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The consequences of labor market flexibility: Panel evidence based on survey data☆

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Abstract

We introduce a new data set on hiring and firing restrictions for 21 OECD countries for the period 1984–1990. The data are based on surveys of business people in the countries covered, so the indices we use are subjective in nature. Controlling for country and time fixed effects, and using dynamic panel data techniques, we find evidence that increasing the flexibility of the labor market increases both the employment rate and the rate of participation in the labor force. A conservative estimate suggests that if France were to make its labor markets as flexible as those in the US, its employment rate would increase 1.6 percentage points, or 14% of the employment gap between the two countries. The estimated effects are larger in the female than in the male labor market, although both groups seem to have similar long-run coefficients. There is also some evidence that more flexibility leads to lower unemployment rates and to lower rates of long-term unemployment. We also find evidence consistent with the hypothesis that inflexible labor markets produce "jobless recoveries" and introduce more unemployment persistence.

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1. Introduction

One of the biggest challenges in economics today is to explain what causes European unemployment. Commentators on the European situation often blame poorly designed

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labor market institutions, a view that sometimes goes by the ugly name of "Eurosclerosis". Economists advising governments on these issues share more or less the same diagnostic: Regulations such as hiring and firing restrictions faced by firms, as well as the generous welfare state that protects the unemployed, are behind the differential labor market performances of Europe and America. A number of countries have taken this view seriously. Great Britain and France are just two examples of countries that followed the economists' advice and reduced hiring and firing restrictions in the mid-1980s to combat high unemployment. This view of the labor market has also inspired large reform programs in the less developed world, where unemployment has recently increased. In fact, deregulation of the labor market is part of what the IMF and the World Bank often call "second-generation reforms". [1]

Since unemployment brings real misery to people's lives, and job security provisions often involve delicate redistribution issues between richer firm owners and poorer workers, one would think that economists giving such advice know what they are doing. More precisely, one would think that there are hundreds of papers studying whether more flexibility does in fact reduce a country's unemployment rate in practice. Sadly, this is not the case. To our knowledge, there is one panel study on the effects of labor market flexibility across countries (Lazear, 1990). And only a couple of cross-section studies, like the early one of Bertola (1990) based on evidence for 10 countries or that in the OECD Jobs Study (1994) based on 21 observations. [2] Addison and Grosso (1996) revise Lazear's data and obtain different estimates with respect to unemployment (they find no evidence favoring the hypothesis that severance pay increase unemployment). [3] Gregg and Manning (1997) review some of the available evidence on the effects of labor market flexibility and argue that it is "*much less persuasive than is commonly believed*", and that the profession's "*faith in the merits of labor market de-regulation is misplaced*" (p. 395). There is, perhaps, no experience more sobering to an economist than to review the state-of-the-art evidence on the effects of firing costs and to reflect on the social costs of unemployment.

The contribution of this paper is empirical. We introduce a new data set on hiring and firing restrictions for 21 OECD countries for the 7-year period covering 1984–1990. The data are based on surveys of business people in the countries covered, so the indices we use are subjective in nature. We also use the new summary measure of the parameters of the unemployment insurance system compiled by the OECD in 1994, which constitutes a significant improvement on previous benefit data available in the profession. We then present an empirical analysis of the effect of flexibility on a number of labor market variables that follows and extends the contributions of Lazear (1990). Controlling for country and time fixed effects, and using dynamic panel data techniques developed by Arellano and Bond (1991), we find evidence that relaxing job security provisions increases the employment rate and the participation rate. The estimated

[1] The IMF suggested that Argentina should increase the flexibility of the labor market, after unemployment reached 18.6% following the pro-market reforms of the early 1990s.

[2] Even in-depth single country studies are relatively rare. The interested reader is referred to the work of Abraham and Houseman (1994), Kugler (1999) and Hunt (1994) and Autor (2003).

[3] They emphasize a number of differences with Lazear's study (e.g. with respect to advance notice requirements), but they do find similar results with respect to three out of four variables.

effects seem large. The fixed effects estimate tells us that, if France were to reform its labor markets and make them as flexible as the American, its employment rate would increase by 1.6 percentage points.[4] This is equivalent to 14% of the employment rate gap between the two countries. In order to express this effect in terms of *GDP* per capita, we note that it implies an increase in total employment of 2.8%. In the short run, the estimated effects are stronger for females than for males, although interestingly both groups have roughly similar long run coefficients. There is also evidence that a more flexible labor market leads to lower unemployment rates and to a lower proportion of long-term unemployed in the unemployment pool. The effect of unemployment benefits on these variables is less clear-cut. As a general point, we think it is reassuring that, in spite of using such a different approach, our results are not out of line with those obtained by Lazear. We also document the basic correlation of flexibility with inflows and the rate of unfilled vacancies, and review the hypotheses that inflexible labor markets produce "jobless recoveries" and introduce more unemployment persistence.

The main empirical evidence on the effect of labor market flexibility that we have available today is presented in Lazear (1990). He uses data on severance pay and periods of notice required before employment termination for 22 developed countries for the period 1956–1984, and finds some evidence that they have a negative relationship with the employment rate and a positive one with the unemployment rate. For example, Lazear finds that the amount of money paid to the worker as severance enters negatively and significantly in univariate regressions on country means (18 observations) explaining the employment rate, the participation rate and the number of hours worked per week. The coefficient on severance pay in the unemployment regression is positive but insignificant, however. In univariate regressions explaining the unemployment rate and the number of hours worked that include country dummies (468 observations), the coefficient on severance pay keeps its sign and turns significant. It is insignificant, however, when explaining the employment rate or the rate of labor force participation (where it also changes sign).

Lazear points out a number of limitations in these data. Amongst them is the fact that information on one type of worker (blue collar with 10 years of service) is used as a proxy for the entire system. Second and more significantly, information on two types of institutions (amount of severance pay and months of advance notice before dismissal) are used to proxy for a large number of employment regulations that affect the flexibility of the labor market.[5] Clearly, flexibility of the labor market could be affected without showing up in these two series. Third, it does not allow for the fact

[4] France is the median country in terms of flexibility, though it is below the mean. See Table 1 for the full data description.

[5] For example, Grubb and Wells (1993) describe five other types of regulations that are relevant besides the restrictions on an employer's freedom to dismiss workers. These include limits on the use (or the legal validity) of fixed-term contracts; limits on the use of temporary work agencies, restrictions on weekly hours of regular or overtime work; limits on shift, weekend and night work; and limits on employer's use of part time work. The OCED Jobs Study (1994) notes that an employer's freedom to dismiss workers can be restricted by a number of requirements other than a requirement of advance notice. These include a requirement of authorization by third parties (e.g. government, or trade union), provisions for appeal against unfair dismissal and the enforcement of these rules.

that countries differ in the degree of enforcement of these laws, and that other, perhaps informal, aspects may be more important than the written laws. Lastly, Lazear points out that "*for the most part*, *rules change once or twice during the period per country*, *so much of the mileage is cross-sectional rather than time-series*" (Lazear, 1990, p. 708). Yet, it is the best data that economists have available to study a most important set of issues.

The flexibility data used in this paper come from the *World Competitiveness Report* (WCR).[6] The WCR requests the opinion of a number of top and middle managers (on average 1,531 each year) on the flexibility enterprises have to adjust things like compensation and employment levels to economic realities in each of the countries covered. By its nature, these data avoid some of the objections raised to the data used by Lazear. For example, it uses information provided by business people who, presumably, are in a position to judge what aspects of flexibility laws actually affect business conditions. Furthermore, it passes simple validation tests. For example, it correlates well with the index of "*strictness of employment protection legislation*" constructed for the OECD Jobs Study (1994), arguably the most complete measure available, for the 1 year where both types of data are available (1989). There are, of course, limitations to the data we use. The time series dimension of the panel is considerably shorter than that of Lazear's (7 versus 29 years). Importantly, the question asked is more vague than what ideally an economist would like to use. Furthermore, a lower set of answers in one country may simply reflect the fact that people there use a different cardinal ranking than people in other countries. Though some of these objections can be tackled in the empirical section, the fact remains that subjective responses should be treated with caution. However, we believe the topic to be of such economic and social significance, and the data that so far has been available to the profession to be of such poor quality, that a willingness to experiment with survey data is justified.[7]

In Section 2, we discuss briefly some of the theoretical background for our study, present our empirical strategy and explain the data used in the paper. Section 3 presents the empirical results while Section 4 concludes.

2. Theory, empirical strategy and data description

2.1. Theory

On the theoretical side, Lazear (1990) points out that if markets are complete, mandated severance payments should not have real effects. The argument is that the firm-worker pair can undo the firing costs imposed on them by a reverse transfer from the worker to the firm at the onset of the employment contract. Bertola (1990) finds that job security legislation does not bias labor demand toward lower average

[6] This is a publication of the IMD/World Economic Forum.

[7] Put another way, the data that we use have different problems to the data previously used in the literature. Thus, we view this paper as complementing Lazear's approach with "hard" data.

employment at given wages in a simple dynamic economy. The intuition is that a firm subject to a positive shock will hire less workers than otherwise, but that firms subject to a negative shock will be less prone to firing. Thus, employment fluctuations are dampened, but average employment may be unchanged. The evidence he presents is based on Emerson (1988) and is consistent with this view. Bentolila and Bertola (1990) present a model where firing costs actually increase long-run employment.

On the other hand, Hopenhayn and Rogerson (1993) study a general equilibrium model with entry and exit of firms and calibrate it using data on firm level dynamics. They show that a tax on job destruction equal to 1 year's wage reduces the employment rate by roughly 2.5%. There are very large welfare costs in their model: The cost of the same tax in terms of consumption is over 2%. The effects of firing costs on investment are also studied by Risager and Sorensen (1997) using Bertola's model. A recent paper by Alvarez and Veracierto (1998) extends Hopenhayn and Rogerson (1993) by introducing frictions in a world without perfect insurance markets. They find that severance payments can increase welfare. The reduction in firm layoffs and the increase in the agents' search efforts (because employment is more desirable) reduce unemployment enough to compensate for lower consumption levels (productivity also falls). Other contributions in the search literature have emphasized different effects of firing restrictions (for a general treatment, see Pissarides (1990); see Mortensen and Pissarides (1999a, b) for a review). Boeri (1998), for example, presents a model where job security provisions, job-to-job shifts and long-term unemployment can coexist. Another paper by Garibaldi (1998) studies how firing restrictions reduce the volatility of job destruction and the amount of job reallocation, with unemployment remaining approximately constant. Interestingly, the effect of firing restrictions on labor force participation is theoretically ambiguous. Such restrictions could lead to higher participation rates if unproductive workers, who would otherwise exit the labor force, are locked into jobs. But they could lead to lower participation if labor supply decisions are made at the household level and a match that is more secure for one member leads other members to stop or postpone their job search activities (see, for example, Pissarides (2001) for a model that gives an insurance role to employment protection in the absence of perfect insurance markets). Saint Paul (1996a) developed a matching model to study the interaction of technological advances with firing costs in the determination of unemployment, while Saint Paul (1997) studies the effect of higher firing restrictions on a country's competitiveness and pattern of trade. Saint Paul also pioneered the study of the determinants of firing restrictions, a topic to which we will return in Section 3.2 (see Saint Paul (1996b) for a review, and Saint Paul (2002) for a compelling model; see also Wright (1986), Di Tella and MacCulloch (2000, 2002) and Hassler et al. (1999) for work on unemployment insurance).

A recent paper by Bertola and Rogerson (1997) shows that we can have similar rates of job creation and destruction across countries despite there being very different degrees of labor market flexibility, if other institutions lead to wage compression. Thus, lower flows due to job security provisions, the argument goes, could be compensated by higher employer-initiated job turnover originating in the generosity of the European welfare state. Thus, the paper points to the importance of controlling for the generosity of the welfare state when investigating the effects of flexibility on the workings of the

labor markets. All regressions in our paper include the new summary measure of the parameters of the unemployment benefit system compiled by the OECD Jobs Study (1994).[8]

2.2. Empirical strategy

In order to study the effect of hiring and firing restrictions on the performance of the labor market, we estimate regressions of the form

$$VAR_{it} = \alpha_1 + \alpha_2 \ Flexibility_{it} + \alpha_3 \ Benefit_{it} + \mu_i + \nu_t + \varepsilon_{it}, \quad (1)$$

where VAR represents the variables of interest. For purposes of comparison with Lazear's results, in the main tables these are the employment rate, the rate of participation in the labor force, the average number of hours worked in manufacturing and the unemployment rate. We also study the effect of flexibility on the proportion of long-term unemployed in the unemployment pool, the vacancy rate and the inflow rate. Variables are defined in Appendix A.

The estimation strategy we use follows Lazear's approach of using a parsimonious reduced form specification. We also show the results of different specifications, rather than committing to one early on. The main differences with Lazear's estimation strategy are that: (1) We do not impose the restriction of a quadratic time trend but report regressions controlling for year fixed effects (i.e. we include year dummies instead of the time trend and the time trend squared used by Lazear); (2) we control in all our regressions for the generosity of the welfare state (as proxied by unemployment benefits); and (3) we report regressions where lagged variables are included since firing costs are sometimes expected to affect the flows (but not directly the stocks) in the labor market.

2.3. Construction of the data set

The indicator of labor market flexibility used in this paper comes from the WCR, a publication of the EMF foundation in Geneva. It covers 21 countries (a list is given in Appendix A) and covers the period 1984–1990. Thus, the first year for which we have data is also the last year covered by the Lazear study. The WCR was used before by economists studying investment and growth (De Long and Summers, 1991) and studying corruption and competition (Ades and Di Tella, 1999), but its use as a source of labor market flexibility data is new. It consists of yearly surveys conducted amongst

[8] It is calculated as the pre-tax average of the replacement ratios for two earnings level, three family situations and three durations of unemployment. Although not perfect, the data begin to address some of the criticisms raised by Atkinson and Micklewright (1991) to the data previously used in the literature. A number of studies have found evidence that unemployment benefit generosity increases unemployment at the micro level (e.g. Katz and Meyer, 1990). Cross-country panel studies, on the other hand, have failed to uncover significant effects of unemployment benefits on the unemployment rate, once country and year fixed effects have been included. One of the potential reasons is that the benefit data used are very poor. For example, Layard et al. (1991) uses the 1985 duration of unemployment benefits as an indicator of generosity for the whole sample.

chief executive officers and economic leaders in the countries covered, who are mailed a questionnaire containing a large number of questions on their country's competitiveness (unfortunately sometimes it can contain as many as 90 questions). The surveys were sent out to "*Company CEO's*, *economic and financial experts*, *bankers*, *heads of foreign owned subsidiaries of multinational companies*, *as well as key personalities of the economic press*, *trade unions and business associations*".

The survey question that is used (classified as 2.17 *LABOR-COST FLEXIBILITY* in 1984) asked the respondents: "*Flexibility of enterprises to adjust job security and compensation standards to economic realities*: 0 = *none at all*, *to* 100 = *a great deal*". This question was changed in 1990 to "*Flexibility of management to adjust employment levels during difficult periods*: 0 = *low*, *to* 100 = *high*". It was dropped altogether in subsequent years. The survey criteria presented in the WCR constitute the average value of respondent's ratings for their respective countries. Respondents were invited to rate the performance of the country in which they resided on 91 criteria, on a scale of 1 to 6. They were "*thus presented with a choice of six values which prevented them from giving a middle value*. *A* 1 *to* 3 *ranking implied a negative assessment*, *and a* 4 *to* 6 *rating a positive one.*" The results presented in the WCR are transformations from the 1–6 to a 0–100 scale. In 1984, there were 5,500 questionnaires sent out and 1,100 were returned. In 1985, there were 7,513 questionnaires sent out and 1,598 returned. In 1986, there were 1,369 returned questionnaires and in 1988 there were 1,937 returned. In 1989, there were 12,000 questionnaires sent out to a similar sample of people of which 1,800 were returned. Finally, in 1990, there were 10,000 survey questionnaires sent out of which 1,384 were returned. The firms are not randomly selected. This has the obvious drawback of not being a truly random selection of firms but the advantage that the firms may share a common benchmark (such as the US).[9] There was no WCR containing 1987 data so the 1986 and 1988 observations were interpolated linearly to obtain observations for 1987.

It is clear that there is a trade-off in using survey data. The data seem to be less precisely defined than what we would ideally like. There is no survey question that is easier to interpret data on than, say, the number of months' written notice required before termination to workers with 10 years of service. On the other hand, our survey measure is more likely to capture the many dimensions that such institutional arrangements associated with employment protection laws encompass. For example, much of the impact of hiring and firing costs comes from the degree of enforcement of the different aspects of the law, such as whether or not there is rightful dismissal, or the appropriate wage/length of employment over which to calculate severance pay. It is also well known that in some countries, like France, advance notice before dismissal given orally is more important than that advance notice administered in written form.[10] It is easier to capture this information through survey questions registering opinions than with easy to quantify data describing the actual laws, unless it is done in

[9] Response rates were similar in later years. For example, in 1991 there were 12,000 questionnaires sent out of which 1,484 were returned. In 1993 WCR, there were 2,160 returns out of 10,300 questionnaires sent out, although this issue did not contain the flexibility question.

[10] Some people call this "fuzzy advance notice".

a very meticulous manner. Another important advantage is that the respondents have actual experience and knowledge of the workings of the labor market in question, so presumably they know the relevance, if any, of changes in the written laws. In any case, measurement error in the data would bias the regression coefficients to zero. Also note that the size of the surveys implies that the variance of the observations would be considerably lower than would be the case with, for example, individual level data.[11]

Another potential source of concern is the fact that the question changes in 1990, omitting any reference to changes in wages. Employers in industrial democracies rarely cut nominal wages, even in countries where it could be done in principle, like the US, so this does not strike us as particularly problematic. As a check, however, we re-estimated our regressions without 1990 and the main results do not change. For example, if we re-estimate the effect of flexibility on our main variables of interest using the LSDV specification with country and time fixed effects without the 1990 observations, we find that the results improve (in terms of size and significance) in every case. Excluding the interpolated year (1987) also improves the main results in the paper.

2.3.1. *Cross-section validation*

As with all subjective data, it is important to see if some of the survey information being used can be related to hard data. The WCR survey measure of labor market flexibility can be compared with other measures that are available for a limited cross section of countries. For example, the OECD Jobs Study (1994) has produced a cross-country index of the "strictness" of labor employment protection legislation for 1989. The OECD index is based on an overall assessment of the extent of regular procedural inconveniences faced by employers such as delays to the start of notice of dismissal, the amount of notice and severance pay for no-fault dismissals, and also the difficulty of dismissal. The difficulty of dismissal includes assessments of the definition of unfair dismissal, trial periods and reinstatements.[12] The correlation coefficient of the WCR survey measure of labor market flexibility in 1989 (where high values denote high flexibility) with the 1989 OECD indicator (where high values denote greater strictness) is -0.75. Higher levels of flexibility measured by the WCR survey responses are strongly associated with lower levels of employment protection strictness measured by

[11] There were more countries covered in the questionnaire than the ones used in this study (because data on other variables of interest is lacking). For example, the 1985 data comes from 1,598 answers from 31 countries, so the average is 52. This may underestimate the average number of respondents for the countries we study in this paper as they are all OECD countries, and it is likely that more questionnaires were sent and returned to these countries than to other countries in the survey (like Mexico, Brazil, Malaysia, Thailand, Korea, etc).

[12] The Jobs Study (1994) notes researchers have constructed various summary indicators to describe the strictness of employment protection in each nation but that "*given the complexity of the phenomenon, summary indicators are inevitably somewhat arbitrary*" (p. 70). Norway and Sweden have relatively high rankings on the OECD scale of strictness of employment protection. This is due to, for example, legislative provisions allowing courts to order reinstatement of unfairly dismissed employees in Norway and the 6-month trial period in Sweden that must be given to dismiss a 35-year-old worker.

the OECD quantification of legal data, as we can reject the hypothesis of independence of the two variables.

A second measure of the strictness of employment protection has been produced by the International Organisation of Employers (IOE) (1988). They assessed the importance of obstacles to termination or use of regular and fixed-term contracts on a 0–3 scale across countries in 1985. Regulatory constraints were classified as insignificant (0), minor, serious or fundamental (3). The correlation coefficient of the WCR survey measure of labor market flexibility in 1985 with the 1985 IOE indicator is −0.59. Higher levels of flexibility measured by the WCR survey responses are associated with lower levels of employment protection strictness measured by the IOE, although the correlation is not as strong as before (independence can again be comfortably rejected). Lastly, we also correlate the WCR data to the index used by Bertola (1990) based on information presented in Emerson (1988), and extended in the OECD Jobs Study (1994) to cover 21 countries. The correlation coefficient is −0.58 and we can reject the null of zero correlation. [13]

2.3.2. Time series validation

Recently, Saint Paul (1996a, b) has coded a number of selected events of changes in job protection legislation that have occurred in Europe over the last 25 years. He classifies each event according to whether job protection legislation has become more or less restrictive. There are 12 events that have occurred in dates and countries for which we also have WCR data. For nine of the 12 events, Saint Paul records an event with the same sign as our data would predict (we create a new variable $\Delta Flexibility_t = Flexibility_t - Flexibility_{t-1}$). Thus, for three events our data disagree with Saint Paul's classification. These are: The UK in 1990 (when there was an increase in the employment duration required to benefit from unfair dismissal protection), Italy in 1987 (when there was a liberalization of determined duration contracts) and Italy in 1990 when there was an extension of unfair dismissal legislation to smaller firms. In the last two events in Italy, however, the variable $\Delta Flexibility$ only registers very small values (5.9% and 5.8% of a standard deviation in $\Delta Flexibility$).

A further concern with the WCR flexibility indicator is that, being assessments of business persons, they may be affected by how well firms are doing. Maybe when a country is in a recession managers will become aware that it is tough to adjust employment levels whereas in a booming economy managers do not recall these difficulties. Or maybe managers are just more positive all round in economic booms. We test the hypothesis that the WCR flexibility variable is correlated with a number of indicators of the business cycle and do not find evidence of such a strong correlation in any of them. For example, in Table 2 in Appendix B we study the correlation between flexibility and measures of (i) aggregate *GDP* (at constant 1985 prices), (ii) the change in *GDP* (ΔGDP), (iii) changes in industrial production (proxied by value-added in

[13] As a further check, we studied the correlation of our flexibility data with a measure of flexibility obtained by Blanchflower (1999) using micro-survey data on individual willingness to move area of residence for 1995. Again we could reject independence between this measure and our *Flexibility* index (for 1990).

industry), (iv) changes in the size of the service sector (proxied by value-added in services) and (v) changes in openness. The disaggregation of ΔGDP into industry and service sectors is done since hiring and firing restrictions may affect these two groups differently. $\Delta Openness$ is included as a proxy for industrial turbulence, since more open economies may be more exposed to external shocks (see Cameron, 1978; Rodrik, 1998). Pearson's correlation coefficient is reported. For example, the correlation coefficient between *Flexibility* and $\Delta Industrial\ production$ is 0.022. It is −0.006 with $\Delta Openness$. All the above five correlations are insignificant (so is Spearman's rho) so we cannot reject the hypothesis that the *Flexibility* variable and each of these measures of the business cycle (and industrial turbulence) are independent. Controlling for country and year fixed effects, ΔGDP has a positive but insignificant estimated effect on *Flexibility* (it is significant at the 85% level) and $\Delta Industrial\ production$ has a positive but insignificant effect (at the 89% level). $\Delta Service\ sector$ and $\Delta Openness$ also have insignificant effects. Still, the empirical section will present regression estimates where flexibility appears lagged 1 year, so the possibility of joint determination of flexibility with economic variables is reduced. We will also present regressions that control for the state of the economy (including ΔGDP and $\Delta Industrial\ production$). In regressions with country and year effects, the relationship between real wages and flexibility is negative (not positive as could be expected if *Flexibility* were just a proxy for the business cycle).

We use the recently published OECD summary measure of the parameters of the unemployment insurance system (OECD Jobs Study, 1994) as a measure of the generosity of a country's welfare state. It is calculated as the pre-tax average of the replacement ratios for two earnings levels (average earnings and two-thirds average earnings), three family situations (single, with dependent spouse and with spouse in work) and three durations of unemployment (first year, second and third years, and fourth and fifth years).[14] It is not weighted by the composition of unemployment in any particular place or period. These data represent a significant improvement over previous measures used. Consider the case of an unmarried worker in Norway. The worker's unemployment benefit replacement rate would be 62% in the first year, 41% in the second and third years and 14% in the fourth and fifth years. These numbers do not vary according to family circumstance. The comparable numbers for the USA would be 24%, 5% and 5%, respectively, but would increase to 26% in the first year if the worker had a dependent spouse and fall to 21% if the worker had a spouse that worked. In the second, third, fourth and fifth years unemployment benefits would be zero if the worker had a spouse that worked and 10% if the spouse was dependent. Atkinson and Micklewright (1991) have emphasized that this is a desirable feature of benefit data since cuts in one type of benefit are often offset by a corresponding increase in another type. Due to the complexity of the OECD calculations of benefit generosity, measurements were made at 2-year intervals. Consequently, observations were interpolated to obtain data for consecutive years.

[14] The pre-tax replacement rate is defined as benefit entitlement over previous earnings, all pre-tax.

We completed our data set with the employment rate (defined as total civilian employment divided by the population aged between 15 and 64 years old), the participation rate (defined as total civilian employment plus total unemployment divided by the population aged between 15 and 64 years) and the average number of weekly hours worked in the manufacturing sector. We also collected the unemployment rate, the rate of unfilled vacancies and the rate of long-term unemployment (defined as the number of workers who have been out of work for 6 months or more as a percentage of total unemployment). The source of these data is the Centre for Economic Performance OECD 1950–1990 updated data set compiled by Bagliano et al. (1992). Pascal Marianna at the OECD kindly provided us with the latest updated file of inflow data (the number of unemployed persons with duration less than 1 month divided by total employment).

Data definitions and summary features of the data appear in Appendix A. The raw data show that countries with more flexible labor markets have higher employment rates, lower unemployment rates and lower proportions of long-term unemployed, though the relationships are by no means monotonic.

3. Empirical results

3.1. Basic evidence on labor market flexibility

We begin our empirical analysis by studying the effect of labor market flexibility on the employment rate. Regression (1) in Table 3A estimates the effect of *Flexibility* and *Benefits* on the employment rate using generalized least-squares random effects (Balestra–Nerlove). For purposes of comparison, Table VI on p. 716 in Lazear (1990) presents hypothesis tests where the lack of independence over time of the error term in a given country has been taken into account. Regression (1) in Table 3A shows that countries with more flexible labor markets also have higher employment rates. The effect of unemployment benefits is negative but insignificant. The estimated flexibility effect is large. If the estimated effects are taken to be causal, a 1 standard deviation increase in the flexibility of the labor market will bring about an increase in the employment rate of 1.9 percentage points, almost 20% of a standard deviation in the *Employment* variable. In other words, if France were to reform its labor market to make it as flexible as that in the United States during this period, then the employment rate would increase by 4.4 percentage points. That is, different degrees of flexibility in the labor market would account for almost 38% of the different employment rates of the US and France in the late 1980s. The estimated effect means that, going from the bottom to the top of the sample (from Spain to the US) in terms of flexibility would increase Spanish employment by almost 6.2 percentage points. In order to estimate its effect on French *GDP* per capita we note that making French labor markets as flexible as those in America would mean that total employment would increase by a large 7.6%. The magnitude of the estimated effects is perhaps surprising (and we will come back to this issue) but we note that the basic evidence is inconsistent with the predictions of Bertola (1990) and Bentolila and Bertola (1990) and are broadly consistent with the Hopenhayn and Rogerson (1993) model.

Another way to correct for the lack of independence over time of the error term in each country is to control for country fixed effects. This method has the considerable advantage of controlling for the incidence of time-invariant omitted variables that may be correlated with the other explanatory variables.[15] The estimators in regression (2) of Table 3A are least squares with dummy variables (LSDV). The effect of flexibility on the employment rate has a similar sign and size to that in regression (1) and is significant at the 5% level, while the coefficient on unemployment benefits is insignificant. This stands in contrast to Lazear (1990, Table V, p. 714) where the effects of severance pay on the employment rate are insignificant once he controls for country dummies. Regression (3) includes year dummies. Controlling for year fixed effects adds the requirement that a country with a higher than average flexibility reading 1 year must also experience a higher than average (for the countries) employment rate (in order for a significant coefficient to be obtained). The effect of *Flexibility* is positive and significant, though of smaller size than the previous estimates. If France were to increase the flexibility of its labor markets to American levels, its employment rate would increase by 1.6 percentage points, now only 14% of the difference in the employment rates of the two countries.[16] In order to express this employment gain in terms of increases in French *GDP* per capita we note that this increases French total employment by 2.8%. There are some negative effects of unemployment benefits (significant at the 9% level only).

As noted in Section 2.3, a potential objection to these results is that there is possible contamination of the data arising from the stage of the business cycle. When the economy is in recession firms are more likely to be firing than hiring and so employment protection legislation may impose binding constraints on firms. If managers' responses to our survey question are subsequently in the direction of greater inflexibility at such times, even though the parameters of the system have not changed, then the interpretation of the results would be different. Alternatively, when the economy is growing the existing employment laws may be of less consequence to firms so managers' responses may indicate a higher degree of labor market flexibility in these times. We attempt to deal with this concern by reporting regressions that control for the change in total *GDP*, ΔGDP, in every one of our tables. The results remain almost identical (that is, the coefficient on *Flexibility* retains its size and sign at the same level of significance). For example, in regression (3), once we control for ΔGDP the coefficient on flexibility changes from 0.053 to 0.052 and the standard error remains equal to 0.026 (reported in note b in Table 3A; see also Table 7 for further tests).

[15] Another reason is that we use Hausman's (1978) specification test to examine if random effects are appropriate. The test statistic for regression (1), which has a chi-squared distribution with two degrees of freedom, has a value of 4.76. The probability of obtaining a value at least as large as 4.76 is consequently 0.0923. Hence, there is some evidence that the assumption of the random effects being uncorrelated with the explanatory variables is incorrect (or that the model is misspecified). Thus, we also report regressions obtained by estimation with LSDV.

[16] We can get another sense of the size of this effect by going from the top to the bottom of the sample. Making the Spanish labor market as flexible as the American means adding another 2.3 percentage points to the Spanish employment rate.

Theoretical models of employment alert us to the possibility that hiring and firing restrictions affect stock variables (like the employment rate) only through its effects on the flows in and out of employment. It is possible, then, that *Flexibility* affects *Employment* with a lag, and that the lag exceeds 1 year. Regression (4) in Table 3A indicates that the 1-year lag of *Flexibility* enters positively and significantly in an employment regression controlling for country and year fixed effects, and is almost 64% larger than the contemporaneous effect estimated in regression (3). If France were to have US flexibility levels, its employment rate would increase 2.6 percentage points a year later, or 22% of the actual difference in employment rates. Using a 2-year lag also yields positive and significant estimate of the effect of flexibility on the employment rate, though the number of observations drops to 102. A virtue of these estimates is that if the flexibility data were still suspect of being contaminated by the economic atmosphere as perceived by the respondents, then this would be less likely to show up when 1- or 2-year lags are used. [17]

An even more stringent test for the hypothesis that flexibility affects labor market performance is to include a lagged dependent variable. Again, from a theoretical perspective, it could be argued that the long-run response of the labor market to flexibility differs in the short- and long runs, or that there exist "adjustment costs" that justify this specification. Another reason we could want to include a lagged dependent variable is that it may help proxy for slower moving omitted variables that are not captured by the controls included. At any rate, it seems natural to keep an open mind at this stage of our empirical (and theoretical) knowledge on the subject. Regression (5) in Table 3A estimates the effect of flexibility on employment controlling for unemployment benefits and lagged employment. The presence of a lagged dependent variable on the right-hand side of (5) introduces a bias when estimation is by LSDV. Perhaps the easiest way to see this is to note that first differencing the data makes the lagged dependent variable correlated with the error term. Since the bias may be particularly large when the time series dimension of the panel is short, we correct for this using the generalized method of moments (GMM) technique (see Arellano and Bond, 1991). Valid instruments are specified in each time period for the first-differenced equations. Regression (5) in Table 3A controls for year fixed effects by including year dummy variables, controls for country fixed effects by first differencing the data, and then controls for the dynamic panel data bias by instrumenting the differenced lagged dependent variable (Δy_{it-1}) with lagged levels of dependent variables dated $t-2$ and earlier. The coefficient on *Flexibility* is still positive and significant. The size is not too different from that in regression (3).

The long-run effects are quite large now. If France were to increase the flexibility of its labor markets to the level of the US, the employment rate would increase by 1.5 percentage points. In the long run, the effect would be to increase the employment rate a full 3.6 percentage points, or 31% of the France–US employment rate gap. The effect of unemployment benefits is insignificant. Lastly, regression (6) in Table 3A includes the more general specification with lags of the dependent and independent variables. The current level of *Flexibility* is still positive and comfortably significant.

[17] As we mentioned in Section 2.2, we did not find evidence of such a correlation.

The first lag of flexibility is positive but insignificant, and unemployment benefits and its lag enter significantly in the employment regression. We cannot reject equality of the unemployment benefits coefficients (in absolute value) so it seems that there is some evidence that positive changes in *Benefits* decrease the employment rate.

Theory leads us to expect different effects of job security provisions across groups, depending on their roles in the labor market.[18] In the next two tables, we study the effect of labor market flexibility on the employment rates of men and women. The general message of Tables 3B and C is that the estimated flexibility effects on female employment rate are larger than the corresponding effects in the labor market for males. The sign and significance of the coefficients in Table 3B are almost identical to those in Table 3A. The size of the coefficients is also very similar. This result would also seem to indicate that managers' responses to the survey question are unlikely to depend on the stage of the business cycle since under this scenario we would expect to find the similar effects for both men and women. Including ΔGDP leaves the coefficients on *Flexibility* almost identical to their previous levels (see footnote b in Tables 3B and C). In terms of size, however, one of the most interesting differences is the estimated long-run effect of flexibility on employment of females compared to that of males. Comparison of the estimated effects in regression (5) in Tables 3B and C seem to indicate that the short-run effect of flexibility is larger for women than for men, but that in the long run they have approximately similar coefficients. If France were to increase the flexibility of its labor markets to levels comparable to those prevailing in the US, regression (5) in Table 3B predicts that there would be an increase in female employment equal to almost 1 percentage point in the short run, and a 1.6 percentage point increase in the long run. Regression (5) in Table 3C predicts that such a movement would increase the employment rate of men by over 0.36 of a percentage point in the short run, and almost 1.3 percentage points in the long run.

We also study the effect of flexibility on labor force participation. As pointed out in the theory section, the expected effect is ambiguous. In Table 4A, we again present a parsimonious, reduced form approach showing a number of different specifications. Regression (1) finds positive and significant effects of flexibility on participation rates. The effect is large: If France turns into the US in terms of flexibility, the participation rate would increase by 3.5 percentage points, over 36% of the actual difference in participation rates between the two countries. Again, in contrast to Lazear, the effect survives the inclusion of country and year dummies, the inclusion of lagged independent variables and the inclusion of a lagged dependent variable (estimated with GMM techniques). In some regressions there are negative effects of unemployment benefits.

The literature suggests some stylized facts about female labor force participation (e.g. it is lower than that for males and it is larger for single women than for married women; see Killingsworth and Heckman, (1986)). This leads us to expect that the insurance effect would be stronger for females. The idea, to put it simply, is that there

[18] For example, Lazear has a short section on the effects of severance pay on the labor market performance of the young.

will be higher female labor force participation when men face higher probabilities of losing their jobs (and higher accessions). Table 4B shows this is largely the case in our sample, with well-defined and positive effects of flexibility on female labor force participation. Table 4C shows that the effect of flexibility for men is weaker all round. When it is significantly different from zero, it is substantially smaller in size than female effects.

Table 5A presents unemployment regressions. Regression (1) finds that random effects estimation suggests that countries with more flexible labor markets have lower unemployment rates. The estimated effects are large. Again taking the relationship to be causal, if France were to reform its labor market to have the flexibility levels observed in the US, it would have an unemployment rate which was lower by more than 1.7 percentage points. That is, different flexibility levels would explain almost 47% of the different unemployment experiences of the two countries during the mid- to late 1980s. Regression (2) shows similar results when controlling for country fixed effects. Importantly, we do not find significant effects of flexibility on the unemployment rate in regression (3), where we control for both country and year fixed effects, although the coefficient is still negative. Using robust regression techniques to reduce the influence of outliers yields a larger, negative coefficient though still insignificant (significant at the 22% level, results available upon request). As we explained earlier, flexibility may affect labor market flows, and thus could affect the unemployment rate with a lag. Regression (4) finds that an increase in flexibility today would only decrease the unemployment rate next year. The effect of flexibility lagged is significant at conventional levels and its size is almost 10% smaller (in absolute terms) than that in regression (2). Regressions (5) and (6) in Table 5A include a lagged dependent variable and only find very weak negative effects of flexibility on unemployment.

A number of economists have predicted adverse effects of inflexible labor markets on the composition of unemployment (e.g. McCormick, 1991). Table 5B studies long-term unemployment. Regressions (1) and (2) show that countries with more flexible labor markets are associated with a lower proportion of long-term unemployed in the unemployment pool when estimation is by random effects and LSDV (country dummies only). Given the effects of flexibility in other regressions, the coefficients are rather small. If the relationship is taken as causal, regression (2) predicts that if France where to reform its labor market in order to match US flexibility levels, the proportion of long-term unemployed would fall 4.6 percentage points, almost 9% of the long-term unemployment gap between the two countries. When we also include year dummies in Eq. (3), the coefficient on flexibility becomes insignificant (though still negative). The lagged specification used in Eq. (4) finds some evidence of negative effects of flexibility, significant only at the 7% level. Re-estimation of regression (3) with robust regression techniques to control for the influence of outliers yields a much higher coefficient on flexibility in absolute value (-0.191, s.e. 0.072), significant at the 1% level. The same is true when regression (4) is re-estimated with robust regression techniques, where the negative coefficient on the lag of flexibility is now significant at the 1% level. Regressions (5) and (6) do not find strong contemporaneous effects. There is some evidence of lagged effects of flexibility on long-term unemployment in Eq. (6) though the number of observations drops as low as 89.

3.2. Causality and non-linearities

It has been pointed out, however, that unemployment may cause changes in labor market institutions (see Lazear (1990) and Saint Paul (1996a, b, 2002) on flexibility and Wright (1986), Atkinson (1990), Di Tella and MacCulloch (2000, 2002) and Hassler et al. (1999) on unemployment benefits). Thus, there may be a simultaneity bias in the flexibility coefficient in unemployment regressions. Lack of suitable instruments leads us to focus on timing to shed at least some light on these issues. If causation runs from say, unemployment to flexibility, we would expect previous experience with unemployment to predict movements in labor market flexibility. The fact that we are working with only seven time periods, however, reduces the usefulness of examining Granger-causality for individual countries. Thus, we first run a panel regression of $\Delta Flexibility_t$ on unemployment lagged, employment lagged and participation rate lagged (this is the specification used in Lazear (1990); see Table IX). None of the coefficients are significant. The same is true if we restrict attention to unemployment. This is shown in regressions (1) and (2) in Table 6A. Comparing regressions (2) and (3), we can see that flexibility lagged is a somewhat better predictor of the change in unemployment than unemployment lagged is a predictor for the change in flexibility, though the effect is not strong. It is also interesting to run separate unemployment and flexibility regressions on unemployment and flexibility lagged once, and unemployment and flexibility lagged twice. The effects are again more supportive of the idea that flexibility causes unemployment than the other way around. Thus, the evidence reported in Table 6A, based on the extremely limited data available for this type of exercise, is not supportive of the reverse causality hypothesis.

We also made an attempt to instrument for *Flexibility* in both the employment and participation rate regressions. The instrument chosen was *Right Wing* politics. This variable that we constructed is meant to capture the degree to which political preferences in the country lean towards the right. It is similar to those employed by political scientists to indicate the left/right position of a government and is constructed in two steps. In the first step, we collect the number of votes received by each party participating in cabinet and express them as a percentage of the total votes received by all parties with cabinet representation. This percentage of support is then multiplied in the second step by a left/right political scale (from Castles and Mair, 1984) and summed across all the cabinet parties to give a continuous variable. A shift to more *Right Wing* government leads to significantly more *Flexibility*. Using this instrument in a two-stage least-squares regression, the coefficient on *Flexibility* in the employment rate equation becomes equal to 0.23, significant at the 10% level (using the same specification as in column (3) of Table 3A). In the participation rate equation, the coefficient on *Flexibility* becomes equal to 0.26, significant at the 5% level (using the same specification as in column (3) of Table 5A).

Lazear suggests that the effect of flexibility in the labor market may be non-linear. He suggests that, once employment restrictions are severe enough, firms may stop firing workers. Unlike Lazear, we find some evidence favoring a specification that includes a quadratic *Flexibility* term, particularly in the female sub-sample with respect

to employment and participation rates. The female employment regression is included in Table 6A as an illustration (see Eq. (6)).

3.3. Adding more controls

Another source of potential concern is that the level of unemployment benefits is the only control included in our regressions. Although Lazear (1990) runs, essentially, univariate regressions, there is a large literature that studies the role of labor market institutions in shaping unemployment (see, for example, Phelps, 1994). Table 6B presents regressions where some of the variables identified in this literature are included as controls. Information is available for only 1 year on some of these variables (e.g. home ownership) when we also have the flexibility data. Thus, we concentrate on regressions that control for random effects.

Regression (1) in Table 6B presents an employment regression similar to the first regression in Table 3A with three extra controls: union coverage, decentralization and home ownership. *Union Coverage* is defined as the percentage of workers covered by collective agreements. In some countries, like France or Spain, this number can be significantly larger than union density (source is Appendix 1.4 in Layard et al., 1991). *Decentralization* refers to the level at which wage bargains occur (the source is a ranking constructed by Calmfors and Driffill (1988)). *Home Ownership* is the percent of households that are owner occupied in 1990 (census data, see Oswald, 1996). The basic result is that the coefficient on *Flexibility* is still positive, comfortably significant and of almost identical size as that in regression (1) in Table 3A. The other variables also enter the regression with the expected sign and are often significant (the exceptions are unemployment benefits and home ownership). In terms of comparative size the effect is only moderate. In an average year, it explains about 26% of the employment difference between Spain and the US, which is less than what the gap in the *Union* and *Decentralization* variables between the two countries can explain. The rest of Table 6B shows that including these three controls does not change the results we obtained earlier with respect to participation rates, unemployment rates and proportion of long-term unemployment in the unemployment pool. The same is true when we study females separately from males (results available on request).

Table 6C provides more robustness tests, but now adding measures of labor taxation. The former variable was the focus of the study by Daveri and Tabellini (2000) about how taxes affected employment outcomes. Our regressions include a full set of country and year fixed effects which means that we are now unable to identify effects of variables for which we do not have time series information (such as home ownership). Comparable results using our base specifications are found in column (3) in Tables 3A, 4A and 5A. For example, in the employment rate regression the coefficient on *Flexibility* changes from 0.053 in Eq. (3) in Table 3A to 0.050 in Eq. (1) in Table 6C, once the fuller set of controls has been added. In the participation rate regression, the coefficient on *Flexibility* changes from 0.059 in Eq. (3) in Table 4A to 0.044 in Eq. (2) in Table 6C with the fuller set of controls. The significance level of *Flexibility* is 6% in the employment regression and 5% in the participation regression. There also exists a negative and significant effect of *Employment taxes* on both employment

and participation (at the 5% level). A one standard deviation increase in employment taxes (equivalent to adding 10.6 percentage points onto employers' contributions as a proportion of total employee compensation) is expected to decrease the employment rate by 0.6 of 1 standard deviation (or 6.1 percentage points) and the participation rate by 0.7 of 1 standard deviation (or 6.4 percentage points). These results all remain robust to controlling for the state of the economy, such as by the inclusion of a term for ΔGDP (reported in the footnotes) as well as terms for Δ*Industrial production* and Δ*Service sector*.[19]

Regressions (4)–(6) add interaction terms between *Flexibility* and *Employment taxes* as well as between *Benefits* and *Employment taxes*. There is no evidence of strong interaction effects in the case of *Flexibility*, but some evidence that the effect of *Employment taxes* is more negative in the presence of high unemployment benefits. We also experimented with controls for openness, terms of trade (i.e. ratio of average value of exports to average value of imports) and measures of the government's budget surplus/deficit. With these three extra controls added (as well as the four control variables included above plus the two interaction terms), the coefficient on *Flexibility* equalled 0.111 in the employment rate regression and 0.097 in the participation regression, both significant at the 5% level.

Table 7 reports results when the state of the economy, proxied by both ΔGDP and Δ*Industrial production*, are included together with interaction effects, as suggested by a referee. If managers tend to stress severance costs when the economy is in a downturn, then the effect of regulation could be overestimated. One way to test for this is to interact the size or sign of ΔGDP and Δ*Industrial production* with the *Flexibility* index. Regressions (1) and (2) include both ΔGDP and its interaction with *Flexibility* in employment and participation rate regressions, respectively. The results remain similar to before. For example, *Flexibility* has a positive effect on the employment rate equal to 0.051, significant at the 5% level, in regression (1). The interaction term is positive but insignificant. Since hiring/firing costs may affect the industrial sector in particular, regressions (3) and (4) include Δ*Industrial production*, interacted with *Flexibility*. The basic results again remain similar. When the sign (i.e. positive or negative) of the growth rate in industrial production is interacted with *Flexibility*, the coefficient on *Flexibility* remains significant at the 5% level in both the employment and participation regressions (equal to 0.056 and 0.065, respectively). Regressions (5) and (6) test robustness using the change in openness (as our proxy for industrial turbulence) with similar results.

3.4. Other hypothesis on the consequences of labor market flexibility

Following the work of Davis and Haltiwanger (1990), there has been growing interest in the profession on the empirical behavior of job creation and job destruction. We were unable to obtain comparable cross-country measures of these variables. We did, however, obtain a measure of unfilled vacancies divided by total employment from the CEP-OECD data set (that in turn collects it from the OECD main economic indicators)

[19] All results reported in the paper but not included in the tables are available on request.

which is as close as we can get to a measure of job creation. And Pascal Marianna at the OECD kindly provided us with the unpublished data for inflows revised in 1998.[20] In Table 8A and B, we study the effect of flexibility on these variables. In spite of some inconsistencies, the results are interesting. For example, regression (1) in Table 8A shows that the pooled data reveal a very strong positive association between inflows and flexibility. However, estimation by random effects makes the coefficient insignificant (and negative). In fact, when country dummies are included in regression (3) the effect of flexibility on inflows is negative and significant. This result does not survive re-estimation with robust regression techniques or the inclusion of year dummies in regression (4).

Regression (5) shows low persistence of inflows and some positive effects of unemployment benefits. The last two regressions in Table 8A control for the state of the business cycle (proxied by the change in *GDP*). As expected there are fewer inflows during expansions. Regression (7) examines if the evidence supports the hypothesis that the relationship between the business cycle and inflows is affected by flexibility by including an interaction term ($\Delta GDP * Flexibility$). The estimated interaction effect has the expected sign but is insignificant. With respect to vacancies, there are again some inconsistencies. Regression (4) in Table 8B shows that, controlling for both country and time fixed effects, countries with higher levels of labor market flexibility have less unfilled vacancies.[21] Presumably, this indicates that in flexible labor markets vacancies are filled more quickly. In regression (6) we find that, somewhat reassuringly, there are more vacancies in a recovery and that there are well-defined negative effects of flexibility on vacancies. Column (7) shows that there are more vacancies in a recovery that occurs in a country with high flexibility (due to the positive interaction term).

Lastly, in Table 9 we examine two other hypotheses that have been proposed in the literature. The first is the jobless recovery hypothesis; the idea that in more inflexible labor markets Okun's law has to be adjusted downwards. Bertola 1990 shows that cross-section evidence is consistent with this view. In order to examine the evidence on these issues, we present regressions (1) and (2) in Table 9 where the dependent variable is the change in the unemployment rate. Regression (1) shows the basic relationship between $\Delta Unemployment$ and ΔGDP, once we control for country and time fixed effects. Regression (2) shows that the interaction term ($\Delta GDP * Flexibility$) is negative and significant at the 10% level, indicating that when *GDP* increases, unemployment falls more in countries with more flexible labor markets.

Second, we test the hypothesis that unemployment persistence is greater in countries with more inflexible labor markets. This hypothesis has been suggested, in one form or another, by Blanchard and Summers (1986) and Lindbeck and Snower (1989). It is also examined in Bertola (1990) who finds evidence consistent with this hypothesis. To test this proposition, we allow for the coefficient on the lagged dependent variable in standard unemployment regressions to vary with the degree of flexibility. In Table 9,

[20] We interpolated four values of inflows, Netherlands 1984 and 1986 and Finland 1988 and 1990. The results do not change if these observations are excluded.

[21] Normalizing by unemployment (instead of employment) produces largely similar results.

regression (3) includes an interaction term, $Unemployment_{t-1} * (1 - Flexibility_{t-1})$, which is positive and significant at the 2% level.[22] In other words, more inflexibility (i.e. corresponding to greater values of $1 - Flexibility_{t-1}$) is associated with a larger coefficient on the lagged dependent variable. Using the coefficients of regression (3), the United States would have a coefficient on lagged unemployment of $0.596 + 0.462 * (1 - 0.727) = 0.72$, ceteris paribus (mean flexibility over the sample period equals 0.727 in the US). On the other hand, France would have a coefficient on lagged unemployment of $0.596 + 0.462 * (1 - 0.423) = 0.86$, ceteris paribus (mean flexibility over the sample period equals 0.423 in France). Furthermore, as we move from the most flexible country in the sample (the US) to the least flexible country (Spain), the coefficient on lagged unemployment is estimated to rise from 0.72 to 0.92 ($=0.596+0.462*(1-0.298)$, since mean flexibility over the sample period equals 0.298 in Spain). Regression (4), which is estimated using GMM, shows a similar effect, with the interaction term again positive and significant at the 2% level. The effect on the coefficient of lagged unemployment of changing the level of flexibility is now larger than in regression (3). A decrease in flexibility equivalent to a shift from the US to France is expected to add 0.25 ($=0.820 * (0.727 - 0.423)$) onto the size of the coefficient on lagged unemployment. Thus, the evidence is consistent with the hypothesis of Blanchard and Summers (1986), and Lindbeck and Snower (1989), as there seems to be less unemployment persistence in flexible labor markets.[23]

4. Conclusion

One of the biggest challenges in economics today is to explain what causes unemployment. Economists who study European unemployment often point out that it must be labor market regulations. This view has been adopted by international institutions like the World Bank and the IMF, which now insist that countries make their labor markets more flexible when providing them with financial support. The evidence available to support this view consists of cross-sections, like that of Bertola (1990) with 10 countries, or the OECD (1994) with 21, and the panel constructed by Lazear (1990). Because the latter focuses on laws for two aspects of flexibility that change little over time, these data are almost like another cross-section. There is, perhaps, no experience more sobering to an economist than to review the evidence we have to support policy recommendations on labor market flexibility and to reflect on the social, economic and personal costs of unemployment.

We introduce a new panel data set on labor market flexibility based on surveys of business people in 21 OECD countries during 1984–1990. One of the virtues of the data is that they originate from people who have to make their living out of roughly understanding how stringent job security provisions actually are in their countries. The

[22] This regression is illustrative. Caution should be exercised when using the absolute values of these coefficients because of the bias in short panels with lagged dependent variables.

[23] Furthermore, the argument that managers' responses to the flexibility survey question may depend on the stage of the business cycle would not seem to be able to explain this persistence effect.

use of a subjective index allows respondents to capture movements in very different kinds of regulations that affect the flexibility of labor markets, such as provisions on part time work, severance payments, interpretation (and enforcement) of what constitutes legal cause for termination and so on. These regulations imply very different costs to normal business operations and would be extremely difficult to document with hard data. There are, of course, limitations to the data we use. The index is more vague than what an economist would ideally like to use. By its nature, our flexibility index does not allow us to distinguish between the effect of the different regulations that are active. And although we present some time series/cross-section validation exercises, the fact must remain that data that are subjective in nature must be treated with care. However, we believe the relevance of the subject matter and the evidence available to the profession to be so out of balance that a willingness to experiment with survey data is justified.

We follow Lazear (1990) and use a parsimonious, reduced form approach to study the effect of flexibility on labor market performance. Our main findings are:

1. Controlling for country and time fixed effects, and using dynamic panel data techniques developed by Arellano and Bond (1991), we find that countries with more flexible labor markets have higher employment rates and higher rates of participation in the labor force. The results on employment are inconsistent with Bentolila and Bertola (1990) and Bertola (1990) and are consistent with the predictions in Hopenhayn and Rogerson (1993).
2. These results are stronger in the female labor market, although the long-run effects are approximately similar across both male and female sub-groups.
3. A potential drawback of these data is their contamination by the stage of the business cycle. When the economy is in recession firms are more likely to be firing than hiring and so employment protection legislation may impose binding constraints on firms. If managers' responses to our survey question were in fact in the direction of greater inflexibility at such times, even though the parameters of the system have not changed, then the interpretation of our results would be different. Consequently, we repeated all our regressions controlling for the state of the business cycle (the change in *GDP*). The results are unaffected. We also note that it we would be hard to explain some of our coefficients if the contamination of the flexibility data with the business cycle was the main factor driving our results.
4. The estimated employment effects seem to be large. A conservative estimate is as follows: if France were to increase the flexibility of its labor markets to US levels, the employment rate would increase by 1.6 percentage points, almost 14% of the actual difference in employment rates between the two countries. In order to estimate the effect of flexibility on French *GDP* per capita, we note that this increase in flexibility would lead to a 2.8% increase in French total employment. Of course, this says nothing about the convenience of such a reform. For that we would need information on the benefits (in terms of employment security, wages and so on) of flexibility, a fact sometimes forgotten in policy debates.
5. The paper only finds some evidence that countries with more flexible labor markets have lower unemployment rates and a lower proportion of long term unemployed.

The problem of the endogeneity of labor market institutions is addressed but still must remain an open issue.

6. In spite of some inconsistencies, the results on inflows and vacancies are interesting. Controlling for country and year fixed effects, we do not find evidence of positive effects of flexibility on inflows. We do however find evidence that more flexibility is associated with lower rates of unfilled vacancies and that there are more vacancies in a recovery that occurs in a country with high flexibility.
7. Lastly, we explore some alternative hypotheses related to flexibility that have been suggested in the literature. First, we examine the jobless recovery hypothesis. We find evidence that Okun's law is steeper in countries with very flexible labor markets (as suggested in Bertola, 1990). We also find evidence consistent with a second hypothesis tested by Bertola and suggested by Blanchard and Summers (1986) and Lindbeck and Snower (1989): that the dynamic structure of unemployment regressions is affected by flexibility. Controlling for country and time fixed effects, we find that unemployment is less persistent in countries with more flexible labor markets.

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Appendix A

A.1. Sample of 21 countries

Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States.

A.2. Definition of the variables

Employment: Total civilian employment divided by the population aged between 15 and 64 years old. From the updated CEP-OECD data set.

Male employment: Total civilian male employment divided by the population aged between 15 and 64 years old. From the updated CEP-OECD data set.

Female employment: Total civilian female employment divided by the population aged between 15 and 64 years old. From the updated CEP-OECD data set.

Participation: Total civilian employment plus total unemployment divided by the population aged between 15 and 64. From the updated CEP-OECD data set.

Male participation: Total civilian male employment plus total male unemployment divided by the population aged between 15 and 64. From the updated CEP-OECD data set.

Female participation: Total civilian female employment plus total female unemployment divided by the population aged between 15 and 64. From the updated CEP-OECD data set.

Unemployment: The unemployment rate, defined as the total number of unemployed workers divided by the total number of both employed and unemployed workers, from OECD Historical Statistics (1997).

Long-term unemployment: The number of workers who have been out of work for 6 months and more as a percentage of total unemployment, from the OECD Employment Outlook (1985–1991).

Benefits: The OECD summary measure of parameters of the UI system. To calculate this measure, the situation of a representative individual is estimated using their unemployment benefit entitlements divided by the corresponding wage. Consequently, the unweighted mean of 18 numbers based on the various combinations of the following scenarios is determined (1) three unemployment durations (for persons with a long record of previous employment)—the first year, second and third years, and fourth and fifth years of unemployment, (2) three family and income situations: a single person, a married person with a dependent spouse, and a married person with a spouse in work; (3) two different levels of previous earnings – average earnings and $\frac{2}{3}$ of average earnings (see OECD Jobs Study, 1994).

Flexibility: The survey question that we use (classified as 2.17 *LABOR-COST FLEXIBILITY* in 1984) asked the respondents: "*Flexibility of enterprises to adjust job security and compensation standards to economic realities*: $0 = none\ at\ all$, *to* $100 = a\ great\ deal$". This question was changed in 1990 to "*Flexibility of management to adjust employment levels during difficult periods*: $0 = low$, *to* $100 = high$". From the WCR, EMF Foundation, Cologny/Geneva.

Inflow rate: Number of people unemployed less than one month divided by the employed. Updated by the OECD in 1998. Unpublished.

GDP: The log of total *GDP* expressed in constant 1985 prices, from the updated CEP-OECD data.

Openness: Exports over *GDP* from the updated CEP-OECD data set.

Industrial production: The log of the total value added in industry expressed in constant 1985 prices, from OECD Historical Statistics (1997).

Service sector: The log of the total value-added in the service sector expressed in constant 1985 prices, from OECD Historical Statistics (1997).

Employment taxes: Employers' total employment tax contributions divided by the total compensation of employees (net of employment taxes), from the updated CEP-OECD data set.

Terms of trade: Ratio of average value of exports to average value of imports (from OECD Historical Stats).

Appendix B

Description of various data for 21 countries for the period 1984–1990 are given in Table 1 and correlation between flexibility and indicators of business cycle (1984–1990) is given in Table 2.

Table 1
Description of the data: 21 Countries, averages for 1984–1990

Country	*Flexibility*	*Benefits*	*Employment*	*Unemployment*	*LTU*
Australia	38.45	0.24	0.66	0.08	0.47
Austria	41.29	0.29	0.64	0.03	—
Belgium	41.83	0.44	0.54	0.11	0.85
Canada	56.90	0.28	0.68	0.09	0.23
Denmark	61.76	0.52	0.76	0.07	0.51
Finland	50.11	0.34	0.73	0.05	0.30
France	42.33	0.36	0.58	0.10	0.65
Germany	41.49	0.28	0.62	0.07	0.65
Ireland	47.57	0.29	0.51	0.16	0.80
Italy	39.87	0.01	0.52	0.11	0.85
Japan	55.43	0.09	0.71	0.03	0.39
Netherlands	46.70	0.53	0.55	0.10	0.67
Norway	40.89	0.38	0.75	0.03	0.19
New Zealand	40.95	0.27	0.67	0.05	0.32
Spain	29.81	0.34	0.45	0.19	0.74
Sweden	40.77	0.28	0.80	0.02	0.22
Switzerland	61.69	0.21	0.75	0.01	—
UK	58.08	0.19	0.67	0.09	0.61
USA	72.66	0.12	0.69	0.06	0.14
Greece	30.28	0.09	0.55	0.08	0.67
Portugal	33.12	0.27	0.65	0.07	0.66

Note: *Flexibility* is presented as in the WCR, on a 0–100 scale. In Table 3A onwards, the data have been scaled down by a factor of 100 (to lie on a 0–1 scale).

Table 2
Correlation coefficients between flexibility and indicators of the business cycle, 1984–1990[a]

	Flexibility	*GDP* per Capita	Δ*GDP* per Capita	Δ *Industrial production*	Δ *Service sector*
Flexibility	1				
GDP per capita	0.013	1			
Δ*GDP* per capita	0.014	−0.071	1		
Δ*Industrial production*	0.022	−0.023	0.601	1	
Δ*Service sector*	−0.078	−0.112	0.659	−0.127	1
Δ*Openness*	−0.006	0.076	−0.161	0.003	−0.224

[a]Based on 126 observations.

Appendix C

Determinants of the employment rates of 21 OECD countries for the period 1984–1990 are given in Table 3. Deteminants of different participation rates for 21 countries (1984–1990) are given in Table 4. Table 5 describes unemployment rates for these countries for the same period. For these countries, Table 6 estimates casualty and non-linear effects, random effect and fixed effects. Table 7 describes determinants of employment and participation, controlling for response bias. Table 8 describes inflow rate and vacancies, while Table 9 gives Okun's law and unemployment persistence.

Table 3
The determinants of the employment, female employment and male employement rates: 21 OECD countries, 1984–1990[a]

	(1) Random effects	(2) LSDV	(3) LSDV[b]	(4) LSDV	(5) GMM	(6) GMM
(A) *Employment rate*						
$Employment_{t-1}$					0.582**	0.436**
					(0.074)	(0.071)
Flexibility	0.144**	0.141**	0.053**		0.050**	0.033**
	(0.022)	(0.022)	(0.026)		(0.013)	(0.009)
$Flexibility_{t-1}$				0.087**		0.001
				(0.029)		(0.009)
Benefits	−0.041	−0.057	−0.129*		−0.037	−0.292**
	(0.071)	(0.081)	(0.075)		(0.044)	(0.097)
$Benefits_{t-1}$				−0.013		0.221**
				(0.077)		(0.065)
Country fixed effects	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes
No. of observations	144	144	144	123	123	102
Adj R^2	0.19	0.97	0.97	0.98	*0.16*	*0.24*
(B) *Female employment rate*						
$Employment_{t-1}$ (female)					0.399**	0.260**
					(0.036)	(0.047)
Flexibility	0.120**	0.119**	0.052**		0.032**	0.028**
	(0.014)	(0.014)	(0.015)		(0.005)	(0.005)
$Flexibility_{t-1}$				0.053**		0.007
				(0.016)		(0.006)
Benefits	−0.032	−0.056	−0.113**		−0.060**	−0.216**
	(0.046)	(0.051)	(0.043)		(0.024)	(0.064)
$Benefits_{t-1}$				−0.041		0.102**
				(0.043)		(0.035)
Country fixed effects	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes

Table 3 (*continued*)

	(1) Random effects	(2) LSDV	(3) LSDV[b]	(4) LSDV	(5) GMM	(6) GMM
No. of observations	144	144	144	123	123	102
Adj R^2	0.17	0.97	0.98	0.98	*0.24*	*0.21*
(C) *Male employment rate*						
$Employment_{t-1}$ (male)					0.711**	0.597**
					(0.069)	(0.065)
Flexibility	0.025**	0.022**	0.001		0.012**	0.006
	(0.011)	(0.011)	(0.014)		(0.004)	(0.005)
$Flexibility_{t-1}$				0.033**		0.011*
				(0.016)		(0.006)
Benefits	−0.015	−0.001	−0.016		0.034**	−0.004
	(0.033)	(0.039)	(0.040)		(0.017)	(0.046)
$Benefits_{t-1}$				0.027		0.020
				(0.041)		(0.040)
Country fixed effects	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes
No. of observations	144	144	144	123	123	102
Adj R^2	0.17	0.95	0.95	0.96	*0.29*	*0.15*

Notes: LSDV is least squares with dummy variables and GMM is generalized method of moments. For the GMM regressions, the Sargan test for the validity of the orthogonality conditions is reported (in italics) in place of the Adj R^2. The WCR flexibility data have been scaled down by a factor of 100 to lie on a 0–1 scale.

[a]Standard errors in parentheses.

*Denotes significance at the 10% level.

**Denotes significance at the 5% level.

[b]If we also control for ΔGDP in column (3) of Table 3(A) the coefficients (standard errors) on *Flexibility*, *Benefits* and ΔGDP are 0.052 (0.026), −0.106 (0.080) and −0.084 (0.097), respectively. The corresponding coefficients (standard errors) in column (3) of Table 3(B) on *Flexibility*, *Benefits* and ΔGDP are 0.051 (0.015), −0.090 (0.045) and −0.083 (0.055), respectively, and in column (3) of Table 3(C) they are 0.001 (0.014), −0.016 (0.043) and −3.2e − 4 (0.052), respectively

Table 4
The determinants of the participation, female participation and male participation rates: 21 OECD countries, 1984–1990[a]

	(1) Random effects	(2) LSDV	(3) LSDV[b]	(4) LSDV	(5) GMM	(6) GMM
(A) *Participation rate*						
$Participation_{t-1}$					0.689**	0.428**
					(0.059)	(0.057)
Flexibility	0.117**	0.114**	0.059**		0.058**	0.052**
	(0.018)	(0.018)	(0.021)		(0.007)	(0.006)
$Flexibility_{t-1}$				0.058**		−0.007
				(0.023)		(0.005)
Benefits	−0.050	−0.082	−0.139**		0.014	−0.168**
	(0.056)	(0.064)	(0.062)		(0.029)	(0.079)
$Benefits_{t-1}$				−0.048		0.078*
				(0.061)		(0.045)

Table 4 (*continued*)

	(1) Random effects	(2) LSDV	(3) LSDV[b]	(4) LSDV	(5) GMM	(6) GMM
Country fixed effects	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes
No. of observations	144	144	144	123	123	102
Adj R^2	0.19	0.97	0.97	0.98	*0.11*	*0.12*
(B) *Female participation rate*						
$Participation_{t-1}$ (female)					0.786**	0.567**
					(0.036)	(0.097)
Flexibility	0.118**	0.118**	0.061**		0.046**	0.040**
	(0.013)	(0.013)	(0.015)		(0.005)	(0.005)
$Flexibility_{t-1}$				0.048**		−0.013**
				(0.016)		(0.004)
Benefits	−0.043	−0.078	−0.133**		0.002	−0.118**
	(0.043)	(0.079)	(0.042)		(0.028)	(0.046)
$Benefits_{t-1}$				0.062		0.052**
				(0.043)		(0.025)
Country fixed effects	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes
No. of observations	144	144	144	123	123	102
Adj R^2	0.13	0.97	0.98	0.98	*0.12*	*0.08*
(C) *Male participation rate*						
$Participation_{t-1}$ (male)					0.504**	0.027
					(0.064)	(0.071)
Flexibility	−8.6e−4	−0.004	−0.002		0.007**	0.005[a]
	(0.007)	(0.007)	(0.010)		(0.002)	(0.003)
$Flexibility_{t-1}$				0.010		0.002
				(0.010)		(0.002)
Benefits	−0.011	−0.004	−0.006		0.022	−0.056**
	(0.023)	(0.026)	(0.028)		(0.022)	(0.023)
$Benefits_{t-1}$				0.014		0.020
				(0.026)		(0.022)
Country fixed effects	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes
No. of observations	144	144	144	123	123	102
Adj R^2	0.01	0.96	0.96	0.97	*0.34*	*0.35*

Notes: LSDV is least squares with dummy variables and GMM is generalized method of moments. For the GMM regressions, the Sargan test for the validity of the orthogonality conditions is reported (in italics) in place of the Adj R^2. The WCR flexibility data have been scaled down by a factor of 100 to lie on a 0–1 scale.

[a]Standard errors in parentheses.

*Denotes significance at the 10% level.

**Denotes significance at the 5% level.

[b]If we also control for ΔGDP in column (3) of Table 4(A) the coefficients (standard errors) on *Flexibility*, *Benefits* and ΔGDP are 0.058 (0.021), −0.103 (0.065) and −0.133 (0.078). The corresponding coefficients (standard errors) in column (3) of Table 4(B) on *Flexibility*, *Benefits* and ΔGDP are 0.061 (0.015), −0.113 (0.045) and −0.071 (0.054), respectively, and in column (3) of Table 4(C) they are −0.003 (0.009), 0.011 (0.029) and −0.062 (0.035), respectively.

Table 5
The determinants of the unemployment, and long-term unemployment rates: 21 OECD countries, 1984–1990[a]

	(1) Random effects	(2) LSDV	(3) LSDV[b]	(4) LSDV	(5) GMM	(6) GMM
(A) *Unemployment rate*						
Unemployment$_{t-1}$					0.840**	0.934**
					(0.035)	(0.039)
Flexibility	−0.056**	−0.053**	−3.5e−4		−0.014	−0.004
	(0.014)	(0.015)	(0.019)		(0.009)	(0.010)
Flexibility$_{t-1}$				−0.048**		−0.022*
				(0.022)		(0.013)
Benefits	0.050	−0.018	0.006		−0.004	−0.014
	(0.044)	(0.054)	(0.053)		(0.021)	(0.030)
Benefits$_{t-1}$				−0.039		−0.030
				(0.057)		(0.033)
Country fixed effects	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes
No. of observations	144	144	144	123	123	102
Adj R^2	0.09	0.93	0.94	0.94	*0.16*	*0.08*
(B) *Long-term unemployment rate*						
Long-term unemployment$_{t-1}$					0.592**	0.439**
					(0.078)	(0.084)
Flexibility	−0.170**	−0.150**	−0.090**		0.011	0.033
	(0.068)	(0.069)	(0.093)		(0.032)	(0.039)
Flexibility$_{t-1}$				−0.200*		−0.073**
				(0.108)		(0.031)
Benefits	0.059	0.001	−0.005		−0.555**	0.202
	(0.258)	(0.356)	(0.364)		(0.195)	(0.292)
Benefits$_{t-1}$				−0.292		0.327
				(0.345)		(0.200)
Country fixed effects	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes
No. of observations	129	129	129	112	108	89
Adj R^2	0.17	0.95	0.95	0.96	*0.26*	*0.18*

Notes: LSDV is least squares with dummy variables and GMM is generalized method of moments. For the GMM regressions, the Sargan test for the validity of the orthogonality conditions is reported (in italics) in place of the Adj R^2. The WCR flexibility data have been scaled down by a factor of 100 to lie on a 0–1 scale.

[a]Standard errors in parentheses.

*Denotes significance at the 10% level.

**Denotes significance at the 5% level.

[b]If we also control for ΔGDP in column (3) of Table 5(A) the coefficients (standard errors) on *Flexibility*, *Benefits* and ΔGDP are −0.001 (0.018), 0.026 (0.055) and −0.079 (0.067), respectively. The corresponding coefficients (standard errors) in column (3) of Table 5(B) on *Flexibility*, *Benefits* and ΔGDP are 0.087 (0.093), −0.010 (0.364) and 0.371 (0.335), respectively.

Table 6

(A) Some evidence on causality and nonlinear effects: 21 OECD countries, 1984–1990[a]

Dependent variable	(1) $\Delta Flexibility$ LSDV	(2) $\Delta Flexibility$ LSDV	(3) Δ *Unemp.* LSDV	(4) *Flexibility* LSDV	(5) *Unemp.* LSDV	(6) *Fem. Emp* LSDV
$Employment_{t-1}$	−6.590					
	(8.364)					
$Participation_{t-1}$	5.031					
	(7.846)					
$Unemployment_{t-1}$	−4.186	0.699		0.317	1.278**	
	(5.900)	(1.205)		(0.833)	(0.075)	
$Unemployment_{t-2}$		0.128		−0.235	−0.716**	
		(1.167)		(1.064)	(0.096)	
$Flexibility_{t-1}$			−0.027*	0.105	−0.026**	
			(0.014)	(0.121)	(0.011)	
$Flexibility_{t-2}$			−0.004	−0.168*	−0.004	
			(0.011)	(0.094)	(0.008)	
$Flexibility_t$						0.172**
						(0.056)
$Flexibility_t$ *squared*						−0.0013**
						(5.6e−04)
$Benefits_t$						−0.151**
						(0.046)
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	125	126	105	105	105	144
Adj R^2	−0.09	−0.11	0.25	0.81	0.99	0.98

(B) Other variables included; random effects: 21 OECD countries, 1984–1990[a]

Dependent variable	(1) *Employment*[b]	(2) *Participation*[c]	(3) *Unemp.*[d]	(4) *Long-term unemp.*[e]
Flexibility	0.144**	0.116**	−0.058**	−0.175**
	(0.022)	(0.018)	(0.015)	(0.069)
Benefits	−0.072	−0.073	0.042	−0.106
	(0.075)	(0.060)	(0.045)	(0.281)
Union coverage	−0.098**	−0.074**	0.044**	0.358**
	(0.039)	(0.032)	(0.017)	(0.095)
Decentralization	−0.015**	−0.011**	0.007**	0.036**
	(0.005)	(0.004)	(0.002)	(0.013)
Home Ownership	−0.114	−0.040	0.124**	−0.431
	(0.136)	(0.113)	(0.059)	(0.421)
No. of observations	139	139	140	122
Adj R^2	0.49	0.43	0.54	0.56

Table 6 (*continued*)

(C) Other variables included, fixed effects: 21 OECD countries, 1984–1990[a]

Dependent variable	(1) LSDV *Employ.*[f]	(2) LSDV *Particip.*[g]	(3) LSDV *Unemp.*[h]	(4) LSDV *Employ.*	(5) LSDV *Particip.*	(6) LSDV *Unemp.*
Flexibility	0.050*	0.044**	−0.017	0.092**	0.079**	−0.028
	(0.027)	(0.022)	(0.020)	(0.046)	(0.037)	(0.034)
Benefits	−0.042	−0.004	0.049	0.111	0.187*	0.064
	(0.087)	(0.071)	(0.063)	(0.133)	(0.107)	(0.098)
Union coverage	−0.095**	−0.089**	0.024	−0.065*	−0.054*	0.025
	(0.036)	(0.030)	(0.026)	(0.039)	(0.032)	(0.029)
Decentralization	−0.012**	−0.009**	0.006*	−0.009*	−0.006	0.006*
	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)
Employment taxes	−0.580**	−0.603**	0.047	−0.097	−0.034	0.066
	(0.207)	(0.170)	(0.149)	(0.334)	(0.270)	(0.247)
Employment taxes ∗ *Flexibility*				−0.149	−0.106	0.057
				(0.178)	(0.144)	(0.132)
Employment taxes ∗ *Benefits*				−1.200	−1.505**	−0.128
				(0.808)	(0.652)	(0.598)
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	122	122	123	122	122	123
Adj R^2	0.98	0.98	0.95	0.98	0.98	0.95

Notes: LSDV is least squares with dummy variables. The WCR Flexibility data have been scaled down by a factor of 100 to lie on a 0 to 1 scale. *Unemp* abbreviates the *Unemployment rate* and *Fem. Emp.* abbreviates *Female Employment*.

[a]Standard errors in parentheses.

*Denotes significance at the 10%.

**Denotes significance at the 5%.

[b]If we also control for ΔGDP in column (1) the coefficient (standard error) on *Flexibility* becomes 0.141 (0.022).

[c]Controlling also for ΔGDP in column (2) the coefficient (standard error) on *Flexibility* becomes 0.112 (0.018).

[d]Controlling also for ΔGDP in column (3) the coefficient (standard error) on *Flexibility* becomes −0.059 (0.015).

[e]Controlling also for ΔGDP in column (4) the coefficient (standard error) on *Flexibility* becomes −0.166 (0.069).

[f]If we also control for ΔGDP in column (1) the coefficient (standard error) on *Flexibility* is 0.050 (0.027).

[g]Controlling also for ΔGDP in column (2) the coefficient (standard error) on *Flexibility* is 0.044 (0.022).

[h]Controlling also for ΔGDP in column (3) the coefficient (standard error) on *Flexibility* is −0.017 (0.020).

Table 7
The determinants of employment and participation, controlling for response bias: 21 OECD countries, 1984–1990[a]

Dependent variable	(1) LSDV Employ. total	(2) LSDV Particip. total	(3) LSDV Employ. total	(4) LSDV Particip. total	(5) LSDV Employ. total	(6) LSDV Particip. total
Flexibility	0.051*	0.056**	0.053**	0.063**	0.055*	0.056**
	(0.030)	(0.024)	(0.027)	(0.022)	(0.031)	(0.025)
Benefits	−0.105	−0.101	−0.116	−0.116*	−0.129*	−0.141**
	(0.081)	(0.066)	(0.080)	(0.065)	(0.076)	(0.063)
ΔGDP	−0.106	−0.186				
	(0.334)	(0.271)				
$\Delta GDP * Flexibility$	0.048	0.116				
	(0.703)	(0.571)				
$\Delta Industry\ production$			−0.068	0.027		
			(0.202)	(0.165)		
$\Delta Industry\ production * Flexibility$			0.039	−0.104		
			(0.452)	(0.368)		
$\Delta Openness$					−0.058	−0.040
					(0.491)	(0.404)
$\Delta Openness * Flexibility$					−0.157	0.209
					(1.034)	(0.850)
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	144	144	144	144	144	144
Adj R^2	0.97	0.97	0.97	0.97	0.97	0.97

Notes: LSDV is least squares with dummy variables. The WCR Flexibility data have been scaled down by a factor of 100 to lie on a 0 to 1 scale. Sign$(x)=1$ if x is positive and 0 if x is negative.

[a]Standard errors in parentheses.

*Denotes significance at the 10% level.

**Denotes significance at the 5% level.

Table 8
The determinants of the inflow rate and vacancies (divided by employment): 21 OECD countries, 1984–1990[a]

	(1) Pooled OLS	(2) Random effects	(3) LSDV	(4) LSDV	(5) GMM	(6) LSDV	(7) LSDV
(A) *Inflow rate*							
Inflow Rate$_{t-1}$					0.402**		
					(0.056)		
Flexibility	2.578**	−0.189	−0.266**	−0.089	0.006	−0.097	−0.030
	(0.386)	(0.134)	(0.130)	(0.166)	(0.051)	(0.163)	(0.190)
Benefits	−0.594*	0.348	0.954	1.141*	1.768**	1.157*	1.271*
	(0.354)	(0.562)	(0.671)	(0.653)	(0.504)	(0.642)	(0.664)
ΔGDP						−1.270**	0.404
						(0.591)	(2.459)
$\Delta GDP * Flexibility$							−3.515
							(5.010)
Country fixed effects	No	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	No	Yes	Yes	Yes	Yes
No. of obs	129	129	129	129	108	129	129
Adj R^2	0.26	0.12	0.98	0.98	*0.49*	0.98	0.98
(B) *Vacancies* (*divided by employment*)							
Vacancies/ Employment$_{t-1}$					0.508**		
					(0.161)		
Flexibility	−0.001	0.005*	0.005*	−0.008**	2.0e−4	−0.007**	−0.013**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Benefits	−0.003	0.003	0.013	0.007	−0.002	−0.009	−0.004
	(0.003)	(0.006)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
ΔGDP						0.048**	−0.068*
						(0.011)	(0.037)
$\Delta GDP * Flexibility$							0.255**
							(0.078)
Country fixed effects	No	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	No	Yes	Yes	Yes	Yes
No. of observations	126	126	126	126	108	126	126
Adj R^2	0.01	0.01	0.66	0.77	*0.14*	0.80	0.82

Note: LSDV is least squares with dummy variables and GMM is generalized method of moments. For the GMM regressions, the Sargan test for the validity of the orthogonality conditions is reported (in italics) in place of the Adj R^2. The WCR Flexibility data have been scaled down by a factor of 100 to lie on a 0 to 1 scale.

[a]Standard errors in parentheses.

*Denotes significance at the 10% level.

**Denotes significance at the 5% level.

Table 9
Okun's law and unemployment persistence: 21 OECD countries, 1984–1990[a]

Dependent variable	(1) $\Delta Unemp.$ LSDV	(2) $\Delta Unemp.$ LSDV	(3) *Unemp.* LSDV	(4) *Unemp.* GMM
$Unemployment_{t-1}$			0.596**	0.286**
			(0.117)	(0.148)
$Unemployment_{t-1}$			0.462**	0.820**
$*(1 - Flexibility_{t-1})$			(0.141)	(0.195)
$\Delta Unemployment_{t-1}$	0.366**	0.318**		
	(0.069)	(0.071)		
ΔGDP	−0.261**	0.123		
	(0.036)	(0.155)		
$\Delta GDP * Flexibility$		−0.481*		
		(0.278)		
$\Delta GDP * Benefits$		−0.546*		
		(0.321)		
Flexibility		0.002	0.011	−0.003
		(0.012)	(0.013)	(0.007)
Benefits		−0.027	−0.040	−0.018
		(0.031)	(0.053)	(0.038)
Country fixed effects	No	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
No. of observations	147	147	126	105
Adj R^2	0.41	0.43	0.98	*0.06*

Unemp. abbreviates the unemployment rate. The WCR Flexibility data have been scaled down by a factor of 100. $1 - Flexibility$ measures labor market inflexibility (on a 0 to 1 scale).

[a]Standard errors in parentheses.

*Denotes significance at the 10% level.

**Denotes significance at the 5% level.

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