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The Effect of Dismissals Protection on Employment: More on a Vexed Theme

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Abstract

This paper presents new results on the relationship between severance pay and labor market performance for a sample of 21 OECD countries, 1956-84. Specifically, it evaluates Lazear's (1990) empirical argument that severance pay reduces employment and elevates joblessness. His findings are shown not to survive correction for errors in the data and the application of correct estimation procedures. Furthermore, adverse labor market consequences of severance pay are not detected in a dynamic characterization of the Lazear model. Limitations of the approach followed here - the focus on a single measure of employment protection and the parsimonious nature of the reduced form model - are also addressed and contextualized.

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1. Introduction

Concern over the adverse employment consequences of employment protection legislation is a recurring theme in labor market analysis. Recent, and conflicting, applied treatments include Scarpetta (1996) and Nickell (1997). The unfolding empirical analysis of the effects of employment protection shares certain similarities with investigation of the covariation of collective bargaining structures and macroeconomic outcomes (see, for example, Calmfors and Driffill 1988; OECD 1997). In both cases, the models are typically reduced form and the empirical evidence rather mixed.

The present paper offers a replication and critique of Lazear's (1990) famous empirical model of employment protection, which is the sole extant empirical treatment to allow for changes in a measure of employment protection over an extended time interval. Our concern is not so much with theoretical issues as with the sensitivity of Lazear's results to data problems. Suffice it to say here that the theoretical analysis of the long-run effects of employment protection produces ambiguous results (e.g. Bentolila and Bertola 1990; Bertola 1991; Bentolila and Saint-Paul 1994; Hopenhayn and Rogerson 1993; Saint-Paul 1995). Accordingly, there is a premium on empirical analysis.[1]

It should perhaps come as no surprise to learn that Lazear did not deny that employment protection could be benign - in perfectly functioning markets the parties would efficiently negotiate around a severance pay mandate via appropriate side payments from worker to firm - or argue that outcomes were independent of the stage of the cycle (see Hamermesh, 1993), or for that matter assert that negative effects would be observed across all outcome indicators (adverse effects being more clear-cut for employment than for unemployment, where discouragement could even generate a reduction in unemployment). Rather, his position was that in the presence of constraints on efficient contracts employment protection rules might be expected to bind in regular (and especially European) markets. His tests were designed accordingly.

Our criticism of Lazear is that, quite apart from errors in his data, he did not adequately investigate the statistical problems stemming from his use of pooled cross-sectional and time-series data, even if recognition of the problems emphasized in the present treatment is apparent in his narrative. We refer in particular to the problems of country heterogeneity and autocorrelation. We shall report that almost all of the statistical significance attaching to Lazear's key employment protection measure evaporates once the appropriate econometric procedures are employed. This is not the end of the story, however, because of the limitations of the employment protection variable and the parsimonious nature of the estimating equations used here. Both issues will be addressed in the context of the developing employment protection research literature.

2. Data and Methodology

The data and variables used in this inquiry in principle follow those of Lazear, to whom we are indebted for supplying us with a diskette containing the raw data (and the programs) used in his study. The data have been corrected for the errors of omission and commission identified by Addison and Grosso (1996). The Data Appendix illustrates the main issues. We note that the data errors do not overturn Lazear's principal findings, taken at face value, even if they do serve substantially to alter the point estimates.

In estimating the employment effects of statutory job protection, Lazear initially uses data on 20 OECD countries for the sample period 1956-84. His data are not complete for all variables and years. Lazear actually collected data on 22 countries but subsequently dropped two nations (Canada and Hong Kong). Our sample of countries and time frame is the same as that of Lazear, with the exceptions noted in the Data Appendix. Suffice it to say that sample differences were not material to any of the results reported below.

Lazear examines the determinants of four outcome indicators: the employment-population ratio (EMPPOP), the unemployment rate (UNRATE), the labor force participation rate (LFPR), and the average hours worked by production workers (HOURS). Values of each of the first three dependent variables differ slightly from those used by Lazear because we were able to obtain updated estimates of the size of the population, civilian labor force, and employment used in their construction. Rather more important changes were introduced into the HOURS variable; chiefly because of the need to provide a consistent time series, and also to substitute a correct measure of weekly hours for Italy to replace the erroneous daily hours measure inadvertently used by Lazear (1990, Table 1, column 3).

Turning to the independent variables, the crucial employment protection measure is severance pay (SEV). This is defined as the statutory entitlement in months of pay due to a blue-collar worker with ten years of service on termination for reasons unconnected with his/her behavior. The measure thus pertains to no-fault individual dismissals for economic reasons. Comment on the efficacy of this measure is provided below.

The remaining covariates are a quadratic time trend, represented by YEAR and YEAR^2; a demographic control for the population of working age (WRKAGE);[2] and the growth in per capita gross domestic product (GROWTH), to accommodate the notion that a growing economy vitiates at least in part the probabilistic costs of a severance pay mandate (see Gavin 1996). Unusually, in his fitted regressions Lazear only enters the latter variable in interaction with severance pay. As is more conventional, we instead include both GROWTH and its interaction with severance pay (GROWTH.SEV).

The majority of Lazear's estimates are from outcome equations that include just the time trend variables TIME and TIME^2 and the key dismissals protection indicator. This has perhaps served to amplify the principal criticisms of the model, namely, that it abstracts from many variables that may be expected to affect structural unemployment and employment rates, while its key independent variable, severance pay, is at best a partial indicator of the legal regulations applying in a particular country. (There is also the neglected issue of other constraints operating through the collective bargaining system.) But we note parenthetically that Lazear (1990, p.720) does at least use his expanded set of variables to explain changes in unemployment rates, in a specification that combines cross-section and time-series variation. He concludes that in some countries more generous severance pay "can go a long way in explaining the changes in employment over time" (Lazear 1990, p.720). Singled out as cases in point are France, Portugal, Italy, and Israel.

Although our interest is primarily in re-assessing Lazear's model per se, both criticisms have to be addressed at this point. Consider first the dismissals protection variable. Recent work in the employment protection area has sought to widen the definition of dismissals protection. Perhaps the best-known measure has been constructed by Grubb and Wells (1993), who identify three elements of a "system" of employment protection: restrictions on dismissals; restrictions on temporary forms of employment contract (so-called "atypical work"); and restrictions on working hours.

The first element covers not only severance pay, as in Lazear, but also procedural delays and unfair dismissal provisions. (Note, however, that there is no recognition of regulations concerning collective dismissals.)[3] The second element encompasses restrictions on the use of fixed-term contracts and temporary agency work, such as the permissible grounds for their use, the maximum number of successive contracts, and their maximum cumulated duration. Finally, hours restrictions cover such things as the length of the normal working week, annual overtime limits, minimum rest periods, and restrictions on night work.

Using simple unweighted averages of rankings, Grubb and Wells (1993, 24) provide summary indexes for each component, together with a grand ranking for the overall strictness of the regulatory climate which is reproduced in column 3 of Table 1. An analogous procedure is employed by the OECD (1994, p.74) to (average) rank countries by the severity of their legal restrictions on regular and atypical work, the results of which are reported in column 4 of the table. In each case, the ranking is from least to most regulated, and the data describe the "situation in the late 1980s." We note that the OECD ranking in column 4 has commonly been used in most of the recent employment protection studies (see below).[4]

[Table 1 near here]

Given that legislative rules may be only part of the story, other researchers have exploited less ambitious but potentially more encompassing "reputation" indexes based on surveys of employers. One such index is provided in column 5 of the table. It uses data from a survey conducted by the International Organization of Employers (1985), as distributed to European and non-European employer federations. The survey seeks to identify the importance of obstacles to the termination of regular employment and the deployment of atypical workers. The entries in column 5 are taken from the OECD (1994, p.74) and measure the reported levels of difficulty on an ascending scale of 0-3 (regulatory constraints were classified as insignificant, minor, serious, or fundamental), averaged over regular and fixed-term contracts.

Two other employment protection indexes derived from employer surveys, have also been used in the literature. Column 6 of Table 1 provides results from an ad hoc survey of some 8,000 industrial firms conducted by the European Community (Commission 1986) at the end of 1985. The survey inquires of the management respondents which of a number of reasons explained their not employing more people at the time of the survey, and asks them to indicate whether each one was "very important", "important," or "not important." One such reason is "insufficient flexibility in shedding labor." The entries in column 6 of the table are obtained by assigning a value of 2 (1) to the percentages of firms responding that this reason was "very important" ("important"). Clearly, other weighting procedures can be used (see, for example, OECD 1994, 73).

A broader-based survey of employers, the *World Competitiveness Report* (WRC), has recently been used to provide another measure of the stringency of cross-country employment regulations.[5] Di Tella and MacCulloch (1999) exploit one question in the WCR survey to obtain an indicator of labor market flexibility. The survey question asks respondents to rate the “flexibility of enterprises to adjust job security and compensation standards to economic realities” on a scale of 0 to 100, where 0 indicates “none at all” and 100 “a great deal.” Column 7 of Table 1 provides values of this flexibility index, averaged over 1984-90 (Di Tella and MacCulloch 1999, Table A).

Yet other researchers have offered more impressionistic indexes of the severity of the regulatory regime. One such index, used by Bertola (1990) and based on an (unstated) mix of employer perceptions and legal rules, is given for completeness in column (2) of Table 1.

Finally, severance pay entitlements as of 1984 for our own sample of 21 countries are given in the first column of Table 1. If these are then ranked in ascending order of generosity, we can compare the relation between our "index" (i.e. the Lazear measure) and that of the other studies - at least for the 11 countries common to each, using OECD (1994, p.74) interpolations for the odd missing values. The rankings evidently display some diversity, our measure of the stringency of employment protection correlating most closely with that of Bertola (1990).

This diversity is inevitable. As Grubb and Wells (1993, p.33) caution, "there is no simple and objective way of defining" an overall index of the severity of the regulatory climate. Their own index fails to consider non-legal constraints (e.g. union restrictions) or, indeed, issues of legal interpretation. Moreover, there is the inevitable problem of additional subjectivity introduced by any weighting scheme - implicit in their case. In the circumstances, might not rankings derived from employer surveys better capture "the many dimensions that such institutional arrangements associated with employment protection laws encompass" (Di Tella and MacCulloch 1999, p.8)? The fact that some employer surveys are available in a time series form might appear an added bonus in this regard. Unfortunately, apart from the issue of their representativeness (the firm heterogeneity point), there is the problem of precision of response. Employers may have difficulty in interpreting questions as to the flexibility or otherwise of the regulatory climate. As a case in point, Addison and Siebert (1999, p.56) have noted that there is little correspondence in ranking as between two subsequent ad hoc employer conducted by the Commission in 1989 and 1994. Major changes in ranking do not seem to correspond to observed changes in national employment protection laws or their application. In addition, employer responses are likely to be mediated by the cycle and it is not clear that this relationship has been convincingly sterilized in extant treatments (e.g. Morgan 1998). Consistency problems may thus severely reduce the usefulness of employer surveys as a partial solution to the moment-in-time limitation of more comprehensive OECD-type indexes of employment protection.

This brings us to post-Lazear studies that not only use a more comprehensive measure of employment protection than severance pay but also a richer array of controls. The best-known treatments are by Nickell (1997) and Scarpetta (1996), who use moment-in-time estimates of employment protection taken from the OECD (1994) Jobs Study. The studies cover almost identical time periods - 1983-94 and 1983-93, respectively - and each employs a comprehensive set of rather similar independent variables. The latter include unemployment benefits, expenditures on active labor market policies, measures of the tax wedge, union density, and the degree of coordination of collective bargaining. The sample of countries is 20 for Nickell and 17 for Scarpetta.

Nickell's dependent variables are short- and long-term unemployment and the employment-population ratio. Estimation is via GLS random effects using averaged data for two subperiods, 1983-88 and 1989-94. He reports that employment protection effects are largely statistically insignificant. Scarpetta focuses on unemployment: overall unemployment, youth unemployment, long-term unemployment, and non-employment. Unlike Nickell, he uses time-series variation in his key variables, other than the employment protection argument. That said, much of this variation is obtained by extending single data point observations. He concludes to the contrary that employment protection causes unemployment, particularly among youths. A harsh reading of both studies would be that the use of a more extensive set of controls is achieved at the cost of no small imprecision of the explanatory variables. Although the lack of variation in the employment protection variable may be less of a problem given the short time frame of the studies, the different conclusions reached as to its impact are disturbing.

Disquiet over this conflicting evidence has generated a wave of new studies. Of these perhaps the most interesting is by Di Tella and MacCulloch (1999), who combine a Lazear-type treatment with a new time series on employment protection derived from the WCR, discussed earlier. The dependent variables follow Lazear, the main difference being the inclusion of a variable proxying the generosity of national unemployment benefits, using the OECD (1994) summary measure of the unemployment insurance system. Using data on 20 countries, 1984-90, Di Tella and MacCulloch report robust, positive associations between flexibility and employment and the labor force participation rate. Less clear-cut, however, is the role of labor market flexibility in reducing unemployment.[6]

Supportive evidence as to the costs of employment protection has also been reported by Garibaldi and Mauro (1999). Following the Nickell procedure of averaging several years of data - over the interval 1980-1998 - the authors examine the association between the growth in total civilian employment and employment protection legislation (using the OECD index) for a 21-country sample. Their controls include the unemployment insurance net replacement ratio, union density, the extent of bargaining coordination, overall taxes, payroll taxes, population growth, and the average change in inflation (to proxy the business cycle and the macro policy regime). They report strongly negative associations between the degree of employment protection and employment growth in cross section. That said, the coefficient estimates of the employment protection variable are imprecisely estimated in panel regressions, which result they attribute to the time-invariant nature of the variable.

Further support for the Lazear argument is provided by Elmeskov, Martin, and Scarpetta (1998), in what is largely an extension of Scarpetta's (1996) analysis. Apart from including more countries and covering a modestly longer time interval, the principal innovation of this follow-up study resides in its use of a second data point for the OECD employment protection index (see below). The dependent variable is structural unemployment, as measured by the predicted nonaccelerating wage inflation rate of unemployment. Broadly speaking, the results reported by the authors suggest that structural unemployment is elevated in more generous employment protection regimes. That said, they also argue that the effects of employment protection, inter al., are mediated by the collective bargaining environment. Specifically, its adverse consequences are reduced in countries characterized by either centralized and coordinated or decentralized collective bargaining than in nations where sectoral wage bargaining predominates with limited coordination.[7]

Finally, a new study by the OECD (1999), which provides updated and revised information on its comprehensive index of job protection, has created considerable controversy precisely because of its finding of insignificant associations between that index and either unemployment or employment (Financial Times 1999). These results are obtained using two-period panel regressions in the manner of Nickell (1997) that employ the now familiar battery of controls. And they hold for disaggregations of the employment protection measure and for the main demographic components of the two outcome measures.

Against this backdrop, the Lazear approach has advantages and disadvantages. The advantage is the use of a long time series on dismissals protection that is consistent across time and requires relatively few judgment calls on the part of the investigator. The disadvantages are twofold. First, that indicator can offer only a partial view of the regulatory apparatus. Second, there is the separate issue of an omitted variables problem. Given our 1956-84 sample period, we were unable to construct a long enough time series on variables such as union density and aspects of unemployment insurance system emphasized in more recent studies. To this extent, our point estimates of the effect of dismissals protection could well be biased, although we have indicated that we are not reassured by extant treatments that have deployed a richer mix of covariates. In evaluating the Lazear model, however, the prime initial consideration must be one of replication, using appropriate econometric techniques. Nevertheless, despite the problems of assembling the relevant time series, future work using more contemporary data must seek to include a wider array of independent variables than considered here. Yet we have also indicated that the construction of a time series on employment protection, the essential contribution of Lazear, remains central. Absent this, discussion over the disemployment effects of employment protection will continue to be mired in controversy.

3. Econometric Issues

The use of a panel of data to study the effects of employment protection on labor market performance seems to be a useful approach because it combines cross-country and within-country variation. Although a simple cross-section data set is able to provide enough variation in both the dependent and independent variables of the model, it necessarily fails to capture the dynamic effects of severance pay entitlements on labor market outcomes. For its part, a single-country time series would probably not produce statistically significant coefficient estimates for the key dismissals protection argument because changes in employment protection are fairly sporadic even over more than two decades.

However, if panel estimation has the strength (and richness) of combining cross-section and time-series information, it can also reflect the problems specific to each. Nations have their own characteristics - in particular, labor market institutions differ widely across countries - so that no single model can be expected to explain the behavior of a given set of outcome variables. In this case, the pooled OLS regression (ultimately favored by Lazear) should be avoided because it will produce biased estimates. In these circumstances, it is conventional to deploy a model with fixed or random effects. Both approaches assume a common slope and different intercepts; intercepts that in the random effects specification shift around an estimated mean in accordance with an individual error component specific to each cross-section unit and an error component associated with time, rather than being assumed to be fixed for each unit.

A second problem stems from the time series nature of the data. We refer to the likely presence of serially correlated errors. If present, the latter have implications for the efficiency of the estimation and may lead to erroneous statistical inference. In these circumstances, transformation of the data is necessary so as to satisfy the standard assumptions of zero covariance between residuals through time.

To anticipate our findings, we do indeed detect the presence of highly correlated residuals in each of the cross-section units in both the fixed effects and random effects models run on the untransformed data. The individual autocorrelation coefficients are highly significant and large in absolute magnitude; a result that may of course flag poor specification. Abstracting from the latter issue, we address the autocorrelation problem by: (a) running a fixed effects model on the pooled data; (b) computing the autocorrelation coefficient, $\hat{r}$; and (c) re-estimating the model (fixed or random effects) on the transformed data, where the transformed variables are given by $y^*_{it} = y_{it} - \hat{r}y_{it-1}, x^*_{it} = x_{it} - \hat{r}x_{it-1}$. This is the asymptotic efficient estimator noted by Hsiao (1986, p.56), who recommends finding a consistent estimate of $\hat{r}$ from a first-stage fixed effects model and then applying the covariance method to the transformed data. A standard LM test was duly performed on the estimated residuals to check whether autocorrelation was removed.

In addition, we employ a feasible GLS procedure that in a first stage - to control for serial correlation - transforms the data using the estimated $\hat{r}$ obtained from OLS (or fixed effects) on the untransformed data, and then applies the GLS estimator, assuming that the residuals are both cross sectionally correlated and heteroskedastic (Kmenta 1997, Ch. 12). This estimation applies a SUR technique (Parks estimator) and is implemented using the software EViews.

Two remaining issues are tackled prior to a final substantive exercise that evaluates the properties of the Lazear model in the specific context of dynamic panel estimation (Arellano and Bond 1991). First, we informally address the potential contribution of influential observations to our central and essentially negative results. Second, given the sporadic nature of changes in the key severance pay argument, which may not therefore have immediate effects on labor markets, we investigate the sensitivity of our results to the use of a smoothed severance pay variable obtained though a Hodrick-Prescott filter.

4. Findings

Table 2 contains results of fitting the Lazear model to the pooled cross-section time-series data without country-specific effects. The regressions are the exact counterpart of those in Lazear (1990, Table VII). As in Lazear, the severance pay argument is statistically significant in the first three regressions. The odd man out is the hours worked equation, where the coefficient estimate of SEV is positive and insignificant at conventional levels. We believe the latter result has more to do with the inaccuracy of Lazear's hours data than anything else.

[Table 2 near here]

The positive association between severance pay and unemployment and the negative relationship between severance pay and employment and labor force participation seem to illustrate the adverse labor market consequences of more generous severance pay regimes. Also consonant with Lazear, the GROWTH.SEV (and GROWTH) covariate is statistically insignificant and the population control, WRKAGE, is strongly significant. It seems therefore that, although our data differ in many respects from those of Lazear, there is broad confirmation of his preferred specification.

When country dummies are included in the regressions, and again abstracting from the hours result, there is again reasonable correspondence with Lazear. The findings in Table 3 are for a fuller specification than in Lazear, who provides fixed-effects results for specifications containing only severance pay and the year variables (see his Table V). The most important (common) result is the positive and statistically significant coefficient of SEV in the unemployment equation. (As in Lazear, SEV is negative and insignificant in the employment equation and positive and insignificant in the labor force participation equation.)

[Table 3 near here]

As we have argued, however, the real issue is whether or not these results can be used to make predictions about the role of severance pay in generating, say, unemployment (see Lazear's Table VIII). The pooled OLS results evidently produce biased estimates, while the fixed effects specification, although controlling for country heterogeneity, may yield misleading statistical inference if there is serial correlation in the error structure. From the LM test statistics in Table 3, it can be seen that the null of no autocorrelation can be rejected. We note parenthetically that implementation of a nonlinear procedure that simultaneously estimates the parameters of the model and the autocorrelation coefficient confirmed that the latter coefficient was indeed highly significant.

There can be no question that Lazear (1990, 716-717) is aware of the problem, but in our view he does not adequately tackle the data problems. Rather, he elects to tackle one problem at a time; that is, he compares (a) the pooled OLS with the fixed effects results, and then (b) the OLS and the random effects model (REM) specifications, before (c) finally addressing the autocorrelation issue.

The point is that in (b) the error component associated with time is not present in his error components specification, only the cross-country effect; while in (c) country fixed effects are ignored. In any event, we note that the REM specification assumes that the autocorrelation of the residuals remains constant irrespective of the time distance between them, while the commonly assumed first-order autocorrelation implies that autocorrelation declines over time.

And, according to our tests, application of random effects to the untransformed data did not remove autocorrelation.

[Table 4 near here]

Given the clear autoregressive pattern of the residuals, the data have to be differenced according to the estimated autocorrelation coefficients. After this transformation, we obtain the fixed effects results given in Table 4. (Results for the alternative random effects specification are similar, and are provided in Appendix Table 1.) The consequences of controlling for the autocorrelation present in the time series are quite dramatic: the effects of SEV are now statistically insignificant throughout. At this stage, then, there is no statistical corroboration of the claim that tougher dismissals protection leads to more unfavorable labor market outcomes.

It is worth pointing out that once we difference the data and re-run the model, the problems of autocorrelation do appear to be solved if we use a REM rather than a fixed-effects specification. The results are provided in Appendix Table 1, where it can be seen that the LM tests on the residuals strongly reject the presence of first order autoregressive errors, with the possible exception of the employment equation. This is because the random effects model additionally corrects for the remaining post-transformation nonzero covariance between residuals over time.

Inspection of the variance of the residuals on each pooled unit in Table 4 (and Appendix Table 1) fails to indicate the presence of heteroskedasticity. As an additional check on the homoskedaticity of the residuals, we applied a weighted least squares regression procedure in which the data was transformed according to the residual variance for each country (see Kmenta 1997, Ch. 12). The results, which are available from the authors on request, do suggest that, after controlling for autocorrelation and introducing country-specific effects, it is indeed correct to assume an homoskedastic error structure.

Another concern is the possible presence of cross-sectional correlation. Here the issue is whether or not the economic interdependence between the countries in the sample influences our results. After controlling for autoregressive errors and cross-section heterogeneity, we applied the SUR weighted least squares option available in EViews. (The results are given in

Appendix Table 2.) Compared with the REM estimation, for example, the parameters generally maintained their signs and magnitudes, and we observe a slight increase in the statistical significance of the GROWTH and GROWTH.SEV point estimates. Again, this conformity is to be expected because the REM model assumes a nonzero correlation coefficient between the residuals of different cross-section units at a given point in time.

It is also worth observing that our results do not appear to be driven by outliers. The unemployment regression is the most susceptible in this regard, since cross-country differences in unemployment are quite large and because unemployment development varies across countries. Visual inspection of our data indicates that Spain and Israel are the most likely outlier candidates. Unemployment in Spain increased from 5.2 percent in 1979 (the first observation for this country) to 20.3 percent in 1984. For its part, Israel shows some sharp movements in unemployment in 1967 and again in 1975. Deleting the two countries from the sample produced virtually no change in the statistical significance of the covariates reported earlier. This is a rather informal statement of the issue. We did not attempt a formal sensitivity analysis for reasons of tractability. First, it is a nontrivial exercise to interpret potential outsiders in a very long panel of data of this type. Second, and more important, the conventional methods used to detect leverage points and influential data are derived assuming a standard linear model (e.g. Belsley, Khu, and Welsch 1980). There is no guarantee that the usual diagnostic checking procedures can easily be applied under the very different estimation methods used here.

[Table 5 near here]

Because regulations on employment protection change only from time to time, a further issue is whether it is appropriate to impose an immediate reaction of employment and unemployment to changes in severance pay. To investigate this issue, we applied a Hodrick-Prescott filter to the severance pay data. As can be seen from Table 5, smoothing the employment protection variable produced only a slight increase in the precision of the point estimates of SEV, none of which achieved statistically significance at conventional levels.

The strong residual autocorrelation detected in the untransformed data points to persistence in the outcome indicators. As a final exercise, therefore, it seems sensible to look in detail at the dynamic properties of the model. Our approach uses the Generalized Method of Moments (GMM) estimator developed by Arellano and Bond (1991); a methodology that extends the first difference instrumental variables method suggested by Anderson and Hsiao (1981) to dynamic fixed effects models. (Fixed effects estimation of an autoregressive model produces biased estimates.) This technique yields asymptotic standard errors that are robust to general cross-section and time-series heteroskedasticity under the null hypothesis of no serial correlation in the errors. To test this hypothesis, Arellano and Bond developed a first- and second-order serial correlation test statistic based on the GMM residuals.

[Table 6 near here]

Results of fitting this dynamic version of the Lazear model are given in Table 6. (The estimation was implemented using the Dynamic Panel Data (DPD) software made available by Dr. Jurgen Doornik of the Oxford University Institute of Economics and Statistics.)[8] The most important result is the failure to observe statistically significant effects of SEV on the key outcome indicators EMPPOP and UNRATE. That said, there is some indication that severance pay has an impact on the remaining outcome indicators, even if the LFPR effect is perverse from a conventional (and Lazear) perspective. In all cases, the coefficient estimates of the autoregressive terms are highly significant. The use of OLS methods can be seen to produce upwardly biased coefficient estimates and understated standard errors. The GROWTH variable also seems to become more important in explaining labor market outcomes than was previously the case, while the WRKAGE covariate evidently loses some of its explanatory power. All the regression statistics perform as expected. Thus, the Wald statistic is very high and the hypothesis that there is no second-order serial correlation in the first difference residuals, given by the m_2 statistic, cannot be rejected. In sum, our dynamic representation of the Lazear model casts further doubt on the argument that severance pay has adverse consequences for employment and unemployment development.

5. Concluding Remarks

This paper has reestimated Lazear's influential empirical model of the effects of dismissals protection on employment and unemployment, using corrected data for all variables and taking account of econometric problems associated with cross-country heterogeneity and serial correlation in the time series for each country. The upshot was that the adverse labor market consequences of more generous severance pay detected by Lazear were not confirmed in either static or dynamic representations of his model.

These results do not of course imply that the effects of dismissals laws, or employment protection legislation more generally, are benign. They pertain solely to the effects of severance pay in the framework of Lazear's parsimonious estimating equation. The use of severance pay to characterize the entire regulatory apparatus of dismissals protection is clearly an oversimplification and possibly a poor proxy for the stringency of that dismissals protection. Clearly, any two countries with the same recorded severance pay entitlements will inevitably differ in other aspects of dismissals protection. Similarly, the sparse formal representation of employment/unemployment determination in the model raises a potentially severe omitted variables problem.

To be sure, recent studies that include a more encompassing measure of dismissals protection and a wider array of control variables have yielded some support for Lazear's empirical conjectures, if not his methodology. More stringent employment protection has thus been linked to elevated unemployment, lower employment/labor force participation, and reduced employment growth. Absence of a suitable time series on variables used in such post-Lazear studies, however, precluded our testing the adequacy of the severance pay argument or the importance of the omitted variables problem. We have also noted the problems attaching to alternative employment protection measures and the dubious pedigree of some explanatory variables popular in the new literature. Moreover, the evidence on adverse labor market consequences of employment protection is not overwhelming. For example, studies using updated measures of the OECD index that provide two data points for this comprehensive measure of employment protection either fail to detect adverse effects on the employment and unemployment aggregates in two-period panel regressions (and first differences), or seem to downplay negative effects, where these are observed, by emphasizing interactions between employment protection and the collective bargaining and tax regimes.

Just as with our own findings, we would not conclude from the latter studies that employment protection is benign after all. A more balanced view would be that the research focus has been too oblique. That is to say, the impact of employment protection on unemployment and employment aggregates is somewhat indirect and difficult to isolate from other causal factors. One alternative research strategy would be to focus on the point at which dismissals protection can be expected more directly to affect behavior. An obvious example is the speed of employment adjustment to demand shocks. Here the immediate goal would be to discover whether observed differences in the employment adjustment process are linked in a systematic way to extant representations of the stringency of national employment protection rules (see Addison and Teixeira 1999). Another example would be to analyze job flow data (see Blanchard and Portugal 1998; Garibaldi 1998). In both cases, the ultimate goal would be to trace the implications of changes in employment adjustment and flows to average levels and durations of employment and unemployment.[9]

Despite the conflicting results reported here, it is assuredly of importance to get a better grip on the effects of employment protection. This is perhaps nowhere more important than in Europe at a time when the European Union is seeking to implement a wide range of job protection and analogous mandates at European level (Addison and Siebert 1999). It would be particularly unfortunate were the results of our own cross-country inquiry and the mixed evidence of the wider literature to encourage the perception that supranational legislation is benign.

Footnotes

1. That said, there is no substitute for carefully parameterized models of individual employment protection mandates. For one such attempt, see Addison and Chilton (1997).

2. On the important contribution of growth in the working age population to employment development, see Krueger and Pischke (1997).

3. The OECD (1999, Table 2.4) has recently incorporated information on collective dismissal regulations in a revised measure of the overall strictness of employment protection legislation (OECD 1999, Table 2.5).

4. A useful survey of these studies is provided by the OECD (1999, Table 2.C.1).

5. The survey covers 21 countries. The number of returned questionnaires varies by year, averaging 1,531 between 1984 and 1990. There was no survey in 1987. The flexibility question was changed in 1990, and subsequently dropped.

6. At issue is whether the short time series of this study is really sufficient to capture actual changes in the regulatory environment, which occur but sporadically. The limited time span may also mean that it is difficult to identify patterns of adjustment implicit in the model's use of lagged variables.

7. A worrying feature of the study is its heavy reliance on collective bargaining systems. Classifying countries by the coordination or otherwise of collective bargaining and the degree of centralization and cooperation is a nontrivial task. It shares certain commonalities with the construction of an employment protection index in this regard.

8. This procedure assumes a homogeneous lagged response, an assumption that is probably violated in the population. Given that the time series is relatively short for analyzing an autoregressive error structure, however, this assumption is unlikely to cause major problems and was necessary to gather sufficient information for parameter estimation.

9. Another option of course would be to temporarily abandon cross-country analyses in favor of national studies, in which framework individual mandates can more easily be parameterized and any tradeoffs rendered more transparent.

Data Appendix

This paper uses data from Lazear (1990) that have been corrected for errors of omission and commission. Full details are supplied by Addison and Grosso (1996). The sample period is identical to Lazear, while the (initial) sample of countries has been expanded by one nation, namely, Finland. Lazear collected data on Finland but treated them as "non-applicable," presumably because there is no requirement in Finnish law for employers to pay severance pay (over and above pay for notice periods). But it became possible for employees to be dismissed for economic reasons in 1979, and so Finland enters our data set from that date and severance pay is duly coded as 0 month.

Unlike Addison and Grosso (1996), we follow Lazear in including Portugal within the sample. Indeed, the Portuguese data cover the entire sample period, 1956-84, rather than 1970-84 as in Lazear. The reasons for this extension have to do with the legal definition of collective dismissals in Portugal. The employment threshold is so low that we treat the law on collective dismissals as applicable to individual dismissals. (For an elaboration of this argument, as well as the problems with Lazear's own measure of Portuguese severance pay, see Addison and Teixeira, 1997).

The countries listed in Table 1 plus Israel make up our 21-nation sample. Israel is not included in the table because of the nature of the comparisons being effected there. By contrast, Lazear's final sample is just 18 countries.

Most of the differences between ourselves and Lazear pertain to the severance pay variable, although many of the same sources are used in its construction (see Addison and Grosso, 1996, fn.5). The more important differences in this regard include the following. First, there are no statutory severance pay entitlements in Denmark, as claimed by Lazear; such obligations are instead fixed under collective bargaining. Denmark is thus coded here as 0 month throughout and not as 0 for 1956-70 and 1 for 1971-84. Second, for four countries - France, Italy, Norway, and Spain - severance pay fails to reach the levels indicated by Lazear. Third, Germany has no statutory severance pay - although service-related compensation for socially unwarranted dismissals is set down under 1969 legislation - and is here coded 0 throughout rather than as 1 in Lazear. Fourth, legislation covering severance pay has long applied in the U.K., but is unaccountably neglected by Lazear; and thus further reduces his sample size. Relatedly, Lazear adopts the convention of discarding all data for a particular country/year if data for any one of the four dependent variables are missing. This procedure serves to further truncate the overall sample size, largely because of missing data on hours. As a practical matter, however, constraining the number of observations to be equal across all four outcome measure regressions did not materially affect any of the results reported here.

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Table 1. Severance Pay and Alternative Indicators of the Stringency of Employment Protection

Country	(1) Severance Pay Entitlement (months)[a]	(2) Bertola ranking	(3) Grubb-Wells ranking	(4) OECD index	(5) IOE employer survey index, 1985	(6) EC Employer survey index, 1985	(7) WCR flexibility index, 1984-90
Belgium	0	9	5	10.5	2.5	113	41.83
Denmark	0	2	2	3.25	1		61.76
France	1	8	6	9.5	2.5	134	42.33
Germany	0	6	7	12	2.5	83	41.49
Greece	1		10	11		130	30.28
Ireland	1.5		3	2.75	1.5	111	47.57
Italy	8.8	10	8	14.25	3	151	39.87
The Netherlands	0	3	4	7.25	2.5	73	46.7
Portugal	10		11	12.5	2		33.12
Spain	6.6		9	11.25	3		29.81
United Kingdom	2.5	4	1	2.25	0.5	39	58.08
Austria	4			9	1.5		41.29
Finland	0			10.5	1		50.11
Norway	0			9.75	1.5		40.89
Sweden	0	7		8.5	2		40.77
Switzerland	0			1.75			61.69
United States	0	1		0.36			72.66
Japan	0	5		3.71			55.43
Australia	0			3.26			38.45
New Zealand	5.5			0.72			40.95
Spearman rank correlation Coefficient[b]		0.654[c]	0.361	0.389	0.097	--	0.434

[a]The entries in column (1) are taken from the present study and pertain to 1984. All other index/ranking values are for the late 1980s, unless otherwise indicated. The construction of the various indexes/rankings is discussed in the text.

[b]The rank correlation exercise is for the first 11 countries in the table, where missing values are based on OECD (1994, Table 6.7, Panel B) interpolations.

[c]Statistically significant at the .05 level.

Sources: Bertola (1990); Commission (1986); Di Tella and MacCulloch (1999); Grubb and Wells (1993); IOE (1985); OECD (1994).

Table 2. Pooled Estimations - No Country Dummies

Independent variable	Dependent Variable			
	EMPPOP	UNRATE	LFPR	HOURS
Intercept	-0.254 (0.048)	0.103 (0.028)	-0.228 (0.045)	48.336 (3.659)
SEV	-0.0045 (0.0008)	0.0021 (0.0005)	-0.0038 (0.0008)	0.095 (0.064)
GROWTH	-0.066 (0.073)	0.029 (0.042)	-0.054 (0.068)	11.274 (5.535)
GROWTH.SEV	-0.002 (0.017)	0.004 (0.01)	0.0004 (0.0162)	-0.753 (1.294)
WRKAGE	1.044 (0.074)	-0.097 (0.043)	1.032 (0.069)	-5.333 (5.633)
R^2	0.38	0.41	0.41	0.29
$\hat{s}$	0.041	0.024	0.038	3.06
n	536	536	536	513

Regressions include YEAR and $YEAR^2$. $\hat{s}$ is the standard error of the estimated regression.

Standard errors of the coefficient estimates are given in parentheses.

Table 3. Fixed Effects Regressions

Independent variable	Dependent variable			
	EMPPOP	UNRATE	LFPR	HOURS
SEV	0.00076 (0.0005)	0.0013 (0.0004)	0.0012 (0.0005)	0.053 (0.045)
GROWTH	0.024 (0.033)	0.016 (0.029)	0.033 (0.032)	12.231 (3.029)
GROWTH.SEV	-0.0087 (0.0074)	0.0084 (0.0063)	-0.0049 (0.0074)	-0.461 (0.67)
WRKAGE	0.548 (0.062)	0.35 (0.056)	0.71 (0.061)	2.65 (5.681)
R^2	0.9	0.77	0.89	0.83
$\hat{s}$	0.017	0.015	0.017	1.51
LM	22.92	17.14	7.34	17.13
n	536	536	536	513

Regressions include YEAR and $YEAR^2$. $\hat{s}$ is the estimated standard error of the regression.

Standard errors of the coefficient estimates are given in parentheses. LM is the first order autocorrelation test statistic. The estimated autocorrelation coefficients $\hat{r}$ resulting from applying a Marquardt nonlinear least squares algorithm that simultaneously estimates the coefficients b and r are 0.94 (0.02), 0.83 (0.03), 0.94 (0.05), and 0.84 (0.02), respectively.

Table 4. Fixed Effects Regressions with Correction for Autocorrelation

Independent variable	Dependent variable			
	EMPPOP	UNRATE	LFPR	HOURS
SEV	-0.00047 (0.00034)	0.0052 (0.00050)	0.00028 (0.00031)	0.0076 (0.0434)
GROWTH	0.022 (0.0091)	0.0218 (0.0150)	0.0071 (0.0087)	3.988 (1.310)
GROWTH.SEV	0.0032 (0.0021)	0.0037 (0.0032)	0.0049 (0.0018)	0.113 (0.276)
WRKAGE	0.337 (0.106)	0.398 (0.132)	0.477 (0.095)	-15.095 (12.260)
R^2	0.35	0.41	0.39	0.34
$\hat{s}$	0.006	0.009	0.006	0.780
LM	39.76	18.37	6.22	18.77
n	536	536	536	513

Regressions include YEAR and $YEAR^2$. $\hat{s}$ is the estimated standard error of the regression.

Standard errors of the coefficient estimates are given in parentheses. LM is the first order autocorrelation test statistic.

Table 5. Fixed Effects Regressions with Correction for Autocorrelation (Hodrick-Prescott smoothed series)

Independent variable	Dependent variable			
	EMPPOP	UNRATE	LFPR	HOURS
SEV	0.0017 (0.0010)	0.00097 (0.00113)	0.0015 (0.0014)	0.119 (0.073)
GROWTH	0.010 (0.006)	-0.029 (0.013)	-0.004 (0.009)	3.471 (0.765)
GROWTH.SEV	0.0004 (0.0042)	0.007 (0.012)	0.0038 (0.0048)	-0.142 (0.312)
WRKAGE	0.309 (0.067)	0.413 (0.155)	0.462 (0.095)	-1.089 (9.514)
R^2	0.41	0.44	0.43	0.37
$\hat{s}$	0.006	0.009	0.005	0.788
LM	18.53	24.51	18.13	13.92
n	536	536	536	513

Regressions include YEAR and $YEAR^2$. $\hat{s}$ is the estimated standard error of the regression.

Standard errors of the coefficient estimates are given in parentheses. LM is the first order autocorrelation test statistic.

Table 6. Dynamic Specification of the Lazear Model: GMM estimates[a]

Independent variable	Dependent variable Y_{it}							
	EMPPOP		UNRATE		LFPR		HOURS	
	OLS[b]	GMM[c]	OLS	GMM	OLS	GMM	OLS	GMM
$Y_{i(t-1)}$	0.93 (0.024)	0.763 (0.134)	0.942 (0.065)	0.722 (0.147)	0.929 (0.032)	0.799 (0.113)	0.886 (0.02)	0.498 (0.224)
SEV	0.00007 (0.00012)	-0.001 (0.0008)	0.00028 (0.00024)	0.000064 (0.0009)	0.00021 (0.00017)	-0.001 (0.0005)	-0.024 (0.016)	0.058 (0.025)
GROWTH	0.07 (0.021)	0.054 (0.029)	-0.134 (0.031)	-0.094 (0.03)	0.0119 (0.0919)	0.01 (0.019)	7.264 (1.713)	5.901 (2.602)
GROWTH.SEV	-0.00099 (0.00272)	0.0059 (0.0036)	0.0081 (0.0045)	0.0033 (0.0037)	0.0031 (0.0021)	0.0092 (0.0022)	0.558 (0.449)	0.251 (0.474)
WRKAGE	0.052 (0.034)	0.12 (0.081)	0.085 (0.058)	0.488 (0.318)	0.095 (0.04)	0.282 (0.097)	-1.912 (3.12)	10.895 (11.858)
M2[d]		-2.11		-1.6		0.22		1.02
Wald[d]		100.8 [5]		299.0 [5]		348.3 [5]		16.6 [5]
R^2	0.88		0.85		0.91		0.88	
n	536	536	535	514	536	515	505	484

[a]The equations were estimated using the DPD software, developed by Arellano and Bond (1991). The version used in this study was made available by Dr. Jurgen Doornik of the Oxford University Institute of Economics and Statistics. Standard errors of the coefficient estimates are given in parentheses.

[b]OLS is the within-group (i.e. fixed effects) estimation in levels of the variables.

[c]The GMM method estimates the model in first differences. The variables SEV, GROWTH, GROWTH.SEV and WRKAGE are assumed exogenous and used as instruments.

[d]The m_2 is a test for lack of second order serial correlation in the first difference residuals; the Wald statistic is a test of the joint significance of the independent variables (degrees of freedom for c^2 are in brackets). The m_2 and Wald tests are both asymptotically robust to general heteroskedascity.

Appendix Table 1. Random Effects Regressions with Correction for Autocorrelation

Independent variable	Dependent variable			
	EMPPOP	UNRATE	LFPR	HOURS
SEV	-0.00054	0.00069	-0.00035	-0.0070
	(0.00034)	(0.00049)	(0.00030)	(0.0424)
GROWTH	0.0022	-0.0207	0.0070	4.016
	(0.0098)	(0.0148)	(0.0087)	(1.303)
GROWTH.SEV	0.0031	0.0039	0.0049	0.108
	(0.0021)	(0.0031)	(0.0018)	(0.274)
WRKAGE	0.364	0.283	0.501	-13.620
	(0.103)	(0.121)	(0.092)	(11.261)
R^2	0.32	0.38	0.36	0.31
$\hat{s}$	0.006	0.009	0.005	0.775
LM	9.05	0.82	1.07	1.86
n	536	536	536	513

Regressions include YEAR and $YEAR^2$. $\hat{s}$ is the estimated standard error of the regression.

Standard errors of the coefficient estimates are given in parentheses. LM is the first order autocorrelation test statistic.

Appendix Table 2. Feasible GLS Estimation for Cross-Sectionally Correlated Residuals

Independent variable	Dependent variable			
	EMPPOP	UNRATE	LFPR	HOURS
SEV	-0.00020	0.00065	-0.00017	0.00006
	(0.00016)	(0.00046)	(0.00014)	(0.01631)
GROWTH	0.0018	-0.0073	-0.0066	4.417
	(0.0042)	(0.0071)	(0.0030)	(0.531)
GROWTH.SEV	0.0033	0.0035	0.0048	-0.216
	(0.0012)	(0.0071)	(0.0015)	(0.178)
WRKAGE	0.304	0.336	0.483	-12.417
	(0.042)	(0.054)	(0.026)	(5.693)
R^2	0.35	0.40	0.38	0.34
$\hat{s}$	0.006	0.009	0.006	0.781
LM	2.00	1.86	2.21	1.96
n	515	465	515	489

Regressions include YEAR and $YEAR^2$. $\hat{s}$ is the estimated standard error of the regression.

Standard errors of the coefficient estimates are given in parentheses. LM is the first order autocorrelation test statistic.

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