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ABSTRACT

The Economics of Employment Protection

Empirical investigation of the labor market consequences of employment protection has mushroomed since Lazear's (1990) pioneering study. Having sketched the theoretical background, we chart the course of the modern empirical literature. We focus mainly on dismissals protection, distinguishing between the themes of employment and unemployment development and labor market dynamics proper. Our discussion of employment and unemployment largely deals with the effect of employment protection on levels of these outcome indicators. We distinguish between overall and compositional effects (e.g., by demographic group and type of contract), between developing and industrialized nations, and identify some key control variables. Our discussion of labor market dynamics focuses on the speed of adjustment issue and on gross flows. It also formalizes the link between analyses of levels and changes in variables. At all times potential offsets to the adverse effects of employment protection receive consideration.

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Keywords: Severance pay, employment protection indices, labor standards, employment, unemployment, employment adjustment, gross flows

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"By comparison [with the effects of unions and social security systems] time spent worrying about strict labor market regulations, employment protection and minimum wages is probably time largely wasted" (Nickell and Layard, 1999, p. 3030).

I. *Introduction*

Employment protection may be described as restrictions placed on the ability of the employer to utilize labor. These restrictions are typically legislated but they may also be set by collective agreements of an *erga omnes* nature or by the decisions of the judiciary through evolving case law. According to this definition, employment protection would cover dismissals protection (procedural inconveniences, notice and severance payments, and the standards/penalties fixed for "unfair" dismissals), limitations on the use of fixed-term and temporary work agency contracts (the terms under which these can be offered, the maximum number of successive renewals, and maximum cumulated duration), the regulation of working hours (maximum weekly/annual normal hours, minimum rest periods, limits on overtime, the scope for flexible distribution of hours across the week/month/year, and restrictions on weekend and night work). However, a wider definition includes such additional "labor standards" as regulations on parental/maternity leave, posted workers, health and safety, equality of treatment of atypical workers, mandatory sick pay, worker representation rights, and minimum wages *inter al*.

The standard competitive model views all such limitations on freedom of contract as imposing resource costs. It is therefore conventional to stress the employment reducing/unemployment increasing consequences of rising overall employment costs, possibly exacerbated by increased wage pressure from employed "insiders," a reduced speed of adjustment of labor markets to exogenous shocks, a reduction in the reallocation of labor from declining to expanding sectors, and dampened job creation. But theory may also be used to blunt or overturn these implications,[1] which become decidedly less transparent once account is taken of market imperfections. Indeed, it has been argued that employment protection legislation can enhance productivity performance by encouraging worker cooperation in the development of the production process, stimulate training investments, and reduce "excessive" turnover, *inter al.*; and, further, that where employment costs are increased by employment protection, the latter may be seen as an alternative to unemployment insurance and also be capable of being offset by other institutional arrangements in the labor market.

Not surprisingly, therefore, economists have increasingly turned their attention to the empirical evidence and the past decade has witnessed an explosion in such inquiries in the wake of Lazear's (1990) famous cross-country study of the impact of *dismissals* protection on employment. In the process, his methodology has been refined and extended, the definition of employment protection widened, and more attention has been paid to dynamics. The result has been a more differentiated pattern of results and, as the above quotation reveals, some areas of real disagreement. In other words, support for Lazear's pessimistic empirical conjectures is mixed.

Herein, having briefly rehearsed the theoretical arguments, we track and evaluate the evolving literature. As an organizing device, we choose to distinguish between employment effects on the one hand and employment adjustment effects on the other. Elements singled out in the former area include compositional effects and potential differences between developing and developed economies. Interactions between employment protection rules and other aspects of the labor market also receive attention. Technical factors are accorded rather more emphasis in the latter discussion, where we also differentiate between net changes in employment and gross employment flows (as well as between aggregate and disaggregate data). A summary draws together the threads of the preceding arguments.

II. *Theoretical Observations*

Employment protection involves both per worker employment costs and employment adjustment costs. Policies affecting employment costs per worker include the "broader" labor standards noted earlier, to which it is conventional to add mandates placing limitations on working hours. Employment adjustment costs are those that accompany gross changes in employment and may be both natural and imposed. Examples of the former are search and training costs. The prime example of the latter is rules governing worker dismissal. Despite their intersection, per worker employment costs and employment adjustment costs merit separate analysis (Hamermesh, 1988, 1993).

As far as employment costs are concerned, the presumption is that rules setting effective labor standards increase labor costs and make it less profitable to produce a given level of output. As a result, output should fall and with it the employment of all inputs including worker hours. The assumption here is that the wage will not fall, and the maintained hypothesis is that the scope for beneficial trades has already been exhausted (see below). If such standards are characterized as a fixed employment cost, there will also occur a substitution effect on the firm's relative demand for employees and hours in favor of the latter. This substitution effect is unambiguous where labor and capital are the only factors of production *and* capital is fixed. If capital is variable, the outcomes depend on whether employment, hours, and capital are complements to or substitutes for one another. Of course, restrictions on hours of work in the form of, say, a shorter work-week provide an incentive to substitute additional workers for longer work-weeks. As shown by Hamermesh (1988, p. 12) the net impact depends on the distribution of hours per worker before the change was imposed because the fixed cost of employment is also raised by the penalty rate multiplied by the reduction in the normal work-week.

Turning to policies that affect employment adjustment costs, it is clear that the inter-temporal pattern of labor will be affected.[2] In periods of declining demand, when the firm would normally lay off workers, the imposition of adjustment costs will lead it to make fewer layoffs because severance pay creates a wedge between the worker's marginal revenue product and the cost of changing employment. In deciding whether or not to add workers at times of rising demand, the firm will take into account not only the wage that must be paid but also the likelihood that severance pay will bite in the future. The imposition of an adjustment cost will increase the amortized costs of a hire and reduce hiring. Employment will therefore fluctuate less over the cycle than in the absence of employment protection: The employer holds employment constant for longer during the downturn and refrains from hiring some workers during the upturn.

In the standard model, not only will there be this reduction in labor fluctuation over the cycle but also (again assuming that wages are fixed or that any reductions in wages only partially compensate employers for the increase in costs) a reduction in average employment. Higher employment in the downturn is dominated by reduced employment in the upturn. The corollary is that unemployment duration is expected to lengthen, and this effect will be intensified by human capital depreciation and stigmatization effects.

This characterization has been criticized by Bertola (1992), who argues that the effect on employment is ambiguous theoretically and hinges on the functional form of labor demand functions, the discount rate, and labor turnover (see also Bentolila and Bertola, 1990; Bertola, 1990). We can fairly crudely illustrate the first point by making certain assumptions about the relative slopes of demand curves during intervals of low and high employment (Blau and Kahn, 1999, p. 1412). If the slope of the demand curve is relatively flat during recessions and relatively steep during booms, then high dismissal costs may *raise* average employment. The number of layoffs deterred by high dismissal costs during recessions might be considerable since in the absence of employment protection it would have required a large reduction in employment to bring about equality between the (currently too high) wage and labor's (reduced) marginal revenue product. On the other hand, the deterrent effect in respect of new hires during boom periods might be small because it would not otherwise have taken many new hires to reestablish the equality between marginal revenue product and wages. Reversal of the hypothesized relative slopes of the two-period demand curves would produce the more familiar result that high firing costs reduce employment on average.

For its part, the effect of discounting is to elevate the costs of firing relative to those of hiring precisely because the former costs are incurred today. Similarly, turnover reduces the probability that severance pay will bite in the future. In both cases, the effects may be sufficient to increase employment even if the slopes of the demand curves are not obliging. Persistence of labor demand fluctuations should underscore this outcome.

Even if these arguments are accepted – and in this area modest reparameterization can yield very different net employment outcomes – there is also the point that employment adjustment costs slow down the reallocation of labor from old and declining to new and dynamic sectors (Hopenhayn and Rogerson, 1993), or that they are in conflict with the need for greater flexibility in sectors producing new goods and services because of their more volatile demand. Nickell and Layard (1999, p. 3063) have countered that such effects may be offset by the turnover of workers. But employment protection might still delay the closure of old plants and hinder the formation of new enterprises while, as noted by Bertola (1999, p. 3010), the reason why firing restrictions bind in reality is precisely because some firms seek to reduce employment by more than can be achieved by relying on quits. This latter observation means that an appeal to *aggregate* turnover rates to downplay the role of mandates imposing dismissal costs is unconvincing.

Thus far we have not introduced wage flexibility. As is well known, if the services provided for under a mandate are valued by workers the supply curve should shift down at the same time as the demand curve shifts down. Indeed, if workers value the service more than the cost of providing it, employment could increase. The problem is that employers should already have exhausted the opportunities for beneficial trades of this nature, that is, have sold to workers all workplace benefits whose value exceeds the costs. Accordingly, the non-provision by the market of benefits analogous to those provided by employment protection mandates implies that they are not valued sufficiently by workers and will be employment reducing. This outcome presupposes distortion-free, full-information markets and, as is equally well known, market failure provides scope for mandatory provision as a result of which employment (i.e., welfare) might be increased.

Categories of market failures that have been frequently invoked in discussions of employment protection are externalities, adverse selection, information asymmetries, public goods aspects of the workplace, and imperfect capital markets (see, for example, Akerlof, 1984; Piore, 1986; Lindbeck and Snower, 1988; Levine, 1991; Kuhn, 1992). Abstracting from the difficult issue of the standard to be fixed under law, it can perhaps be conceded in such circumstances that the employment effects of job protection may be muted if not positive. But some wage flexibility is typically required. The problem is that employment protection raises the bargaining power of incumbent workers (i.e., insiders) and is likely to result in an increase in wages. Moreover, there is every incentive for these groups to lobby for increases in job protection (and to actively resist its attenuation).

A related concern is the effect of employment protection on the composition of employment. Dismissals protection raises the costs of a bad hire and, other things equal, should serve to make firms more choosy in selecting employees.[3] The suggestion is that youth and older workers are at risk, especially in markets where a floor is placed on wages or where wage setting behavior maintains or compresses skill differentials. Compositional effects also follow directly from the incomplete coverage of employment protection rules. Most obviously perhaps, we would anticipate a growth in self-employment. And to the extent that atypical work is also excluded or less tightly regulated than open-ended employment, temporary employment should also increase; this phenomenon would counter the tendency to toward reduced cyclical fluctuation in employment. Compositional effects of this nature do serve to qualify separate equity arguments in favor of employment protection that we do not examine herein.

III. *Employment Protection and Employment: Preliminaries*

In this and the next section we survey cross-country evidence on the link between employment protection and employment and unemployment development, although we shall also mention individual country studies and touch on some other outcomes. Here we take Lazear's (1990) famous study as our starting point and then trace the more important steps in the evolution of the empirical model. Specifically, we consider the refinements made to the employment protection measure and the justification for additional covariates capturing other labor market institutions and policy variables.

The Lazear Model. In the first multivariate cross-country analysis of the effects of severance pay on employment, Lazear (1990) offered a parsimonious representation of the determinants of four labor market aggregates: the employment-population ratio, the unemployment rate, the labor force participation rate, and average hours worked. Apart from the dismissals protection measure, the other independent variables are a quadratic time trend, the growth in per capita GDP (to accommodate the notion that a growing economy vitiates at least in part the probabilistic costs of severance pay), and a demographic control (the population of working age). The model was estimated over a sample of 20 countries for the sample period 1956-1984.[4] The crucial variable is his severance pay measure, defined as the amount of statutory severance pay due to a blue-collar worker with 10 years of service dismissed for reasons unconnected with his or her behavior. The majority of Lazear's estimates are from equations that include just the dismissals indicator and the time trend variable rather than the fuller specification.

For a specification that excludes country dummies, Lazear reports that his measure of employment protection is negatively related to the employment-population ratio and the labor force participation rate (and also to hours worked, where it is speculated that employers make greater use of part-time work to avoid the strictures of legislation) but positively associated with unemployment. Allowing for country fixed effects confirmed each result other than that for unemployment, where the coefficient estimate for severance pay was no longer statistically significant. Lazear nevertheless chooses a version of the first specification – augmented with the growth and demographic controls – to calculate how much of the changes in unemployment rates over time are explained by changes in severance pay.[5] He concludes that although the evidence is mixed, the generosity of severance pay can go a long way toward explaining higher unemployment in a number of countries: more than half the increase in the cases of France (59.6 percent) and Portugal (71.2 percent). Finally, Lazear also addresses the question of causality (see below) and the issue of whether younger workers (aged 16-25 years) are particularly disadvantaged by employment protection legislation. In the former case, he finds little evidence to suggest that changes in employment and unemployment precipitate changes in the law. In the latter case, regressions of the ratio of younger-to-older worker employment and unemployment on severance pay yielded weak evidence that younger workers suffered more.

Lazear's study caused considerable controversy at the time, not least because of the sharpness of its results against the backdrop of the ambiguities in theory. It also upset the cozy complacency in some policy-making circles that had been encouraged by the mixed messages conveyed by theory and some early empirical work at the nation-state level (see, for example, Nickell, 1982; Buechtemann, 1993, pp. 35-44). Criticism of Lazear swiftly followed. It centered on the parsimomious nature of his estimating equations and the nature of his employment protection measure which was at best viewed as only a partial indicator of dismissals protection and at worst as a poor indicator of the overall regulatory climate. As we shall see, these criticisms were to shape the course of empirical work over the following decade. Interestingly, much less criticism was directed at errors in Lazear's raw data and the frailties of his estimation procedures (see, respectively, Addison and Grosso, 1996; Addison et al., 2000). Suffice it to say here that errors of omission and commission in data, while material, do not overturn his findings but that proper accounting for country heterogeneity and serial correlation seemingly do.

The Quest for an Improved Indicator of Employment Protection. Post-Lazear, investigators have sought a more inclusive measure of employment protection. The most comprehensive first attempt was made by Grubb and Wells (1993) for a sample of 11 EU nations.[6] Grubb and Wells identify three elements of a system of employment protection: restrictions on (individual) dismissals; restrictions on temporary forms of employment contract (i.e., atypical work); and restrictions on working hours. The first element includes months of severance pay and notice for no-fault dismissals, as in Lazear but now calculated over three intervals of tenure. It also covers procedural delays and complications (e.g., prior authorization) before notice can be activated, as well as the perceived difficulty of dismissal as indexed by the legal conditions defining "fair" or "unfair" dismissal (encompassing trial periods, compensation payable at 20 years of tenure, and extent of reinstatement). In each case, a rank order of countries is derived from the respective scores and an overall ranking is obtained by taking the unweighted average of the component rankings and then ranking these averages. As far as the regulation of atypical work is concerned, the components are essentially threefold: the objective grounds for entering into such arrangements (and permitted derogations), the maximum number of successive contracts, and their maximum cumulated duration.[7] Overall rankings are derived from the component rankings in the same manner as for restrictions on dismissals. Finally, restrictions on working time cover "maximum normal work time" and "flexibility of working time." The former category relates to normal annual weeks and normal annual hours. Rankings for normal work time are based on collective bargaining provisions in each case, although legal provisions per se are used as a tie-breaker. Flexibility of working time covers maximum annual overtime, flexibility in the distribution of hours, maximum weekly rest hours at weekends, and restrictions on night work. Overall rankings are derived as before but this time separately for the two categories of maximum work time and flexibility of working time.

We have spent some time on the Grubb-Wells study for a number of reasons. First, as a practical matter, it provides the basis for the most widely used employment protection indicator in the empirical literature (see immediately below). Second, the complexity of the exercise suggests the difficulty of establishing a time series for the summary measure(s) of employment protection. (Note that the various rankings assembled by Grubb and Wells pertain to the "late 1980s.") Third, and relatedly, there is an obvious problem of subjectivity in the construction of the indices. This is reflected in the implicit weighting scheme, the inherent bluntness of ordinal rather than cardinal measures, the difficulty of attributing scores on the basis of legal provisions that may be applied differently in practice (possibly varying in severity with the stage of the cycle) that involve different levels of penalties and that may be subordinate to collective bargaining arrangements. (Statistics on the latter are, of course, even more difficult to assemble.) Fourth, there is lingering ambiguity as to the number of categories over which one would wish to average rankings and then re-rank. Finally, we observe that nowhere do Grubb and Wells offer an analysis of the sensitivity of the ranking exercise to alternative component weightings.

As noted earlier, most empirical investigations of the effects of employment protection on economic aggregates have used a variant of the Grubb-Wells index, specifically that constructed by the OECD (1994). The are three basic differences between this initial OECD measure and Grubb-Wells. First, the OECD ignores restrictions on working hours. Second, it excludes the regulation of temporary agency work. Third, it extends the sample by five countries (Australia, Finland, Norway, Sweden, and Switzerland). Two summary measures are provided by the OECD, namely, a strictness ranking for dismissals protection for (a) regular contracts and (b) fixed-term contacts. When averaged and re-ranked these show some differences from the Grubb-Wells counterparts (cf. OECD, 1994, Table 6.7; Grubb and Wells, 1988, Table 9, 2000, Table 1).

Again the initial OECD measure pertains to the late 1980s. In recognition of the limitations of a time-invariant indicator, the OECD (1999) has recently updated (and revised) its overall and component measures of employment protection. The innovations are the use of a different technique to calculate the summary measures (by converting first-level indicators into cardinal scores and in some cases subsequently using uneven weights), the inclusion of temporary agency work in the calculation of an overall value/ranking of the severity of regulations applying to atypical work, and the construction of a new index for the regulation of *collective* dismissals. Although the OECD provides summary values and rankings for the overall strictness of employment protection legislation for these three components, in its subsequent empirical analysis (see below) an attempt is made to uncover differences in the impact of each. Note that the new measures pertain to the "late 1990s." Corresponding summary values/rankings of the strictness of regulations covering regular and atypical work (though not collective dismissals) for the late 1980s are also given, thus providing a discrete time-varying measure(s) of employment protection for 19 countries (the new nations are Canada, the United States, and Turkey).

Other indicators of the severity of employment protection may be constructed from surveys of employers, even if these are widely viewed as much more subjective than OECD-type indicators. The two best-known such instruments are the periodic surveys of employers conducted by the European Commission and a one-off survey conducted by the International Organization of Employers (IOE). The former, large-scale surveys of employers – there have been three to date, the last being in 1994 – asked managers to identify those factors that militated against their employing more labor, and whether these were "very important," "important," or "not important." In the 1985 survey, for example, the respondents cited current and expected levels of demand as the major reason, followed by price competitiveness and nonwage labor costs (European Commission, 1986). "Insufficient flexibility in hiring and shedding labor" ranked next, ahead of such factors as rationalization or the introduction of new technologies, direct wage costs, insufficient profit margins, and insufficient productive capacity. Differences in the share of firms answering that inflexibility was either very important or important have been used to construct country rankings of the severity of employment protection (see OECD, 1994, Table 6.7). Such indices may also be constructed by exploiting responses from other parts of the survey dealing with the specific labor market changes perceived to be most conducive to employment growth.

Although it has no longitudinal content and is not geared to employment growth, the IOE (1985) study also provides direct evidence on obstacles to terminating employment, as well as the severity of constraints on the management of working time. The study is based on a questionnaire distributed to 18 European and 2 non-European (New Zealand and Canada) employer federations. With regard to the former element, regulatory constraints are identified as "insignificant," "minor," "serious," or "fundamental," providing an obvious basis for ranking countries. As far as constraints on working time are concerned, these pertain not just to fixed-term contracts and temporary work agencies but also to part-time work. The same categories of response are identified, so that analogous rankings may again be derived for all or some of these forms of atypical contract (see OECD, 1994, Table 6.7; OECD, 1999, Table 2.6).

As a matter of fact, both the EU and IOE surveys are important building blocks in Emerson's (1988) influential paper on the scope for *deregulating* European labor markets. Arguably his study was more important than that of Lazear (1990) in stimulating cross-country analyses of how employment protection affects economic outcomes. Certainly, results from the IOE/EC surveys, together with other institutional detail contained in Emerson, provided the basis for the familiar Bertola (1990) index.

More recently, analysts have used even broader based surveys of employers that attempt to measure economic freedom and competitiveness. One such survey is the annual World Competitiveness Report (WCR), which covers some 21 countries. Top management is asked a large number of questions about national competitive performance. In 1990, for example, the report used over 300 criteria to measure competitiveness, mixing quantitative data and qualitative assessments by managers. The overall index of competitiveness provided by the WCR is a weighted linear sum of the components. Note that in addition to the market friendliness of economic institutions, the survey includes measures of actual economic performance and indicators of human capital. If for this reason there are difficulties in using the overall published rankings, it is nonetheless possible to use responses to specific questions. Di Tella and MacCulloch (1999) have recently exploited responses to the question that asks managers to rate the "flexibility of the enterprise to adjust job security and compensation standards to economic realities" on a scale of 0 to 100, where 0 indicates "none at all" and 100 indicates "a good deal." The time series is limited since this question was only asked between 1984 and 1990, when it was discontinued. In addition, there was no WCR containing 1987 data and the nature of the question changed in 1990.[8]

Apart from the WCR there are three other broad-based indices of economic freedom, provided by the Fraser Institute, the Heritage Foundation/Wall Street Journal, and Freedom House (Freeman, 2001). Their appeal is that they provide a time series. The downside is that the surveys on which they are based pay comparatively little attention to labor market institutions and employment protection per se, the focus instead is on private property rights, freedom to operate a business, free trade, and freedom of capital, etc. Of the indices, Freeman has argued that the five-year country ratings provided by the Fraser Institute – dating back to 1970 – are the most useful for assessing changes over time in the market orientation of countries. He reports that the index is reasonably highly correlated, albeit at one point in time, with the narrower index of employment protection provided by the OECD (1999) as well as with indicators of the labor market institutions of centralized bargaining and union density/coverage.

In the spirit of the competitiveness and economic freedom indices but altogether more specific is the new OECD database on indicators of product market regulations, based on member-state responses to a survey requesting information on approximately 1,300 *administrative* laws and regulations. Measures of the extent of product market regulation based on the questionnaires are provided by Nicoletti et al. (2000). Regulation is analyzed along three domains: (a) direct state control of economic activities, (b) barriers to entrepreneurial activity, and (c) regulatory barriers to international trade and investment. Factor analysis is used to analyze the extent of regulation within each dimension and also in aggregating across domains to provide an overall measure of regulation. As we shall see, both the grand measure and the disaggregated measures of the extent of product market regulation have been used to explain differences in cross-country nonagricultural employment rates (Nicoletti and Scarpetta, 2001).

The Appendix Table provides an overview of country rankings obtained from the various employment protection indicators. In each case, higher rankings correspond to increasing coerciveness of the regulatory framework. The Spearman rank correlation coefficients at the foot of the table provide evidence of some consistency between the various measures (see also OECD, 1999, Table 2.6). But note that the summary statistics conceal some notable differences in country rankings as between measures. Also note that we do not provide the scores on which these rankings are based.

Finally, note the restrictive focus in all of this on developed nations.[9] Recently Heckman and Pagés (2000) have derived a cardinal measure of employment protection index for 20 Latin American and Caribbean-basin countries (plus 16 other, largely European nations). Familiarly, the Heckman-Pagés index has a basis in the extent of severance pay and advance notice set by legislation but in a new departure it also exploits information on (a) the full tenure-severance pay profile and (b) the worker's probability of being dismissed (albeit assumed common across countries and set at U.S. levels). It is reported that even after a decade of often-substantial reforms, the level of severance pay in developing nations (measured in multiples of monthly wages) is considerably higher than in the industrialized countries. Note that this measure of employment protection pertains only to individual dismissals, considers only open-ended contracts, and while also including unfair dismissal (the product of severance pay for unjustified dismissal and the probability that economic difficulties of the firm are considered just cause) abstracts from dismissal costs that are ruled by a judge if a firm is taken to court (thus the U.S. is assigned a zero value in the index).

Omitted Variables. We next consider variables omitted from Lazear's empirical model that might bias the coefficient estimate for employment protection by virtue of their correlation with that measure and with the dependent variable. We also consider variables whose exclusion although not biasing the coefficient estimate for employment protection might nevertheless influence (amplify or reduce) the effect of policy on the economic aggregates and hence reveal more the simple "average" effect. In addition, we briefly examine two other policy variables that have been included in post-Lazear exercises, the inclusion of which should at least improve the precision with which the effects of employment protection are estimated.

Among the most important variables omitted from the Lazear study are collective bargaining, unemployment insurance, and product market regulation. Most attention has focused on the first variable. If employment protection is positively associated with union density/coverage (i.e., passage of legislation or more stringent legislation is more likely the higher is union density/coverage), and assuming that the effects of both variables are adverse, then omission of the union variable in the employment (unemployment) equation will produce a negative (positive) bias. In other words the adverse effects of employment protection will be overstated in each case. If on the other hand there is no correlation between union density and employment protection, the measured effect of employment protection will be unbiased but the "average" effect of the latter may hide more than it reveals, perhaps concealing important differences in the effect of employment protection in regimes of low, medium, and high union density. Interestingly, although post-Lazear studies typically include a measure of union density, much more attention has been accorded the *structure* of collective bargaining (and its interaction with employment protection). A commonly encountered argument is that a *centralized* bargaining process should result in better employment outcomes by taking the welfare of all workers into account and not simply those of insiders. Alternatively put, the more workers who are included in the bargaining unit, the better able is the union to internalize what would be externalities under a (more) decentralized bargaining regime. More concretely, in the model of Calmfors and Driffill (1988), totally decentralized bargaining represents a situation in which there is little scope for a union to raise its members' wages. As bargaining comes to cover more than the plant, bargaining power increases and with it the ability of the union to raise wages. Wage increases may feed through into higher prices but have little local effect on employment or prices more generally. As bargaining becomes yet more centralized – ultimately, fully encompassing – the price effects of wage increases become more obvious as do the disemployment effects and the price (and tax) consequences of excessive wage increases. Accordingly, so the argument runs, unions will take into account the effects of wage increases on all workers. The result is a humped or inverse U-shaped relation between wage restraint and centralization. Full decentralization and centralization yielding equivalent beneficial outcomes and sector-level bargaining with high union density produces the worst of all worlds.

A number of issues arise here. Exclusion of a bargaining structure variable à la Lazear is *prima facie* inappropriate since there is a potential omitted variables problem via the likely association between centralization and employment protection, especially if unions are construed as social partners. We note that Freeman (2001, p. 15) reports a negative correlation between his freedom index and degree of centralization (and union density). Second, even in the absence of any such correlation, it is interesting to examine whether the inclusion of the centralization variable modifies the effects of employment protection on employment/unemployment, and the literature has duly sought to interact the structure of collective bargaining variable with employment protection. Third, however, the calculation of a centralization score or index has occasioned considerable disputation as to the characterization of countries. For this reason, researchers have increasingly relied on the notion of *coordination* rather than (just) centralization, because the model relies on behavior rather than the fact of centralization. In practical terms, although economy-wide bargaining is perforce coordinated, highly coordinated bargaining need not be centralized. Coordination is conventionally measured by allotting a subjective score (say 1-3) to union and employer coordination and then summing/deploying separately, or by merging such scores with a degree of centralization measure. Needless to say, the identification of countries by the degree to which collective agreements are coordinated is fraught with difficulty and for reasons that magnify the subjectivity of the exercise; the coalitions across which coordination is practiced are presumably inherently unstable. A final observation is that increased trade calls into question the viability of the basic model; in particular, it undermines the notion that sectoral bargaining regimes (the *bête noir* in the story) can after all be viewed as different in kind from the two polar cases.

With regard to unemployment benefits, it has sometimes been argued that strict employment protection may be a substitute for unemployment insurance benefits. If so, the exclusion of benefits from the employment/unemployment equations will certainly bias the estimated impact of employment protection. Thus, if UI reduces employment, the bias in omitting the variable from the outcome equation will be positive. Assuming that the effect of employment protection is also to lower employment, omitting UI will understate the deleterious effect of employment protection. Similarly, the negative bias in the unemployment equation will mean that the effect of employment protection in elevating unemployment is understated. The converse obtains if there is a positive association between UI and employment protection. Having entered UI in the outcome equations, however, few analysts have sought to interact it with employment protection. Rather, it has been more common to interact the variable with a proxy for active labor market policy (see below). Theory suggests that the variable clearly belongs in employment equations irrespective of omitted variable bias or any mediating effect it may have on employment protection. When included in the relevant outcome equation, the unemployment benefit variable should attempt to reflect the full measure (i.e., generosity) of the UI system and not simply focus on replacement rates. In this context, we note that the OECD (1994) has derived a cross-country summary index of unemployment benefits based on an average of after-tax replacement rates for individuals with two earnings levels, two different jobless durations, and three different family situations. Even so, synthetic series of this nature are inevitably *ad hoc* given the complexity of national benefit systems.

A more likely source of omitted variables bias stems from the failure to model product market regulation. Very recent work, reviewed below, has argued that the two forms of regulation are positively correlated – and have the same directional effects on the economic aggregates of employment and unemployment. It thus appears that the omission of product market regulation imparts negative bias to the coefficient estimate on employment protection in the employment equation and positive bias in respect of the unemployment equation. But the more general point attaching to the positive association between the two forms of regulation is that it directs our attention to the politics side or, alternatively put, to the determinants of institutions. It serves as an important counterpoint to the suggestion that the institution of employment protection is an efficient response to a market alternative that is ruled out by incentive incompatibilities or enforcement problems. *Vulgo*: Institutions may be the result of the actions of employed insiders who, disregarding the interests of unemployed outsiders, either initiate legislation or manipulate existing legislation to reduce the pressure on their wages and jobs. Suffice it to say here that the literature has with some notable exceptions (principally Saint-Paul, 1993, 1996) paid scant attention to the endogeneity of employment protection legislation other than through largely cursory attempts along Granger-causality and similar lines (the principal exception is the study by Dertouzos and Karoly, 1993, reviewed below).

The two most commonly used additional (policy) variables used post-Lazear are the tax wedge and active labor market policies.[10] The tax wedge is the gap between the gross labor costs to employers and the consumption wage paid to employees (i.e., the wage after deduction of direct and indirect taxes). Reflecting the argument that switching between the components is largely immaterial (Nickell, 1997, pp. 68-69), most researchers have used the sum of these costs as a regressor rather than using, say, payroll costs.[11] The issue is of course the extent to which these taxes are shifted back to labor. Although in the long run, full shifting is often implied, this should not apply in the case of low-paid workers for whom statutory and collectively bargained wage minima and social welfare provisions will establish binding floors. Accordingly, their employment should fall with increases in non-wage labor costs.

In principle, active labor market policy offers the prospect of a reduction in unemployment and an increase in employment: directly by improving search efficiency and indirectly by reducing wage pressure. By the same token, it might insulate wage bargainers from the consequences of their actions by mopping up unemployment. It is an obvious candidate for inclusion in employment equations and is widely encountered in the employment protection literature, where it is typically measured by the expenditures on such measures per unemployed individual relative to output per capita. (The use of a per capita metric is justified by potential nonlinearities, namely, the possibility that active measures are more effective when unemployment is higher.) One problem with this measure is simultaneity bias in circumstances where greater expenditures on active labor market are triggered by rising unemployment. One partial solution favored in the employment protection literature is to "instrument" active labor market policy by normalizing on past values of unemployment; another is to treat the policy variable as a fixed effect. There is a further problem, however, associated with the administrative treatment of participants in such programs as not unemployed when many of them will in fact be looking for work. There are obvious political benefits to policymakers massaging the unemployment rate by reclassifying participants as not unemployed.

Finally, with respect to the dependent variable(s), and abstracting from measures of employment and unemployment dynamics reviewed in the next section, there have been some modest innovations post-Lazear. Perhaps the main development has been the further disaggregation of the employment and unemployment indicators by demographic group and to a much lesser extent by type of contract. In addition, researchers have used measures of structural unemployment, short- and long-term unemployment, and overall labor supply as dependent variables. The definition of labor market performance has also been widened to include productivity growth.

IV. *Employment Protection and Employment: Post-Lazear Outcomes*

Results from 15 studies that examine the effects of employment protection on employment and unemployment – including Lazear for completeness – are summarized in Table 1. Some additional findings are also provided in the text. Dynamic considerations are largely remitted to the next section although, as we shall see from that discussion, the distinction is in part artificial.

(Table 1 near here)

Consider first the results for employment. We do not further discuss the Lazear study or its replication by Addison et al. (2000) in the first two rows of the table. The preponderance of the remaining studies support the Lazear conjecture that countries with stricter employment protection rules have lower employment-population ratios (and possibly lower employment growth as well). Thus, with the major exception of the OECD study in row 9, therefore, and to a lesser extent the studies by Nickell and Layard (rows 7 and 8), all other estimates point to a reduction in total employment in more onerous/generous employment protection regimes, despite differences in the employment protection measure, time period, econometric specification, and underlying model. Each of the three "dissenting" studies provides some evidence of a negative correlation between employment protection (ranked least to most restrictive) and employment; that is, no study reports positive coefficient estimates for the employment protection indicator. Note that Nickell/Nickell and Layard downplay their finding of lower employment in stricter employment protection regimes, arguing that much of this correlation reflects low participation rates among married women in southern European nations which also happen to have strict employment protection rules. But this is unconvincing because participation rates may be low because of the more limited employment prospects caused by a stricter employment protection regime. We shall return to this issue below in addressing results by demographic group.

But if all studies point to a negative association between overall employment and employment protection, the estimated effects of employment protection differ markedly. Again this is hardly surprising given differences in the employment protection indicator, the time periods covered, the controls used, and the econometric specifications of the tests. More interesting in this regard are the results for some of the other variables and their interactions with employment protection. Potentially most important, not least in the light of the unemployment results reviewed below, is the association between collective bargaining and employment protection. Virtually all studies using conventional union power variables (union density and coverage) find them to be associated with reduced employment. However, most studies also include measures of the structure of bargaining as well, to test the argument that centralization/coordination may be beneficial by internalizing bargaining externalities. The strongest evidence as it pertains to employment is contained in the studies by Nickell/Nickell and Layard (rows 7 and 8), where it is reported that although union presence reduces employment this effect can be nullified by coordination. Here the argument is simply that the positive coefficient estimate for union and employer coordination dominates the negative effects of union density and union coverage. Only one employment study (row 11) conditions employment protection on the degree of coordination, and it reports a highly statistically significant negative coefficient estimate for employment protection interacted with medium coordination. (The interaction with high coordination is also negative and statistically significant in some specifications.) This study by Nicoletti and Scarpetta also examines the interaction of bargaining structure with the intensity of business regulations, and ostensibly provides stronger evidence favoring the corporatist notions in this regard.

Few employment studies have examined employment effects by demographic group and type of employment, and as one might expect the results are more varied than for overall employment. The first point is that virtually no study produces statistically significant negative results for *prime-age males*. The one exception is Heckman and Pagés (row 13), who report statistically significant negative effects in two out of three specifications, although this seems to stem from the inclusion of Latin American and

Caribbean countries in the sample (see below). This prime-age male result is consistent with the insider hypothesis, noted earlier, but the question arises as to whether other pieces of evidence are equally consistent with this view. Abstracting from type of employment, the answer seems to be a qualified yes. Thus the most recent OECD study (row 9) reports negative, albeit statistically insignificant, coefficient estimates for the employment protection variable in equations estimated over prime-age females and youth. Di Tella and MacCulloch (row 12) find that their flexibility index – an inverse measure of employment protection – is consistently associated with higher female employment, and Heckman and Pagés (row 13) confirm this result for prime-age females (in advanced industrialized nations) and report especially strong negative results for youth employment.

As far as type of employment is concerned, there is some evidence favorable to the hypothesis. Thus, two studies report that self-employment is higher in regimes with stricter employment protection (rows 4 and 9).[12] On the other hand, Heckman and Pagés (row 13) find no consistency in the relation. And there is little systematic evidence at cross-country level regarding atypical employment. Although the simple regressions of Grubb and Wells (row 4) indicate that employment in temporary work is highly correlated with the stringency of dismissals protection for regular workers, the much more detailed multivariate analysis of the OECD (row 9) detects no evidence that the share of temporary employment in total employment varies directly with the overall measure of employment protection. Indeed, it reports a statistically significant *negative* coefficient estimate for an employment protection measure indexing the strictness of the rules applying to regular, open-ended contracts. However, nonlinearities might be important here. As noted by Boeri et al. (2000), a relatively small difference in employment protection between regular and temporary and permanent contracts may lead to more significant shifts in one or the other than in countries with less restrictive overall regulatory regimes.[13]

We noted earlier that levels of employment protection are broadly higher and more variable in some blocs (say Latin American countries) than in others (say, OECD-Europe). Using a broad measure of economic freedom, Freeman (row 14) has exploited this argument to support his conjectures on the leeway European, if not developing countries, have to operate different degrees of employment protection without obvious disadvantage to their national economies. This is the notion of social space. But note that Heckman and Pagés (row 13), using a direct measure of employment protection, report that with some exceptions the adverse employment effects of employment protection characterize Latin American and OECD countries alike.

Finally, just two studies in the table cover employment growth (rows 9 and 10). The basic finding seems to be that strong negative associations between employment protection and employment growth are observed in cross section but that the effects are imprecisely estimated using panel methods.

Not surprisingly when we turn to consider simple unemployment rates (standardized or otherwise), the evidence is mixed. Thus, there is disagreement as to the sign of the effect of employment protection on overall unemployment (cf. rows 7, 8, and 9 with rows 12 and 13). That said, we should not exaggerate these differences since the results are generally statistically insignificant. (Note the two studies in rows 12 and 13 pointing to statistically significant increases in overall unemployment find that this outcome is rather sensitive to methodology). Moreover, as we have seen, there is also the suggestion that participation rates are reduced in more stringent regulatory climates. Note finally, the broad agreement across studies that the prime-age male unemployment rates is either reduced or unaffected by employment protection (rows 9 and 13).

The strongest evidence of adverse unemployment effects is found for other measures. Thus, using a measure of *structural unemployment* – defined as the difference between the actual level of unemployment and its cyclical component – the studies in rows 5 and 6 obtain positive and generally highly significant coefficient estimates for employment protection. Possibly reflecting the major disagreement between the structural unemployment study in row 7 and the recent OECD study in row 9, despite the use of otherwise similar variables, Elmeskov et al. (row 8) are at pains to argue that the adverse effects of employment protection may be offset by coordinated bargaining. In the Scarpetta study (row 7), coordination is measured independently of centralization and the effects of the two variables are opposite in sign. In Elmeskov, Scarpetta, and Martin the coordination variable now combines the two arguments and the coefficient estimate for the combined variable is strongly negative for the highest level of "corporatism" and strongly positive for "intermediate corporatism" (the omitted category being decentralized bargaining). Indeed, in separate estimations the authors suggest that adverse effects of employment protection are found only in "intermediate corporatism" regimes.

Certain other interactions reported by Elmeskov et al. are of more general interest. First, their interactions of employment protection with unemployment benefits provide no indication that strict employment protection acts as a substitute for unemployment benefits (which in almost every study are associated with significantly higher unemployment). Second, the interaction of the structure of bargaining variable with the tax wedge – although positive – is only statistically significant for intermediate and noncorporatist regimes. This result is suggestive because it might offer support for the notion that higher taxes are construed as part of the social wage under corporatism (see Summers *et al*., 1993). But any such conclusion would be premature. Thus, the basis for identifying statistically significant differences between the three (positively) signed interaction terms is not as clear-cut as the authors suggest, while no parallel iterations are provided for employment. In addition, this interpretation is further qualified by the favorable performance of the broader-based flexibility indicators (see, respectively, the studies in rows 11 and 12).

At the disaggregated level, the recent OECD study (row 9) fails to detect any effect of employment protection on female, youth, or low-skilled unemployment. It reports just one marginally significant association – a negative one – between employment protection and male prime-age unemployment. Diametrically opposing results for youth unemployment are reported by Scarpetta (row 7), however, while Heckman and Pagés (row 13) obtain generally positive coefficient estimates for employment protection in their prime-age male, prime-age female, and youth unemployment equations, though the pattern of the results on this occasion does vary by national grouping. The mixed evidence on long-term unemployment is further addressed in section V.

The last study in Table 1 by Blanchard and Wolfers should perhaps be left to the next section. Our justification for including it in the table is that it blames unemployment on adverse economic shocks rather than on employment protection directly, noting that the regulatory apparatus was in place before the rise in unemployment (and European unemployment in particular). But if not the direct cause of unemployment, labor market institutions including employment protection interact with these shocks and can – albeit temporarily according to the authors, as the shocks fade and institutions become more employment-friendly – increase the equilibrium rate of unemployment or the persistence of unemployment. The results in row 15 suggest that employment protection, as well as generous unemployment benefits and the extent of the tax wedge, can indeed make the unemployment situation worse. That is, Blanchard and Wolfers report a uniformly significant positive coefficient estimate for a fixed-in-time measure of employment protection interacted with different representations of the shocks – which are first treated as time effects and then as observable and country-specific effects (see also Bertola et al., 2001). By the same token, the interaction term(s) for coordinated bargaining is negative and significant suggesting that a greater degree of coordination can improve the unemployment outturn. However, when the authors substitute changes in employment protection (and UI benefit generosity) for their static counterpart(s) not only does the fit of the unemployment equation worsen but the respective interaction terms become statistically insignificant. Of course the static and dynamic measures of employment protection differ in their construction, and this may explain one important source of instability in the measured effects of employment protection observed both here and in wider the literature. For their part, Blanchard and Wolfers simply caution that the data used to construct the time-varying series (of employment protection and shocks) may be poor and that they are looking at the product of the two series.

Finally, very little attention has been paid to the potential simultaneity between unemployment and employment protection. Where attempted in the cross-country literature, the approach follows that first used by Lazear and focuses on timing. Thus, for example, Di Tella and MacCulloch (row 12) report that their flexibility index (an inverse measure of job protection) lagged is a better predictor of the change in unemployment than is lagged unemployment a predictor of the change in employment protection. More progress in adjusting for the simultaneity bias in the employment protection coefficient in outcome equations has been made in country studies, particularly by Dertouzos and Karoly (1993) in modeling the employment effects of the erosion of the hire-at-will common law principle in the United States as the result of the incursions of state judiciaries. The degree of incursion is a function of the scope of the legal exceptions to hire-at-will and the nature of the remedy. Specifically, using state data for 1980-1987, the authors distinguish states according to which of three (hybrid) wrongful dismissal doctrines their courts have embraced and whether or not the remedies provided are contractual or tort based. The employment equation includes as regressors the level and growth of gross state product, year and state dummies, and the presence of these wrongful termination doctrines/remedies. The latter are predicted values based on logistic models of the probability that a state has a particular wrongful-termination doctrine or remedy. Factors included in the logit equations include legal spillovers from contiguous states, the percentage change in lawyers per capita, whether or not the state is right to work, and the degree of union density, *inter al*. The simultaneity corrected outcome equation indicates that aggregate employment is on average 2.9 percent (1.8 percent) lower following a state's recognition of tort (contractual) damages for wrongful dismissal. Regressions run for other combinations of doctrine and remedy confirm that it is tort remedies rather than type of exception that drives the disemployment result.[14] This study again makes the point that court decisions matter and optimally need to be reflected in cost of dismissal measures used in the literature. As far as we know, the only other study to recognize these subtleties is Ichino et al. (2001), who investigate firing litigation in Italy. They show that an objective criterion for fair dismissal – a worker's misconduct – may not be adjudged sufficient in Italian courts in loose labor markets. The implication from both studies is that high unemployment may increase firms' firing costs.

V. *Speed of Adjustment Issues/Studies*

Studies Using Aggregate Data. We begin by analyzing the impact of employment protection in the context of standard labor demand models in which aggregate employment appears as a function of output demand and input prices. In this framework, the fluctuations of the stock of employment are the prime concern, and the effects of such rules are evaluated by looking at aggregate measures of employment inertia and through the derived indicators of the speed of adjustment of labor demand in response to exogenous shocks in output.

Examples of studies using this approach are Abraham and Houseman (1993, 1994) and Hamermesh (1993). By specifying employment only as a function of output (plus a linear or quadratic time trend) the main goal was to inquire into the nature of employment-output relationship over a sufficient long period of time either by using industry or economy-wide data. The typical – Koyck – specification in these studies is as follows (in logs):

$$l_t = \alpha + \lambda l_{t-1} + (1-\lambda)\phi y_t + \delta_1 T + \delta_2 T^2 + \varepsilon_t \qquad (1)$$

where l denotes employment, y is output (taken as exogenous), and T is the time trend. λ, $0<\lambda<1$, determines the speed of adjustment: the higher is the parameter λ, the lower is the speed of employment adjustment to changes in output.

Based upon equation (1) the effect of a regime change in employment protection can be tested using:

$$l_t = \alpha + \alpha_1 D + (\lambda + \lambda_1 D) l_{t-1} + (1-\lambda-\lambda_1 D)\phi y_t + \delta_1 T + \delta_2 T^2 + \varepsilon_t\,, \qquad (2)$$

where D is the regime change dummy, and λ_1 measures the regime change specific effect.

As a practical matter, none of the implementations of models (1) and (2) in core OECD countries were able to detect a discernible impact of changes in job security regulations – introduced in the 1980s – on the speed of employment adjustment.

More general lagged models have also been estimated, typically of the form:

$$l_t = \alpha_0 + \alpha l_{t-1} + \beta T + \sum_{i=0}^{k} \gamma_i y_{t-i} + \varepsilon_t\,, \qquad (3)$$

which yields a mean lag in employment adjustment of $\alpha/(1-\alpha)$ periods. An interesting extension of (3) is the following model (Hamermesh, 1993):

$$l_t = \alpha_0 + \alpha l_{t-1} + \alpha' T l_{t-1} + \beta T + \sum_{i=0}^{k} \gamma_i y_{t-i} + \sum_{i=0}^{k} \gamma'_i T y_{t-i} + \varepsilon_t\,, \qquad (3')$$

which allows assessment of the speed of adjustment over time either directly through the parameter $\alpha + \alpha' T$ or by simulating the impact of a steady-state increase in output demand on the path of employment. Using these derived measures, countries can be then compared and the impact of legislative changes analyzed by implementing standard Chow tests to evaluate the impact of a given liberalization package. In the United States, for example, there seems to be a trend toward a reduced speed of adjustment. Using separate regressions on nine two-digit industries, Hamermesh (1993) concludes that due to the erosion of the hire-at-will common law doctrine (see section IV), workers in specific sectors seem to have become more isolated from shocks to product demand.

Apart from the finding that liberalizing moves in employment protection in the 1980s and 1990s had little or no effect, the broad conclusion of this early literature was that the speed of adjustment in Anglo-Saxon countries exceeded that in European nations. The caveat was that such differences were muted if labor is measured in hours rather than number of employees; that is, continental European nations, especially Germany and France, tend to adjust mostly through hours, while the United States and the United Kingdom do so mainly via employment.

Models (1)-(3') exemplify the econometric literature in empirical labor demand. They can be criticized for their failure to adequately specify the dynamics of the employment-output relationship and to control adequately for the effects of technology and input prices (wages, materials, energy, capital, etc.). But there is a different strand of literature – the time-series tradition – which departs from the previous modeling strategy by fully specifying the labor demand model, including a detailed statistical analysis of the relevant time series variables and direct modeling of the dynamics of employment adjustment, mostly via Error Correction Models (ECM).

If the time series in labor demand analysis are stationary after first differences, one can estimate the labor demand model by simply taking first differences of equation (3) and use OLS methods to estimate:

$$\Delta l_t = \alpha_0 + \beta T + \sum_{i=0}^{k} \gamma_i \, \Delta y_{t-i} + \varepsilon_t \, . \qquad (3'')$$

In this case the response of employment to changes in output is given by the coefficients γ_i. This is the method used by Abraham and Houseman (1993). The problem is that simply using the model in first differences will introduce a misspecification bias because it fails to account for the underlying relationship between the level of the variables. In these circumstances and provided that the labor demand variables are cointegrated – that is, provided there is any vector α such that $l_t - \alpha X_t = u_t$ is stationary, where X_t denotes the set of exogenous variables – the most appropriate procedure is to formulate an ECM model in which both the long- and short-run components of labor adjustment are taken into account in characterization of labor demand.

In this approach, the first step is therefore the specification of the static, long-run relationship between the relevant labor demand variables, that is, the specification of a standard labor demand derived from a cost minimizing firm in which output and input prices are taken as exogenous variables (e.g., Addison and Teixeira, 2001a):

$$l_t = \alpha_0 + \beta T + \alpha_1 y_t + \sum_{i=1}^{n} \gamma_i z_{it} + \varepsilon_t \, , \qquad (4)$$

where l denotes labor demand, y is output demand, the z_i are the input prices, and T is a deterministic trend term that controls for those changes in employment over time unexplained by output and wage growth. From equation (4) one can derive then the long-run employment elasticities (with respect to output and input prices). Its main role however is to facilitate analysis of the dynamics of labor demand. This is typically achieved through the specification of a single- or two-stage error correction model.

In the first stage of the two-step Engle-Granger ECM model, residual-based tests are applied to the OLS regression on the levels of the labor demand variables and then the speed of adjustment of labor demand to deviations from the long-run relationship (say, in response to an exogenous change in output demand) is then estimated (Engle and Granger, 1987). Formally, in the first stage one estimates (4) and in the second stage:

$$\Delta l_t = \delta_0 + \lambda ecm_{t-1} + \sum_{i=1}^{k} \delta_i \Delta l_{t-i} + \sum_{j=1}^{n} \sum_{i=0}^{l} \gamma_j \Delta x_{j,t-i} + \varepsilon_t \, , \qquad (5)$$

where ecm_t are the residuals from equation (5), and X_t denotes the right-hand-side variables included in the model.

Typically, the length of the time series used in labor demand studies is very short (two to three decades of quarterly data). As a result, the power of residual-based cointegration tests used to test for the presence of the long-run cointegrating relationship is suspect (see the critique and extensive survey in Maddala and Kim, 1998). An alternative approach is to formulate a single-stage ECM model in which the long-run labor demand coefficients (elasticities) and the error correction term λ are jointly estimated by nonlinear methods. In this case, the model is specified as:

$$\Delta l_t = \mu + \lambda [l_{t-1} - \sum_{j=1}^{n} \alpha_j x_{j,t-1} - \beta T] + \sum_{i=1}^{k} \delta_i \Delta l_{t-i} + \sum_{j=1}^{n} \sum_{i=0}^{l} \gamma_j \Delta x_{j,t-i} + \varepsilon_t \, . \qquad (6)$$

In the employment protection literature, the parameter λ (the error correction term) is of course the focal point: the higher is λ, the faster is the short-term reaction to deviations from the estimated long-run employment-output equilibrium. However, given the methodology – in particular, the presence of many right-hand-side lagged variables and the need to simplify the initial over-parameterized model – the dynamics of labor adjustment are subsumed in all lagged parameters of the model, not just in the error correction term. Therefore, and since the parameters included in the final empirical specification of the model other than the long-run elasticities are not simple to interpret, a common procedure in the literature is to simulate the impact of some exogenous shock (e.g., an exogenous and permanent change in output demand or a once-for-all exogenous shock) on employment adjustment. Countries might be expected to differ in the pattern of labor demand adjustment in a manner that reflects the stringency of their employment protection.

(Figure 1 near here)

Examples of this approach can be found in Flaig and Steiner (1989), Burgess et al. (2000), and Addison and Teixeira (2001a). Panels (a) and (b) of Figure 1 provide some illustrations of this procedure for a sub-set of OECD countries. Since the main goal of the exercise is to estimate the speed with which employment converges towards its long-run equilibrium, we simulate the impact of a once-for-all exogenous shock in the employment equation, implied by the corresponding model estimates. Panel (a) provides results for four countries using aggregate quarterly manufacturing data, 1977-1997. Panel (b) provides results for four countries, 1960-1992, but this time using two-digit SIC industry level data. In both cases the simulations are based on coefficient estimates derived from the single-stage ECM model (6).[15]

At the risk of some simplification, the main finding is that the Anglo-Saxon countries (the United States and the United Kingdom) tend to present a higher rate of employment adjustment than southern European countries (Italy and Spain in particular). For its part, Germany shows simultaneously a more erratic behavior and, say, a medium-range speed of adjustment. Particularly surprising is the case of Portugal, which, despite its reputation as an exemplar of stringent employment regulation, evinces an above-average speed of employment adjustment.

Simulation exercises of this type, based on equations (4)/(5) and (6), are of course indirect attempts to capture the effects of employment protection legislation. An interesting extension of this approach is to relate the labor demand adjustment parameter directly to labor market institutions.

The general framework for this exercise can be specified as follows (see Kraft, 1993; Nickell and Nunziata, 2000):

$$\Delta l_t = \mu + \lambda_t \, [l_{t-1} - \sum_{j=1}^{n} \alpha_j x_{j,t-1} - \beta T\,] + \sum_{i=1}^{k} \delta_i \Delta l_{t-i} + \sum_{j=1}^{n} \sum_{i=0}^{l} \gamma_j \Delta x_{j,t-i} + \varepsilon_t \,, \quad (7)$$

where $\lambda_t = \theta_0 + \sum_{j=1}^{J} \theta_j Z_{jt}$, and Z_j indicates various labor market institutions in index form as proxies for adjustment costs such as employment protection, labor standards more widely, union density/coverage, and union coordination. The specification may also include interaction terms involving any pairwise combinations of these variables (see also Blanchard and Wolfers, 2000; Burgess, 1988). More cumbersome versions of equation (7) include δ_i and γ_j as a function of Z_j as well, but the usual short length of the series is a serious limitation. As in equation (7), the most common procedure is to assume that the long-run parameters are constant.

Within this framework and using a cross-country panel of 20 countries and 32 annual observations, Nickell and Nunziata find that employment protection has a negative impact on the speed of adjustment, as does union density, but that when these two variables are interacted the effect is to increase the speed of adjustment. The net effect is nonetheless a reduced speed of adjustment. For their part, union coordination, union coverage, and labor standards raise adjustment speeds. It is argued that labor standards reduce operational flexibility and throw the burden of adjustment on to employment. Similarly, the authors speculate than when employers have wages imposed on them from without, they have to focus on the employment margin, again leading to more rapid adjustment.

Kraft (1993) uses a similar but much simpler model to test for changes in the speed of adjustment over time and whether the changes in German labor legislation have had any impact on employment flexibility. The specification of vector Z includes a time trend, a change dummy, the union density, and the unemployment rate. The model is fitted to annual data for 21 West-German manufacturing industries, 1970-1987. No evidence of decreased flexibility (as hypothesized) is reported nor do changes in labor market regulations aimed at stimulating job creation seem to have produced any visible effect in the speed of adjustment. More surprising perhaps is the absence of any effect from unionization.

Despite its richness, even in its most parsimonious version, the model in equation (7) is not immune to criticism. In single-country studies, the researcher needs detailed and accurate data on changes in employment protection over a meaningful time span.

Unfortunately this is nontrivial requirement and may be also a source of measurement error with ad hoc manipulations of the data. The lack of detailed time-series single-country data can be offset by collecting cross-country data with fewer data points. In this case, however, and abstracting from the difficulty in reconciling national idiosyncrasies in data collection, any statistically significant effect will arise from cross-country variations and any effects of changes in legislation will be hard to capture.

Firm-Level Data. The above analysis of employment adjustment was developed using data on employment changes at the industry or economy level. But the implicit assumption of a representative agent in such aggregate studies flags the problem of aggregation bias. In particular, aggregation over single units with possibly very distinct patterns of employment adjustment can produce a much smoother adjustment process than would be observed if an appropriate disaggregation of the data were used. It this case, attributing observed differences in adjustment behavior to changes in labor market regulations either in cross-country or single-country time-series studies can only be done very cautiously because the speed of adjustment in aggregate models depends on adjustment costs parameters and on the distribution of shocks across firms. In short, the use of aggregate data may mask relevant and heterogeneous micro behavior, likely hampering precise parameter inference (Varejão, 2001).

In standard aggregate models of dynamic labor demand, adjustment costs are assumed to be quadratic, reflecting the presumption of smooth employment adjustment to its long-run equilibrium. However, a smooth pattern can be found even if the true micro structure of adjustment is lumpy (due to the presence of fixed adjustment costs). In these circumstances of large and infrequent micro-level adjustments, the synchronization of the micro units' actions will be crucial in shaping the aggregate adjustment path. The risk is that the less synchronized are the individual actions, the smoother will be the observed aggregate pattern. The speed of adjustment parameter in this case should be interpreted as representing the proportion of firms that keep their employment level unchanged or the fraction of the sample period in which, on average, their employment level is fixed (Anderson, 1993).[16]

We next assemble some indirect evidence on the effects of employment protection from firm-level studies, without suggesting that disaggegation is a panacea. For example, in most cases the time dimension of the micro data panel is not adequate to test for regime changes, and typically the output variable and input prices are subject to substantial measurement errors (in greater degree than in time-series data). There are also the issues of frequency of observation and representativeness. Based on the parameters of the panel estimation we can derive the mean adjustment lag (a summary indicator of aggregate labor market dynamics) and compare it with the time-series estimates reflected in Figure 1.

Following Nickell (1984), Layard and Nickell (1986), Dolado (1987), and Bentolila and Saint-Paul (1992), the standard labor demand model in panel studies can be specified as follows (in logs):[17]

$$l_{it} = l\, l_{t-1} + b¢(L)X_{it} + m_i + n_t + e_{it}, \tag{8}$$

where L is the lag operator, and b is the vector of coefficients of exogenous variables. All unobservable variables specific to the individual firm are captured in the time-invariant firm-specific component m_i, macroeconomic events (aggregate demand shocks) specific to a given year are represented by n_t, and e_{it} is a white noise residual. The input prices of labor and materials are usually treated as endogenous variables given that, in most cases, they are obtained by dividing total costs by total employment and because wages and employment may be jointly determined under collective bargaining. Given the presence of lagged dependent variables on the right hand side of equation (8), the standard panel techniques will produce biased and inconsistent estimates. The most commonly used technique is the Generalized Method of Moments (GMM) estimator developed by Arellano and Bond (1991) which extends the first-difference instrumental variables method suggested by Anderson and Hsiao (1981) to dynamic fixed-effects models.

(Table 2 near here)

In Table 2 we present some applications of this technique. The estimates are often very sensitive to instruments used, a result that has been interpreted by some authors as further evidence that the fixed-cost hypothesis rather than the quadratic cost structure is probably more appropriate in modeling dynamic labor demand (Varejão, 2001). In the table we only report results from annual data. The main reason for this procedure is the lack of appropriate panel data at quarterly level – the existing panels at quarterly frequencies are all derived from interpolation from raw annual information.

Table 2 shows that panel studies in general yield longer lags than the corresponding time-series studies. Preliminary evidence suggests that temporal aggregation (i.e., the use of annual rather than quarterly data) is less dangerous than spatial aggregation (i.e., aggregation over single units), especially in less turbulent labor markets (Varejão, 2001). Broadly speaking, the findings in Table 2 confirm country rankings established in time-series studies (Addison and Teixeira, 2001a), with the United Kingdom showing the lowest employment persistence and Spain the highest.

Some of these studies have also provided additional measures of the effects of employment protection. Bentolila and Saint-Paul (1992), for example, attempt to link the effect of macro and idiosyncratic shocks on the pattern of employment adjustment over the cycle in relation to country-specific features of employment protection. In particular, they look at fixed-term contracts in Spain, a country noted for its extensive use of such temporary employment (more than 30 percent of paid employment). The model's main prediction is that if an economy places strong limitations on the use of regular, open-ended contracts, then not only will there be a greater preponderance of workers in short-term jobs but also that shocks will have a different impact over the cycle. Specifically, lower employment inertia will be expected in expansions (as well as higher wage elasticities). The empirical evidence gives some indication that firms in expansion tend to adjust more quickly to unexpected changes in demand. Specifically, employment inertia is slightly lower during expansions. But there is no supportive cross-country evidence permitting generalization of this result. These and other findings (Bentolila and Dolado, 1994), however, do suggest that firm-level panel estimation may offer a useful additional check on the impact of different employment protection regimes.

VI. *Gross Job Flows*

In the previous section we focused on the effects of employment protection on the speed of adjustment using both aggregate and firm-level data. Our discussion showed the importance of having a good understanding of fluctuations in the stock of employment over time since these provide an aggregate view of the dynamics of firm behavior. For example, a slow reaction of employment or highly persistent employment behavior will not be desirable if the economy requires material restructuring.

But if the main concern is job turnover – defined as the sum of job creation and job destruction across individual firms or establishments – and the quality of job matching, analysis of net flows will be insufficient. The same rate of employment adjustment can conceal very different rates of job reallocation and have very different efficiency effects. In this section we discuss the relationship between employment protection and gross job flows and consider some recent contributions aimed at reconciling theory and some seemingly contradictory cross-country evidence. In particular, we will examine the impact of firing restrictions on employment and unemployment flows and on the behavior of job turnover over the cycle. The interaction between employment protection rules and wage setting is also addressed.

At the outset, we note that the data on gross employment flows are poor. Not only is it more difficult to collect information on flows than stocks but the data also vary extensively in the manner of their collection (e.g., employment coverage, sectoral classification, and frequency.) Attempts at standardization are necessarily somewhat ad hoc and not surprisingly cross-country studies are scarce and disparate.

We begin by stating the proposition that stricter employment protection should lead to lower rates of job destruction and job creation but have ambiguous effects on average employment and unemployment. This is the argument that employment protection has more of an impact on dynamics than levels of variables. Whatever the sophistication of the theoretical models underlying the argument, the empirical evidence is basic. The focus has been on simple correlation exercises and parsimonious regression analysis.

To illustrate, we summarize successive OECD findings reported in two issues of the publication *Employment Outlook*. In the first study, rank correlations are provided between job turnover, unemployment inflows/outflows, and long-term unemployment. These are always signed in the expected manner – positive in the first two cases, negative in the third – but they are not statistically significant except in the case of long-term unemployment (all establishments) and unemployment inflows (continuing establishments) (OECD, 1996, Table 5.4). Similarly, the rank correlations between job turnover and different measures of employment protection are not statistically significant except in two (out of ten) cases (OECD, 1996, Table 5.6). In the second study, and with the provision of the new OECD employment protection index for the late 1990s (see section III), there is a visible improvement in significance levels for the overall measure of employment protection. Thus, both unemployment duration and long-term unemployment are now positively and significantly correlated with the index, and a significant negative association is reported for unemployment inflows (OECD, 1999, Table 2.12). However, there is no statistically significant relation between job turnover and this index, and disaggregation of the latter into its component parts yields weaker results throughout. These findings are broadly confirmed in regression analysis. For example, bivariate regressions provide no evidence that employment protection is an important determinant of differences in job turnover, although stricter regulation does seem to be associated with lower flows into and out of unemployment as well as longer jobless duration (OECD, 1999, Chart 2.3). Controlling for other variables and using the overall measure of employment protection confirms the rank correlations. In particular, the use of a two-period panel regression yields statistically significant coefficients for the regulation in the cases of unemployment inflows/outflows and unemployment duration (though not long-term unemployment)(OECD, 1999, Table 2.13). As before, disaggregation of the index produces poorer results. Just two (out of 12) significant coefficient estimates are obtained: unemployment duration increases with restrictions on regular employment, and regulation of temporary employment reduces unemployment inflows. There is thus some weak evidence to suggest that the stringency of employment protection impacts the dynamics of unemployment. But of considerable interest is the seeming failure of employment protection to influence annual job destruction and creation rates.

Much recent research has focused on the latter surprising result that major differences in employment protection do not translate into lower job turnover rates. In an attempt to reconcile the theory with the data, Boeri (1999) argues that employment protection restrictions lead to a higher proportion of short-term jobs and that the holders of these jobs compete with the unemployed for both open-ended and temporary employment, thereby reducing the job finding prospects of the existing unemployed. This effect is reinforced by another source of job competition emanating from about to be displaced workers who take advantage of the procedural delays and advance notice requirements of employment protection legislation to engage in on-the-job search. Boeri predicts that the main effects of more stringent employment protection are threefold: (a) a greater proportion of short-term jobs (specifically, workers on fixed-term contracts), (b) reduced transitions from unemployment to employment, and (c) a substantial share of long-term unemployment. No effect on overall job turnover is implied. Only the mechanism of labor adjustment will be different: occurring through short-term jobs in more sclerotic labor markets and via an active unemployment pool in less regulated ones. Countries with less regulation will evince higher than average unemployment flows that are "balanced" by a greater intensity of job-to-job shifts in their more regulated counterparts.

To test the implication that the unemployment outflow rate is decreasing with the share of temporary workers, Boeri regresses the job finding probability of the unemployed on the incidence of short-term employment (proxied by the proportion of workers under fixed-term contracts). (His grouped-logit model also includes a quadratic trend term and the growth rate of GDP lagged one period as a proxy for missing data on vacancies). The model is fitted using EU Labor Force Survey data on unemployment flows and four gender/age groups are distinguished. He found that a higher incidence of short-term jobs always leads to a reduced job-finding probability. The effects are stronger for older unemployed females and males (aged 25 or more) than for their younger counterparts.

The impact of employment protection on employment dynamics can also be tackled by analyzing the pattern of job creation and job destruction over the cycle. One such exercise is conducted by Garibaldi (1998), using a search-theoretic matching model. Garibaldi seeks to explain why job turnover has been counter-cyclical in Anglo-Saxon countries and acyclical (or even pro-cyclical) in continental Europe. He argues that employment protection, and especially firing delays, are the root cause: the greater the limitations placed on firing workers, the higher is the correlation between job turnover and net change in employment. In the absence of procedural delays but with costly hiring, job destruction is instantaneous while job creation is expected to take more time to implement. In this situation, job creation will persist more during recessions than will job destruction during expansions. In other words, job destruction will tend to fluctuate more than job creation. As a result, and all else being equal, job turnover will be higher when the net employment change is negative (during recessions) and lower when the net employment change is positive (during expansions), producing the hypothesized negative correlation between job turnover and net employment change when firing restrictions are low. But with an increase in firing costs, job destruction is no longer instantaneous. It will therefore tend to persist more in expansion than in a regime without firing delays and become less volatile. As a result, job turnover becomes positively (or less negatively) correlated with changes in net employment.

The time-series simulation results produced by the author's numerical model coincide with the model's main predictions, namely that: (a) job creation is procyclical and job destruction counter-cyclical for all levels of firing restrictions; (b) the relative variance of job destruction to job creation falls as firing restrictions increase; and (c) the correlation between job turnover and the change in net employment increases with firing delays.

Bertola and Rogerson (1997) explore a different route to the same end. If theory predicts lower job turnover but one observes approximately the same rate of job creation and job destruction across countries, then the similarity in job reallocation must reflect differences in wage setting. In continental Europe, so the argument runs, not only is the employment protection regime more stringent but there is also greater relative-wage compression from centralized wage bargaining. This wage compression is said to produce a higher volume of employer-initiated job turnover as well as a greater number of job-to-job shifts (which will be amplified for the reasons noted by Boeri). If firms cannot adjust wages in the face of adverse demand shocks, they will perforce adjust through more intense employer-initiated labor shedding (and hiring). The consequences are a higher frequency of job-to-job shifts in continental Europe and lower unemployment flows and a higher proportion of long-term unemployment. We again see in this dynamic approach the same emphasis on the interaction between employment protection and the structure of collective bargaining as is placed in much of the levels-of-variables literature.

Most recently, Blanchard and Portugal (2001) have countered that it is crucially important to analyze gross job flows at the appropriate frequency of observation. In particular, they claim that similar annual gross flows can be produced by completely different within-year labor dynamics. Although the study is based on data from just two countries (the United States and Portugal), it offers an important challenge to analyses based on the stylized fact of similar job turnover across countries. Specifically, Blanchard and Portugal confirm that lower quarterly flows (in Portugal) produce lower unemployment flows but find no corroboration of the notion that lower reallocation of jobs through unemployment is offset by higher job-to-job-shifts.

The authors also provide a calibrated flow model with job destruction to assess the impact of employment protection on output (and welfare). It is shown that higher firing costs lead to lower outflows from employment (i.e., lower layoffs and quits). Given that employment protection strengthens the bargaining power of workers, higher firing costs generate the major model's prediction: stricter employment protection does not necessarily lead to a higher rate of unemployment. But there is a cost. The duration of unemployment increases with firing costs and total output decreases. The latter result arises because firms incur firing costs and because the quality of job matches is reduced as a result of the lower flows through the market. Note the explicit acceptance in this study that a higher job reallocation rate is intrinsically better as means of improving the efficiency of the economy. Left unstated is the issue of "how much higher;" or, expressed differently, how many desirable separations does employment protection sacrifice/eliminate and what is the balance between the pros of short jobs and the pros of long jobs?

The inescapable conclusion of this review of the literature on gross flows is that there is a pressing need to supplement the aggregate studies with industry and especially firm data.[18] In the process, multivariate regression analysis (substituting for reliance on the numerical properties of derived models) and more detailed examination of the outcomes directly linked to labor reallocation should help identify the specific role of employment protection (including the potentially important impact of employment thresholds under legislation) and address the issue of the 'adequacy' of job turnover.

VII. *Conclusions*

Interpretation of data is something of an art in applied labor economics research. Nowhere is this truer than in assessing the impact of employment protection on economic outcomes because of the piecemeal nature of research, the focus on reduced forms, and daunting data problems. The opening citation from Nickell and Layard has to be viewed in this light.

Frankly, not all the evidence investigated herein is of equal quality and, assuredly, inadequate attention has been paid to the issues of parameterization and statistical inference. But we think it fairer to emphasize the very real problems that analysts have had to face and the ingenuity they have displayed in dealing with these difficulties. Consider the difficulties in formulating a measure of employment protection. Researchers have addressed the problem in a number ways. Some have opted for a narrow measure for reasons of tractability. Thus, focusing on legal severance pay entitlements has allowed the construction of a cardinal measure of the stringency of employment protection as well as a reasonable time series that in principle offers a solution to the inevitable problem of country heterogeneity. But consideration of the monetary costs to employers requires adding in factors that reflect the probability that severance pay will bite and in respect of whom. Thus, data are also required on voluntary turnover and the distribution of the labor force by the self-same characteristics as define the entitlements for severance pay (i.e., the occupational and tenure distributions). Absent some heroic assumptions, the virtue of simplicity is soon lost.

Other analysts have sought measures that capture more of the regulatory milieu. The favorite measures have included such things as the procedural inconveniences surrounding layoffs, severance and notice entitlements, and the difficulty of dismissal. More often than not the composite employment protection indicator combines these with restrictions on the use of temporary contracts. Not surprisingly the more ambitious the measure, the more difficult it is to generate a time series. Specific problems arise from the subjectivity involved in assessing the stringency of a particular component of employment protection where there is no unambiguous metric, the implicit or explicit weighting of the components (though factor analysis might help here), errors in interpreting legal provisions, and the vexed question of the application of laws which may be related to the outcome under investigation. Once such an index is constructed, however, it displays considerable persistence.

Yet other researchers have attempted to fashion an employment protection indicator from responses to specific questions in employer surveys as to the perceived seriousness of the constraints imposed by the regulatory climate. This strategy allows a longer time series to be assembled and arguably the measure can capture the varied dimensions of dismissals protection. But there are also some obvious problems – the consistency of responses when the economic conditions facing firms in the sample differ, changes in the identity of the manager respondent, and even changes in the relevant question – while aggregation across questions in surveys raises the same difficulties that arise in the previous case.

In the light of these difficulties, it is not surprising that some other observers have focused on outcomes expected to be most directly affected by employment protection legislation, such as the speed of adjustment of employment to exogenous changes in output. Such outcomes are then compared with the "reputations" of these countries as either flexible or sclerotic.

Despite these measurement problems, however, we do not subscribe to the view that little has been learned about the effects of employment protection or that the concern with employment mandates is a *divertissement*. What, then, has been learned? In the first place, it appears that employment *is* reduced on net. But this is an average effect and the consequences have been more discernible for some groups than others. It seems fairly clear that prime-age male workers have *not* been adversely impacted. This is an important result because there are also indications that other groups – most notably younger persons – have been negatively affected. Second, there is a positive association between employment protection and self-employment. Further progress in understanding this association requires that more attention be paid to the relationship between labor and product market regulation because policies that make it more difficult to start and operate a business will limit the growth in self-employment. More work is also needed on the opportunity costs of self-employment, including the relative level of security contributions that have to be paid by the self-employed. Finally, although it is widely supposed that atypical work is also stimulated by employment protection legislation, this perception does not receive ringing endorsement in cross-country data – though suggestive findings have been reported in country studies – once we move from bivariate to multivariate analysis. One problem here is that researchers have failed adequately to incorporate in their analysis the rules governing the regulation of atypical work. What is perhaps needed is a measure of the relative stringency of the rules governing regular open-ended contracts vis-à-vis those applying to fixed-term and temporary agency employment in the same jurisdiction. In this connection, we are not inclined to place much reliance at this time on results pointing to a reduction in hours worked in more stringent employment protection regimes. This is because inadequate attention has been devoted to the fixed costs of employment protection and to the specific mandates that govern working hours.

The results for average unemployment are not unexpectedly less clear-cut. Indeed, the coefficient estimates for employment protection in equations for overall unemployment are of mixed sign. That being said, statistically significant coefficient estimates where observed are positive in sign and stricter employment protection unambiguously increases *structural unemployment*. Factors mediating the association between employment protection and the aggregate unemployment rate are the tendency of stricter employment regimes to evince lower participation rates and higher inactivity transitions. At the disaggregate level, a number of studies have reported statistically significant positive (negative) associations between employment protection and *youth* (prime-age male) unemployment. Finally, the relationship between employment protection and long-term unemployment is always positive but is less robust in panel studies than in their pooled cross-section/time-series counterparts.

That part of the literature dealing with employment dynamics reveals some surprises. One such surprise is the seeming ability of some supposedly "sclerotic" nations to adjust the labor input to fluctuations in output rather more quickly than one might suppose. Partly this result has to do with their enhanced ability to substitute hours for workers albeit at some cost (which should presumably be reflected in lower employment on net, as is observed). But the result remains something of a puzzle. The other puzzle is that annual rates of job reallocation (i.e., job flows) are often as high in nations with stringent job protection as in countries with weak regulation. This awkward empirical regularity has prompted some ingenious explanations. Thus, it has been argued that the effect of employment protection in slowing down job flows is offset by greater wage inflexibility at plant level in regimes with rigid rules of this nature. The result is a wash, allowing countries with very different degrees of employment protection to record roughly similar rates of job creation and destruction. Alternatively, it has been asserted that job-to-job transitions are higher in countries with tighter employment protection rules because of their larger number of temporary and soon to be displaced workers who compete with the employed for work. The two former groups tend to transition into employment without an intervening spell of unemployment and the unemployed stay unemployed for longer. In other words, lower flows into unemployment and out of unemployment are nevertheless compatible with large job reallocation rates in countries with tighter employment protection because of the higher volume of direct job-to-job flows.

Yet more recently it has been asserted that the focus on annual rates of job creation and destruction is misleading since *quarterly* rates of job creation and destruction may be considerably lower in more regulated labor markets than in their less regulated counterparts. The argument here is that movements in job creation and destruction reflect the transitory and permanent components of desired employment. Job flows in countries with more flexible markets will have a larger transitory component. That component will be smoothed in countries with tight employment protection precisely because of their higher separation costs, but employers will have no option other than to react to the permanent component. The lower the frequency of observation (annual versus quarterly observations for example), the more relevant the permanent component and the less relevant the transitory component, meaning that differences in job flows between countries will be muted and much less informative on the consequences of employment protection for job creation and job destruction. One problem with this argument is that it is based on the experience of just two countries (cross-country quarterly flow data are generally unavailable). A related problem is the real ambiguity as to what constitutes an optimal job flow.

Empirical analyses of the role of employment protection in influencing flows into and out of employment suggests that it operates in the predicted manner to reduce both – and to increase jobless duration. But the strongest results are found for pooled data using OLS. The effects of employment protection are less robust when using more sophisticated panel data methods. This is not unexpected given the limitations of the flow data. More progress clearly awaits the exploitation of firm data sets, to include measurement of the effects of employment protection of firm formation and dissolution as well as on contraction and expansion. In the process, the intriguing issue of the contribution of employment size thresholds (below which firms are exempted from legislation) can and must be addressed.

The remaining issue raised by Nickell and Layard's statement is the suggestion that, whatever its sign, the economic significance of employment protection is modest. This assertion has two strands. First, are the effects of employment protection small? Second, are its effects important when considered alongside other features of a country's labor market. As far as the first question is concerned, it has now been openly conceded that the employment effects of employment protection can be substantial (Nickell and Nunziata, 2000, p. 12). This question need not detain us unduly, even if there remains the issue of proximate versus fundamental causation. On the second question, however, the balance of the empirical evidence firmly suggests that there are offsets to potentially costly employment protection rules. We refer in particular to the favorable association between coordinated collective bargaining and economic outcomes. The magnitude of the point estimates for coordinated bargaining are often such as to completely offset the adverse impact of employment protection on employment and unemployment. The empirical interplay between the structure of bargaining, employment protection and economic outcomes is troublesome nonetheless. In particular, many of the problems that arise in constructing an index of employment protection apply to the measurement of employer and employee coordination. There is inevitably a problem of "research Darwinism" (Blanchard and Wolfers, 2000, p. 22) given that the measure is constructed by researchers *ex-post facto*. After all, problems with the centralized bargaining variable led to its replacement by a measure emphasizing coordination. There is also the point that researchers have been content on occasion to gloss over seeming inconsistencies in the effects of coordination across different outcome indicators and to have largely limited its interaction effects to employment protection. Beyond the argument that corporatist economies control rent seeking better than others, little direct support for the argument has been offered and it remains a black box (but see Teulings and Hartog, 1998). Be that as it may, we are for the moment stuck with the robustness of the results, while accepting that they do not offer much by way of policy prescription since few observers have seriously argued that a country might improve its employment performance by embracing the collective bargaining institutions of another.

The broader question is the degree of freedom countries have to pursue different institutional arrangements. In applications of his economic freedom indicator, Freeman (2001) has concluded that that capitalism is a sturdy economic system that allows for diversity in institutional arrangements. The evidence we have assembled instead suggests that there are important competitive constraints on and hence employment consequences of ambitious employment protection schemes. But we have only been able to go so far, and it is the task of future research to establish the precise range within which institutional experimentation has small effects on outcomes. However, it is a moot point whether these limits can be identified with cross-country data or whether more progress can be made by a careful investigation (i.e., parameterization) of individual policies at the country level.

NOTES

[1]There is of course the general (Coasian) argument that in the absence of transaction costs the reassignment of job property rights under legislation will be neutral for employment, being fully offset by up-front payments from workers to their employers.

[2]As noted earlier, employment adjustment costs also imply substitution effects in favor of hours although, other things being equal, the total amount of hours worked should still fall.

[3]Pagés and Montenegro (2000) show how severance pay that is increasing in tenure can aggravate the position of younger workers.

[4]The countries are Austria, Australia, Belgium, Denmark, France, Germany, Greece, Ireland, Israel, Italy, Japan, the Netherlands, Norway, New Zealand, Portugal, Spain, Switzerland, Sweden, the United Kingdom, and the United States.

[5]The method is to compare the growth in a country's unemployment rate between 1956/1959 and 1981/1984 with the change in average severance pay requirements over the two periods multiplied by the coefficient estimate for severance pay in the unemployment equation.

[6]Note, however, that protean elements of a regulatory index are contained in Emerson (1988) (see below) and subsequently exploited by Bertola (1990).

[7]Neither Grubb and Wells nor subsequent analysts offer a measure of the severity of the regulatory apparatus governing part-time work.

[8]The question now asking the respondent to rate the "flexibility of management to adjust employment levels during difficult periods," again on a scale of 0 (=low) to 100 (=high).

[9]The Fraser Institute index also covers developing countries. As we shall see, this extended coverage is exploited by Freeman (2001).

[10]We abstract from controls for terms of trade and real interest rates, barriers to geographical mobility, cyclical proxies such as the inflation rate or the output gap, and the stance of macroeconomic policy, etc.

[11]For a detailed comparative study of the employment and wage effects of social security financing and taxes, see Tyrväinen (1995).

[12] See also Centeno (2001).

[13]But for an important recent study suggesting that the growth in employment protection in the United States attendant on the erosion of the common law hire-at-will principle has led to a 20 percent increase in temporary employment, see Autor (2000).

[14]Despite the strength of the employment effect, the authors nonetheless speculate that the benefits of dismissal protection may be worth the employment sacrifice by reducing uncertainty about the enforcement of implicit labor contracts and allowing the parties to more fully reap the benefits of long-term contractual relationships.

[15]Industry effects seem to be relevant because the common industry adjustment speed is clearly rejected in the study by Burgess et al. (2000). If sustained, these industry effects may raise issues of causality, with a given country specializing in industries that present relatively more favorable job security provisions or the latter being endogenously determined by the existing pattern of industry specialization and the resulting degree of labor market turbulence.

[16]These shortcomings of aggregate studies are not such as to preclude the use of aggregate estimates of the adjustment lag, where the main concern is to determine how exogenous changes in output demand impact the dynamic process of labor demand behavior.

[17]These studies assume quadratic adjustments costs. Although in many countries the data do not seem to clearly reject the lumpy adjustment case, researchers have been unable to draw clear-cut results because the competing hypotheses are non-nested.

[18]Early such studies include Blanchflower and Burgess (1996) and Burgess (1994).

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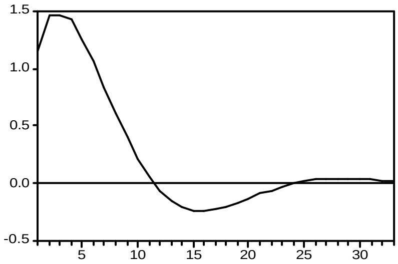
Figure 1

Percentage Change in Employment in Response to a One-Time Positive Demand Shock, Selected Countries

(a) *Manufacturing aggregate data*

Germany



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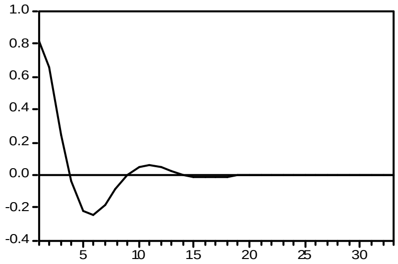
Portugal



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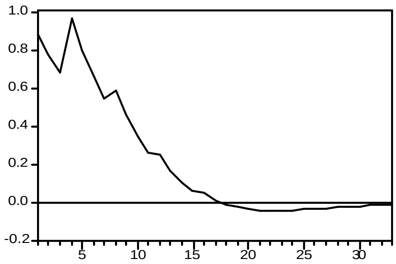
Spain



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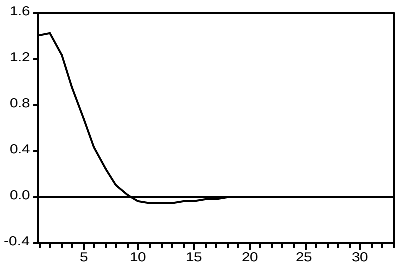
United Kingdom



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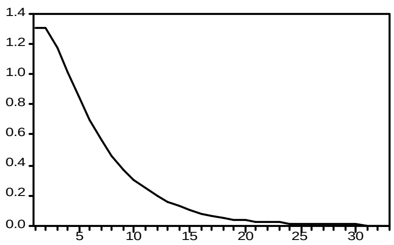
(b) *Industry (2-digit) data*

Germany



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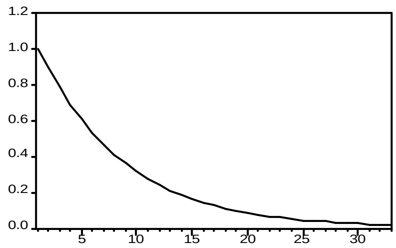
Italy



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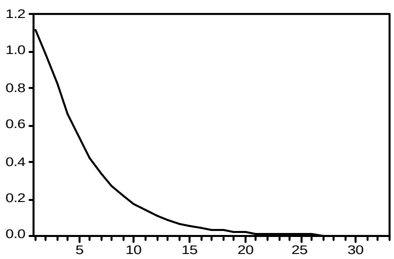
United Kingdom



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United States

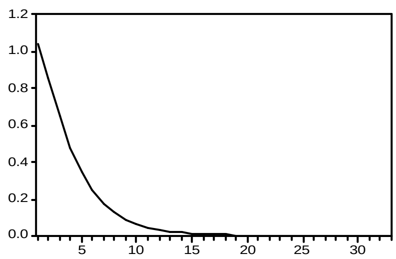

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Table 1

Effects of Employment Protection (EP) on Employment and Unemployment, Selected Studies

	Study	Sample	EP measure	Outcome indicator(s)	Other variables	Methodology	Finding
1	Lazear (1990)	20 countries; 1956-1984.	Severance pay due blue-collar workers with 10 years' service. Time-varying measure.	Employment-population ratio, participation rate, unemployment rate, average hours worked per week.	Quadratic time trend plus, in some specifications, controls for population of working age, and growth in per capita GDP (interacted with EP measure).	Pooled time-series/cross-section estimates. Selective corrections for fixed effects, random effects, and auto-correlation.	In favored specification, EP raises unemployment and reduces employment participation, and hours.
2	Addison, Teixeira, and Grosso (2000)	As above.	As above.	As above.	As above but uses full Lazear specification.	Fixed and random effects, with correction for autocorrelation, plus FGLS estimates.	EP is statistically insignificant.
3	OECD (1993)	OECD 19 countries; 1979-1991.	Severance pay and notice periods combined across blue-and white-collar workers. Moment-in-time indicator.	Long-term unemployment.	UI benefit duration; active labor market policy (ALMP) expenditures divided by UI benefit expenditures.	Pooled time-series/cross-section estimation.	EP has positive effects on jobless duration, especially in southern Europe.
4	Grubb and Wells (1993)	11 EU countries; 1989.	Authors' own indicators of restrictions on overall employee work (ORDW) dismissal of regular workers (RDSM), fixed-term contracts (RFTC), and temporary work agencies (RTWA)	Employment; self-employment; part-time work, temporary work; agency work.	None.	Simple cross-section regressions.	ORDW reduces employment, increases self-employment, and reduces part-time work. RDSM (RFTC) increases (decreases) temporary work. RTWA but not RDSM reduces temporary agency work.
5	Scarpetta (1996)	17 OECD countries; 1983-1993.	OECD strictness ranking for regulation of dismissal averaged over regular and fixed-term contracts (OECD, 1994, Table 6.7, Panel B, col.2).	Structural unemployment, plus separate regressions for youth unemployment, long-term unemployment, and non-employment rates.	ALMP calculated as expenditure on active measures per person relative to output per capita; summary index of UI benefits (OECD, 1994, Chapter 8); union density; union coordination, employer coordination, and their sum; centralization of collective bargaining; tax wedge; proxy for product market competition; real interest rates; output gap.	Random effects, FGLS.	EP raises structural unemployment, with stronger effects for youth and long-term unemployment. EP increases non-employment rate.
6	Elmsekov, Scarpetta, and Martin (1998)	19 OECD countries; 1983-1995.	OECD (1994, Table 6.7, Panel B, col. 2) ranking, but modified to take account of changes since late 1980s. Two-observation, time-varying indicator.	Structural unemployment.	ALMP (as above); UI benefits (as above); union density; dummies for the degree of coordination on the employer and union sides; dummies for degree of centralization of collective bargaining; tax wedge; output gap, minimum wage relative to average wage.	Random effects, FGLS.	EP raises structural unemployment but interaction effects are important. EP not statistically significant in either highly centralized/coordinated or decentralized bargaining regimes.

7	Nickell (1997)	20 OECD countries; 1983-1988 and 1989-1994.	OECD (1996, Table 6.7, Panel B, Col. 5) ranking. Also use of labor standards measure covering in addition to EP working time, minimum wages, and employee representation rights (OECD, 1994, Table 4.8, col. 6).	Employment-population ratio for whole working-age population and for prime-age males. Overall labor supply, defined as actual annual hours divided by normal annual hours multiplied by employment-population ratio. Log unemployment rate and component short- and long-term rates.	UI benefit replacement rate; UI benefit duration in years; union density; union coverage index; sum of indices of union and employer coordination; instrument for ALMP expenditure; tax wedge; change in inflation.	GLS random effects using two cross sections.	EP reduces overall employment rate but not that of prime-age males. EP also reduces overall labor supply. For unemployment, EP effect is negative but statistically insignificant. EP reduces short-term unemployment and increases long-term unemployment. Coefficient estimate for worker labor standards variable is statistically insignificant in unemployment regression.
8	Nickell and Layard (1999).	As above.	As above.	As above, plus measures of labor and total factor productivity growth, 1976-1992.	As above, plus owner-occupation rate as a negative proxy for geographic mobility.	As above. OLS for analysis of productivity growth.	As above. EP is positive and statistically significant in labor and total factor productivity equations, but effect vanishes with correction for initial productivity gap.
9	OECD (1999)	19 OECD countries; 1985-1990, 1992-1997.	OECD (1999, Table 2.5) measures for "late 1980s" and "late 1990s." Single overall indicator and also separate indicators for regular employment, temporary employment and collective dismissal. In some specifications further disaggregations for regular and temporary employment.	Log unemployment rate, log employment-population ratio, and changes in unemployment and employment. For unemployment: separate results for prime-age males, prime-age females, youth, and low-skilled. For employment: separate results for prime-age males, prime-age females, youth, share of self-employment, share of temporary employment, and temporary share in youth employment.	UI benefit replacement rate; UI benefit maximum duration; ALMP expenditures as percentage of GDP; degree of centralization of collective bargaining; degree of coordination of collective bargaining; trade union density; trade union coverage; tax wedge; output gap.	Two-period panel estimated by random effects, GLS. (Changes in levels model estimated by OLS.)	Irrespective of the form of the indicator, EP coefficient estimate is statistically insignificant for overall unemployment. It is positive and statistically significant for prime-age male unemployment (overall indicator only). For all other demographic groups EP is statistically insignificant. Further, changes in EP do not affect changes in unemployment for other than prime-age females, where the effect is negative and statistically significant (strictness of EP with respect to regular employment). For employment, the coefficient estimates for EP are negative but statistically insignificant for overall, prime-age female, youth, and temporary employment. Otherwise they are positive and in the case of self-employment statistically significant (overall EP measure and its regular employment variant). Further, changes in EP have statistically insignificant effects for overall employment and for all demographic groups. For self-employment and the share of temporary employment, some statistically significant negative effects are observed.

10	Garibaldi and Mauro (1999)	21 OECD countries; 1980 - 1998.	OECD (1994, Table 6.5, Panel B, Col. 5) ranking. Moment-in-time measure.	Average growth in total civilian employment.	Average change in inflation; average total taxation as share of GDP; average payroll taxes as share of GDP; average UI benefit net replacement rate for an unemployed worker (OECD, 1994, Chapter 8); union density; index of the coordination collective bargaining; time dummies.	Random effects, GLS: six-year averages of data (1980-1985, 1986-1991, 1992-1997).	There is a strong negative association between EP measure and employment growth in cross section (for 24 out of 27 cases), but in panel regressions the association is less precisely estimated and is statistically significant in one of five specifications only.
11	Nicoletti and Scarpetta (2001)	20 OECD countries; 1982-1998.	Two indicators of the stringency of the regulatory apparatus. The first is EP per se, and is based on the time-varying OECD (1999, Table 2.5) measure. The second is a measure of the degree of product model regulation and is both static (based on Nicoletti et al., 1999) and time varying (based on the authors' evaluation of regulation and market conditions in 7 energy and service industries, 1970-1998).	Nonagricultural employment rate.	Public employment rate; tax wedge; union density; dummy variables for high and intermediate coordination of bargaining based on a summary indicator combining centralization and coordination; UI benefit replacement rate composite measure (OECD, 1994, Chapter 8); and the output gap.	Fixed effects without product market regulation indicator; random effects with static product market regulation indicator; and two stage regression approach, the second stage involving regression of fixed country effects on the static product market regulation indicator. Also fixed effects panel estimates with time-varying EP and product market indicators.	In initial fixed effects specification, EP is associated with a statistically significant reduction in employment. When EP enters in interaction with the coordination of collective bargaining dummies, its effects are negative and statistically significant for both intermediate and high coordination. The same results obtain for the random effects and second stage regressions. In each case, the negative effect on employment is stronger in countries with an intermediate degree of coordination. The effect of the static product market regulation variable is statistically significant and negative. Finally, for the fixed effect panel regressions, EP is negative and statistically significant in the basic specification. In interactive form, however, the negative coefficient estimate for EP is only statistically significant for the intermediate coordination measure. In interaction with the coordination measure, the product market regulation variable is negative throughout, but is statistically significant for low and intermediate coordination.

12	Di Tella and MacCulloch (1999)	21 OECD countries; 1984-1990.	World Competitiveness Report data. Indicator of flexibility (see text). Time-varying measure with five data points.	Employment-population ratio; participation rate; unemployment rate; long-term unemployment rate; and average hours worked per week. For the first two variables, disaggregations by gender are provided.	UI benefit composite measure (OECD, 1984, Chapter 8), plus level of GDP. Selective results are also provided for a specification that includes union coverage, a dummy for decentralized collective bargaining, and degree of home ownership.	Random effects, LSDV with country fixed effects, LSDV with country and time fixed effects, and GMM estimates for each outcome indicator.	Statistically significant positive association between flexibility indicator and overall employment population ratio across all specifications. By demographic group this effect is much stronger for females than for males. Parallel results are obtained for the participation rate. Some evidence that flexibility increases average hours worked. The association between flexibility and the unemployment rate is negative throughout but not always statistically significant. The results for long-term unemployment are less precisely estimated.
13	Heckman and Pagés (2000)	43 countries from Latin America, the Caribbean, and OECD; 1980-1997 (max.).	Authors' own cardinal measure based on severance pay, notice interval, and compensation for unfair dismissal (see text). Two-period time-varying measure.	Employment: total, prime-age male, prime-age female, youth, and self-employment. Unemployment: total, prime-age male, prime-age female, youth, and share unemployed for more than 6 months.	Level of GDP, GDP growth, and two demographic controls, namely, female participation rate and proportion of the population aged 15-24 years.	Pooled cross-section/time series, random effects, and fixed effects. Results for full sample and separate samples of OECD and Latin-American nations.	EP effect is negative and statistically significant for total employment for each estimating procedure. Similar results obtained for males and youth – but not females – the impact of EP on male employment being half the total employment effect and the youth effect is almost double the average effect. EP effects for females and self employment vary widely across estimating procedure. The results for unemployment depend on methodology and there is no statistically significant effect of EP on longer-term unemployment. Disaggregation by broad national grouping reveals that the employment effects of EP by demographic group are negative and mostly statistically significant. The exception is females in the Latin-American grouping. The effects of EP on unemployment are nearly always positive and stronger for the OECD grouping.
14	Freeman (2001)	23+ countries; 1970-1990.	Fraser Institute index of economic freedom (see text). Time-varying measure with 6 data points.	Level of log GDP per capita, log employment-population ratio, log GDP per employee; and unemployment rate. Also changes in levels for the first three variables.	Squared freedom index term (in some specifications); country dummies; time dummies.	Cross section and "panel" estimates.	Countries with a high degree of economic freedom have higher GDP per capita, high employment-population rates, high GDP per employee, and low unemployment – at least in terms of levels. With the exception of unemployment these results do not survive the inclusion of country fixed effects. Estimating GDP per capita in levels and change form for a sample of less developed countries produces statistically significant positive coefficient estimates for the freedom indicator in cross-section and panel estimates.

15	Blanchard and Wolfers (2000)	20 OECD countries; 1960-1999, 8 five-year averages of data.	Static and time-varying measures. Static measure taken from Nickell (1997). Time-varying measure taken from Lazear (1990) and updated.	National unemployment rates (i.e., non-standardized). Basic argument is that unemployment can be explained by shocks which interact with labor market institutions. Shocks are first modeled as common and unobservable and then as country specific.	Basic specification uses 7 (other) labor market institutions taken from Nickell (1997). Alternative specification(s) uses two measures of UI benefits (authors' own calculations) that are deployed in fixed and time-varying form.	Nonlinear least squares with time effects interacted with fixed institutions/time-varying institutions. Robustness checks offered. Nonlinear least squares with country-specific observable shocks (total factor productivity growth, the real rate of interest, and a labor demand shift measure) that are interacted with all 8 labor market institutions. As before, estimates provided for fixed and time-varying institutions.	Shock-EP interaction terms point to amplification of the effects of adverse shocks. Essentially the same is true for the remaining institutional variables with two exceptions. The exceptions are coordination of collective bargaining and active labor market policies, which ameliorate the effects of adverse shocks. In general, much weaker interaction effects and poorer fit when static EP (and UI) measures replaced by their time-varying counterparts.

Table 2

Speed of Adjustment Estimates, Selected Dynamic Panel Studies

Study	Sample	Speed of Adjustment	Long-run elasticities	
			Labor cost	Demand shock
Arellano and Bond (1991)	Unbalanced panel of U.K. manufacturing firms; 1976-1984 (n=140; T=9).	0.68	-0.24	0.05
Addison and Teixeira (2001b)	Balanced panel of Portuguese manufacturing firms; 1970-1977 (n=1,552; T=8).	0.75	-0.71	0.03
Bentolila and S.Paul (1992)	Balanced panel of Spanish manufacturing firms; 1985-1988 (n=1,214; T=4)	0.86	-1.81	0.09

Notes: The estimates were obtained using the general model specified in equation (8) in the text. The speed of adjustment is given by *l* (the corresponding mean adjustment lag is given by *l/(1 - l)*. The demand shock is given by the log change in industry demand (firm sales) in Arellano/Bond (Addison/Teixeira and Bentolila/Saint-Paul). The estimated models also include the stock of capital and the input price of materials.

Sources: Arellano and Bond (1991), Table 4,column (c); Addison and Teixeira (2001b), Table 4, column (1); Bentolila and Saint-Paul (1992), Table 4, column (1).

Appendix Table

Rankings of the Regulatory Climate in Ascending Order of Stringency/Inflexibility

Country	Bertola (1990) [late 1980s]	Grubb and Wells (1993) [late 1980s]	Nickell (1997) [late 1980s]	Freeman (2001) [1985]	Freeman (2001) [1997]	Di Tella and MacCulloch (1999) [1984-1990]	Nicoletti *et al.* (2000) [1997/1998]	OECD (1994) [late 1980s]	OECD (1999) [late 1990s]
Australia			4	7	5	18	3	8	5
Austria			16	12	16	13	6	12	12
Belgium	9	5	17	5	10	11	17	15	10
Canada			3	3	5	5	11	3	3
Denmark	2	2	5	12	9	2	6	7	8
Finland			10	10	12	7	14	15	9
France	8	6	14	16	16	10	18	13	17
Germany	6	7	15	6	14	12	6	19	15
Greece		10		21	21	20	19	17	20
Ireland		3	12	15	4	8	2	6	4
Italy	10	8	20	19	20	17	21	21	19
Japan	5		8	9	10	6	11	9	14
Netherlands	3	4	9	7	7	9	6	10	11
Norway			11	14	14	15	19	14	16
New Zealand			2	18	1	14	5	2	7
Portugal		11	18	20	16	19	14	20	21
Spain		9	19	17	12	21	13	18	18
Sweden	7		13	11	16	16	6	11	13
Switzerland			6	1	7	3	16	4	6
United Kingdom	4	1	7	3	3	4	1	5	2
United States	1		1	2	2	1	4	1	1
Spearman rank correction coefficient	0.733**	0.964*	0.797*	0.705*	0.880*	0.710*	0.697*	0.867*	-

Note: *, ** denote statistical significance at the .01 and .10 levels, respectively.

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