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FIRING COSTS, EMPLOYMENT FLUCTUATIONS AND AVERAGE EMPLOYMENT: AN EXAMINATION OF GERMANY

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ABSTRACT

West Germany's Employment Promotion Act of 1985 facilitated the use of fixed term contracts and increased the number of dismissals above which the employer is required to establish a "social plan" (involving severance payments). The effect of this reduction in "firing costs" on movements in employment is assessed using manufacturing data by detailed industry for the period 1977-1992: a dynamic specification using the data as a panel, and allowing coefficients to vary by industry (random coefficients) is employed. Compared to the 1977-1981 period, adjustment of blue collar hours was more flexible from 1982-1988, and less flexible in the subsequent period. There is weaker evidence that adjustment of blue collar workers became less flexible in the years following the new legislation and that white collar workers' flexibility fluctuated over the period examined. The timing and direction of these changes, as well as the direction of relative changes in flexibility between industries with high and low sales variability, suggest they are not the result of the Employment Promotion Act.

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One of the features which distinguishes most European labor markets from the United States labor market is the existence of higher "firing costs": costs associated with the termination of contracts of small or large groups of employees. These costs, whose most salient features may be summarized as severance payments and notice periods, are the subject of a vigorous debate centering on their effect on average employment. The unions which lobbied for job security laws in the early 1970s hoped that these firing costs would reduce firing sufficiently to raise average employment. By the 1980s, however, several European governments had become persuaded that the costs might deter hiring even more than firing, and legislated reductions in firing costs.

Theoretical models such as those of Bertola (1992) and Bentolila and St.Paul (1992a,b) have ambiguous predictions about the effect of firing costs on average employment, but generally predict firing costs will slow adjustment of employment to shocks. A small number of empirical studies has tried to analyze the effect of policy changes, but results on changes in employment adjustment are mixed. Abraham and Houseman (1994) find no effect of a 1985 law facilitating use of fixed term contracts on (West) German employment adjustment, while Kraft (1993) finds German employment adjusted more slowly after 1985. Bentolila and St.Paul (1993a) find evidence of faster employment adjustment after fixed term contracts became widespread in Spain, but rely on data from after the contracts became legal. In a paper with a different emphasis, Dertouzos and Karoly (1993) find that U.S. states making legal redress against wrongful dismissal easier experience a fall in employment of 2-3%.

This paper reexamines the impact of the legislation introduced in Germany in 1985 aimed at reducing firing costs in order to boost employment.[1] It achieves more precise estimates than those of previous studies by using monthly data for a panel of detailed industries, rather than using yearly data or time series estimation. A further improvement on existing work using panel data is the use of a random coefficients model, which allows for possible differences in coefficients by industry. Data on employment and sales for the period January 1977 to December 1992 are used in a dynamic specification.[2]

The timing and direction of the changes in employment flexibility identified in this paper, as well as the direction of relative changes in flexibility between industries with high and low sales variability, suggest they are not the result of the Employment Promotion Act. This is in broad agreement with Kraft (1993) and Abraham and Houseman (1994).

INSTITUTIONAL BACKGROUND

In common with many other European countries, Germany has laws which make individual or collective dismissals or redundancies costly either in terms of time, money or procedural complexity. These termination costs became much higher at the beginning of the 1970s, then were relaxed by the *Beschäftigungsförderungsgesetz* (Employment Promotion Act) of 1985.[3] Before 1985, the 1969

Kündigungsschutzgesetz (Protection Against Dismissal Act), the 1972 *Betriebsverfassungsgesetz* (Works Constitution Act) and the *Bürgerliches Gesetzbuch* (Civil Code) set out the following procedures for individual redundancies (contract terminations on economic grounds). Before an employer may proceed, the works council must first be consulted (about two thirds of private sector workers are represented by a works council - most of those unrepresented work in small firms). If the works council does not agree, the worker must be retained pending an appeal to the Labor Court. Notice periods must be given which are a function of the worker's tenure, age and type of job (Arbeiter or Angestellte: blue or white collar), and vary from two weeks to six months.

A collective redundancy was defined until 1985 as dismissal within a thirty day period of more than five workers in companies with 21-59 employees, 25 workers or 10% of workers in companies with 60-499 workers, and 30 workers in companies with over 500 workers. Consultation with the works council is more extensive than in the individual case, and includes discussion of who should be laid off. The employment office of the state (Land) must be informed. The employer must then wait one month before proceeding (the employment office may increase this period to two months, or reduce it). The same notice periods as in individual cases apply, but the one month waiting period makes the notice period a minimum of one month. Individuals or the works council may appeal. Companies conducting collective redundancies are required to negotiate a "social plan" with the works council, which include severance payments and costs of retraining programs. The amounts are not specified, but the median settlement is typically worth 15-25 weeks blue collar pay.[4]

Workers may be dismissed without going through these procedures during their six month probationary period, if they are on a fixed term contract, or if they are temporary agency workers.

The provision of the 1985 law considered most important concerns the use of fixed term contracts. Prior to 1985 a fixed term contract could last only six months, and the employer had to demonstrate that the work was by nature temporary. The 1985 act permitted fixed term contracts of 18 months (or two years under some circumstances) with no justification required. The percent of workers on fixed term contracts rose from 4.0% in 1984 to 6.3% in 1986 (see Büchtemann 1993: note that these two numbers are from different sources), with blue collar workers more likely to hold fixed term contracts than white collar workers.[5] If a worker on a fixed term contract is to be retained on its expiry, the contract must be converted to a permanent contract. (A fixed term contract is permitted if more than four months have elapsed since any previous contract, whether permanent or fixed term.) The law also permitted hiring of workers from temporary help agencies for up to six months rather than three.

The 1985 change also raised the number of redundancies above which the collective redundancy rules apply. The new thresholds are 6 workers or 20% of workers in companies with 20-59 employees, 37 or 20% in companies with 60-249 employees, 60 or 15% in companies with 250-499 employees, and 60 or 10% in companies with 500 or more employees.

In part due to the existence of these restrictions on firing, temporary lay-offs as exist in the United States are unknown. However, the use of short time (reduction of hours below standard hours) is widespread: workers put on short time receive short time benefits, which replaces the same proportion of their lost earnings as unemployment insurance would were they fully unemployed. Short time benefits may last up to two years during recessions (up to three in the steel industry).[6]

At the same time as the Employment Promotion Act came into effect, a transition in standard working hours began. The engineering union *IG Metall* accomplished the first step towards its goal of a 35-hour week in the 1984 bargaining round, when it negotiated a 38.5 hour standard working week effective 1985. The printing union *IG Druck und Papier* concluded a similar contract one week later, and over the next several years the lead of these two unions in reducing the 40-hour week was followed by unions in other sectors. The 35-hour week was negotiated in the engineering and printing sectors in the 1990 bargaining round, to be phased in by 1995. In return for conceding shorter hours, employers in some sectors obtained the right to deploy their workers more flexibly, which in some cases permitted the plants to operate longer hours. This flexibility took the form of allowing workers to work more than the standard hours during peak periods as long as the average conformed to agreed hours over a period as long as six months, permitting different workers to work different hours as long as the average across workers conformed, allowing hours worked over the agreed hours to be compensated with additional days off, and permitting more Saturday shifts. If a flexibility clause was negotiated with the union, it was then up to an individual employer to implement the details with its works council. It is unclear how important these clauses were either in their ability to reverse the effects of restrictions on overtime, for example, or in their extent (surveys suggest most employers did not use them, however those that did were the large employers), but their possible impact on flexibility of hours per worker must be considered.

In some sectors such as the chemical industry, early retirement programs compelling the employer to allow up to 5% of the workers to retire early were negotiated instead of a reduced working week. These programs were able to take advantage of government measures in place from 1984 to 1988 to facilitate early retirement. These measures provided a state subsidy to workers retiring early from age 58 under programs agreed between firms and unions. This subsidy was only paid, however, if the firm demonstrated that it had replaced the retired worker with a person registered as unemployed, or unable to find a job after completing an apprenticeship. From 1989 through 1992 the government subsidized wages of workers 59 or older who came to an agreement with their employer to work only half time. This subsidy too was conditional on replacement of the lost hours with an unemployed person's labor. Due to this conditionality, it is unclear to what extent the government intervention is likely to have increased employment flexibility.

PREVIOUS THEORETICAL AND EMPIRICAL WORK

The relevant theoretical literature includes both a European body motivated by the same questions as this paper, and an American body which examines the implications of (incomplete) unemployment insurance experience rating.[7] A general equilibrium model is difficult to construct, and both strands of the literature have weaknesses. The European strand, while modelling firm behavior over the whole (possibly stochastic) business cycle, assumes the wage to be exogenous. The American strand endogenizes the wage, but often focuses on the firing decision, does not allow for stochastic shocks, and sometimes neglects factors shown in the European literature to have important consequences, such as quit rates and discount rates.

In this section a model of the European variety is outlined. An important modelling decision concerns the nature of adjustment costs. If it is assumed that firms do not simultaneously hire and fire, one natural assumption is that costs are linear in net employment changes, but differ according to whether the change is positive or negative. This model has received considerable attention in recent theoretical literature. The model often preferred by empirical researchers assumes that adjustment costs are quadratic in the net employment change, which implies that firms will spread their adjustment over time. As was described above, firing costs in Germany do rise with the number of workers to be fired. In addition, if aggregate data are used, adjustment may appear to be smoothed over time, and the quadratic model may be a better fit (see Hamermesh 1989).

Consider first a model with adjustment costs quadratic in the net employment change caused by hiring or firing, $\dot{Z}$. Output is produced by labor only, shocks to demand are represented by θ, and workers quit at an exogenous rate δ.[8] The firm seeks to maximize

$$J = E_t\int_0^\infty \left[F(L_t;\theta_t) - wL_t - b\dot{Z}_t^2\right]e^{-rt}\,dt \qquad (1)$$
$$s.t.\ \dot{L}_t = -\delta L_t + \dot{Z}_t$$

This maximization may be performed with standard techniques using the Hamiltonian, which yield the following differential equation for the path of employment:

$$2b\ddot{L}_t - 2br\dot{L}_t - 2b\delta(\delta+r)L_t + F'(L_t;\theta_t) - w = 0 \qquad (2)$$

In the steady state there is a wedge between the marginal product of labor and its wage :

$$F'(L^*) = w + 2b\delta(\delta+r)L^* \qquad (3)$$

Bertola (1992) considers a version of this model where the adjustment costs are a linear function of employment change, and shocks are non-stochastic. The Euler equation is:

$$F'(L_t) = \begin{cases} w + (\delta+r)h & \text{if } \dot{Z}_t > 0 \\ w - (\delta+r)f & \text{if } \dot{Z}_t < 0 \end{cases} \qquad (4)$$

where h is the hiring cost and f the firing cost.

Bertola demonstrates the following points, building on earlier work by Nickell (1978). In the presence of turnover costs, firms will neither hire nor fire around the peaks and troughs, but rather let employment decline through quits. The speed of adjustment at other times may differ from the no cost case, and the effect on upward and downward adjustment will be opposite in direction, but the nature of the change will depend upon the functional form of labor demand and shocks to it. The effect of turnover costs on the average level of employment over the cycle is ambiguous, and may be broken down into two types of effect. The relative sizes of labor hoarded in the troughs, and not hired at the peaks depends upon whether the marginal product of labor is a steeper function of employment during slumps or booms. In a theoretical sense this depends upon the functional form of labor demand (and shocks to labor demand). The second type of effect is through discounting and attrition of labor. Hiring costs increase the average marginal product of labor, and thus imply lower average employment, while firing costs decrease the average product of labor, and imply higher average employment. Thus the relative sizes of hiring and firing costs as well as the form of labor demand determine the effect of turnover costs on the average level of employment.

Bertola's numerical example illustrates a case where the functional form of labor demand tends to decrease average employment over the cycle. For small firing costs, average employment is a decreasing function of firing costs, while when firing costs become larger the discounting and attrition effects dominate, and average employment may exceed that in the no turnover cost case. Firing costs are more likely to increase average employment if the business cycle is long.

The finding that the effect of turnover costs on average employment is ambiguous is reproduced in several papers with slightly differing models. In a model with stochastic shocks, designed to examine the introduction of fixed term contracts, Bentolila and St.Paul (1992a) find the same result, and predictions of Bentolila and St.Paul (1992b) are also very similar.

The transition to empirical estimation is most commonly made by considering the quadratic adjustment cost case, which may be solved analytically if the labor demand function and shocks to it are assumed to have a convenient form (notice that Bertola's work suggests that this may not be innocuous). If the quadratic cost case above is considered in discrete time, and $F(L_t;\theta_t)$ is assumed to have the form: $F(L_t;\theta_t) = \theta_t(\alpha_1 L_t - \frac{1}{2}\alpha_2 L_t^2)$, where θ_t represents shocks, it may be shown that

$$L_{t+j+1} = \lambda_1 L_{t+j} - \frac{\lambda_1}{2b(1-\delta)} \sum_{i=0}^{\infty} \left(\frac{1}{\lambda_2}\right)^i E_t\{w_{t+j+1+i} - \alpha_1 \theta_{t+j+1+i}\} \tag{5}$$

where β is the discount factor. λ_1 and λ_2 are both non-linear functions of b,δ,β,α_2, and (since shocks are multiplicative) θ. If shocks to labor demand and wages follow a first order process, and firms form expectations based on the information available up to the present, the equation may be rewritten:

$$L_t = \lambda_1 L_{t-1} - \frac{\lambda_1}{2b(1-\delta)} \left\{\left(1-\frac{\rho_w}{\lambda_2}\right)^{-1} w_t - \alpha_1 \left(1-\frac{\rho_s}{\lambda_2}\right)^{-1} \theta_t\right\} \tag{6}$$

where ρ_W and ρ_S are the autocorrelation coefficients for wages and shocks. As b approaches zero, λ_1 (which is between 0 and 1) approaches zero, while λ_2

approaches infinity. Smaller firing costs thus imply a smaller coefficient on lagged employment and larger magnitude coefficients on the other terms.

The large empirical literature estimating some form of this equation is summarized in Hamermesh (1992), and only the most closely related will be discussed here. No consensus has been reached on the effect on employment of policy changes connected with firing costs. Abraham and Houseman (1994) use the same data as used in this paper to address the same question. They estimate an equation similar to equation 6 for all workers, blue collar workers, and blue collar hours over the period 1972-1990, with industry sales proxying for shocks to demand, with a quadratic time term to represent the capital stock, but omitting the wage term. Firing costs are not modelled directly, but a time dummy for the post May 1985 period is interacted with the coefficients to see if adjustment patterns changed following the introduction of the new laws. The equations are estimated separately by industry, as the authors reasonably assume that the coefficients will differ by industry, but this combined with the fact that the monthly data are smoothed into quarterly data means that the number of observations per regression is small. This is probably why the dummies are found to be insignificant, both in their analysis of Germany, and in parallel analysis of France and Belgium. Their use of seasonally adjusted data also removes some useful variation. Abraham and Houseman (1993b) examine workers, hours and inventories jointly, and find that inventories play only a small role in the process of adjustment to shocks.

Kraft (1993) forms a panel from yearly data for 1970-87 on twenty-one German manufacturing industries to estimate a similar equation for all workers. His time dummy is significant, and of a sign suggesting *reduced* flexibility for 1985-7.

Bentolila and St.Paul (1992) perform a similar exercise with yearly data on a panel of firms in Spain, which in 1984 introduced fixed term contracts similar to the German ones. They find evidence of faster adjustment after fixed term contracts became widespread, but do not have data on the period before the contracts actually became legal. They also find evidence that the effect on adjustment during upturns was smaller than that on adjustment during downturns, suggesting that the effect on average employment may have been negative.[9]

Dertouzos and Karoly (1992) take advantage of changes in legal environment in U.S. states. They estimate a static specification for yearly employment in five sectors on a panel of states. Two dummy variables which vary over time and across states indicate whether the state had adopted one of two forms of legal redress against wrongful dismissal. One of these dummies is significant and suggests employment falls 2-3% when this type of protection is adopted.

MODEL

The data are used here as a panel of monthly seasonally unadjusted observations on detailed industries (201 industries over the period January 1977 to December 1992) with the aim of uncovering smaller effects than Abraham and Houseman (1994) were able to. To accommodate the concern that coefficients may differ by industry, a random coefficients model is used which allows some industry variation in coefficients. Because the data are aggregated, the quadratic cost model is considered more appropriate, and equation 6 is used as a guideline for reduced form estimation.

The Random Coefficients Model

The model used is one in which some of the regression coefficients may be viewed as random variables with a probability distribution:

$$y_i = X_{1i}\beta_1 + X_{2i}\beta_{2i} + u_i \tag{7}$$

where

$$\beta_{2i} = \beta_2 + \nu_i$$

Notice that random effects is a special case of this where only the intercept has a random coefficient. The assumptions adopted are those of Swamy (1974) (this description also draws on Hsiao 1986):

$$\begin{gathered} E(\nu_i) = 0 \\ E(\nu_i \nu_i') = \begin{cases} \Delta & \text{if } i=j \\ 0 & \text{if } i \neq j \end{cases} \end{gathered} \tag{8}$$

$$E(X_{1i}\nu_i) = E(X_{2i}\nu_i) = 0$$

The last assumption is violated if one of the X's is the lagged dependent variable, as will be the case, and thus an instrumental variables approach is required in the generalized least squares (GLS) framework.[10] Acknowledgement that the errors are likely to be serially correlated is an additional reason instrumental variables is necessary:

$$E(u_i u_j') = \begin{cases} \sigma_i^2 \Omega_i & \text{if } i = j \\ 0 & \text{if } i \neq j \end{cases} \tag{9}$$

where

$$\Omega_i = \frac{1}{1-\rho_i^2} \begin{bmatrix} 1 & \rho_i & \rho_i^2 & \cdots & \rho_i^{T-1} \\ \rho_i & 1 & \rho_i & \cdots & \rho^{T-2} \\ \vdots & \vdots & \vdots & \vdots & \vdots \\ \rho_i^{T-1} & \rho_i^{T-2} & \rho_i^{T-3} & \cdots & 1 \end{bmatrix}$$

This introduces a second source of correlation of the lagged dependent variable with the error term. Thus,

$$y_i = X_{1i}\beta_1 + X_{2i}\beta_2 + X_{2i}\nu_i + u_i \tag{10}$$

and if W_i is a set of instruments, the variance-covariance matrix of the stochastic term is block-diagonal, with the i th diagonal block equal to

$$\Phi_i = W_i' X_{2i} \Delta X_{2i}' W_i + \sigma_i^2 \Omega_i \tag{11}$$

The GLS estimator is

$$\begin{bmatrix} \hat{\beta}_1 \\ \hat{\beta}_2 \end{bmatrix} = \left\{ \sum_i \begin{bmatrix} X_{1i}' \\ X_{2i}' \end{bmatrix} W_i \Phi_i^{-1} W_i' (X_{1i}, X_{2i}) \right\}^{-1} \left\{ \sum_i \begin{bmatrix} X_{1i}' \\ X_{2i}' \end{bmatrix} W_i \Phi_i^{-1} W_i' y_i \right\} \tag{12}$$

The first step in the estimation is to get estimators of β_1 and β_{2i} in an instrumental variables regression of y on X_1 and X_2 allowing the coefficients on X_2 to vary by i. From this estimators of ρ_i may be found, and Cochrane-Orcutt iteration performed to get new estimates of β_1, β_{2i}, ρ_i and lastly σ_i. This first set of steps is difficult if the lagged dependent variable is permitted to have a random coefficient while at least one X has no random coefficient, as the procedure then implies instrumenting 201 variables (the lagged dependent variable interacted with industry dummies). If all variables have a random coefficient, these steps may be done industry by industry. An estimator for Δ may then be calculated according to the formula

$$\hat{\Delta} = \frac{1}{N-1}\sum_i (\hat{\beta}_{2i} - \frac{1}{N}\sum_i \hat{\beta}_{2i})(\hat{\beta}_{2i} - \frac{1}{N}\sum_i \hat{\beta}_{2i})' - \frac{1}{N}\sum_i \sigma_i^2 (X_{2i}'X_{2i})^{-1} \tag{13}$$

The estimators of ρ_i, σ_i^2, and Δ may then be used in equations 11 and 12 to calculate the GLS estimator.[11]

Specification

Three measures of employment are used: number of blue collar workers, number of white collar workers, and blue collar hours (hours worked, rather than hours paid). Lagged industry sales, SAL_{it-1} are chosen as a proxy for shocks to labor demand in an industry.[12]

The wage variable available, created by dividing a wage bill by an employment measure, is problematic. Changes in it may reflect changes in hours or worker quality rather than actual wage changes, and it will be correlated with the error term. In addition, end of year bonuses give wages a peculiar seasonal pattern. Early regressions showed that the coefficients on variables involving the wage were very sensitive to the specification, and in the results presented here the wage variables are dropped. Dropping the wage variables altered other coefficients only slightly.[13]

The issue of technological progress is dealt with by including a time trend among the variables allowed to have a random coefficient. Information by industry on capital prices is unavailable.[14]

Adjustment is permitted to be different in upturns and downturns, and two definitions of the upturn dummy UP_{it} are used: one (UPS) defines the month to be an upturn if sales in that month are usually higher than in the previous month (thus exploiting a predictable seasonal pattern). UPB captures information about the business cycle: the (log) sales series are seasonally adjusted by taking the residuals from regressions of sales on month dummies, and UPB is set to one if the average of the previous two months' sales exceeds the average of sales lagged three and four times by more than the average month to month change. UPB has quite a lot of within industry variation, some of which is noise.

A dummy for the period preceding May 1985 - BEF_t - is used to test for a break in the model, and the change is allowed to differ in seasonal upturns and downturns (empirically the change never appears to differ in business cycle upturns and downturns, so these interactions were dropped). Following standard procedure, the logs of the variables (except dummies and the time trend) are used. The most appealing random coefficients model is:

Thus the intercept, lagged employment, sales and time trend correspond to X_2 in the previous section, and the interaction terms to X_1. This specification will be referred

$$L_{it} = (\alpha_0+\nu_{0i}) + (\alpha_1+\nu_{1i}+\beta_1 BEF_t+\beta_2 UPS_{it}+\beta_3 BEF_t * UPS_{it}+\beta_4 UPB_{it}) L_{it-1} \\ + (\alpha_2+\nu_{2i}+\beta_5 BEF_t+\beta_6 UPS_{it}+\beta_7 BEF_t * UPS_{it}+\beta_8 UPB_{it}) SAL_{it-1} \\ + (\alpha_3+\nu_{3i}) t + u_{it} \tag{14}$$

to as random coefficients (four). However, as mentioned, it is very hard to use instrumental variables with such a specification, so in addition a simple random effects model is examined, and also one in which all variables have a random coefficient:

$$L_{it} = (\alpha_0+\nu_{0i}) + (\alpha_1+\nu_{1i}) L_{it-1} + (\alpha_2+\nu_{2i}) SAL_{it-1} + (\alpha_3+\nu_{3i}) t \\ + (\beta_1+\eta_{1i}) BEF_t * L_{it-1} + (\beta_2+\eta_{2i}) UPS_{it} * L_{it-1} \\ + (\beta_3+\eta_{3i}) BEF_t * UPS_{it} * L_{it-1} + (\beta_4+\eta_{4i}) UPB_{it} * L_{it-1} \\ + (\beta_5+\eta_{5i}) BEF_t * SAL_{it-1} + (\beta_6+\eta_{6i}) UPS_{it} * SAL_{it-1} \\ + (\beta_7+\eta_{7i}) BEF_t * UPS_{it} * SAL_{it-1} + (\beta_8+\eta_{8i}) UPB_{it} * SAL_{it-1} + u_{it} \tag{15}$$

This specification will be referred to as random coefficients (all). If firing costs slow adjustment, it is expected that β_1 will be greater than zero, and β_5 less than zero. If adjustment in the upward direction is slowed more, β_3 will be greater than zero, and β_7 less than zero. A more flexible specification estimated uses several time dummies rather than just BEF.

The instrument used for lagged employment is sales lagged twice. The interactions involving lagged employment must also be instrumented: for example, $UPS_{it} * L_{it-1}$ could be instrumented with $UPS_{it-1} * SAL_{it-2}$. However, UPS_{it} and UPS_{it-1} are not very highly correlated, so the instrument used is $UPS_{it} * SAL_{it-2}$.

The analysis would benefit greatly if a cross-section measure of firing costs were available. The best proxy available is a measure of variation in industry sales (VAR). Although the pattern of shocks to an industry enters the model in its own right, variation of sales may also be seen as capturing effective firing costs: an industry whose sales vary little and thus desires less variation in employment will be less affected by changes in firing costs than an industry whose sales vary a lot. Three measures of the variation of sales are used, capturing variation of different frequencies: the coefficient of variation of sales over the whole period, the average of the coefficients of variation of four sub-periods, and the average absolute month to month change in log sales. If the variation of sales and its interactions with BEF, UPS and UPB are interacted with lagged employment and sales, significant coefficients on $VAR*BEF*L_{t-1}$ (positive) and $VAR*BEF*SAL_{t-1}$ (negative) will suggest that adjustment was slowest for high variance industries in the before period, evidence of the effects of higher firing costs before the law change. In the blue collar hours regressions this could also indicate greater inflexibility of union contracts before 1985, while if government subsidization of early retirement programs reduced firing costs this could also be reflected in the regressions.

A variable for the average quit rate over time by industry is available[15], but it is clear that the quit rate has a very complicated effect, entering the coefficients on the principle variables, as well as possibly directly. A simple way to capture this is to allow interactions of the quit rate with lagged employment and sales, as well as including the quit rate and its interactions with BEF and UP directly. Since the quit rate does not vary over time, its coefficient is not allowed to have a random component, and a minor modification to the initial stage of the GLS estimation procedure is necessary. A similar modification is necessary for regressions including VAR, which is not time-varying.

RESULTS

Figure 1 shows the aggregate movements in the most important variables, with the date of the law change indicated with a vertical line. Both the before and after periods have a business cycle upturn and downturn in sales, and corresponding fluctuations in blue collar workers. The blue collar hourly wage been scaled to fit on the same graph as the monthly wages: notice the important seasonal pattern, with wages particularly high in the month of November when bonuses are received, and, when calculated hourly, in December and the summer when fewer hours are worked. Figure 2 shows sales and blue collar workers in four industries chosen to illustrate the variety of industry patterns.

Tables 1, 2 and 3 show the results of estimating equations with the variables of equations 14 and 15 using random effects with and without instrumenting the lagged employment terms, random coefficients in the four uninteracted variables, and random coefficients in all variables with and without instrumenting, for the three measures of employment.[16] Consider first the uninteracted variables: lagged employment, sales and time trend have the expected signs. The coefficient on lagged employment falls as more variables are permitted to have random coefficients, while the coefficient on sales rises (both these changes suggest more rapid adjustment of employment). The magnitude of the coefficient on the time trend also increases moving across the tables. The magnitude of the coefficients suggests much more rapid adjustment of hours than workers, something evident from Figure 1. Interaction of these variables with the Up_s dummy suggests that blue collar workers and hours adjust more slowly in a seasonal upswing than in a downswing. Interactions with the Up_b dummy are generally insignificant.[17]

Consider now the interactions with the Before dummy: if firing costs slow adjustment, the coefficient on Before*$Empl_{t-1}$ will be positive, on Before*$Sales_{t-1}$ negative. For blue collar workers (Table 1), the coefficient on Before*$Empl_{t-1}$ is negative and significant in the random effects and random coefficients (four) specifications, but insignificant in the instrumental variables and random coefficients (all) specifications. The coefficient on Before*$Sales_{t-1}$ is insignificant in all specifications. The coefficient on Before*Up_s*$Empl_{t-1}$ is negative and significant, and on Before*Up_s*$Sales_{t-1}$ positive and significant in the random effects and random coefficients (four) specifications, but insignificant in the instrumental variables and random coefficient (all) specifications. The effect of instrumenting is thus to render insignificant the coefficients on interactions with Before: inspection of the results of the first step of GLS estimation and of Figure 1 suggests that sales are not well correlated enough with blue collar workers to be a good instrument, however, and instrumenting may thus add little information. Similarly, allowing all variables to have a random coefficient may have as its main effect to reduce the efficiency of estimation. It is interesting that the random coefficients (four) specification cannot be rejected by a Hausman test in favor of the random coefficients (all) specification (random effects can be rejected in favor of random coefficients (four), however). Since the uninstrumented specifications are inconsistent, however, the Hausman test is strictly speaking invalid. In summary, coefficients on the Before interactions are insignificant in most specifications, but where significant suggest that adjustment may have been faster before 1985, especially in seasonal upswings, the opposite of the expected result.

For white collar workers (Table 2) the coefficients on variables involving Before are insignificant in all cases. As for blue collar hours (Table 3), the coefficients on Before*$Empl_{t-1}$ are positive and significant in most specifications, while those on Before*$Sales_{t-1}$ are negative, and significant in most specifications. This suggests that hours have adjusted more flexibly since 1985. The coefficients on the Before*Up_S interactions, significant in specifications other than random coefficients (all) and random coefficients (all) IV, suggest that upward flexibility may have fallen. Here the preferred specification is random coefficients (all) IV, however.

This combination of apparently more flexible hours and possibly less flexible workers since 1985 is hard to explain by referring to the new legislation. If firms were able to circumvent the firing costs fully by adjusting hours per worker, it is conceivable that the legislation might have left hours unaffected, while increasing worker flexibility. If the firing costs did constrain the hours chosen by employers, both hours and worker flexibility might have been expected to rise, while if the firing costs did not bind firms at all, there might have been no changes. It is possible that the observed combination is rather the result of the union contracts negotiated beginning in 1984. The increased flexibility in hours per worker may have made adjustment of hours more attractive compared to adjustment of workers.

The coefficient on lagged employment, λ_1 (α_1), may be used to calculate the adjustment half-life τ (the time taken to move half way to the new steady state after a shock): $\lambda_1^{\tau} = 0.5$. Although the different specifications yield qualitatively similar results, their implications about the magnitudes of flexibility changes vary considerably. Thus, in Table 3, the hours random coefficients (four) coefficient 0.5101 implies a lag of 1.0 months. If this is adjusted using the Before interaction coefficient 0.0033, the implied lag rises 1%. The same calculation for the random coefficient (all) specification suggests the half-life was 0.8 months in the post-1985 period, and 20% higher before. For blue collar workers the adjustment implies a fall in the lag of 1% for the random coefficients (four) specification (from 10.0 months) and 3% for the random effects specification. The size of the coefficient on Before*$Sales_{t-1}$ compared to the coefficient on $Sales_{t-1}$ also varies somewhat across specifications. These half-lives indicate that yearly data are ill-suited to an examination of employment adjustment.

The fact that the slowing of blue collar worker adjustment is especially marked in the seasonal upswing, and that the faster blue collar hours adjustment is greater in the downswing, makes it more likely that the changes will lead to lower average employment. The impact of the Before dummies not interacted with Up_s must also be considered, however, and the net effect will depend on the path of shocks to an industry.

If the variables Before, Up_s, Before*Up_s and Up_b are included directly in the regression as well as interacted with lagged employment and sales, the coefficients on the interaction terms of interest are less consistent across specifications (these results are not shown). However, the results of the preferred specifications, random coefficients (four) for blue collar workers, and random coefficients (all) IV for blue collar hours, are similar to the results discussed above.

The regressions of Tables 1-3 were also repeated with the quit rate interacted with lagged employment and sales as additional covariates. For blue collar workers the results indicate faster adjustment in high quit rate industries, as would be expected, and other coefficients are little changed. For blue collar hours a higher quit rate does not speed adjustment (other coefficients are little changed). For white collar workers the coefficients on the quit rate terms are insignificant, and the other coefficients of interest remain insignificant. The quit rate and its interactions with the dummy variables may be entered directly without altering results significantly for the preferred specifications.

The regressions of Tables 1-3 may be run on sub-groups of industries. The 201 industries in those tables may be grouped into more aggregated categories. Twenty-five of these categories contain five or more detailed industries, and the regressions were repeated in these twenty-five cases. The coefficients on the Before*Up_s and Up_b interactions were rarely significant, and for white collar workers the other interaction coefficients were also generally insignificant. For blue collar workers and hours the pair of coefficients on the Before interactions were often significant, but with varying signs (although always opposite in sign from eachother): thus for blue collar workers or hours, flexibility of adjustment was increasing for some industries and decreasing for others. For no industry group did the coefficients indicate a significant fall in flexibility of hours and a significant rise in flexibility of workers. Of the five engineering sectors, four showed a significant rise in hours flexibility.

The blue collar worker equations of Table 1 have been repeated including lagged hours per blue collar worker and its interactions with Before, Up_s and Up_b as covariates. (As hours per worker are calculated by dividing worker hours by workers, this variable is correlated with the error, however.) The coefficients on interactions of Before with lagged employment and sales are similar (both are significant in the random coefficients (four) specification). The coefficients on Before*Up_s interactions are insignificant.

Table 4 investigates more closely the timing of changes noted in Tables 1-3, focusing on one of the specifications which suggested changes occurred for both blue collar workers and hours, random coefficients (four). (Recall, however, that this uninstrumented specification leads in principle to inconsistent results. For all three measures of employment the random coefficients (all) interaction coefficients are insignificant.) The "before" time period is divided into two periods of equal length (T1 and T2), as is the "after" period (T3 and T4). The omitted period is T1, which means that, for example, if the increase in hours flexibility began with the law change in 1985, the coefficients on interactions with T2 should be insignificant, while those on T3 and T4 interactions with lagged employment should be negative and with lagged sales positive.

For blue collar workers the coefficients on interactions with T2 are insignificant, while the coefficients on the interactions with T3 and T4 confirm the earlier result of slower adjustment after May 1985, and suggest that adjustment may have been slower in period T4 than in period T3. This is consistent with a break in 1985.

The white collar worker random coefficient (four) results indicate that adjustment for this type of employment was fastest in periods T3 (at least in the downward direction) and T2, thus explaining why the coarser division into before and after 1985 yielded coefficients suggesting insignificantly different adjustment.

Blue collar hours results indicate that adjustment was faster in T2 and T3 compared to T1, especially in the downward direction, and that adjustment in period T4 may have been slower in the upward direction (and similar in the downward direction) compared to T1. It therefore does not seem that the timing of changes in hours adjustment corresponds to the timing of the law change, nor is it apparent that the changing union contracts could explain the pattern.

In the regressions of Table 5, interactions with the coefficient of variation of (log) industry sales (over the whole period), a proxy for effective firing costs, are added to the basic specification of Tables 1-3. The results of random coefficients (four) are focused on (random effects results are similar, while in the random coefficients (all) specification coefficients on interactions with Var are almost all insignificant). The coefficient on the interactions of Var with lagged sales and employment (the interactions of Var*Up_S in the case of blue collar hours) suggest industries with greater variation in sales adjust employment more flexibly, as seems intuitive. For blue collar workers the interactions of Var*Before with lagged sales and employment although significant only at the 10 and 20% levels, have the signs expected if firing costs fell in 1985. The further interactions with Up_S are significant and opposite in sign. If the interactions of Var*Before are considered to have coefficients of zero, the coefficients on Var*Before*Up_S provide evidence against a fall in firing costs in 1985; if point estimates are relied upon, there is evidence for higher firing costs before 1985 affecting downward adjustment. The point estimates imply for an industry with coefficient of variation equal to 0.27 (the highest except for one outlier), the fall in half life after 1985 in response to a downward shock is 3%, while for an industry with average sales variation of 0.06, the fall is 1%. For white collar workers and blue collar hours the interactions with Var*Before are insignificant.

These regressions were repeated using the two other measures of sales variation (these results are not shown). Using the average of the coefficients of variation for four sub-periods, to avoid confounding volatile industries with those with long term trends, yielded very similar results. Using the measure based upon average month to month variation yielded quite different results: this time the relevant coefficients for blue collar hours are significant and indicate slower downward adjustment for high variation industries before 1985, but the relevant coefficients for blue collar workers are completely insignificant. This measure picks up short term volatility which has greater implications for hours, and the workers and hours results together suggest a change in flexibility in hours per worker is being reflected here.

Thus, the timing of hours flexibility changes noted in Table 4, and the coefficients on the Var interactions in Table 5 both indicate hours flexibility is increasing for reasons other than the law change. There is slim evidence based on coefficients on Var interactions significant at only the 10% level, that downward flexibility of blue collar workers may have increased after 1985 for high variation industries relative to low, which would be consistent with lower firing costs. If this is the case, however, the absence of change for blue collar hours suggests that the constraints on worker flexibility could be made up for by flexibility in hours per worker.

CONCLUSIONS

The analysis performed has been able to detect changes in the speed of employment adjustment in German manufacturing over the period 1977-1992, but the timing and direction of these changes, as well as the direction of relative changes in flexibility between industries with high and low sales variability, indicate that they are unlikely to have been caused by 1985 legislation aiming to reduce firing costs. These results are in broad agreement with earlier work on Germany of Abraham and Houseman (1994) and Kraft (1993), who also found no evidence that firing costs were lower after 1985. The lack of impact of the new legislation could either be evidence that firing costs have little effect, that fixed term contracts do not give firms freedom in a useful dimension, that the restrictions on rolling over fixed term contracts limited the usefulness of their availability, or that works councils or unions were able to pressure firms into converting more fixed term contracts into permanent ones than they wished. The direction of the changes is more compatible with the hypothesis that union contracts permitting more flexible hours per worker was an important factor, although the timing does not seem quite right. This hypothesis, as well as the effect of reduced standard working hours, remains to be investigated more fully.

Compared to the 1977-1981 period, adjustment of blue collar hours was more flexible from 1982-88, and less flexible in the subsequent period, and the relative flexibilities across industries of different sales variability did not change (for the preferred measure of sales variability). Estimation of blue collar worker equations was less reliable, due to the lack of suitable instruments, but some specifications suggest the adjustment of blue collar workers became less flexible in years following the new legislation. Examination of blue collar worker adjustment in high variability industries compared to low gives weak support to the hypothesis that firing costs were lower after 1985. If this is the case, however, under the higher firing costs employers must have been able to adjust hours per worker sufficiently that worker hours were unconstrained. White collar worker flexibility appeared to fluctuate over the period examined. Estimates of the magnitude of changes in adjustment half life vary from 1% to 20%, depending upon the specification and employment measure. Analysis of different industry groups separately, however, suggests that the results for all industries together mask different trends for different industries.

It was found that blue collar workers and hours adjust to shocks more slowly in seasonal upswings than in seasonal downswings, and that while blue collar workers adjust more slowly in business cycle upswings, blue collar hours adjust more quickly. Asymmetries in white collar worker adjustment were harder to identify.

It is important to note whether the trends in flexibility described come about through changes in downward or upward flexibility, in order to know their implications for average employment over the seasonal cycle. The changes identified in the paper imply in most cases a reduction in upward relative to downward flexibility, which tends to lower employment over a cycle, although the net effect of the changes will depend on the path of shocks.

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NOTES

1. Others countries were considered less suitable for study: France, for example, changed laws governing firing costs several times in the 1980s, sometimes reducing firing costs, sometimes increasing them. The time between the changes was considered too short for an assessment of their impact. Spain introduced changes similar to Germany's in 1984, but has only yearly data available.

2. See the data appendix for a full description of the data. Service sector data are unfortunately unavailable, as are firm-level data for the relevant period.

3. See the *European Industrial Relations Review* May 1985 and July 1985 for a good summary of the relevant laws.

4. See Abraham and Houseman (1993a).

5. See Schömann and Kruppe (1993).

6. For more details on relevant German institutions see Abraham and Houseman (1993a).

7. In the United States firms with a large number of recently laid-off employees claiming unemployment insurance pay a higher tax rate for the UI fund. This higher tax rate is therefore a firing cost in the same way as the European costs described above.

8. In reality, of course, quits are procyclical. This is not modelled.

9. Although the papers cited here estimate reduced forms of models using adjustments costs that are quadratic in net employment change, there exist papers examining other forms of adjustment cost, notably costs that depend on gross employment changes, and papers that perform structural estimation. See, for example, Hamermesh (1993b), Hamermesh and Pfann (1992), Pfann and Palm (1993).

10. Maximum likelihood estimation is not used as it is extremely slow.

11. The estimator for Δ need not be positive definite. In cases where it is not, the term in σ^2 is not subtracted. This means that the estimated Δ is no longer unbiased.

12. Employment measured in numbers of workers is measured at the end of the month, so sales during that month are used as SAL_{it-1}. For blue collar hours, which are the sum of hours over the month, sales from the previous month are used.

13. A possible solution to some of the problems is to instrument the wage with bargained industry wages for particular occupations. This is not pursued as so many other covariates also require instrumenting. A possible solution to the seasonal pattern is to use seasonally adjusted data, but it shall be seen that seasonal variation appears to play an important role for employment adjustment.

14. Information on materials prices is available, but classified by the industry of the firm, not the plant, like the other data.

15. The source for this variable is different from that of all the others - see the data appendix.

16. In general the Δ estimated for the random coefficients (all) both instrumented and uninstrumented is not positive definite, and is therefore corrected. See footnote 11.

17. A simpler definition of Up_b (whether sales in the previous month were more than 5% higher than sales a year before) yielded more significant coefficients, and evidence that blue collar workers adjust more slowly in business cycle upturns, while blue collar hours adjust more quickly. This definition requires dropping an additional eight months of data, however, which lowers the significance of interactions with BEF, the main focus of the paper. A third definition, based on the residuals of a regression of log sales on a polynomial in time as well as month dummies, yields results similar to those of the year to year change definition. For none of the Up_b definitions are the coefficients on Before*Up_b interactions significant.

DATA APPENDIX

All data except those on quits are published data on manufacturing obtained from the Statistisches Bundesamt. Firms with at least twenty employees are required to report the data to the statistical office at the end of each month. The data used here refer to establishments belonging to these firms. Nominal values are made real using the producer price index. Wages are computed from the wage bills: the blue collar monthly wage is the ratio of the blue collar wage bill and blue collar workers, blue collar hourly wage is the ratio of the blue collar wage bill and blue collar hours, and the white collar monthly wage is the ratio of the white collar wage bill and white collar workers. There exist no data on white collar hours. The months used are January 1977-December 1992. A small number of industries were excluded from the analysis due to problems with their data. Earlier data are not used due to a change in industry classification.

The quit rate by industry is calculated using individual level data from the German Socio-Economic Panel. The industry of each worker is known, and it is possible to tell if a worker quit a job between interviews, allowing an industry quit rate to be computed. The industry categories are much more aggregated than those used in the main analysis (there are twelve categories), but do not correspond to the Statistisches Bundesamt more aggregated categories, and an imperfect match must thus be made. The quit rates are calculated from data for 1984-1989, and the average for these years is used. The sample sizes are not enormous: the average manufacturing quit rate of 3.4% is based on 346 quitters, but the average quit rate tallies well with Büchtemann (1993).

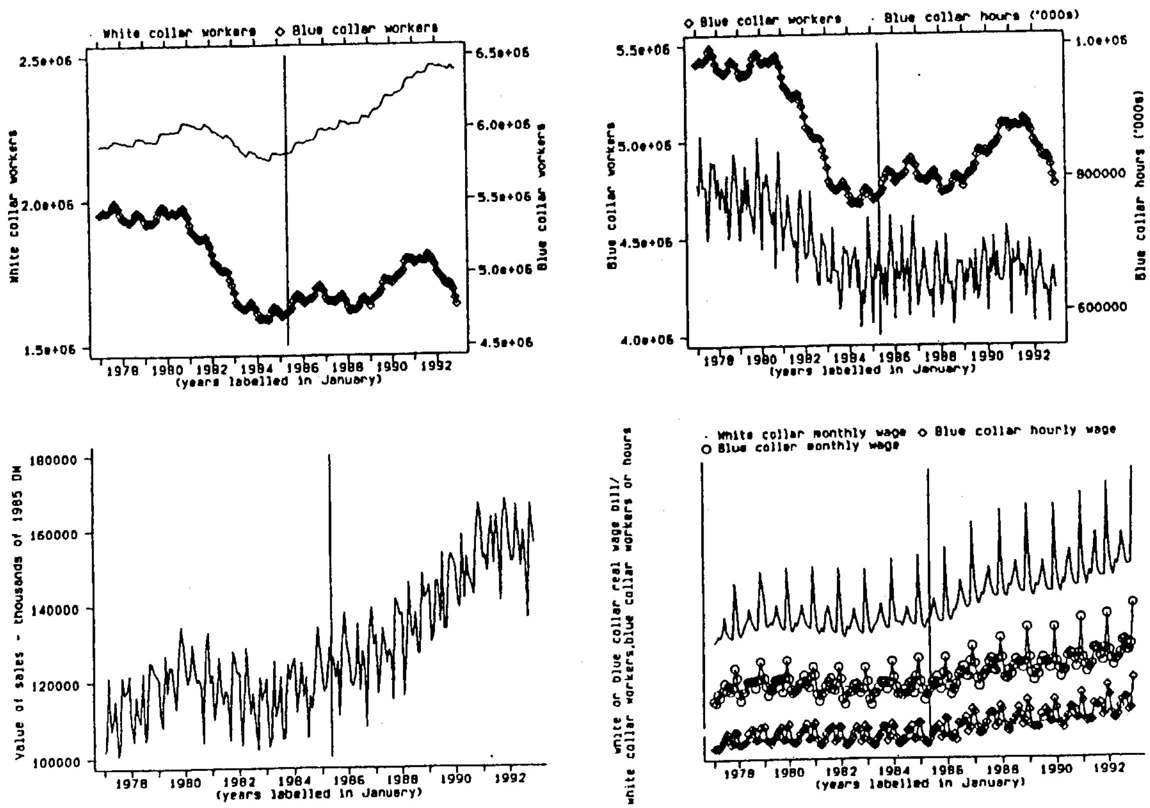

Figure 1: Aggregate Trends in Main Variables

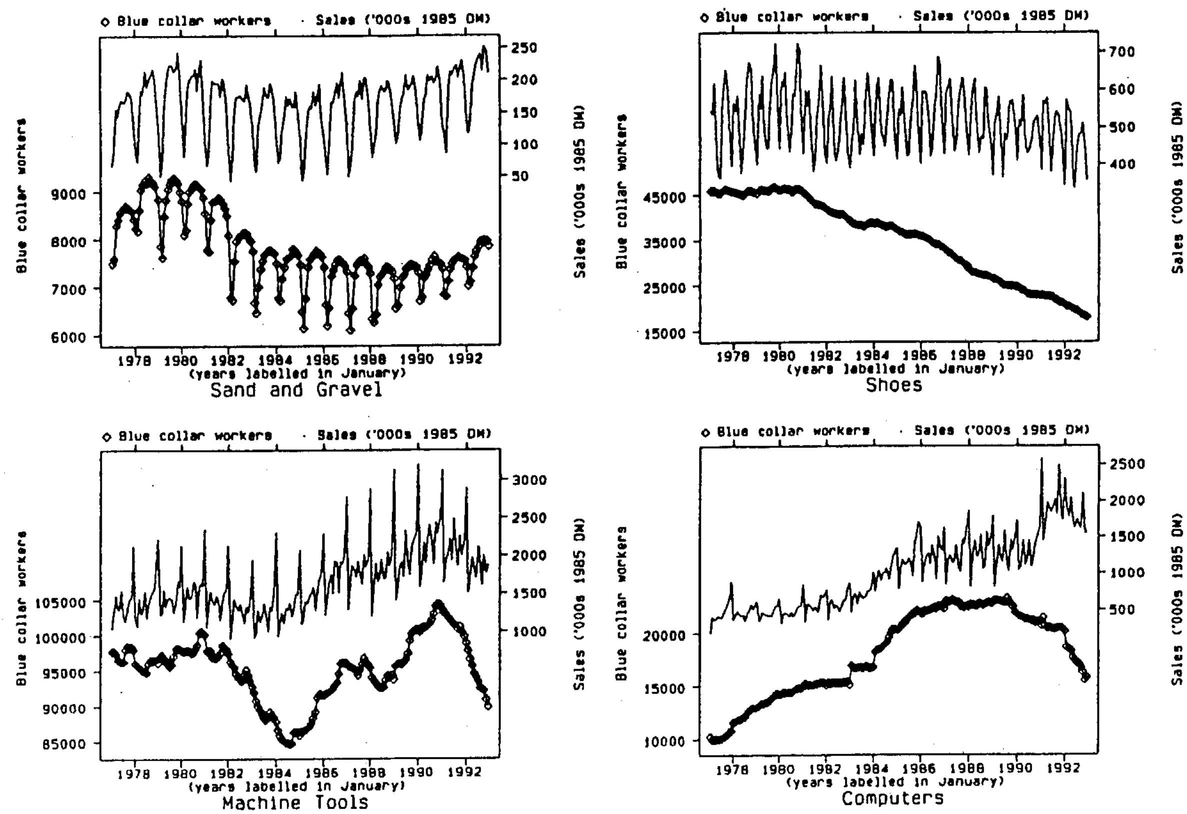

Figure 2: Employment and Sales in Selected Industries

Table 1: Determinants of Employment: Blue Collar Workers
(Standard Errors in Parentheses)

Variable	Random Effects	Random Effects IV	Random Coeffs (Four)	Random Coeffs (All)	Random Coeffs (All) IV
$Employment_{t-1}$	98.431 (0.067)	97.287 (0.306)	93.308 (0.719)	92.789 (0.707)	91.075 (1.579)
$Sales_{t-1}$	1.299 (0.047)	2.261 (0.194)	3.350 (0.421)	3.470 (0.418)	4.083 (0.490)
Time trend*10	-0.056 (0.003)	-0.071 (0.013)	-0.138 (0.017)	-0.140 (0.024)	-0.172 (0.057)
Before * $Empl_{t-1}$	-0.045 (0.016)	-0.041 (0.063)	-0.039 (0.022)	0.097 (0.202)	0.088 (0.220)
Before * $Sales_{t-1}$	0.022 (0.025)	0.024 (0.103)	0.009 (0.035)	-0.182 (0.383)	-0.133 (0.413)
Up_s * $Empl_{t-1}$	0.128 (0.017)	0.142 (0.062)	0.068 (0.016)	0.076 (0.255)	0.354 (0.315)
Up_s * $Sales_{t-1}$	-0.197 (0.027)	-0.232 (0.100)	-0.105 (0.024)	-0.082 (0.459)	-0.466 (0.611)
Before * Up_s * $Empl_{t-1}$	-0.051 (0.023)	-0.060 (0.083)	-0.042 (0.021)	-0.201 (0.243)	-0.314 (0.303)
Before * Up_s * $Sales_{t-1}$	0.075 (0.037)	0.095 (0.137)	0.065 (0.033)	0.314 (0.434)	0.416 (0.581)
Up_b * $Empl_{t-1}$	0.033 (0.012)	0.012 (0.048)	0.019 (0.011)	-0.072 (0.134)	-0.124 (0.164)
Up_b * $Sales_{t-1}$	-0.030 (0.019)	-0.002 (0.080)	-0.012 (0.017)	0.154 (0.245)	0.247 (0.320)
Hausman $\chi^2(12)$			87.6	4.7	

Notes:
a. 37128 observations on 201 industries from May 1977 to December 1992.
b. Variables other than dummies are in logs. All coefficients multiplied by 100.
c. Estimation is by GLS. The random coeffs (four) specification allows the intercept and the uninteracted variables to have a random coefficient. Random coeffs (all) allows all variables a random coefficient.
d. $Employment_{t-1}$ is the lag of the dependent variable. Before indicates the period May 1977-April 1985. Up_s is a seasonal indicator, while Up_b is a business cycle indicator (see text).
e. In the IV specifications terms involving lagged employment are instrumented with lagged exogenous variables (see text).
f. The Hausman test are of random coeffs (four) versus random effects, and random coeffs (all) versus random coeffs (four). The critical value is 21.0.

Table 2: Determinants of Employment: White Collar Workers
(Standard Errors in Parentheses)

Variable	Random Effects	Random Effects IV	Random Coeffs (Four)	Random Coeffs (All)	Random Coeffs (All) IV
$Employment_{t-1}$	98.536 (0.069)	98.732 (0.203)	94.155 (0.467)	93.050 (0.570)	95.339 (1.664)
$Sales_{t-1}$	0.935 (0.047)	1.239 (0.154)	2.347 (0.317)	2.314 (0.402)	1.991 (0.470)
Time trend*10	-0.023 (0.002)	-0.018 (0.009)	-0.069 (0.014)	-0.061 (0.017)	-0.085 (0.052)
Before * $Empl_{t-1}$	-0.040 (0.023)	-0.055 (0.073)	0.031 (0.032)	0.177 (0.290)	0.013 (0.332)
Before * $Sales_{t-1}$	0.033 (0.033	0.072 (0.109)	-0.068 (0.046)	-0.201 (0.510)	-0.054 (0.564)
Up_s * $Empl_{t-1}$	0.046 (0.025)	0.045 (0.081)	-0.003 (0.024)	-0.705 (0.369)	-0.693 (0.423)
Up_s * $Sales_{t-1}$	-0.067 (0.036)	-0.069 (0.120)	0.003 (0.034)	1.092 (0.626)	1.075 (0.705)
Before * Up_s * $Empl_{t-1}$	-0.047 (0.034)	-0.024 (0.109)	-0.030 (0.032)	0.311 (0.429)	0.084 (0.550)
Before * Up_s * $Sales_{t-1}$	0.063 (0.049)	0.026 (0.164)	0.044 (0.046)	-0.484 (0.731)	-0.153 (0.939)
Up_b * $Empl_{t-1}$	0.016 (0.017)	0.007 (0.054)	0.005 (0.017)	-0.083 (0.177)	0.034 (0.214)
Up_b * $Sales_{t-1}$	-0.016 (0.025)	-0.006 (0.081)	-0.004 (0.024)	0.129 (0.293)	-0.059 (0.353)
Hausman $\chi^2(12)$			121.5	22.8	

Notes:
a. 37128 observations on 201 industries from May 1977 to December 1992.
b. Variables other than dummies are in logs. All coefficients multiplied by 100.
c. Estimation is by GLS. The random coeffs (four) specification allows the intercept and the uninteracted variables to have a random coefficient. Random coeffs (all) allows all variables a random coefficient.
d. $Employment_{t-1}$ is the lag of the dependent variable. Before indicates the period May 1977-April 1985. Up_s is a seasonal indicator, while Up_b is a business cycle indicator (see text).
e. In the IV specifications terms involving lagged employment are instrumented with lagged exogenous variables (see text).
f. The Hausman test are of random coeffs (four) versus random effects, and random coeffs (all) versus random coeffs (four). The critical value is 21.0.

Table 3: Determinants of Employment: Blue Collar Hours
(Standard Errors in Parentheses)

Variable	Random Effects	Random Effects IV	Random Coeffs (Four)	Random Coeffs (All)	Random Coeffs (All) IV
$Employment_{t-1}$	83.273 (0.359)	96.141 (0.367)	51.013 (1.879)	40.352 (2.442)	54.089 (3.864)
$Sales_{t-1}$	4.834 (0.268)	-0.756 (0.268)	11.196 (1.101)	18.967 (1.893)	12.416 (2.212)
Time trend*10	-0.320 (0.016)	-0.043 (0.014)	-1.042 (0.091)	-1.220 (0.106)	-0.878 (0.147)
Before * $Empl_{t-1}$	0.379 (0.114)	0.164 (0.119)	0.328 (0.160)	6.525 (1.488)	3.459 (1.516)
Before * $Sales_{t-1}$	-0.467 (0.140)	-0.172 (0.150)	-0.347 (0.201)	-8.488 (2.031)	-4.391 (2.107)
Up_s * $Empl_{t-1}$	3.763 (0.119)	3.952 (0.142)	3.260 (0.109)	7.079 (1.179)	10.804 (1.857)
Up_s * $Sales_{t-1}$	-3.607 (0.146)	-3.787 (0.178)	-3.100 (0.134)	-7.901 (1.586)	-13.115 (3.276)
Before * Up_s * $Empl_{t-1}$	-0.896 (0.158)	-0.909 (0.188)	-0.634 (0.143)	-0.917 (1.419)	-2.589 (1.793)
Before * Up_s * $Sales_{t-1}$	0.934 (0.198)	0.924 (0.240)	0.649 (0.179)	0.713 (1.958)	2.883 (2.494)
Up_b * $Empl_{t-1}$	-0.109 (0.080)	-0.066 (0.079)	0.002 (0.076)	-0.407 (0.563)	-0.809 (0.803)
Up_b * $Sales_{t-1}$	0.118 (0.100)	0.065 (0.101)	-0.040 (0.095)	0.502 (0.810)	1.241 (1.388)
Hausman $\chi^2(12)$			470.2	41.0	

Notes:
a. 37128 observations on 201 industries from May 1977 to December 1992.
b. Variables other than dummies are in logs. All coefficients multiplied by 100.
c. Estimation is by GLS. The random coeffs (four) specification allows the intercept and the uninteracted variables to have a random coefficient. Random coeffs (all) allows all variables a random coefficient.
d. $Employment_{t-1}$ is the lag of the dependent variable. Before indicates the period May 1977-April 1985. Up_s is a seasonal indicator, while Up_b is a business cycle indicator (see text).
e. In the IV specifications terms involving lagged employment are instrumented with lagged exogenous variables (see text).
f. The Hausman test are of random coeffs (four) versus random effects, and random coeffs (all) versus random coeffs (four). The critical value is 21.0.

Table 4: Determinants of Employment and Hours - Additional Time Dummies
(Standard Errors in Parentheses)

Variable	Blue Collar Workers	White Collar Workers	Blue Collar Hours
$Employment_{t-1}$	92.713 (.710)	93.676 (.467)	49.379 (1.876)
$Sales_{t-1}$	3.375 (.422)	2.129 (.321)	8.399 (1.074)
Time trend*10	-0.164 (.018)	-0.070 (.014)	-1.066 (.095)
Up_s * $Empl_{t-1}$	0.041 (.021)	-0.032 (.032)	2.298 (.137)
Up_s * $Sales_{t-1}$	-0.062 (.034)	0.042 (.046)	-2.135 (.175)
Up_b * $Empl_{t-1}$	0.023 (.015)	0.003 (.023)	0.001 (.101)
Up_b * $Sales_{t-1}$	-0.025 (.024)	-0.008 (.034)	-0.054 (.129)
T2 * $Empl_{t-1}$	0.007 (.025)	-0.068 (.036)	-1.319 (.170)
T2 * $Sales_{t-1}$	-0.015 (.040)	0.071 (.053)	1.253 (.216)
T3 * $Empl_{t-1}$	0.069 (.035)	-0.113 (.049)	-1.146 (.249)
T3 * $Sales_{t-1}$	-0.042 (.056)	0.161 (.070)	0.968 (.315)
T4 * $Empl_{t-1}$	0.166 (.045)	0.020 (.062)	0.178 (.337)
T4 * $Sales_{t-1}$	-0.143 (.072)	0.001 (.089)	-0.203 (.418)
T2 * Up_s * $Empl_{t-1}$	-0.030 (.029)	0.000 (.043)	0.485 (.188)
T2 * Up_s * $Sales_{t-1}$	0.043 (.046)	0.007 (.063)	-0.486 (.240)
T3 * Up_s * $Empl_{t-1}$	0.050 (.030)	0.090 (.045)	0.975 (.199)
T3 * Up_s * $Sales_{t-1}$	-0.076 (.047)	-0.127 (.065)	-1.023 (.251)
T4 * Up_s * $Empl_{t-1}$	0.003 (.030)	-0.022 (.047)	0.715 (.206)
T4 * Up_s * $Sales_{t-1}$	-0.008 (.048)	0.034 (.067)	-0.709 (.257)
Hausman $\chi^2(20)$	117.4, 43.2	851.5, 68.0	6413, 335.7

Notes:

a. 37128 observations on 201 industries from May 1977 to December 1992.
b. Variables other than dummies are in logs. All coefficients multiplied by 100.
c. Estimation is by GLS random coefficients, allowing random coefficients in four variables.
d. $Employment_{t-1}$ is the lag of the dependent variable. Up_s is a seasonal indicator, while Up_b is a business cycle indicator. T2 indicates May 1981-April 1985, T3 May 1985-January 1989, T4 February 1989-December 1992.
e. The first Hausman test is of random coefficients (four) versus random effects, the second of random coefficients (all) versus random coefficients (four). The critical value is 31.4.

Table 5: Determinants of Employment and Hours - Interactions with Coefficient of Variation of Sales (Standard Errors in Parentheses)

Variable	Blue Collar Workers	White Collar Workers	Blue Collar Hours
Before * $Empl_{t-1}$	-0.072 (.032)	-0.015 (.046)	0.392 (.226)
Before * $Sales_{t-1}$	0.055 (.050)	-0.010 (.064)	-0.366 (.277)
Up_s * $Empl_{t-1}$	0.045 (.023)	-0.009 (.035)	3.299 (.153)
Up_s * $Sales_{t-1}$	-0.085 (.036)	0.002 (.048)	-3.261 (.184)
Before * Up_s * $Empl_{t-1}$	-0.011 (.031)	-0.043 (.047)	-0.598 (.201)
Before * Up_s * $Sales_{t-1}$	0.008 (.048)	0.058 (.065)	0.567 (.246)
Up_b * $Empl_{t-1}$	0.038 (.016)	0.039 (.025)	0.052 (.106)
Up_b * $Sales_{t-1}$	-0.031 (.025)	-0.045 (.034)	-0.071 (.131)
Var * $Empl_{t-1}$	-13.083 (3.470)	-18.354 (3.170)	-17.571 (16.299)
Var * $Sales_{t-1}$	15.267 (5.183)	20.843 (4.403)	15.553 (16.238)
Var * Before * $Empl_{t-1}$	0.888 (.505)	0.849 (.659)	1.432 (2.668)
Var * Before * $Sales_{t-1}$	-1.367 (.954)	-1.021 (1.077)	-3.967 (3.994)
Var * Up_s * $Empl_{t-1}$	0.067 (.398)	-0.303 (.539)	-3.676 (1.972)
Var * Up_s * $Sales_{t-1}$	0.403 (.711)	0.744 (.843)	7.893 (2.874)
Var * Bef*Up_s*$Empl_{t-1}$	-1.003 (.520)	0.104 (.730)	-2.326 (2.599)
Var * Bef*Up_s*$Sales_{t-1}$	1.934 (.952)	0.005 (1.160)	4.399 (3.967)
Var * Up_b * $Empl_{t-1}$	-0.109 (.270)	-0.441 (.378)	0.160 (1.350)
Var * Up_b * $Sales_{t-1}$	-0.181 (.498)	0.403 (.601)	-1.144 (2.063)
Hausman $\chi^2(22)$	1155, 3.92	1281, 30.5	488.3, 136.7

Notes:
a. 37128 observations on 201 industries from May 1977 to December 1992.
b. Variables other than dummies are in logs. All coefficients multiplied by 100. Covariates include $Employment_{t-1}$, $Sales_{t-1}$ and Time Trend.
c. Estimation is by GLS random coefficients, allowing random coefficients in four variables.
d. $Employment_{t-1}$ is the lag of the dependent variable. Before indicates the period May 1977-April 1985. Up_s is a seasonal indicator, while Up_b is a business cycle indicator. Var is the coefficient of variation of industry sales over the whole period.
e. The first Hausman test is of random coefficients (four) versus random effects, the second of random coefficients (all) versus random coefficients (four). The critical value is 33.9.