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Working Paper

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severance pay? : Further iterations of Lazear et al.

IZA Discussion Papers, No. 943

Provided in Cooperation with:

IZA – Institute of Labor Economics

Suggested Citation: Addison, John T.; Teixeira, Paulino (2003) : What have we learned about the employment effects of severance pay? : Further iterations of Lazear et al., IZA Discussion Papers, No. 943, Institute for the Study of Labor (IZA), Bonn

This Version is available at:

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Further Iterations of Lazear et al.**

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ABSTRACT

What Have We Learned About the Employment Effects of Severance Pay? Further Iterations of Lazear et al.*

In this study we examine the contribution of severance pay to employment and unemployment development using data on industrialized OECD countries. Our starting point is Lazear's (1990) *empirical dictum* that severance payment requirements adversely impact the labor market. We extend his sample period and add to his parsimonious specification a variety of fixed and time-varying labor market institutions. While the positive effect of severance pay on unemployment garners some support, there is no real indication of adverse effects for (the three) other employment outcomes identified here. Moreover, with the possible exception of collective bargaining coordination, the role of institutions is also more muted than suggested in the literature.

JEL Classification: E24, J23, J64, J65

Keywords: severance pay, employment protection, employment, labor force participation, unemployment, long-term unemployment, time-varying labor market institutions

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* This paper was presented at the World Bank/IIASA/ Ludwig Boltzmann Institute for Economic Analysis *Workshop on Severance Pay Reform: Toward Unemployment Savings and Retirement Accounts*, Laxenberg/Vienna, November 7-8, 2003.

I. Introduction

Refocused by the work of Lazear (1990), analysis of the impact of job security provisions on labor market outcomes was among the most studied topics in labor economics during the decade of the 1990s¹ and, now extending beyond proximate causation, shows every sign of continuing to be a key research theme in the first decade of the new millennium (see, in particular, Botero *et al.*, 2003). Interest in employment protection remains keen because of continuing high unemployment and sluggish growth in much of Europe. But the economics profession has failed to provide consistent results on the consequences of employment protection, as is evidenced by the very pessimistic conclusions of, say, Heckman and Pagés (2000) on the one hand and the guarded optimism of the OECD (1999) on the other.

Although theory can provide the basis for different expectations regarding the effects of employment protection on labor market outcomes, data limitations would seem in this case to have played a more important role than usual in accounting for diversity of finding (on which, see Addison and Teixeira, 2003). The data problems are reflected in models that are parsimonious in both the range of explanatory variables deployed and in the time frame examined. To be sure, in the years following Lazear's pioneering analysis the data situation has improved in terms of refinements to the key independent variable and with the availability of information on new regressors. But data constraints have continued to cast a long shadow over the economic analysis of employment protection. In particular, the needs of wider country coverage and an extended time series have consequences for the number of explanatory variables than can be included in the empirical model. The tradeoffs that have to be made might be expected to encourage more humility on the part of investigators than is apparent in the literature.

The purpose of the present paper is twofold. First, we seek to document the problems arising from the prototypical parsimonious model, using Lazear's famous paper as an organizing device. In updating Addison, Teixeira, and Grosso (2000), we will further discuss the robustness of Lazear's major predictions as to the role of his preferred measure of job security (*viz.* the no-fault severance pay granted to a blue-collar worker with 10 years service, or *SEV*) on various employment indicators. Our sample covers an (extended) interval of more than four decades, namely, from 1956 to 1999. In a new

departure, we will also address the course of long-term unemployment – and drop the average hours worked measure used by Lazear – albeit for a much shorter period (1979-1999).

Our second goal is to discuss the sensitivity of the basic employment protection result to a different and more comprehensive measure of job protection (e.g. the OECD composite index of the severity or coerciveness of employment laws) and other labor market institutions. Replacement of the partial indicator *SEV* by some composite index capturing other seemingly important aspects of the job regulatory framework – such as a measure of procedural delays in dismissals and the permissibility of fixed-term contracts – is expected to improve the explanatory power of the model, although this advantage might be compromised or undercut by a shortened time series. For its part, the failure to include variables such as the degree of collective bargaining coverage or the degree of employee and employer coordination in wage bargaining might be a more serious source of bias in the estimated parameters than the use of such a partial indicator of employment protection.

As a practical matter, most measures of ‘labor market institutions’ are seldom available in a continuous form. Rather, they are one-off purpose-built constructs or at best only observed at a few points in time.² This reality leaves the researcher with two options: either assume these variables are roughly constant over time and run the model on fixed institutions (in which case the analysis can be extended to cover almost half a century), or instead assume time-varying institutions and trade a presumably more informative set of institutions off against a substantially smaller number of observations (via the reduction in the respective time series). To conduct our ‘robustness test’ of the parsimonious specification used by Lazear, therefore, we shall follow two routes. First, we use annual data in conjunction with fixed institutions throughout (i.e. from 1956 up to 1999). Second, and this is our preferred route, we average our annual data on (four) labor market performance indicators over 5-year periods and use time-varying institutions for which we have observations at different moments in time (at least three). The sample period in this case is necessarily shorter and covers the period 1970-99 (1979-99 in the case of the long-term unemployment equation).

Assuming fixed labor market institutions over a period of almost half a century – the first route – seems at first glance rather heroic,³ but the pooling of cross-section and time-series data offers an indication of the effect of time-varying severance pay over an extended period that is embedded within in a richer institutional context. The second route, by dropping the assumption of fixed institutions, reduces both the sample period and the number of institutions, but eliminates the need for interpolation in order to obtain (artificial) annual time-varying data. This approach, which also allows the researcher to focus on the long-term impact of policy intervention in labor markets, has found some favor in the literature (see Nickell, 1997; Blanchard and Wolfers, 2000; and Bertola, Blau, and Kahn, 2001). Note that our set of time-varying labor institutions and range of labor market performance indicators is wider than has been used in this literature.

II. Modeling and Data

Specification

The sample panel structure of our database allows for a wide range of sensitivity tests. In the most favorable case, we will be able to work with data on 21 OECD countries over 44 consecutive years.

We will begin with the standard Lazear specification containing country specific effects, which can be written:

$$y_{it} = c_i + \sum_j X_{ijt} b_j + e_{it}, \quad (1)$$

where y denotes the labor market outcome, X is the set of explanatory and control variables, and c captures the country specific effect. In the original model, the vector X included severance pay, the proportion of the population that is aged between 25 and 65, and the growth in GDP per capita. Lazear also adds a quadratic time trend. At this stage, the model assumes away any reverse causation or endogeneity of the explanatory variables – although Lazear (pp. 722-23) subsequently addresses the causality issue by, inter al., regressing changes in severance pay between t and $t+1$ on levels at time t of three of the four dependent variables. In our fitted regressions we will only address directly the problems arising from autocorrelation, where our first-pass solution will be to assume a (common) first-order serially correlated error term. Specifically, this approach

will be applied in generating Tables 2 through 4 which cover the sample period 1956-1999.⁴

Extending the vector X of explanatory variables in equation (1) to include measures of labor market institutions requires a slight change to the model specification and estimation procedures. In particular, since the inclusion of the additional regressors reduces substantially the length of the panel – especially if the model includes time-varying variables – GLS random effects estimates will be used rather than the standard fixed effects model. Within this framework we will also introduce time dummies to proxy unobserved cross-country (common) shocks. This approach is followed in Tables 5 through 7, and the general formulation can be described as follows:

$$y_{it} = c_i + d_t + \sum_j X_{ijt} b_j + e_{it}, \quad (2)$$

where t denotes the 5-year periods, d_t is the time effect for period t , and X now contains the institutional variables.

Our final model extension includes the interaction of labor market institutions and time (i.e. unobserved shocks). The interaction terms are intended to capture the ‘product’ of shocks and institutions, the presumption being that a particularly unfavourable labor market regulation will impact labor market performance more severely in bad times. In this case, we use a nonlinear specification of the following type:

$$y_{it} = c_i + d_t (1 + \sum_j X_{ijt} b_j) + e_{it}. \quad (3)$$

Our findings using this specification are contained in Table 8, with all variables being expressed in terms of deviations from the sample mean.⁵

Data

Our database contains observations on a maximum of 21 OECD countries: Austria, Australia, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Japan, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, the United States, Greece, and Israel. Information on the country sample, labor market outcomes, and explanatory variables for the period 1956-84 is provided in Addison, Teixeira, and Grosso (2000). For present purposes, we limit our comments to how we updated this information between 1985 and 1999.

The employment population ratio (*EMPPOP*), the unemployment rate (*UNRATE*), and the labor force participation rate (*LFPR*) were updated using the OECD publication Labor Market Statistics. The same source was used to compute the right hand side variable capturing the share of the population aged 25 to 65 years (*WRKAGE*) and the long-term unemployment rate (*LTUNRATE*) (in this case from 1979 to 1999). The growth in GDP per capita (*GROWTH*) was calculated from the International Financial Statistics Yearbook (2002).

We also updated the severance payment variable (i.e. the statutory entitlement in months of pay due to a blue-collar worker with 10 years of service at termination, separated for reasons unconnected with his/her behaviour), using the detailed information on dismissals procedures for 1992-99 contained in Bertola, Boeri, and Cazes (2000). This material covers only 11 OECD countries – Australia, Austria, Denmark, France, Germany, Ireland, Italy, New Zealand, Spain, the United Kingdom, and the United States. We therefore supplemented it with data for other countries from the OECD Employment Outlook (1999, Table 2.2) which documents the changes in severance pay for no-fault individual dismissals in the 1990s. Using this procedure, it was possible to code the severance payment variable after 1992 for all countries other than Greece and Israel.

(Table 1 near here)

To set our severance pay measure in wider relief, we also obtained data on the severance pay due to a 40 year-old white-collar employee made redundant after 10 years of service. These two indicators were then converted into rankings (in ascending order of stringency) and are reported in columns (1) and (2) of Table 1. Column (3) of this table also gives the country ranking order derived from Heckman and Pagé's (2000) cardinal measure of firing costs (which controls for the entire tenure-severance pay profile), while in columns (4) through (8) we introduce some other widely used indices of stringency of employment protection laws, including the employment protection index used by Nickell (1977). Finally, columns (9) and (10) of the table present the corresponding country rankings of more narrowly defined indices of industrial relations and social security laws, respectively.

As shown by the Spearman ranking correlation at the foot of Table 1, the three measures of severance pay in columns (1) to (3) are, as expected, highly correlated but the correlation of severance pay with the broad employment protection indicators in columns (4) through (8) offers a less consistent pattern. And the correlations between severance pay and the indicators of industrial relations and social security are extremely low. For their part, the broader indicators of the stringency of employment protection laws are strongly correlated: the correlation coefficients between the column (5) measure – OECD, late 1990s – and columns (4), (6), (7), and (8) are all statistically significant at the .01 level. There is, however, no correlation between the OECD measure and the index of social security reported by Botero et al. in column (10).

Finally, six labor market institutions are identified in the present treatment. These are the unemployment insurance replacement rate (*UIRR*); the maximum duration of unemployment benefits (*MDUB*); expenditure on active labor market policy (*ALMP*); union density (*UDEN*); collective bargaining coverage (*UCOV*); overall employee and employer coordination in wage bargaining (*TCOOR*); and the tax wedge (*TXWEDGE*). As mentioned above, since none of these series is available on a yearly basis, we constructed 5-year averages (1970-99) using the interpolations described in Appendix Table 1. Data sources and variable definitions are also included in this table, as well as Nickell's (1997) time-invariant employment protection index.

III. Findings

As noted earlier, Lazear's pioneering study acted as the catalyst for more intensive and systematic study of the effects of job security provisions on labor market performance. After more than a decade since its publication, the Lazear argument that severance pay reduces employment and elevates joblessness (in imperfectly competitive markets) not only remains a mainstay of orthodoxy but also continues to attract broad empirical support (see the survey by Addison and Teixeira, 2003). In what follows while we do not claim to detect any evidence suggesting pro-employment effects of stringent labor regulation, we will nonetheless contend that the more flamboyant empirical findings in the spirit of Lazear need to be interpreted with caution.

To begin with, we take Lazear's parsimonious model and re-estimate it using an additional 15 years of data. Next, in recognition that much data on labor market institutions has only become available in recent years, we test the robustness of the original model to the inclusion of an extended set of such explanatory variables, and in the process address some more contemporary issues. To repeat, in this stage of our empirical analysis we shall look at the effects of severance pay on unemployment and employment for a longer sample period than does Lazear (as previously noted, the exception is the long-term unemployment outcome measure that we substitute for Lazear's working time indicator and for which we have a shorter run of data), and in a framework that accommodates time-varying labor market institutions.

(Table 2 near here)

Table 2 shows the results of applying the most restrictive version of the Lazear model, namely, estimation of the effects of severance pay (inter al.) on labor market outcomes using pooled cross-section time-series data with no country dummies. As in Addison, Teixeira, and Grosso (2000, Table 2), the results of this specification are broadly supportive of Lazear's empirical proposition that job protection, proxied by the *SEV* variable, adversely impacts employment, labor force participation, and overall unemployment. (Also consistent with Lazear is the statistical insignificance of the *GROWTH.SEV* interaction and the well-determined effects of the population control *WRKAGE*.) Using a shorter time-series, it also appears that the association between *SEV* and long-term unemployment, *LTUNRATE*, is negative and well determined (column 3).

(Table 3 near here)

Since there no obvious reason to neglect national idiosyncracies, Table 3 shows the effect of introducing country fixed effects. Robustness is clearly an issue. The introduction of country dummies renders the coefficient estimates of *SEV* statistically insignificant in both the *EMPPOP* and *LTUNRATE* regressions. The association between *SEV* and *UNRATE* remains positive and well determined while there is a sign reversal in the case of *LFPR*. The absence of country fixed effects is statistically rejected in all regressions at the .01 level.

(Table 4 near here)

We also tested for the presence of autocorrelation. Table 4 gives the results of fitting the fixed effects model assuming a first-order autocorrelation term. It can be seen that the null of no serial (first-order) correlation is clearly rejected. As it is apparent, the re-estimation takes no prisoners: none of the coefficient estimates for *SEV* is any longer statistically significant at conventional levels.

At this point we are of course reminded of the parsimonious nature of the Lazear model. This is next issue to be tackled. But thus far at least we would conclude that the Lazear model has failed to pass muster. This conclusion is also reached by Addison, Teixeira, and Grosso (2000). The difference here is that we are updating the database with information for more recent years that, with the uptick of unemployment, might perhaps have been expected to offer a more promising milieu for the model.

The parsimony of Lazear's specification has been addressed in various ways in the subsequent literature. But one amendment has proved increasingly popular, namely, the class of models whose general specification is described in equation (2) above. Their distinctive feature is the introduction of time-varying measures of labor market institutions, on the one hand, and time dummies as proxies of unobservable shocks, on the other. One of the first authors to apply this specification was Nickell (1997) who combined two-time periods – 6-year averages of data for 1983-88 and 1989-94 – with a wide set of explanatory variables.

(Table 5 near here)

We begin with a quasi-replication of Nickell's (1997) approach in Table 5. In this exercise the left hand side variables are again extracted from our own database, whereas the right hand side variables are taken from Nickell. The surprising result is the statistical insignificance of most of the parameter estimates. But there is some support for Lazear's findings: the higher the *EPL* ranking (i.e. the more generous employment protection), the lower the employment population ratio and labor force participation. Moreover, the long-term unemployment rate – but not overall unemployment – is also impacted unfavourably by employment protection legislation. Appendix Table 2 reports a somewhat different exercise in which both the right-hand side and left-hand side variables are taken from our own database, with the exception of *ALMP* and *benefit duration*. There is obvious

corroboration of the findings in Table 5. Taken together, these results suggest that *quasi*-cross-section data (two data points spanning two decades) if they do not make a strong case for labor market institutions do offer a measure of support for Lazear.

Tables 6 and 7 show the more interesting case in which the number of data points has been enlarged. But this extension is not achieved without cost. Thus, in Table 6, we have a maximum of nine periods covering the entire sample period 1956-99 and seven *fixed* labor market institutional variables (the *replacement rate*, *benefit duration*, *ALMP*, *union density*, *union coverage*, *union and employer coordination*, and the *tax wedge*) plus the severance pay variable. In Table 7 the sample period is 1970-99, but we have a smaller number of labor market institutions which are now *time varying*. The Nickell study considered eight institutional variables of which one is time-invariant (*EPL*).

(Tables 6 and 7 near here)

Clearly, these innovations produce an improvement in the precision of the institutional variables in the case of *UNRATE* (in both tables). For the other regressions (*EMPPPOP*, *LTUNRATE*, and *LFPR*), the coefficient estimates are statistically significant in just 6 out of 24 cases in Table 6 and in only 3 out 18 cases in Table 7. But the *SEV* coefficient is now well determined only in 3 out of 8 regressions (taking Tables 6 and 7 together), while in Table 5 and the Appendix Table 2 it was well determined in 6 out of 8 cases.

We should also report the results from a different exercise using annual data (1956-99) in which we added seven fixed institutional variables (including the tax wedge) to the full set of original Lazear regressors.⁶ In this procedure 14 out of 32 (i.e. 8 x 4) coefficients estimates were found to be statistically significant, which is a slight improvement over Table 6, for example, where 11 such estimates were well determined. In particular, the *SEV* variable was positively signed and statistically significant in the *UNRATE* and *LTUNRATE* equations (albeit only at the .10 level in the latter). Bearing in mind the results from Table 4 above, it can be seen that the *SEV* coefficient estimate does show some sensitivity to the inclusion of labor market institutions. Based on the same type of augmented-Lazear specification we then made an attempt to determine the degree of sensitivity of the severance pay coefficient in Table 4 to the introduction of all possible combinations of institutional variables (viz. the seven fixed measures mentioned above).

From this exercise it emerged that the *SEV* coefficient estimate was never statistically significant in the *EMPPOP* regression, but was always positive and well determined in the in the *UNRATE* regression. The ‘addition’ of the institutional covariates to the *LTUNRATE* regression yielded a marginally statistically significant coefficient estimate for *SEV* in roughly 50 percent of the cases, while in the *LFPR* equation the estimate was statistically significant (although on this occasion at both the 0.05 and 0.10 levels) in approximately two-thirds of all cases. In sum, while the sensitivity of the *SEV* coefficient in the *EMPPOP* equation seems to be low, in the other three cases – *UNRATE*, *LFPR* and *LTUNRATE* – sensitivity to model specification cannot be ignored. Nonetheless, the consequences for standard Lazear equations of ignoring labor market institutions are arguably less severe than might be expected, although a more definite conclusion necessarily awaits the provision of better (i.e. annual) data on institutions.

We should also note that we experimented with alternative measures of employment protection legislation in substitution for *SEV* and Nickell’s (1997) *EPL* index. But the broad picture is unchanged: the role of institutions is less ‘active’ than one might expect. Our finding that institutions seem to be of greater importance in explaining overall unemployment than the other indicators is also worthy of note. To some degree, it parts company with the notion that the impact of labor regulations on unemployment is more ambiguous than its effects on employment.

These remarks bring us finally to model (3). In this model, it is hypothesized that labor institutions only reveal their true ‘color’ in conjunction with adverse economic conditions (e.g. negative shocks). Accordingly, if a given country is ‘endowed’ with a non-employment friendly set of labor laws, the unfavourable impact of the latter may not surface if that nation fails to experience hard times. The non-linear specification of equation (3) is particularly suited to address the interaction between (observed or unobserved) shocks and institutions.

In fitting this model to the data we again consider the sample period 1970-99 and the same 5-year averages as before. The set of time-varying institutions is also the same; in particular, we retain *SEV* variable as our indicator of employment protection. In other words, we are implementing here the ‘work-in-progress’ part of Blanchard and Wolfers’ (2002, p. C23) analysis, that is to say, a model in which all institutional regressors are

allowed to vary over time. We note that Blanchard and Wolfers attempted to run the model with time-varying institutions, but only in a limited way, using just the employment protection and unemployment insurance covariates. Blanchard and Wolfers also focus exclusively on the course of unemployment, and so do not consider the *LTUNRATE*, *LFPR*, and *EMPPPOP* outcome indicators considered here. (Bertola, Blau, and Kahn, 2001, likewise concentrate on unemployment developments.) Finally, observe that although data on observable shocks is available we restrict our attention to the case of unobservable shocks which we proxy with time dummies.

(Table 8 near here)

The results of this analysis are presented in Table 8. In only 7 out of 24 cases are the labor market institutional parameters estimated with precision. The *SEV* variable is statistically significant in just the *UNRATE* equation, while *UCOV* and *TCOOR* are never statistically significant. Most surprisingly, developments in long-term unemployment are almost solely explained by country and time effects, with no role reserved for labor market institutions. We note parenthetically that the *restricted* version of model (3) – that is, the model in which X_{ij} is time invariant – fully replicates Blanchard and Wolfers findings for unemployment (e.g. their Table 1), with all variables being identically signed and statistically significant (the active labor market policy and union density covariates are not statistically significant). Applying the same model to the *EMPPPOP*, *LFPR*, and *LTUNRATE* outcomes revealed approximately the same pattern as described in Table 8. In the case of the long-term unemployment regression, none of the coefficient estimates is statistically significant. What these results show is the seemingly inability of labor market institutions as a whole to materially impact labor market outcomes under the more realistic scenario of time-varying indicators. The *SEV* variable, with the exception of the unemployment case, does not seem to play any particularly prominent role either. The attenuated role of collective bargaining coordination is further weakened.

IV. Conclusions

The effects of job security provisions on job turnover (i.e. job creation and job destruction) and on unemployment flows are fairly well established. Net effects are less firmly established, despite widespread acceptance of the view that stronger employment

protection will entail lower employment and higher unemployment. In the present treatment we have offered a wide-ranging combination of empirical strategies in which the effects of institutions on labor market aggregates are analyzed across a variety of sample periods, explanatory variables, and estimation techniques.

Our starting point was the influential Lazear study of the role of severance pay in influencing employment and joblessness. By adding more regressors – specifically, labor market institutions – to the original Lazear model, we found little slippage of the unemployment result. Much weaker was the evidence linking severance pay to the rate of long-term unemployment and to the employment population ratio and the labor force participation rate. Surprisingly, in virtually all model specifications, and irrespective of the empirical strategy used, we found low statistical significance of the other institutional variables. Even the performance of the union and employer coordination variables, often viewed as favorable to labor market development, was not impressive overall.

We cannot of course conclude from the foregoing exercise that labor market institutions – and job protection in particular – do not matter. Rather, our findings indicate that we simply do not yet know enough about the role of such institutions, or, expressed differently, that the extent of their adverse impact on the labor market is not easily gauged. For instance, we cannot exclude the possibility that different combinations of labor institutions and regulations may produce quite similar outcomes. It may even be the case that the quest for improved labor market performance is better directed elsewhere, although we would resist this interpretation, arguing that lingering uncertainty as to the impact of the institutions identified here is an inescapable consequence of the vintage of research in this area.

Endnotes

1. For the flavor of the earlier literature, see Buechtemann (1993).
2. This raises the specter of research Darwinism, alluded to by Blanchard and Wolfers (2000, p. C22)
3. Assuming fixed labor market institutions over a period of almost half a century seems rather heroic, but with the exceptions of Portugal (in 1974) and Spain and Greece (1975 and 1973, respectively), it might be argued that none of the balance of our sample – apart from 1979-1990 Britain under Mrs. Thatcher – has experienced dramatic political changes.
4. Addison, Teixeira, and Grosso (2000) discuss the problems arising from a panel specification such as equation (1).
5. In this case, the difference between the coefficient estimate for the first time dummy and the last time dummy gives the change in y_{it} due to exogenous shocks (if $X_{it} = \bar{X}$, then $\hat{y}_{it} = \hat{c}_i + \hat{d}_t$). We do not discuss the case of observable shocks.
6. This exercise was carried out using the GLS random effects model to allow the presence of time-invariant regressors.

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Table 1: Severance Pay and Job Security Rankings by Selected OECD Countries (in inverse order of stringency)

Countries	(1) Severance pay	(2) Mercer Consulting	(3) Heckman and Pagés	(4) Nickell	(5) OECD	(6) OECD	(7) World Competitive Report	(8) Botero et al.	(9) Botero et al.	(10) Botero et al.
	1984	2003	Late 1990s	1985-94	1989	Late 1990s	1984-90	1997	1997	1997
Belgium	6	5.5	9	16	14.5	9.5	10	17	9.5	3
Denmark	6	5.5		4	6	7	2	4	9.5	20
France	12.5	11.5	6	13	12	16	9	14	18	15
Germany	6	5.5	5	14	18	14	11	13	15	5
Greece	12.5	5.5	11	17	16	19	19	18	11	14
Ireland	14	5.5		11	5	3.5	7	6	8	8
Italy	19	15		20	20	18	16	12	16	12.5
Netherlands	6	5.5		8	9	9.5	8	15	12	1
Portugal	20	13	13	18	19	20	18	20	20	9.5
Spain	18	16	12	19	17	17	20	19	17	16
United Kingdom	15	11.5	8	6	4	2	4	5	1	6.5
Austria	16	14	10	15	11	11.5	12	1	6	6.5
Finland	6	5.5		9	14.5	8	6	16	7	17
Norway	6		4	10	13	15	14	10	19	18
Sweden	6	5.5		12	10	11.5	15	7	13	19
Switzerland	6		7	5	3	5.5	3	9	5	12.5
United States	6	5.5	1	1	1	1	1	2.5	2	2
Japan	6	5.5		7	8	13	5	11	14	4
Australia	6		3	3	7	3.5	17	2.5	4	11
New Zealand	17		2	2	2	5.5	13	8	3	9.5
Spearman rank correlation coefficients: ^a										
	----	0.812***	0.573**	0.501**	0.288	0.366	0.481**	0.194	0.167	0.038
	0.366	0.467*	0.674**	0.821***	0.862***	----	0.665***	0.697***	0.876***	0.286

Sources: Entries in column (1) are taken from the present study. The remaining columns were derived from Mercer Human Resource Consulting (2003), Chart 4; Heckman and Pagés (2000), Table 1; Nickell (1997), Table 4; OECD (1994), Table 6.7, column (2); OECD (1999), Table 2.5, column (7); Di Tella and MacCulloch (1999), Table A, column (1); and Botero et al. (2003), Tables II and III (columns (8), (9), and (10) provide rankings of the stringency of employment, industrial relations, and social security laws, respectively).

* **, and *** denote statistical significance at the 0.1, 0.05, and 0.01 levels, respectively.

TABLE 2
Pooled Estimations - No Country Dummies (1956-99)

Independent variable	Dependent variable			
	<i>EMPPPOP</i>	<i>UNRATE</i>	<i>LTUNRATE</i>	<i>LFPR</i>
Intercept	-0.1305 (0.0393)	0.1129 (0.0276)	-0.1372 (0.3414)	-0.1080 (0.0349)
<i>SEV</i>	-0.0064 (0.0007)	0.0032 (0.0005)	0.0243 (0.0045)	-0.0055 (0.0007)
<i>GROWTH</i>	-0.1200 (0.0718)	-0.0024 (0.0504)	1.5883 (0.4882)	-0.1320 (0.0638)
<i>GROWTH.SEV</i>	0.0097 (0.0180)	-0.0005 (0.0126)	0.0445 (0.1471)	0.0118 (0.0160)
<i>WRKAGE</i>	0.8703 (0.0614)	-0.1582 (0.0431)	-1.2188 (0.3117)	0.8438 (0.0546)
N	833	832	348	833
F(k, N-(k+1))	72.5	83.9	23.7	108.1
R ²	0.35	0.38	0.29	0.44

Standard errors are given in parentheses.

Note: The regression includes *YEAR* and *YEAR*² terms. The *LTUNRATE* series only begins in 1979. N denotes the number of countries multiplied by the number of observations per country.

TABLE 3
Fixed Effects Regressions (1956-99)

Independent variable	Dependent variable			
	<i>EMPPOP</i>	<i>UNRATE</i>	<i>LTUNRATE</i>	<i>LFPR</i>
<i>SEV</i>	0.0005 (0.0005)	0.0017 (0.0005)	0.0084 (0.0071)	0.0011 (0.0005)
<i>GROWTH</i>	0.0089 (0.0368)	-0.0668 (0.0360)	1.5776 (0.2114)	-0.0237 (0.0327)
<i>GROWTH.SEV</i>	-0.0008 (0.0088)	0.0064 (0.0086)	0.0444 (0.0630)	0.0033 (0.0078)
<i>WRKAGE</i>	0.5724 (0.0422)	0.0875 (0.0412)	-0.2573 (0.2180)	0.6356 (0.0374)
N	833	832	348	833
F(k, N-(k+1))	67.3	121.1	44.0	199.9
R ²	0.83	0.65	0.23	0.83

Standard errors are given in parentheses.

Note: The regression includes a constant plus *YEAR* and *YEAR*² terms. The *LTUNRATE* series only begins in 1979. The null hypothesis that the country fixed effects are jointly equal to zero is rejected in all cases.

TABLE 4

Fixed Effects Regressions with Correction for Autocorrelation (1956-99)

Independent variable	Dependent variable			
	<i>EMPPOP</i>	<i>UNRATE</i>	<i>LTUNRATE</i>	<i>LFPR</i>
<i>SEV</i>	-0.00052 (0.00046)	0.00085 (0.00054)	0.0043 (0.0073)	-0.00022 (0.00040)
<i>GROWTH</i>	-0.00039 (0.01220)	-0.02576 (0.01461)	0.9240 (0.1391)	-0.01235 (0.01064)
<i>GROWTH.SEV</i>	0.00318 (0.00268)	0.00295 (0.00320)	0.1003 (0.0432)	0.00498 (0.00233)
<i>WRKAGE</i>	0.25701 (0.05260)	0.03743 (0.06009)	-0.0018 (0.2720)	0.30429 (0.04604)
N	812	811	329	812
F(k, N-(k+1))	10.7	10.1	13.4	20.5

Standard errors are given in parentheses.

Notes: The regression includes a constant plus *YEAR* and *YEAR*² terms. The *LTUNRATE* series only begins in 1979. The null hypothesis that the country fixed effects are jointly equal to zero is rejected in all cases. The null hypothesis that the error term is not first-order autoregressive is also rejected.

TABLE 5

Random Effects (GLS) Regressions with Eight Labor Market Institutions and Two Data Points
(6-year averages, 1983-88 and 1989-94)

Independent variable	Dependent variable			
	<i>EMPPOP</i>	<i>UNRATE</i>	<i>LTUNRATE</i>	<i>LFPR</i>
<i>Employment protection</i> (1-19)	-0.0055 (0.0025)	0.0015 (0.0020)	0.0216 (0.0063)	-0.0055 (0.0019)
<i>Replacement rate</i> (%)	0.0008 (0.0005)	-0.0002 (0.0004)	-0.0010 (0.0014)	0.0005 (0.0004)
<i>Benefit duration</i> (years)	-0.0070 (0.0059)	0.0051 (0.0054)	0.0264 (0.0176)	-0.0063 (0.0045)
<i>ALMP</i> (%)	-0.0003 (0.0002)	0.00004 (0.0002)	-0.0004 (0.0007)	-0.0002 (0.0002)
<i>Union density</i> (%)	0.0003 (0.0007)	-0.0001 (0.0005)	0.0008 (0.0017)	0.0003 (0.0005)
<i>Union coverage</i> (1-3)	-0.0144 (0.0204)	0.0287 (0.0194)	0.0549 (0.0580)	-0.0041 (0.0158)
<i>Union and employer coordination</i> (2-6)	0.0134 (0.0093)	-0.0153 (0.0081)	-0.0510 (0.0287)	0.0129 (0.0071)
<i>Tax wedge</i> (%)	-0.0001 (0.0012)	0.0004 (0.0009)	-0.0018 (0.0028)	-0.0002 (0.0009)
R ²	0.68	0.55	0.75	0.74
Wald χ^2	25.1	16.1	40.6	36.47
N	38	38	34	38

Standard errors are given in parentheses.

Notes: The model includes a constant term and a time dummy representing the 1989-94 period. All explanatory variables are taken from Nickell (1997) while the dependent variables are from our own dataset. The results are virtually unchanged when the dependent variables are expressed in logs.

TABLE 6

Random Effects (GLS) Regressions with Eight Labor Market Institutions and Nine Data Points
(5-year averages, 1956-99)

Independent variable	Dependent variable			
	<i>EMPPOP</i>	<i>UNRATE</i>	<i>LTUNRATE</i>	<i>LFPR</i>
<i>SEV</i> (months)	0.0004 (0.0013)	0.0017 (0.0010)	0.0046 (0.0110)	0.0010 (0.0012)
<i>Replacement rate</i> (%)	-0.0002 (0.0006)	0.0007 (0.0003)	0.0017 (0.0023)	0.00002 (0.00059)
<i>Benefit duration</i> (years)	0.0008 (0.0078)	-0.0019 (0.0040)	-0.0164 (0.0289)	0.0003 (0.0072)
<i>ALMP</i> (%)	-0.0012 (0.0011)	0.0008 (0.0006)	0.0031 (0.0038)	-0.0008 (0.0010)
<i>Union density</i> (%)	0.0002 (0.0007)	0.0004 (0.0004)	0.00002 (0.00243)	0.0004 (0.0007)
<i>Union coverage</i> (1-3)	-0.0557 (0.0220)	0.0264 (0.0114)	0.2543 (0.0842)	-0.0481 (0.0203)
<i>Union and employer coordination</i> (2-6)	-0.0301 (0.0120)	0.0246 (0.0061)	0.1017 (0.0420)	-0.0218 (0.0111)
<i>Tax wedge</i> (%)	-0.0016 (0.0011)	0.0013 (0.0005)	0.0033 (0.0037)	-0.0010 (0.0010)
R^2	0.55	0.68	0.68	0.56
Wald χ^2	54.3	267.2	176.2	155.3
N (countries, years)	162	162	71	162

Standard errors are given in parentheses.

Notes: The model includes a constant term and year dummies. ALMP and Union and employer coordination are set to negative. All right hand side variables are taken from Blanchard and Wolfers (2002) with the exception of the SEV variable. (See description in the Appendix Table 1.)

TABLE 7

Random Effects (GLS) Regressions with Six *Time-Varying* Labor Market Institutions and Six Data Points (5-year averages, 1970-99). (No interaction between institutions and unobservable shocks.)

Independent variable	Dependent variable			
	<i>EMPPOP</i>	<i>UNRATE</i>	<i>LTUNRATE</i>	<i>LFPR</i>
<i>SEV</i>	-0.0019 (0.0018)	0.0031 (0.0014)	0.0202 (0.0118)	-0.0007 (0.0014)
<i>Replacement rate (%)</i>	0.0003 (0.0004)	0.0003 (0.0003)	0.0007 (0.0021)	0.0005 (0.0003)
<i>Union density (%)</i>	0.0002 (0.0003)	-0.0004 (0.0002)	-0.0006 (0.0013)	-0.0001 (0.0003)
<i>Union coverage (1-3)</i>	-0.2430 (0.0132)	0.0157 (0.0085)	0.0675 (0.0493)	-0.0170 (0.0114)
<i>Union and employer coordination (1-3)</i>	-0.0171 (0.0124)	0.0137 (0.0079)	0.0090 (0.0424)	-0.0098 (0.0107)
<i>Tax wedge (%)</i>	-0.0004 (0.0007)	0.00006 (0.0005)	0.0011 (0.0028)	-0.0004 (0.0007)
R^2	0.31	0.55	0.50	0.19
Wald χ^2	22.7	88.0	119.9	92.6
N	92	92	62	92

Standard errors are given in parentheses.

Notes: The model includes a constant term and year dummies. Union and employer coordination are set to negative. All right hand side variables were extracted from our own database. (See description in Appendix Table 1.)

TABLE 8

Nonlinear Least Squares Regressions with Six Time-Varying Labor Market Institutions and Six Data Points, with Interaction between Institutions and Unobservable Shocks (5-year averages, 1970-99)

Independent variable	Dependent variable			
	<i>EMPPOP</i>	<i>UNRATE</i>	<i>LTUNRATE</i>	<i>LFPR</i>
<i>SEV</i> (months)	-0.00005 (0.0003)	0.1755 (0.0843)	0.0739 (0.1042)	0.00007 (0.0002)
<i>Replacement rate</i> (%)	0.00008 (0.00006)	0.0358 (0.0132)	-0.0313 (0.0195)	0.0009 (0.0004)
<i>Union density</i> (%)	-0.0002 (0.00006)	-0.0052 (0.0076)	-0.0211 (0.0134)	-0.0002 (0.00004)
<i>Union coverage</i> (1-3)	0.0003 (0.0026)	0.1651 (0.2372)	0.4720 (0.3995)	0.00007 (0.0019)
<i>Union and employer coordination</i> (1-3)	0.000004 (0.002)	0.0067 (0.2284)	-0.5170 (0.4062)	0.00037 (0.0017)
<i>Tax wedge</i> (%)	-0.0004 (0.0002)	-0.0052 (0.0138)	0.0283 (0.0252)	-0.0003 (0.0001)
R ²	0.90	0.83	0.94	0.93
F	22.3	11.0	21.9	32
N	92	92	62	92

Standard errors are given in parentheses.

Notes: The model specification is given in equation (3). All right hand side variables were extracted from our database. (See Appendix Table 1.)

APPENDIX TABLE 1

Variable/source		Definition/range	Raw year/ period	Interpolated periods
Employment protection (<i>EPL</i>)	Fixed measure (OECD, 1994, Table 6.7).	Ranking of employment protection legislation by “strictness”. It is an average ranking based on four different indicators. 1 denotes the lowest rigidity.	1985-93	1970-99, five- year periods.
	Time-varying (OECD, 1994, Table 8.B.1).	Summary measure of benefit entitlements on a gross basis.	1971 1981 1991	1970-74; 1975-79 1980-84; 1985-89 1990-94; 1995-99
Benefit duration (maximum duration of unemployment benefits) (<i>MDUB</i>).	Fixed measure (*) (Blanchard and Wolfers, 2000).	Share of past earnings replaced by unemployment benefits.	1983-88 and 1989-94	1970-99, five- year periods.
	Fixed measure (*) (Blanchard and Wolfers, 2000).	Duration of unemployment benefits (in years; 4 years denotes indefinite duration).	1983-88 and 1989-94	1970-99, five- year periods.
Active labor market policies (<i>ALMP</i>)	Fixed measure (*) (Blanchard and Wolfers, 2000).	Active labor market spending per unemployed individual as a percentage of GDP per member of the labor force.	1983-88 and 1989-94	1970-99, five- year periods.
	Time varying measure (OECD, 1997, Table 3.3).	Trade union density.	1970 1980 1990 1994	1970-74; 1975-79 1980-84; 1985-89 1990-94 1995-99
Union density (<i>UDEN</i>)	Fixed measure (*) (Blanchard and Wolfers, 2000).	Trade union density.	1983-88 and 1989-94	1970-99, five- year periods.
	Time varying measure (OECD, 1997, Table 3.3).	Share of workers actually covered by union bargaining: 1 denotes less than 25 percent; 2 means from 25 to 75 percent; and 3 means over 70 percent.	1980 1990 1994	1970-74; 1975-79; 1980-84; 1985-89 1990-94 1995-99
Union coverage (<i>UCOV</i>)	Fixed measure (*) (Blanchard and Wolfers, 2000).	Employer and union coordination in bargaining. It is assigned a value of 1 if there is no economy-wide coordination/centralization and 3 if the degree of coordination/centralization is very high.	1983-88 and 1989-94	1970-99, five- year periods.
	Time varying measure (OECD, 1997, Table 3.3).	Employer and employee coordination in bargaining. It is coded as between 1 and 6 (the sum of employer and employee coordination).	1980 1990 1994	1970-74; 1975-79; 1980-84; 1985-89 1990-94 1995-99
Union and employer coordination (<i>TCOOR</i>)	Fixed measure (*) (Blanchard and Wolfers, 2000).	Overall tax wedge (in percentage of average production worker earnings).	1983-88 and 1989-94	1970-99, five- year periods.
	Time-varying measure (OECD, 1997, Table 25).	Tax burden. It is measured as the sum of the average payroll, income and consumption tax rates.	1978 1985 1994	1970-74; 1975-79 1980-84; 1985-89 1990-94; 1995-99
Tax wedge (<i>TXWEDGE</i>)	Fixed measure (*) (Blanchard and Wolfers, 2000).		1983-88 and 1989-94	1970-99, five- year periods.

Notes: (*) The data on the fixed measures was generously made available by Prof. Olivier Blanchard at <http://www.mit.edu/blanchar/www.articles.html>). Blanchard and Wolfers (2000) took a simple average of Nickell's (1997) original data over two periods, 1983-88 and 1989-94.

APPENDIX TABLE 2

Random Effects (GLS) Regressions with Eight Labor Market Institutions and Two Data Points
(6-year averages, 1983-88 and 1989-94)

Independent variable	Dependent variable			
	<i>EMPPOP</i>	<i>UNRATE</i>	<i>LTUNRATE</i>	<i>LFPR</i>
<i>Severance pay</i> (months)	-0.0077 (0.0037)	0.0044 (0.0031)	0.0442 (0.0140)	-0.0076 (0.0033)
<i>Replacement rate</i> (%)	-0.0010 (0.0010)	0.0009 (0.0008)	0.0050 (0.0037)	-0.0008 (0.0009)
<i>Benefit duration</i> (years)	-0.0008 (0.0075)	0.0022 (0.0067)	0.0323 (0.0200)	-0.0020 (0.0060)
<i>ALMP</i> (%)	-0.0004 (0.0003)	0.0001 (0.0003)	0.0004 (0.0007)	-0.0004 (0.0002)
<i>Union density</i> (%)	0.0015 (0.0005)	-0.0007 (0.0004)	0.0001 (0.0017)	0.0011 (0.0004)
<i>Union coverage</i> (1-3)	-0.0008 (0.0007)	0.0006 (0.0006)	-0.0002 (0.0020)	-0.0004 (0.0006)
<i>Union and employer coordination</i> (2-6)	0.0150 (0.0181)	-0.0200 (0.0156)	0.0683 (0.0630)	0.0017 (0.0156)
<i>Tax wedge</i> (%)	0.0000 (0.0011)	-0.0001 (0.0009)	-0.0022 (0.0035)	-0.0001 (0.0009)
R ²	0.69	0.53	0.42	0.63
Wald χ^2	22.8	12.9	15.8	18.8
N	32	32	32	32

Standard errors are given in parentheses.

Notes: The model includes a constant term and a time dummy representing the 1989-94 period. All right hand side variables except ALMP and benefit duration are extracted from our database. (See Appendix Table 1.)

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