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The Consequences of Labour Market Flexibility: Panel Evidence Based on Survey Data

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The Consequences of Labour Market Flexibility: Panel Evidence Based on Survey Data[1]

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Abstract

We introduce a new data set on hiring and firing restrictions for 21 OECD countries for the period 1984-90. 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. The analysis of inflows and vacancies present some inconsistencies, although there is some evidence that the correlation between inflows and the business cycle is stronger in more flexible labor markets (again this is stronger for females). We also find some evidence consistent with the hypothesis that inflexible labor markets produce "jobless recoveries" and introduce more unemployment persistence.

JEL Classification: J65

Keywords: Job security provisions, subjective data, employment, and unemployment.

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I. 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 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 economist's advice and reduced hiring and firing restrictions in the mid 1980's to combat high unemployment. This view of the labour 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 Washington Consensus often calls "second generation reforms".[2]

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 in fact does 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 (Lazear (1990)). And only a couple of cross-country 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. 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 1984-90. 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. 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 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.[3] This is equivalent to 14% of the employment rate gap between the two countries. In order to express this effect in terms of

[2] Labor flexibility is one of the main features of the IMF's recent agreement with Argentina, where unemployment reached an alarming 18.6% after the first 5 years of pro-market reforms.

[3] France is the median country in terms of flexibility. See Table A for a full description of the data.

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. There is weaker evidence that flexibility leads to longer hours worked in manufacturing. The effect of unemployment benefits on these variables is less clear cut. 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", introduce more unemployment persistence and reduce the country's vulnerability to external shocks.

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 to 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 the 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 ten 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.[4] Clearly, flexibility of the labor market could be affected without showing up in these two series. Third, it does not allow for the fact that countries differ in the degree of enforcement of these laws, and that other, perhaps informal, aspects maybe 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). 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

[4] 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 authorisation by third parties (e.g. government, or trade union), provisions for appeal against unfair dismissal and the enforcement of these rules.

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 nicely with the index of "*strictness of employment protection legislation*" constructed for the OECD Jobs Study (1994), arguably the most complete measure available, for the one 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 years versus 29). 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.[5]

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 labour demand toward lower average 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 one 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%. 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 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).

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 unemployment benefit

[5] Put another way, the data that we use have different problems to the data previously used in the literature. Thus, we

system compiled by the OECD Jobs Study (1994). 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.[6]

In section II we present the data while section III presents the empirical results. Section IV concludes.

II. Empirical Strategy and Data Description

II.a. 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

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

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 1.

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.

II.b. Construction of the Data Set

The indicator of labour market flexibility used in this paper comes from the *World Competitiveness Report* (WCR), a publication of the EMF foundation in Geneva. It covers 21 countries (a list is given in Appendix 1) and covers the period 1984-90. 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 (1994)), but its use as a source of labor market flexibility data is new. It consists of yearly surveys conducted amongst chief executive officers and economic leaders in the countries covered, who are mailed a questionnaire

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view this paper as complementing Lazear's approach with "hard" data.

[6] 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* (1990) uses the 1985 duration of unemployment benefits as an indicator of generosity for the whole sample.

containing a large number of questions on their country's competitiveness (unfortunately sometimes it can contain as many as 90 questions). The number of returned questionnaires varies every year: 1,100 in 1984, 1,598 in 1985, 1,369 in 1986, 1,937 in 1988, 1,800 in 1989, and 1,384 in 1990.[7] There was no WCR containing 1987 data so the 1986 and 1988 observations were interpolated to obtain observations for 1987. The survey question that is used (classified as 2.17 *LABOUR-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.

First, 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.[8] Second, it is worth emphasising that with survey data it is feasible to capture the many dimensions that the types of 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 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 a very meticulous manner. Another important advantage is that the respondents have actual experience and knowledge of the workings of the labour market in question, so presumably they know the relevance, if any, of changes in the written laws.

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 labour 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.[9] The correlation coefficient of the WCR survey measure of labour market flexibility in 1989

[7] For 1989 the WCR just says, "over 1,800" questionnaires were returned. It only reports the number of questionnaires sent out for 1984 (5,500 sent out, for a response rate of over 20%) and 1985 (7,513 sent out for a response rate of 21%). We do not know response rates for later years, except for the 1993 WCR (2,160 returned out of 10,300 sent out), although this issue did not contain the flexibility question.

[8] 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).

[9] The Jobs Study notes researchers have constructed various summary indicators to describe the strictness of employment protection in each country but that "*given the complexity of the phenomenon, summary indicators are inevitably somewhat arbitrary*" (page 70). Norway and Sweden have relatively high rankings on the OECD scale of strictness of employment protection. This is due to, for example, the legislative provisions allowing courts to order the

(where high values denote high flexibility) with the 1989 OECD indicator (where high values denote greater strictness) is -0.754. Higher levels of flexibility measured by the WCR survey responses are strongly associated with lower levels of employment protection strictness measured by the OECD quantification of legal data, as we can reject the hypothesis of independence of the two variables. These two measures are plotted in Figure 1 in Appendix 2.

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 labour market flexibility in 1985 with the 1985 IOE indicator is -0.586. 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). These two measures are plotted in Figure 2 in Appendix 2. 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 -0.578 and we can reject the null of zero correlation. The data are plotted in Figure 3 in Appendix 2.

Time Series Validation: Recently, Saint-Paul (1996) 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 weather 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 9 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 disagrees with Saint Paul's classification. These are: the United Kingdom 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 liberalisation 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 $\Delta Flexibility$ registers only very small values (5.9% and 5.8% of a standard deviation in the $\Delta Flexibility$ variable).

A further concern with the WCR flexibility indicator is that, being assessments of businesspersons, 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 correlation in any of them. For example, we study the correlation between flexibility and a measure of aggregate GDP (at constant 1985 prices). Pearson's correlation coefficient is negative and insignificant (so is Spearman's rho) so we cannot reject the hypothesis that the two variables are independent. Controlling for time and fixed effects, the estimated

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reinstatement of unfairly dismissed employees in Norway and the 6-month trial period in Sweden which must be given to dismiss a 35 year old worker.

effect of aggregate GDP on flexibility is positive and insignificant (it is significant at the 79% level). Still, the empirical section will present regression estimates where flexibility appears lagged one year, so the possibility of joint determination of flexibility with economic variables is reduced.

As a measure of the level of unemployment benefits in a country, we use the recently published OECD summary measure of the parameters of the unemployment insurance system (OECD Jobs Study (1994)). 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 year, and fourth and fifth year).[10] It is not weighted by the composition of unemployment in any particular place or period. This data represents 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 don't 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 emphasised 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 two-yearly intervals. Consequently observations were interpolated to obtain data for consecutive years.

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) 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 this data is the Centre for Economic Performance OECD 1950-88 data set compiled by Bagliano *et al* (1992) and updated by Brian Bell (see Appendix 1 for all data definitions). 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 one month divided by total employment).

Data definitions appear in Appendix 1, while summary features of the data are described in Table A in Appendix 1. The raw data show that countries with more flexible labour markets have higher employment rates, lower unemployment rates and lower proportions of long-term unemployed, though the relationships are by no means monotonic. The raw data are graphed in Appendix 2.

III. Empirical Results

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

III. a. Basic Evidence on Labor Market Flexibility

We begin our empirical analysis by studying the effect of labor market flexibility on the employment rate. In Figure 4, in Appendix 2, we graph the raw flexibility and employment data. Regression (1) in Table IA estimates the effect of *Flexibility* and *Benefits* on the employment rate using Generalised Least Squares random effects (Balestra-Nerlove). For purposes of comparison, Table VI on page 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 IA shows that countries with more flexible labour 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 one standard deviation increase in the flexibility of the labor market will bring about an increase in the employment rate of 1.9%, 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%. 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 80's. 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. We can also do a simple linear calculation to convert these employment gains into income. To see 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.[11] The estimators in regression (2) of Table IA 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) where the effects of severance pay on the employment rate are insignificant once he controls for country dummies (Table V, p.714). Regression (3) includes year dummies. Controlling for year fixed effects adds the requirement that a country with a higher than average flexibility reading one 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

[11] 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 2 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.

employment rate would increase by 1.6 percentage points, now only 14% of the difference in the employment rates of the two countries.[12] 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).

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 one year. Regression (4) in Table IA indicates that the one-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 two-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 with one-year or two-year lags are used.[13]

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 run, 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 IA, 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. 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. By a similar logic, LSDV is also an inconsistent estimator, particularly as the time series dimension of the panel is short (see Nickell (1981) for estimates of the bias for different *T*). To correct for this, the Generalized Method of Moments (GMM) technique developed by Arellano and Bond (1991) was used. Valid instruments are specified in each time period for the first-differenced equations. Regression (5) in Table IA 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 (Dy_{it-1}) with lagged levels of dependent variables dated *t-2* and earlier. For details, see Arellano and Bond (1991). 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

[12] 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% to the Spanish 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 IA includes the more general specification with lags of the dependent and independent variables. The current level of *Flexibility* is still positive and comfortably significant. 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 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.[14] 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 IB and IC 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 IB are almost identical to those in Table IA. The size of the coefficients is also very similar. 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 IB and IC seems 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 IB 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 IC 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. Theoretically, we expect there to be higher participation rates in less regulated markets. The traditional explanation, pointed out by Lazear, is that more inflexible labor markets mean there is less employment (as Table I seems to indicate). Then, we would expect that some of the workers who become unemployed drop out of the labor force. Another mechanism could emphasise insurance effects. If layoffs are discouraged by greater regulation, then wage income is less risky as there is less probability of losing one's job. This "insurance" effect may also mean that more flexible labor markets are associated with higher participation rates, particularly if labor supply decisions are made at the household level. In Table IIA we explore this hypothesis. Again we 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

[13] As we mentioned in section *II.b.* above, we did not find evidence of such a correlation.
[14] For example, Lazear has a short section on the effects of severance pay on the labor market performance of the young.

negative effects of unemployment benefits.

The literature suggests some stylised 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 will be higher female labor force participation when men face higher probabilities of losing their jobs (and higher accessions). Table IIB shows this is largely the case in our sample, with well defined and positive effects of flexibility on female labor force participation. Table IIC 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 III studies the effects of flexibility on the average number of hours worked per week in manufacturing. There is some evidence of a positive relationship. To get a feel for the size of the effect, regression (3) indicates that, if the French were to have American regulations in the labor market, they would be working 27 more minutes per week, almost 21% of the gap in hours worked per week between the two countries. There appears to be very little persistence in hours worked (even in LSDV regressions). We could not find data to study the effects for males and females separately.

Table IVA presents unemployment regressions. Regression (1) finds that countries with more flexible labor markets have lower unemployment rates in a regression controlling for random effects. 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 late 80's. 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 than that in regression (2). Regressions (5) and (6) in Table IVA include a lagged dependent variable and only find very weak negative effects of flexibility on unemployment. In Figure 5, in Appendix 2, we graph the raw data.

A number of economists have predicted adverse effects of inflexible labor markets on the composition of unemployment (e.g. McCormick (1991)). Table IVB 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 equation (3) the coefficient on flexibility becomes insignificant (though still

negative). The lagged specification used in equation (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 equation (6) though the number of observations drops as low as 89.

III. b. Causality and Other Controls

It has been pointed out, however, that unemployment may cause changes in labor market institutions (Lazear (1990) and Saint Paul (1996) on flexibility and Wright (1986), Atkinson (1990) and Di Tella and MacCulloch (1995) 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 7 time periods, however, reduces the usefulness of examining Granger-causality for individual countries. Thus, we first run a panel regression of $_{D}Flexibility_{t}$ 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 VA. 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 VA, based on the extremely limited data available for this type of exercise, is not supportive of the reverse causality hypothesis.

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 literature that studies the role of labor market institutions in shaping unemployment (e.g Layard, Nickell and Jackman (1991), Oswald (1996)). Table VB presents regressions where some of the variables identified in this literature are included as controls. The information is available for only one year when we also have the flexibility data. Thus, we concentrate on regressions that control for random effects. Regression (1) in Table VB presents an unemployment regression similar to the first regression in Table IVA with three extra controls: union coverage, centralisation 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, Nickell and Jackman (1991)). Centralisation 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 negative,

comfortably significant and of almost identical size as that in regression (1) in Table IVA. The other variables also enter the regression with the expected sign and are often significant (the only exception is unemployment benefits). In terms of comparative size the effect is only moderate. On an average year, it explains about 19% of the unemployment difference between Spain and the US, which is less than what the gap in the Union and Centralisation variables between the two countries explain. The rest of Table VB shows that including these three controls does not change the results we obtained earlier with respect to employment, participation rates, and the proportion of long term unemployment in the unemployment pool. The same is true when we study females separately from males (results not reported but available upon request).

III. c. 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 behaviour 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 (which in turn collects it from the OECD Main Economic Indicators), 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.[15] In Table VIA and VIB we study the effect of flexibility on these variables. In spite of some inconsistencies, the results are interesting. For example, regression (1) in Table VIA shows that the pooled data reveal a very strong positive association between inflows and flexibility. This is what most economists expect and in Figure 6, in Appendix 2, we graph the raw inflow and flexibility data. 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 nor the inclusion of year dummies.

Regression (5) shows low persistence of inflows and some positive effects of unemployment benefits. The last 3 regressions in Table VIA control for the state of the business cycle, proxied by the log of total GDP in the country expressed in constant 1985 prices. 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 (GDP * Flexibility). The estimated effects have the expected signs though are insignificant (the interaction is only significant at the 11% level). Regression (8) examines the same hypothesis in the female sub-sample. Interestingly, we again find evidence that the effect of flexibility is more precisely estimated in the female labor market. Forcing slightly the interpretation of the variables used, the evidence suggests that recessions lead to more job destruction in very flexible countries (this is as close as we can get with the data available to testing the hypothesis that regulation interferes with job destruction). With respect to vacancies, there are again some inconsistencies. Regression (4) in Table VIB shows that, controlling for both country and time fixed effects, countries with higher levels of labor market flexibility have less unfilled vacancies.[16] Presumably, this

[15] We interpolated 4 values of inflows, Netherlands 1984 and 1986 and Finland 1988 and 1990. The results do not change if these observations are excluded.

[16] Normalising by unemployment (instead of employment) produces largely similar results.

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. No evidence is found that vacancies increase more in a recovery that occurs in a country with high flexibility, though the interaction term is positive.

Lastly, in Table VII we examine three other hypotheses that have been proposed in the literature concerning flexibility of labor markets. 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 VII where the dependent variable is the unemployment rate. Regression (1) shows the basic relationship between a country's total GDP and unemployment, once we control for time and country fixed effects. Regression (2) shows that the interaction term (*GDP * Flexibility*) is negative, indicating that when GDP increases unemployment falls more in countries with more flexible labor markets. The estimated effect, however, is insignificant, although its sign is consistent with Bertola's predictions. Stronger effects are found (the interaction term is negative and significant), when the dependent variable is the female unemployment rate.[17]

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 VII, regression (3) includes an interaction term, $Unemployment_{t-1}*(1-Flexibility_{t-1})$, which is positive and significant at the 2 per cent level.[18] 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 U.S.). 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 U.S.) 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 per cent 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 U.S. 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

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[17] All regressions reported but not included in the paper are available upon request.

[18] This regression is illustrative. Caution should be exercised when using the absolute values of these

(1986), and Lindbeck and Snower (1989), as there seems to be less unemployment persistence in flexible labor markets.

Lastly, we examine the basic evidence on the hypothesis that the effect of openness is amplified in flexible labor markets. The idea is again based on the recent differential performance of the American and European labor markets. To test this hypothesis at this general level, we repeated the analysis but included a measure of openness (imports plus exports over GDP) and an interaction term (Open * Flexibility). The evidence in regressions with country and year dummies is mixed. For a number of different dependent variables there were no significant effects. For example, the interaction term was insignificant in unemployment, employment and participation regressions, with the exception of male participation that seemed to fall with higher flexibility and higher openness but had a positive and significant interaction term. In the last two regressions in Table VII we present the regressions where the effects seem to be stronger. Regression (5) is an hours regression. It shows that, controlling for fixed effects, countries with more flexible labor markets or that are more open to trade, work longer hours. It also shows that the effect of openness on hours is lower the more flexible is the labor market. Regression (6) is a wage regression showing that, controlling for fixed effects, countries with higher flexibility and more openness to trade have lower real wages (average weekly wages in manufacturing adjusted by the consumer price index). The interaction term is negative and significant, indicating that the effect of openness gets smaller as flexibility increases. If the effects are taken to be causal, this result could be of interest to economists interested in reforms. It implies that a country that opens up to trade and makes its labor markets more flexible at the same time is likely to experience a smaller reduction in real wages than a more cautious country that decides to only open up to trade. Thus, at least from the workers' perspective, little could be worse than doing one thing at a time (sequential reforms).

IV. 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 we have 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, this data is 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 1984-90. One of the virtues of the data is that it originates from people who have to make their living out of roughly understanding how stringent job security provisions actually are in

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coefficients because of the bias in short panels with lagged dependent variables.

their countries. The 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 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 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). We also find weaker evidence of a positive effect of flexibility on the number of hours worked per week.
2. These results are stronger in the female labor market, though the long run effects are approximately similar across both male and female sub groups.
3. 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 see its effect 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.
4. 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 given the unavailability of convincing instruments, must remain an open issue.
5. 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 find, however, evidence suggesting that recessions are associated with higher inflow rates in more flexible labor markets (this is as close as we can get with the data available to testing the hypothesis that regulation interferes with job destruction). This effect is again stronger in the female labor market. We find some evidence that more flexibility is associated with lower rates of unfilled vacancies.
6. 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 that Okun's law is steeper in countries with very flexible labor markets (as suggested in Bertola (1990)), though the effect is insignificant. We do, however, 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. Lastly, we do not find evidence that the effect of foreign competition on the labor market is amplified by flexibility.

Appendix 1: Sample of 21 countries (with country codes if included in figures 1 and 2)Australia (aus), Austria (aut), Belgium (bel), Canada (can), Denmark (den), Finland (fin), France (fra), Germany (ger), Ireland (ire), Italy (ita), Japan (jap), Netherlands (net), New Zealand (nz), Norway (nor), Spain (spa), Sweden (swe), United Kingdom (uk), United States (us).

Greece, Portugal and Switzerland are included in our sample, though not in figures 1 and 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 from OECD Historical Statistics (1991).

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 the parameters of the UI system. In calculating this measure, the situation of a representative or average individual is estimated (using the (pre-tax) 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: -3 unemployment durations (for persons with a long record of previous employment); the first year, the 2nd and 3rd years, and the 4th and 5th years of unemployment. -three family and income situations: a single person, a married person with a dependent spouse, and a married person with a spouse in work; and -2 different levels of previous earnings: average earnings and 2/3 of average earnings (see OECD Jobs Study (1994)).

Flexibility: The survey question that we use (classified as 2.17 *LABOUR-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 World Competitiveness Report, EMF Foundation, Cologny/Geneva.

Inflow: 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.

Open: Exports plus imports over GDP, from the updated CEP-OECD data set.

Wages: Index of hourly earnings in the manufacturing sector deflated by the consumer price index, from the updated CEP-OECD data set.

Hours: Average weekly hours worked in manufacturing sector, from the updated CEP-OECD data set.

TABLE A: Description of the Data: Averages for 1984-1990

Country	Flexibility	Benefits	Emp.	Unemp.	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 to 100 scale. In Table IA onwards, the data have been scaled down by a factor of 100, to lie on a 0 to 1 scale.

Appendix 3

TABLE IA
The Determinants of the Employment Rate, 21 OECD countries 1984-90.

	(1) Random Effects	(2) LSDV	(3) LSDV	(4) LSDV	(5) GMM	(6) GMM
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*

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE IB
The Determinants of the Female Employment Rate, 21 OECD countries 1984-90.

	(1) Random Effects	(2) LSDV	(3) LSDV	(4) LSDV	(5) GMM	(6) GMM
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
N of Observations	144	144	144	123	123	102
Adj R^2	0.17	0.97	0.98	0.98	*0.24*	*0.21*

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE IC
The Determinants of the Male Employment Rate, 21 OECD countries 1984-90.

	(1) Random Effects	(2) LSDV	(3) LSDV	(4) LSDV	(5) GMM	(6) GMM
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*

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE IIA
The Determinants of the Participation Rate, 21 OECD countries 1984-90.

	(1) Random Effects	(2) LSDV	(3) LSDV	(4) LSDV	(5) GMM	(6) GMM
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)
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*

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE IIB
The Determinants of the Female Participation Rate, 21 OECD countries 1984-90.

	(1) Random Effects	(2) LSDV	(3) LSDV	(4) LSDV	(5) GMM	(6) GMM
Participation t-1 (female)					0.786** (0.086)	0.567** (0.094)
Flexibility	0.118** (0.013)	0.118** (0.013)	0.061** (0.015)		0.046** (0.005)	0.040** (0.005)
Flexibility t-1				0.048** (0.016)		-0.013** (0.004)
Benefits	-0.043 (0.043)	-0.078 (0.049)	-0.133** (0.042)		0.002 (0.028)	-0.118** (0.046)
Benefits t-1				0.062 (0.043)		0.052** (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*

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE IIC
The Determinants of the Male Participation Rate, 21 OECD countries 1984-90.

	(1) Random Effects	(2) LSDV	(3) LSDV	(4) LSDV	(5) GMM	(6) GMM
Participation t-1 (male)					0.504** (0.064)	0.027 (0.071)
Flexibility	-8.6e-4 (0.007)	-0.004 (0.007)	-0.002 (0.010)		0.007** (0.002)	0.005* (0.003)
Flexibility t-1				0.010 (0.010)		0.002 (0.002)
Benefits	-0.011 (0.023)	-0.004 (0.026)	-0.006 (0.028)		0.022 (0.022)	-0.056** (0.023)
Benefits t-1				0.014 (0.026)		0.020 (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*

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE III
The Determinants of Weekly Hours Worked, 21 OECD countries 1984-90.

	(1) Random Effects	(2) LSDV	(3) LSDV	(4) LSDV	(5) GMM	(6) GMM
Hours Worked t-1					0.124	-0.082
					(0.111)	(0.110)
Flexibility	0.376	0.199	1.479*		0.640**	0.509*
	(0.618)	(0.605)	(0.800)		(0.323)	(0.292)
Flexibility t-1				1.097		0.311
				(0.870)		(0.608)
Benefits	-1.867	0.998	1.819		1.256	0.657
	(2.248)	(2.375)	(2.455)		(1.060)	(2.119)
Benefits t-1				-0.456		1.196
				(2.603)		(1.675)
Country Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	Yes	Yes	Yes
No. of Observations	137	137	137	116	116	96
Adj R^2	0.33	0.98	0.98	0.99	*0.63*	*0.35*

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE IVA
The Determinants of the Unemployment Rate, 21 OECD countries 1984-90.

	(1) Random Effects	(2) LSDV	(3) LSDV	(4) LSDV	(5) GMM	(6) GMM
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	0.005	-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	147	147	147	126	126	105
Adj R^2	0.09	0.93	0.94	0.94	*0.16*	*0.08*

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE IVB
The Determinants of the Long Term Unemployment Rate, 21 OECD countries 1984-90.

	(1) Random Effects	(2) LSDV	(3) LSDV	(4) LSDV	(5) GMM	(6) GMM
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*

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE VA
Some evidence on Causality.

Dependent Variable:	(1) *DFlexibility* LSDV	(2) *DFlexibility* LSDV	(3) *DUnemp.* LSDV	(4) *Flexibility* LSDV	(5) *Unemp.* 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)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
No of Observations	125	126	105	105	105
Adj R^2	-0.09	-0.11	0.25	0.81	0.99

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. 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.

TABLE VB
Other Variables Included; Random Effects

Dependent Variable:	(1) Unemp.	(2) Employment	(3) Participation	(4) Hours Worked	(5) Long Term Unemp.
Flexibility	-0.058**	0.144**	0.116**	-0.246	-0.175**
	(0.015)	(0.022)	(0.018)	(0.520)	(0.069)
Benefits	0.042	-0.072	-0.073	-2.504	-0.106
	(0.045)	(0.075)	(0.060)	(1.957)	(0.281)
Union Coverage	0.044**	-0.098**	-0.074**	0.544	0.358**
	(0.017)	(0.039)	(0.032)	(1.385)	(0.095)
Centralisation	0.007**	-0.015**	-0.011**	0.604**	0.036**
	(0.002)	(0.005)	(0.004)	(0.172)	(0.013)
Home Ownership	0.124**	-0.114	-0.040	-9.143*	-0.431
	(0.059)	(0.136)	(0.113)	(4.866)	(0.421)
No. of Observations	140	139	139	130	122
Adj R^2	0.54	0.49	0.43	0.58	0.56

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. All regressions are estimated using random effects (Balestra-Nerlove). The WCR Flexibility data have been scaled down by a factor of 100 to lie on a 0 to 1 scale.

TABLE VIA
The Determinants of the Inflow Rate, 21 OECD countries 1984-90.

	(1) Pooled OLS	(2) Random Effects	(3) LSDV	(4) LSDV	(5) GMM	(6) LSDV	(7) LSDV	(8) LSDV Women
Inflows t-1					0.402**			
					(0.056)			
Flexibility	2.578**	-0.189	-0.266**	-0.089	0.006	-0.202	1.317	1.974**
	(0.386)	(0.134)	(0.130)	(0.166)	(0.051)	(0.160)	(0.961)	(0.845)
Benefits	-0.594*	0.348	0.954	1.141*	1.768**	1.089*	1.282**	1.293**
	(0.354)	(0.562)	(0.671)	(0.653)	(0.504)	(0.618)	(0.625)	(0.549)
GDP						-1.369**	-1.415**	-0.666**
						(0.378)	(0.376)	(0.331)
GDP * Flexibility							-0.117	-0.156**
							(0.073)	(0.064)
Country Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes	Yes	Yes
No. of Obs	129	129	129	129	108	129	129	129
Adj R^2	0.26	0.12	0.98	0.98	*0.49*	0.98	0.98	0.99

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE VIB
The Determinants of Vacancies (divided by Employment), 21 OECD countries 1984-90.

	(1) Pooled OLS	(2) Random Effects	(3) LSDV	(4) LSDV	(5) GMM	(6) LSDV	(7) LSDV
Vacancies/ Employment t-1					0.508**		
					(0.161)		
Flexibility	-0.001	0.005*	0.005*	-0.008**	2.0e-4	-0.006**	-0.014
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.021)
Benefits	-0.003	0.003	0.013	0.007	-0.002	0.001	-0.001
	(0.003)	(0.006)	(0.010)	(0.009)	(0.009)	(0.009)	(0.010)
GDP						0.018**	0.017**
						(0.008)	(0.008)
GDP * Flexibility							5.7e-4
							(0.002)
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*	0.77	0.77

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. LSDV is Least Squares with Dummy Variables and GMM is Generalised 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.

TABLE VII
Okun's Law, Unemployment Persistence & External Vulnerability, 21 OECD countries 1984-90.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	*Unemp.* LSDV	*Unemp.* LSDV	*Unemp.* LSDV	*Unemp.* GMM	*Hours* LSDV	*Wages* LSDV
Unemployment t-1			0.596**	0.286**		
			(0.117)	(0.148)		
Unemployment t-1 * (1-Flexibility t-1)			0.462**	0.820**		
			(0.141)	(0.195)		
GDP	-0.294**	-0.292**				
	(0.036)	(0.036)				
GDP * Flexibility		-0.009				
		(0.007)				
Open					9.577**	-0.552**
					(1.580)	(0.130)
Open * Flexibility					-6.862**	0.584**
					(1.622)	(0.130)
Flexibility	-0.027*	0.087	0.011	-0.003	6.228**	-0.562**
	(0.015)	(0.091)	(0.013)	(0.007)	(1.409)	(0.112)
Benefits	0.075*	0.095**	-0.040	-0.018	-1.635	0.450**
	(0.043)	(0.046)	(0.053)	(0.038)	(2.210)	(0.183)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of Observations	147	147	126	105	137	144
Adj R^2	0.96	0.96	0.98	*0.06*	0.99	0.66

Note: Standard errors in parentheses. * denotes significance at the 10 percent level, ** denotes significance at the 5 percent level. Dependent variable is unemployment for regressions (1)-(4); average weekly hours worked in manufacturing for regression (5); and real wages in manufacturing in regression (6). *Unemp.* abbreviates the unemployment rate. The WCR Flexibility data have been scaled down by a factor of 100. *1-Flexibility* measures labour market inflexibility (on a 0 to1 scale).

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