		(0.016)	(0.018)	
EPLR × EPLT × Layoff		0.026			0.000			0.063			−0.017	
		(0.144)			(0.044)			(0.181)			(0.019)	
EPL (summary index) × Layoff			−0.273*			−0.100**			−0.072			0.012
			(0.141)			(0.046)			(0.172)			(0.020)
R^2	0.329	0.329	0.328	0.330	0.330	0.328	0.371	0.371	0.367	0.371	0.371	0.367

Notes: All estimates contain country-by-year dummies and industry-by-year dummies; the layoffs estimates are based on 4,180 observations, the job turnover estimates are based on 2,860 observations. When interacted with one another, EPLR and EPLT are expressed in deviation from the sample average. Robust standard errors in parentheses. ***, **, *: significant at the 1% 5% and 10% level, respectively.

EPLT is a key confounding factor that we have omitted so far, even though it can affect productivity. Second, we also include an interaction between EPLR and EPLT since the latter is likely to matter more for the overall regulatory stance in countries where the former is more stringent (see e.g. Nunziata and Staffolani, 2007). Finally, we look at the implications of differences in the impact of the two types of regulation for the measured association between the overall index of EPL and TFP growth.

Table 8 shows the results of this exercise. They are presented for both the turnover-based and layoff-based classifications of EPL-binding industries. A turnover-based classification is arguably more appropriate than a layoff-based one in this case, since EPL for temporary contracts concerns hiring as much as dismissals. In all specifications, stricter regulation for temporary contracts has no or positive impact on TFP. By contrast, it appears that, controlling for EPLT, the estimated effect of EPLR on TFP remains negative, significant and of virtually the same magnitude as in our baseline specifications. In other words, partial EPL reforms, which liberalize only the rules on temporary contracts, do not appear the most promising route to boost productivity. Not surprisingly, the contrasting effects of EPLR and EPLT on TFP are reflected in the weak and often insignificant association between the overall index of EPL and TFP growth.

5.4. Does the impact of regulations depend on the distance from the frontier?

Firms that operate with technologies that are far from the technological frontier often improve their efficiency by adopting more efficient technologies developed by and/or already in use by industry leaders or elsewhere. Frequently, adoption of new technologies requires downsizing and/or other staff adjustments to cope with new skill needs (Cappelli, 2000). To the extent that dismissal regulations slow real-

Table 9. EPLR and TFP growth: the effect of EPLR on the speed of convergence

Indicator of layoff propensity and TFP measure	(1) Qualitative, fully adjusted	(2) Quantitative, fully adjusted	(3) Qualitative, broad measure	(4) Quantitative, broad measure
Log relative TFP	−0.040***	−0.041***	−0.038***	−0.039***
	(0.004)	(0.004)	(0.004)	(0.004)
EPLR × Layoff	−0.361**	−0.172***	−0.348**	−0.161***
	(0.167)	(0.052)	(0.165)	(0.051)
EPLR × Layoff × Log rel. TFP	−0.006	−0.001	−0.006	−0.001
	(0.006)	(0.002)	(0.006)	(0.002)
R^2	0.330	0.332	0.336	0.338

Notes: All estimates contain country-by-year dummies, industry-by-year dummies and additional implicit interactions; the estimates are based on 4,180 observations. When interacted with one another, EPLR and log relative TFP are expressed in deviation from the sample average. All specifications control for implicit additional interactions. Robust standard errors in parantheses. ***, **: significant at the 1% and 5% level, respectively.

location of resources across activities, firms and industries, one can expect that they dampen the pace of technology adoptions and, thereby, the speed of convergence towards the productivity frontier. If this were the case, reforms of overly strict dismissal regulations would be particularly important in countries that are, on average, further from that frontier. This possibility is explored in Table 9, where we modify our preferred specifications by letting the effect of relative TFP vary as a function of EPLR, where the latter is, as always, multiplied by the indicator of layoff propensity. If EPLR had a negative impact on the speed of adoption, and thereby the speed of convergence to the frontier, we would expect this additional interaction term to have a positive coefficient, pointing at a stronger effect in industries where EPL is more binding. Surprisingly, however, no significant interaction effect is estimated (Columns 1 and 2).

In many cases, however, industry followers adopt new vintages of capital equipment already in use by productivity leaders, resulting mainly in embodied technological change. We can suspect, therefore, that the reason EPLR does not appear to slow down convergence is related to the use, as the dependent variable, of a proxy for disembodied technological change only. To check for this possibility, we replace 'fully adjusted' TFP with 'broadly defined' TFP, which arguably captures both embodied and disembodied technological change (see Section 2.1), and redefine relative TFP accordingly. Yet, no significant interaction effect appears (Columns 3 and 4 of Table 9).

Although the evidence presented here is far from being conclusive, it yields little empirical support to the idea that dismissal regulations negatively affect the pace of technology adoption. In fact, the depressing effect of stringent job protection legislation on TFP does not appear to vary with the distance from the technological frontier, suggesting that EPL is perhaps less important for the adoption of existing, albeit more advanced, technologies than for the development of new innovative ones.

5.5. Other possible heterogeneous effects: non-linearities and interactions with other institutions

Do other institutions and policies affect the relationship between EPLR and productivity? This question is key for policy purposes. In fact, our estimates might capture only relationships prevailing on average in our sample of countries. If the heterogeneity of institutional systems matters, our results might be of limited interest for policy-makers from countries whose institutional framework is far from the OECD average. In particular, the literature points out that coordinated industrial relation systems favour the development of specific skills and internal labour markets (e.g. Hall and Soskice, 2001, among others), which may make dismissal regulation less binding or even positively related to productivity, at least at low stringency levels (see also Section 2.2). Alternatively, it has been argued (e.g. Thesmar and

Table 10. EPLR and TFP growth: heterogeneous effects

Indicator of layoff propensity	Qualitative			Quantitative		
	(1)	(2)	(3)	(4)	(5)	(6)
Log relative TFP	−0.039***	−0.038***	−0.039***	−0.039***	−0.039***	−0.039***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
EPLR (low segment) × Layoff	−0.443			−0.182		
	(0.388)			(0.128)		
EPLR (medium segment) × Layoff	−0.446			−0.064		
	(0.495)			(0.151)		
EPLR (high segment) × Layoff	−0.618			−0.515***		
	(0.634)			(0.186)		
EPLR × Layoff		−0.784*	−0.460**		−0.290**	−0.220***
		(0.445)	(0.184)		(0.145)	(0.059)
High corp. × Layoff		−0.108			0.019	
		(1.074)			(0.344)	
Medium corp. × Layoff		0.204			0.668	
		(1.726)			(0.490)	
EPLR × High corp. × Layoff		0.306			0.089	
		(0.573)			(0.184)	
EPLR × Medium corp. × Layoff		0.179			−0.144	
		(0.707)			(0.213)	
Stock market cap. × Layoff			−0.067			−0.187
			(0.543)			(0.190)
EPLR × Stock market cap. × Layoff			−0.224			−0.076
			(0.420)			(0.144)
Observations	4180	4180	4142	4180	4180	4142
R^2	0.329	0.329	0.330	0.331	0.331	0.332

Notes: All estimates contain country-by-year dummies and industry-by-year dummies. Robust standard errors in parentheses. ***, **, *: significant at the 1%, 5% and 10% level, respectively.

Thoenig, 2004) that financial market development, by improving risk sharing between owners of listed firms, increases the willingness of these firms to take risks. This in turn increases firm-level uncertainty in sales, employment and profits, more so if the labour market is flexible. To the extent that risk-taking behaviours are required to experiment with new technologies, and the willingness to take risks is affected by dismissal regulations, one might expect that dismissal regulations have a greater impact on productivity in countries where financial markets are more developed.

We take a look at these possible heterogeneous effects in Table 10. First, we examine possible non-linearities in the effect of EPLR, by fitting a continuous piecewise linear function, with knots approximately corresponding to the 33rd and the 67th percentiles of the distribution of EPLR (Columns 1 and 4). Looking at point estimates, it appears that the effect of dismissal regulations is stronger for high-stringency levels. However, specification tests show that differences across stringency levels are not significant, although this might simply be the result of insufficient variation in the data, as shown by the large standard errors. In other words, our evidence is not inconsistent with, but nevertheless not very supportive of, the hypothesis that the effect of job security provisions on productivity varies according to the stringency level.

Next, we investigate the role played by the degree of corporatism of the industrial relation system in shaping the relationship between firing restrictions and productivity. We find that, although EPLR attracts a less negative coefficient in highly corporatist countries, with coordinated and/or centralized wage-bargaining systems, differences across wage-bargaining types do not appear statistically significant (Columns 2 and 5). More importantly, the effect of dismissal regulations that we estimate for highly corporatist countries is always approximately equal to the effect we estimate for the whole sample in our preferred specifications (cf. Table 2). Overall, these results suggest that layoff legislation is likely to matter for TFP growth independently of the prevailing wage-bargaining system, although there might remain some second-order differences in the intensity of the relationship that we are unable to detect, given our data.

Finally, we roughly quantify the level of financial and stock market development by taking the ratio of stock market capitalization to GDP, drawn from the World Bank's Financial Structure Dataset and Beck *et al.* (2000). We then examine whether the relationship between EPLR and TFP growth is more intense when this ratio is larger. We find that, even though the interaction term attracts the right sign, its coefficient is largely insignificant (Columns 3 and 6). To put it another way, we find little evidence that financial development significantly affects the impact of dismissal regulations on TFP growth. If any, the effect of financial development on this relationship appears to be of second order.

6. POLICY IMPLICATIONS

Let us summarize our results. First, we find that mandatory dismissal regulations have a depressing impact on TFP growth in industries where layoff restrictions are more likely to be binding. We present a large battery of robustness checks that suggest that our finding is robust. Furthermore, the effects of dismissal regulations on TFP appear to carry over to labour productivity. We also argue that from our results one can infer that layoff restrictions have a negative impact on aggregate labour productivity growth, even if we are able to provide only a lower bound estimate to the average effect of these regulations.

Second, we find no evidence that these regulatory restrictions affect the technological catch-up with the industry productivity frontier. On the contrary, their impact does not appear to vary significantly with the distance to the frontier or across different types of institutional frameworks.

Third, the dampening impact of EPL on productivity appears to be entirely due to the effect of dismissal regulations, while restrictions on the use of temporary employment have, if any, a positive impact on TFP growth.

There are two key policy implications that can be drawn from these findings. First, reforms of overly strict dismissal regulation in many OECD countries can be justified on the grounds of fostering productivity growth. Lack of conclusive evidence on the employment impact of EPL is not a good reason for policy inaction. However, relaxing layoff restrictions will be particularly valuable for firms that do not rely solely on adoption of technologies developed elsewhere for their productivity growth. Second, partial EPL reforms, facilitating the use of fixed-term and atypical contracts, are unlikely to have an important impact on efficiency and technological change and cannot therefore be a substitute for comprehensive EPL reforms whereby dismissal restrictions for open-ended contracts are also weakened. In other words, even though in recent years many countries have chosen to ease regulations on temporary and atypical contracts to make their labour market more flexible, the productivity growth pay-off that can be expected from these reforms is very low. Italy, for example, made several reforms in the past 15 years, which created, and eased the use of, a multiplicity of atypical contracts, without however addressing the difficulty of dismissing workers with open-ended contracts (see also Figure 2 above). While these reforms might have delivered some benefit in terms of employment (see, e.g., Boeri and Garibaldi, 2007), it is perhaps not surprising that the Italian productivity and GDP per capita growth was among the lowest in OECD countries during the same period (see, e.g., OECD, 2007).

Reforming regular contracts is, however, difficult, since it often raises vocal opposition by workers. In this context, one interesting reforming strategy has been recently followed by Austria, which in 2003 introduced a system of individual savings accounts to replace redundancy payments for dismissals. Before the reform, employers were required to make severance payments to employees with more than three years' service in the event of termination. The size of the payment increased with employees' tenure with the employer. Under the new rules, employers now pay a premium of 1.54% of the payroll into an account for each employee for the entire period of the employment contract. In the event of termination, an employee with more than three years of tenure with their current employer chooses between receiving a payment from their savings account and putting the amount in the account towards a future pension. If an employee quits, or is dismissed before reaching three years of tenure, the balance of the account is conserved and additional contributions are made by future employers. The employee continues to accumulate funds over his/her working life, with the balance accessible upon retirement. As the enactment of this individual accounts system entailed only a moderate increase in the financial risk born by workers, it left essentially little scope for opposing its implementation. Nevertheless, this system is likely to significantly increase mobility by removing disincentives for dismissals and voluntary separations.[24] Such

[24] Although it may also increase labour costs for employers if they are not able to transfer the contribution to employees in the form of lower wages.

a reform amounts to a drop of 0.55 points in the EPLR indicator used in this paper. Taking our estimates at face value, in the long run this would imply that Austria will raise its annual labour productivity growth in EPL-binding industries by about 0.3 percentage points, which translates into an average estimated growth rate of about 0.1 percentage points for the whole economy. Although this figure might not seem huge, it would represent an increase in GDP per capita growth of about 5% with respect to the Austrian average of the previous 20 years. And, as we discuss in this paper, the real effect could well be much greater. In other words, this might be an example of a reform path that other countries wish to imitate.

Discussion

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This paper contributes to the discussion on the effects of employment protection legislation (EPL) on the performance of the economy. Although we begin to understand the effects of EPL on unemployment, the authors argue correctly that we know a lot less about the effects of EPL on productivity.

From a theoretical point of view, EPL may affect firms or workers and it may increase or decrease productivity. This leads to four possibilities: (a)–(d) in Table 11. As mentioned in the paper, all four possibilities have been modelled in the literature.

In case (a), EPL stimulates the firm to invest to raise workers' productivity. Due to this higher productivity the firm avoids laying off workers which is expensive with strict EPL. With less strict EPL, the incentive to invest is reduced since firing relatively unproductive workers is easy. This effect is reminiscent of the literature that stresses the positive effects of competition on innovation: by making life hard for firms, you stimulate firms to raise their performance (see, for instance, Aghion *et al.*, 2001).

In case (b), EPL reduces firms' investments in productivity enhancing technologies. This theory comes in two flavours. The first simply observes that labour saving technologies are less attractive with strict EPL as the workers that become

Table 11. Four theoretical possibilities for the effect of EPL on productivity

EPL	Affects firm	Affects worker
Increases productivity	(a)	(c)
Reduces productivity	(b)	(d)

redundant are expensive to fire. This is not very convincing. It is exactly when EPL is strict that firms have an incentive to reduce the number of workers that may have to be fired in a downturn. Hence forward-looking firms have a big incentive to adopt labour saving technology under strict EPL (as argued under (a)) and use natural attrition to avoid the costs associated with EPL. The second – more sophisticated – version of this argument stresses that adopting new technology implies a risk. If the implementation of the new technology goes wrong, a firm may need to fire workers quickly to absorb the blow. EPL makes this very expensive. This version of the argument comes with a testable implication: EPL only reduces firms' investments in technologies that are risky to adopt. I come back to this below.

Case (c) starts from the following hold up problem. A worker investing in firm-specific knowledge loses the value of this investment in case he is fired. This threat of losing his job reduces the incentive to invest. EPL reduces this hold up problem by creating a commitment that the worker will not be fired. Hence EPL stimulates productivity enhancing (firm specific) investments by workers.

Finally, the reduction in the threat of being fired may make workers 'lazy'. In case (d), the workers feel less pressure to keep learning and increasing their productivity under strict EPL. In this case EPL reduces productivity (growth).

Although this is not the first empirical paper on EPL, the paper makes the point that most of the previous literature has used cross-country/time-series data. This makes it hard to control for confounding factors. This paper circumvents this problem by using industry level data and distinguishing industries where EPL is (likely to be) binding from industries where this is not the case. In this way, the analysis controls for all other unobserved effects that affect productivity growth in EPL-binding and non-binding industries in the same way.

The paper finds that EPL (for regular contracts) reduces productivity growth in industries where EPL is binding (compared to industries where EPL is not binding). Roughly speaking, for most industries cases (a) and (c) in Table 1 are not relevant.

Hence we are left with cases (b) and (d). The paper presents a result that suggests (indirectly) that case (b) is the most relevant. Here we can use distance to frontier results. For explanation (d) it does not matter where the increase in productivity comes from; the problem is the worker who has little incentive to learn how to work with the new technology which thereby reduces productivity. Hence under explanation (d) catching up to the industry frontier should be hampered by EPL. The firm invests in cutting edge technology, but workers do not feel enough pressure to bring the technology to its full potential, for instance through training and learning how the technology should be used.

However, Table 9 suggests that catching up to the industry frontier is not different between EPL binding and non-binding industries. This is consistent with (b) if one is willing to assume that adopting technologies that are already used in the industry is not very risky. However, if this technology is already used in another country, the firm can learn how to adopt it successfully. Hence the risk of things going so wrong that numerous workers need to be fired is small. Therefore EPL does not hinder the adoption of known technologies and does not affect firms catching up to the industry frontier. The mechanism under (b) is that EPL makes firms reluctant to adopt new technologies that may go seriously wrong, thereby forcing the firm to shed labour.

If it is indeed the case that EPL hinders the adoption of brand new technologies and thereby hinders innovation, the question becomes what the best policy response is. Reducing the strictness of EPL for regular contracts is clearly one option. However, as argued in the paper, EPL may also have an efficiency rationale in a world with capital market (and other) imperfections. Perhaps as an additional instrument, policy-makers can also consider how to stimulate innovation and R&D. This is usually done through subsidies and tax breaks. Another way to spend (part of) this money may be through relaxing EPL restrictions in innovative industries and compensating workers that are indeed fired if the adoption of a new technology backfires.

Given the strength of the EPL effect found in the paper it would be interesting to see in future research which is the best way to spend money stimulating R&D and innovation. Directly subsidizing R&D or compensating workers in firms where the adoption of a new technology went wrong and employment protection is reduced to allow the firms to absorb the blow.

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Job protection legislation is an important labour market policy instrument in many (particularly European) countries and many policy-makers have the strong prejudice that stringent EPL might be responsible for the often weak employment and productivity performance in these countries. Despite these policy debates, economic research has had a hard time to establish the empirical relevance of this claim, in particular with respect to the impact of EPL on aggregate employment.

The authors approach the issue by looking at the impact of EPL on industry-specific (total factor) productivity growth, an outcome that has not been studied so far. They use panel data from the EUKLEMS database which provides disaggregate information for 19 industries and 11 countries over a period of 21 years (1982–2003) and end up with 4,180 observations. They also use an extended (unbalanced) country panel including five additional countries which increases the number of observations to 5,139. This seems a very interesting and reasonable data set to address the question of interest.

Their empirical approach is a version of the difference-in-difference strategy suggested by Rajan and Zingales (1998) who study the impact of financial development on long-run growth. The basic idea in this paper is that EPL affects various industries in a quite different way. Industries with high layoff rates should be confronted with stronger constraints and consequently larger effects on economic outcomes, whereas EPL should be weak or even irrelevant in industries where layoff rates are low. To the extent that the economy can be classified into industries where EPL is binding and those where it is not, a standard difference-in-difference approach can be applied. Binding industries are 'treated' and non-binding industries serve as a 'control' group. I found this a very interesting paper adopting an elegant approach to shed new light on the impact of EPL on economic outcomes. In particular, the paper performs a very careful empirical analysis and is extremely careful in addressing many obvious arguments that come to mind. The paper is fun to read.

Having said that, it is important to keep in mind that this empirical approach has potentially important limitations. To identify the impact of EPL on TFP growth, the authors focus on a comparison of treated and control industries, but the classification into these two types is by itself far from clear. The basic idea is that technology varies across industries causing 'natural' layoff rates that would prevail in the absence of EP rules. It is assumed that the industry ranking with respect to the extent to which industries are EPL constrained does not change over time and does not vary across countries. This is certainly a strong (and certainly not innocent) assumption. Countries differ strongly in industry structure and in the speed of structural change. For instance, in the United States the services sector is much larger than in most European countries. Differences in the education system may have an impact on labour mobility and labour reallocation (for instance, the German apprenticeship system leads to higher incentives to invest in firm-specific human capital) with unclear effects on the hypothetical ranking of industries in the absence of EPL. Comparative advantages and changes in the international division of labour may lead to a situation where an industry that experiences expanding international markets in one country may experience stagnation and layoff pressures in another country. It is also important to note that industries are very heterogeneous entities and the composition, firm size distribution, skill-intensity, and adopted technologies may be substantially different across the different countries and may have changed in a different way during the observation period.

A second (and related) comment is a standard objection against difference-in-difference analyses. The effect identified with such an empirical strategy relies on the assumption that there are no differential trends in TFP growth between treated and control industries. While the analysis controls for time-, country- and industry-fixed effects, it could be that, for reasons totally unrelated to job protection legislation (catch up, technology adoption, deregulation of labour and credit markets, changes in the education system, removal of trade barriers) that the treated industries grow faster than control industries. One way to deal with this problem would have been to randomly assign changes in the EPL index to some dates around the actually observed change in the EPL index. If the estimated treatment effect does not vanish by polluting the EPL measure in such a way the estimated effect is mostly likely the results of differential *ex-ante* productivity trend rather than an effect of EPL. However, if the estimated treatment effect remains, the analysis would be much more reliable. The authors are aware of this problem and, in one of their robustness tests they assign a random value of EPLR to each country/year couple and re-run their baseline models many times. This check should lead to lower coefficients if results are due to EPL effects rather than country-specific industry trends. In fact, in sensitivity analyses they find a downward bias suggesting that specific country-industry trends do not drive our main finding. In another sensitivity test the authors check whether results change when all countries where the binding/non-binding TFP growth rate differential is greater than 1 percentage point are excluded from the analysis. They find that results are strikingly robust even to this test.

The authors are very careful in addressing these issues and most results confirm their proposition that the estimated effects do in fact provide a reasonable estimate of the impact of EPL in productivity growth. However, the question remains how powerful these robustness checks are. Their robustness tests would be highly convincing if variation in EPL in their sample is within-country variation and within-country changes in the EPL index were sufficiently strong so that meaningful inferences of their effect on within-country productivity growth could be drawn. However, such within-country variation is limited in their sample. Most of the variation in EPL is the result of persistent EPL differences across countries. This weakens the power of their robustness tests as numerous confounding factors could be at work causing country-specific productivity differences across the various industries.

Third, the focus of the analysis is the difference in TFP growth rates between EPL binding and EPL non-binding industries. While this is an important question *per se*, the question of main interest is how EPL affects aggregate productivity growth. The TFP-growth difference between the two types of industries is only informative if important other general equilibrium effects can be neglected. More precisely, the adopted basic model estimates the effect of interest under two rather special circumstances: (1) EPL should not affect productivity growth in non-binding industries and (2) the relative importance (in terms of value added) of binding and non-binding industries should be unaffected by EPL. The authors are well aware of the potential importance of such general equilibrium effects and briefly address this issue in Section 5.2. The presented evidence suggests that stronger EPL tends to increase the value added (but not the employment) share of non-binding industries. As non-binding industries have lower productivity growth rates, this implies that EPL does not only depress TFP growth in binding industries but also withdraws resources from the more dynamic sectors of the economy. While the authors state in Section 5.2 that a more systematic analysis is beyond the scope of their paper, a thorough discussion of these issues would have deserved more attention. A related point is that TFP growth is one, but not the only, important productivity outcome potentially affected by job protection legislation. For a full picture on the impact of job protection legislation on productivity it seems equally important to address the issue of how job protection affects the incentives to invest in physical and human capital.

To sum up, I found this paper a very interesting and stimulating contribution that highlights the potential importance of EPL on productivity growth rates – an outcome that has not yet been studied so far. The paper presents a very careful empirical analysis and performs numerous robustness checks concerning obvious confounding factors. While the authors do a very good job in addressing potential confounders, limitations of their data (in particular, little within-country variation in EPL) do not allow drawing definite conclusions. Future research should look for within-country policy changes and work out clean experimental settings to test how EPL changes affect productivity and the allocation of resources across industries. In any case, the paper provides a very good motivation for future researchers for the potentially important effects of EPL on productivity growth.

Panel discussion

Referring to his work on financial development and growth, Ashoka Mody was confronted with reverse causality, too. To investigate whether 'finance matters' he and his co-authors compared differences of growth rates across industries and countries. They did not even try to pin down levels, like an instrument could do in a growth accounting regression. This paper should be modest as well: its conclusion can be that some industries' growth rates are more strongly affected by EPL, while nothing can be said about the overall speed of growth. Accordingly, the authors should be careful about policy implications.

Jacques Delpla noticed that in France dismissals are perceived to be difficult not only because of legislation but also because of slow and expensive court procedures, which also lead to out-of-court settlement. So, he wondered whether this factor can be taken into account in the data. Following this comment, Georges de Menil asked for more information about the components and building strategy of the EPL index. He also found interesting the suggestion that more flexible labour market legislation may have a positive effect on productivity. As noticed by Ashoka Mody, usually EPL is studied in relation with unemployment, not productivity. In the paper the two are not considered together, otherwise they would have been reinforcing each other, and should lead to a boom. Tito Boeri noticed that to interpret the results it is important to distinguish between regular and temporary contract protection, as well as between intensive and extensive margins, and sector-specific skill intensities.

WEB APPENDIXES

Available at http://www.economic-policy.org

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