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Michael Keane

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Institute for Empirical Macroeconomics
Federal Reserve Bank of Minneapolis
250 Marquette Avenue
Minneapolis, Minnesota 55480

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INDIVIDUAL HETEROGENEITY AND INTERINDUSTRY WAGE DIFFERENTIALS

Michael P. Keane*

Institute for Empirical Macroeconomics
Federal Reserve Bank of Minneapolis

and University of Minnesota

ABSTRACT

Estimates of interindustry wage differentials are obtained using a fixed-effects estimator on a long panel, the National Longitudinal Survey of Young Men (NLS). After controlling for observable worker characteristics, 84 percent of the residual variance of log wages across industries is explained by individual fixed-effects. Only 16 percent of the residual variance is "explained" by industry dummies. Since no controls for specific job characteristics are used, job characteristics that vary across industries could potentially explain this rather small residual across-industry log wage variance that is not attributable to individual effects. Clearly then, these data do not force us to resort to noncompetitive explanations of interindustry wage differentials, such as efficiency wage theory. Furthermore, efficiency wage theories predict that wages in efficiency wage paying (or primary) industries should be relatively rigid. Therefore, industry wage differentials should widen in recessions. However, no such tendency is found in the data.

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1. Introduction

This paper provides more efficient and reliable estimates of the interindustry wage structure than those obtained in previous work by using a fixed effects estimator on a long panel, the National Longitudinal Survey of Young Men (NLS). It also provides the first estimates of labor force quality constant (or offer) wage movements at the 1-digit industry level over the business cycle.[1] The paper focuses on two questions concerning the interindustry wage structure that are of particular relevance to the efficiency wage literature. First, can industry differences in labor force quality explain industry wage differentials? Second, are the movements of these differentials over the business cycle consistent with the predictions of efficiency wage theory?

Competitive theories of the labor market imply that job attributes which do not affect the utility of workers should not affect their wages. However, there exists substantial evidence that workers with identical observable characteristics in jobs with identical observable characteristics receive different wages (on average) depending on the industry in which they are employed. These "interindustry wage differentials" have received a great deal of attention in recent years because they have been taken as evidence in favor of efficiency wage theories. In these theories some firms, because of characteristics of their industry (such as market structure or the production process), find it profitable to pay wages above the going rate for labor of the type they need to attract.

There are, however, potential competitive explanations for interindustry wage differentials. First, they may be due to unobserved differences in worker quality between industries. Second, they may be due to differences in job characteristics between industries which generate compensating wage differentials. Krueger and Summers (1988) present evidence that controlling for unobserved worker heterogeneity does not eliminate interindustry wage differentials. They also find that observed working condition variables do not explain industry wage differentials, while Murphy and Topel (1986) and Abowd and Ashenfelter (1981) find that industry differences in the probability and duration of unemployment cannot fully explain these differentials either.

[1]Heckman and Sedlacek (1986) examine manufacturing and nonmanufacturing wage movements over the cycle using selection bias correction techniques to control for variation in labor force quality as do Keane, Moffitt, and Runkle (1988). Quality constant cyclical wage movements at the 1-digit (11 industry) level have not been estimated.

The results of the present paper indicate that, after controlling for observed worker characteristics, industry dummies still explain 8.63 percent of the total variance in log wages in the NLS. However, unobserved differences in worker quality across industries explain a substantial 84 percent share of this residual interindustry log wage variance. Since no attempt is made to control for systematic differences in specific job characteristics across industries, it does not seem impossible that such differences may explain the 16 percent of residual across industry log wage variance that is not attributable to individual heterogeneity.[2] Clearly these data do not force us to resort to models with noncompetitive wage setting, such as efficiency wage models, as providing the only possible explanation for this rather small remaining portion of variance.

A fundamental problem with any attempt to prove that industry wage differentials are incompatible with competitive theory is that, while fixed effects estimators can be used to control for unobserved worker heterogeneity, it is impossible to control for unobserved characteristics of jobs. Therefore, competitive explanations for industry wage differentials based upon unobservable job characteristics and tastes can never be ruled out. In light of this problem, a more fruitful approach to testing efficiency wage theory is to ask, not whether unexplained wage differentials exist, but whether they behave in a manner consistent with the theory.

In this paper I consider the predictions of efficiency wage theory for the cyclical behavior of wage differentials. The various types of efficiency wage models all generate cyclical unemployment by making wages unresponsive to shocks. In the model of Weiss (1980), firms in primary industries where skilled labor is important in the production process avoid wage cuts following adverse demand shocks because "the best workers would quit." In the Akerlof and Yellen (1985) framework, efficiency wage paying firms have no great incentive to reduce wages following adverse nominal or real shocks because their losses from failure to do so are only second order (this results from the fact that effort increases with

[2]Although many empirical studies conclude that effects of measured job characteristics on wages are insignificant or have the wrong sign, Duncan and Holmlund (1983) find significant and properly signed effects in first difference equations. Specifically, they find that changes in twelve self-reported working condition variables explain 2.2 percent of the total variance of log wage changes in data from the 1968 and 1974 Swedish Level of Income Surveys (13.8 percent of the total variance is explained by changes in human capital variables). Duncan-Holmlund argue persuasively that differencing reduces bias resulting from the measurement errors and individual idiosyncrasies present in self reports of job characteristics.

wages, so the wage is a choice variable for these firms). In the Stiglitz (1985) framework, firms in primary industries have to be concerned about their wages relative to other firms in the industry. Following a unilateral wage reduction, such a firm would experience a higher quit rate and lower profits. So, due to coordination problems, primary industry firms tend not to cut wages following adverse nominal or real shocks.

Thus, in all three of these efficiency wage models, adverse real or nominal shocks cause a widening of wage differentials as firms in competitive (or secondary) industries reduce wages more than firms in efficiency wage paying (or primary) industries. Concurrent with this widening of differentials are layoffs from the primary sector and increased unemployment as queues for primary sector jobs increase in size. Thus, industry wage differentials should widen in recessions.[3]

In fact, the movement of industry wage differentials over the business cycle casts doubt on an efficiency wage interpretation for these differentials. The results of the present paper indicate that there is no systematic tendency for industry wage differentials to be greater in periods of higher unemployment. In fact, industry wage differentials have a slight tendency to narrow in recessions. Those industries with significantly negative wage differentials—wholesale trade, services, retail trade and agriculture—do not have above average wage procyclicality. Construction, one of the highest wage industries, has well above average wage procyclicality. Thus, contrary to the predictions of efficiency wage theory, real wage rigidity does not appear to be greater in high wage industries.[4]

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[3]The idea that a test for countercyclical variation in industry wage dispersion is a test of the proposition that wage premiums exist for equally skilled workers who obtain rationed jobs in "high-wage" industries is due to Wachter (1970). As he states ". . . high-wage industries can be viewed as hiring off a labor queue . . . As labor markets tighten . . . low wage industries attempt to improve their competitive standing by narrowing the wage structure." He goes on to argue that high-wage industries will allow wage structure compression, since they still have labor queues (although smaller ones) when labor markets tighten, and since their monopoly power gives them incentives not to change wages. Thus, wage differentials widen in recessions. As he points out (see his footnote 5), there is no reason to expect such countercyclical movement in industry wage dispersion if wage differentials are supported by competitive forces, because then there are no queues for high-wage jobs.

[4]The claim that industry wage differentials should widen in recessions may appear to contradict the fact that, in efficiency wage models with only one industry, increases in unemployment <u>reduce</u> the efficiency wage. For example, in Shapiro and Stiglitz (1984), workers value their jobs more when unemployment is high, because this implies long unemployment spells in the event of losing one's job. Thus, a lower wage is necessary to elicit work effort. This scenario may appear consistent with a <u>narrowing</u> of wage differentials in recessions. However, when a competitive (secondary) sector is added to the model, as in Bulow and Summers (1986), workers who cannot obtain primary sector jobs need not be unemployed.

2. Estimating Industry Wage Differentials

The first objective of this paper is to estimate interindustry wage differentials using a fixed effects estimator on NLS data. Using fixed effects is equivalent to estimating wage differentials solely from wage changes of industry changers. As Lang and Kahn (1990) point out: "If high wage industries employ better workers, industry wage differentials should largely disappear when measured only for a sample of changers while they should be largely unaltered if the efficiency wage model is correct."

Krueger-Summers estimate interindustry wage differentials using a first difference estimator on CPS data, which is similar to using fixed effects. However, there are two important problems with their dataset. First, the census bureau cannot match individuals who change their address during the year, so the 30 percent of respondents who move are lost (this is a potentially serious selection problem, because moving is probably highly correlated with industry switching and because the most mobile workers are being eliminated). Second, they tend to use sample periods of only a few years. Given sectoral shifts in labor allocation (see Lilien 1982), it is difficult to interpret wage differentials estimated from first differences using a short sample period because most labor flows may be in a single direction. For example, when Krueger-Summers look at the January 1984 CPS displaced workers survey, they find that only workers in the mining industry receive a significant wage differential, and that this is positive. However, during 1980-83 workers were leaving mining in large numbers due to declining demand for coal induced by falling oil prices (mining employment dropped by 7.3 percent according to BLS establishment data), so that transitions out of mining heavily outnumbered those into mining. Hence, the positive wage differential estimated for mining could just result from the wage declines of workers who were displaced from mining and therefore lost their industry-specific capital.

Both these problems can be avoided or mitigated by the use of NLS rather than CPS data. In the NLS workers who change geographic location are not eliminated from the sample, thereby avoiding problem one. Furthermore, workers are observed over a reasonably large number of years (16), so that

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They are only unemployed if they queue for primary sector jobs. In this more realistic multi-industry framework, Bulow-Summers find that adverse productivity shocks generate increases in the ratio of primary to secondary sector wages. Thus, relative wage differentials widen in recessions. This type of result is probably rather general, because we would expect the size of queues for primary sector jobs, and therefore the extent of unemployment, to be an increasing function of the difference in present value between primary and secondary sector jobs.

the sample period is not likely to be dominated by any one sectoral shock, thereby mitigating problem two. Because of the length of the NLS panel, it is possible to gauge the sensitivity of fixed effects estimates of wage differentials to the choice of sample period. Results obtained using different waves of the NLS data reveal that these estimates are sensitive to changes in sample period (see Table 4 below). Thus, it is essential to use a long panel to obtain reliable fixed effects estimates.

A problem with fixed effects or first difference estimation is that, if random industry misclassification error generates spurious industry transitions, then such estimators of industry wage differentials may be more sensitive to measurement error than the OLS estimator.[5] This problem can only be addressed under very special circumstances. Krueger-Summers implement a correction for measurement error, but the validity of this correction rests on a set of assumptions which certainly do not hold in their CPS data, and which also do not hold in the NLS data used here.[6] Murphy-Topel (1986), in their study of industry wage differentials, attempt an instrumental variable correction for measurement error, but the assumptions necessary for their instrument to be valid are also certainly violated.[7] Since the assumptions on which these measurement error corrections rest do not hold, I do not implement them here.

While an econometric solution to the measurement error problem is not available, one may try to reduce the problem by choice of data. There are two reasons why the true rate of industry transitions would be higher in the NLS than in the CPS. First, while the CPS is a random sample of the population,

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[5]Freeman (1984) shows (in the context of a union status dummy in a wage equation) that when error in measuring status is modest and the number of true status changers is fairly small, then the downward bias in cross-sectional estimates of the status dummy coefficient is less than that of fixed effects estimates.

[6]First, the Krueger-Summers procedure requires that the industry classification error rate be known. Dickens and Katz (1987) challenge the assumptions which Krueger-Summers use to estimate this error rate, and point out that their estimates are very sensitive to changes in these assumptions. Second, their assumption that "the change in industry status is probably orthogonal to the change in other independent variables" is invalid in the NLS, and probably in the CPS as well. Third, their assumption that "the distribution of industry employment is in a dynamic steady state" is certainly violated over the 1966–81 period of the NLS and also over Krueger-Summers period of 1974–75, 1977–78, and 1979–80 in the CPS.

[7]Murphy-Topel assume that if an individual reports the same time t industry in both the time t and time t+1 surveys then time t industry is measured correctly. Then, time t industry may be used as an instrument for industry change. However, suppose that due to the complexity of the operations of many large corporations, there is error in how respondents perceive their industry. Then, time t industry may well be measured with error even when it is consistent with time t−1 industry, and it will not be a valid instrument for the time t−1 to t industry change.

would be higher in the NLS than in the CPS. First, while the CPS is a random sample of the population, the NLS includes only young men, who are much more mobile. Second, unlike the CPS, the NLS does not omit from the sample the geographic movers—who are also the most likely industry movers. Thus, assuming the NLS does not have a higher rate of measurement error than the CPS, it will have a higher signal/noise ratio of true to false industry transitions, making it superior to the CPS for the purpose of studying wage differentials.[8]

Turning to the econometric techniques, I begin by specifying a wage equation of the form:

$$\ln W_{it} = X_{it}\beta + I_{it}D + \mu_i + \epsilon_{it}, \quad t=1,\ldots,T. \tag{1}$$

Here W_{it} is the real hourly wage of person i at time t, X_{it} is a vector of observable characteristics of worker i at time t and β is the corresponding coefficient vector, and ϵ_{it} is an i.i.d. error term. The μ_i are unobservable characteristics of workers (possibly correlated with the X_{it} and the I_{it}) which affect their productivity. I_{it} is a $1 \times J$ vector of indicator variables for whether person i locates in industry j at time t, $j=1, \ldots, J$. D is the corresponding vector of industry wage differentials.

If one simply estimates this equation by OLS, one will obtain biased estimates of the D_j unless the μ_i are uncorrelated with the regressors, including the industry specific constants in I_{it}. If, for example, workers with the highest values of μ_i tend to locate in industry j, an OLS regression will give an upward biased estimate of D_j. In this case, a positive value of D_j arises not because of efficiency wage payments, but only because unobserved labor force quality is high in industry j.

In order to control for unobserved labor force quality a fixed effects estimator is used. This is obtained by using OLS to estimate the transformed equation $\ln \tilde{W}_{it} = \tilde{X}_{it}\beta + \tilde{I}_{it}D + \tilde{\epsilon}_{it}$ where $\tilde{Z}_{it} = Z_{it} - T^{-1} \sum_{t=1}^{T} Z_{it}$. Here, the error term $\tilde{\epsilon}_{it}$ is i.i.d. and uncorrelated with the regressors, so OLS

[8]Existing estimates of spurious transition rates in the CPS are biased towards overstating these rates. For example, Murphy-Topel claim that most industry transitions in the CPS are spurious. They assume that anyone who, at time t, reports no change in job from time $t-1$ to t, while also reporting a different time $t-1$ industry than they reported previously in the time $t-1$ survey, must have an error in time $t-1$ industry classification. However, since the questions asked at time t about change in job from $t-1$ to t and industry at $t-1$ are retrospective, it is more likely that any response error occurred in these, rather than in the time $t-1$ current industry status response (for example, if someone recalled changing job and industry 13 months previously, when in fact it was only 11, the type of contradiction noted by Murphy-Topel would emerge).

estimates of β and D are consistent and efficient.[9] The difference $D_j - D_k$ may be interpreted as the average wage change of workers who move between industries j and k. If $D_j - D_k > 0$, then workers typically receive wage increases (decreases) when they move from k to j (j to k), and j is a "high wage" industry compared to k.

The second objective of the present paper is to estimate the movements in interindustry wage differentials over the business cycle. In the simple Solow (1979) efficiency wage model, efficiency wage paying firms set the wage so that the elasticity of worker productivity with respect to the wage is one. Thus, wages are completely determined by production technology and are rigid in the face of cyclical fluctuations. Extensions of this simple model generate wage smoothing rather than complete rigidity. As Carmichael (1990) points out, if efficiency wage paying industries have different production technologies, they set different wage levels. This breaks any deterministic relationship whereby higher wage firms always have more rigid wages. Nevertheless, wages in efficiency wage paying industries should be higher and more rigid than wages in secondary industries. Thus efficiency wage models do predict a widening of the wage differential between high wage primary industries and low wage secondary industries in recessions.

One may estimate cyclical movements in industry wage differentials using the wage equation:

$$(2) \qquad \ln W_{it} = X_{it}\beta + U_t I_{it}\alpha + I_{it}D + \mu_i + \epsilon_{it}$$

where U_t is the cyclical indicator (in this case the aggregate unemployment rate) and α is a $J \times 1$ vector of effects of the business cycle on the wage differentials, such that the differential for industry j at time t is $D_j + \alpha_j U_t$. The estimates of the α_j capture the cyclical behavior of industry wage differentials measured in the manner of Krueger-Summers and the present paper. As Heckman and Sedlacek (1985)

[9]This discussion assumes, of course, that the mean of ϵ_{it} conditional on i being employed in industry j in period t is zero. Otherwise, the coefficient estimates are subject to selection bias. To see how selection bias could arise, suppose that industries have different returns to unobserved ability and workers with high (low) unobserved ability are sorted gradually into high (low) return industries (because ability level is only gradually revealed to workers and/or employers over time). Then, sorting will tend to produce positive industry wage differentials for industries with high returns to unobserved ability (Gibbons and Katz 1987 develop such a model). Under this scenario, industry wage differentials would remain even after controlling for individual fixed effects. This type of selection cannot be controlled for, because industry specific individual effects cannot be identified separately from true industry effects. Differentials may also arise competitively if industries have differently sloped wage-experience profiles. Then, workers may consistently take wage cuts to move to industries with steeper profiles, and vice-versa.

and Keane, Moffitt and Runkle (1988) discuss, if a shock causes high or low wage workers to enter or leave a particular industry, then the average wage in that industry is affected by the change in labor force quality in addition to any direct effect of the shock. Thus, movements of the average wage in an industry will be biased estimates of shifts in the mean of the offer wage distribution.

Fixed effects estimates of the α_j control for cyclical variation in industry j labor force quality that arise if high or low unobserved ability workers (that is, workers with high or low μ_i values) are more or less likely to enter or leave industry j in a recession. Nevertheless, the α_j do not provide estimates of labor force quality constant cyclical wage movement in industry j. For example, if changes in U_t cause workers with systematically high or low values of the time varying unobserved productivity component (that is, high or low values of ϵ_{it}) to enter or leave industry j, then a fixed effects estimate of α_j will be a biased estimate of the change in the mean offer wage in industry j. Alternatively, if workers have industry specific unobserved ability components (μ_{ij}), or if there are industry specific returns to the human capital variables (β_j), then equation (2) does not provide consistent estimates of labor force quality constant industry wage movements.[10]

In this paper I obtain consistent estimates of changes in mean industry offer wages using a fixed effects version of the self selection model of Heckman (1974). In this model, the wage equation for an industry is estimated jointly with a probit choice equation which determines whether a worker locates in that industry. The model may be written as:

$$\ln W_{ijt} = X_{it}\beta_j + U_t\alpha_j + \mu_{ij} + \epsilon_{ijt}$$

$$\text{observed iff } I_{ijt} = 1$$

$$I^*_{ijt} = Z_{it}\gamma_j + U_t\delta_j + \psi_{ij} + \omega_{ijt} \tag{3}$$

$$I_{ijt} = \begin{cases} 1 \text{ if } I^*_{ijt} \geq 0 \\ 0 \text{ if } I^*_{ijt} < 0 \end{cases}.$$

[10]If there are industry specific μ_{ij} and workers with high or low μ_{ij} are more or less likely to enter or leave j in a recession, then the mean of μ_{ij} among workers employed in j is correlated with U_t, and α_j is affected by labor force quality variation. If β_j varies by industry, then controlling for X using the same β for all industries does not control for the effect of cyclical changes in observed labor force quality in industry j.

Here I^*_{itj} is the latent index of a probit employment equation which determines whether a worker is employed in industry j. Z_{it} is a row vector of regressors which affect probability of employment in industry j, and γ_j is the corresponding coefficient vector. δ_j measures the affect of changes in the unemployment rate on the probability of employment in industry j, ψ_{ij} is an individual fixed effect in the industry j employment choice equation.

The model of equation (3) is estimated by maximum likelihood, assuming that ϵ_{ijt} and ω_{ijt} have a bivariate normal distribution with correlation ρ_j and respective standard deviations σ_{ϵ_j} and 1 (the variance of the probit equation error must be normalized to 1 for identification). It can be shown that the OLS estimate of α_j is biased downward if ρ and δ_j have the same sign and upward if they have opposite signs. The selection model uses the estimate of ρ_j to adjust the estimate of α_j for the effect of selection bias.[11]

3. Data

The data set used in this study is the National Longitudinal Survey of Young Men (NLS). This is a nationally representative sample of 5,225 males aged 14 to 24 which was drawn in 1966. These individuals were interviewed in 12 of the 16 years from 1966 to 1981, with data collected on their employment status, wage rates and sociodemographic characteristics. I have restricted the sample to those at least 21 years of age at the interview date, who had completed their schooling and military service, and who had available data for all variables used in the study. The final analysis sample contains 4,439

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[11]An important point is that fixed effects selection model estimates are inconsistent for finite T, because estimates of the choice equation fixed effects are biased for finite T, and this bias is transferred to the wage equation estimates. Heckman (1981) provides Monte-Carlo evidence indicating that the bias is minor for $T > 8$. However, in the present case this turns out to be a moot point because, as will be seen in Section 4, the estimates of ρ_j obtained here are insignificant and close to zero for all eleven 1-digit industries. This obviates any practical problem of bias being transferred from the choice to the wage equation.

Finally, observe that if the individual effects in equations (1), (2), and (3) are uncorrelated with the regressors, then efficiency is lost by estimating fixed effects rather than random effects models. As will be apparent in Section 4, some coefficient estimates change substantially between the OLS and fixed effects estimators, indicating that fixed effects are in fact present. However, if some subset of the regressors is uncorrelated with the individual effects, it is possible to construct instrumental variable estimators that are more efficient than the fixed effects estimator (see Breusch, Mizon, and Schmidt 1989). This was not attempted in the present paper due to a desire to adhere to the estimation methodology used in previous work on industry wage differentials.

males and 23,927 person-year observations, giving an average of 5.4 observations per person. The sample used here is identical to that used by Keane, Moffitt and Runkle (1988) except that they used a random half sample while I use the full sample. A detailed description of the number of observations lost due to each data screen can be found in appendix B of Keane, Moffitt, Runkle.

Table 1 reports sample means for some of the key individual specific variables. The workers are classified into eleven 1-digit industries on the basis of 3-digit census industrial classification (CIC) codes. A complete list of industries, their corresponding CIC codes, and the sample size for each are reported in Table 2. The average rate of 1-digit industry transition from one wave to the next in the NLS is 19.5 percent.

The wage measure used in the analysis is an hourly straight-time measure in 1967 consumer price index dollars. This is a point-in-time wage measure, taken as of the date of the interview, rather than an annual measure (such as annual earnings divided by annual hours). Keane, Moffitt and Runkle describe the biases that can result from using annual wage measures. Because of these biases, it is preferable to use point-in-time wage measures such as those contained in the NLS rather than annual measures such as those in the Michigan PSID. A straight-time wage measure was used because it is not possible to construct a wage measure which includes overtime for every year of the data (see Keane, Moffitt and Runkle on this point). To adjust for nonwage compensation, such as variation in fringe benefits across industries, the hourly wage rate for each worker was multiplied by the ratio of total labor costs to wages in the corresponding industry, as in Krueger-Summers (the data are from the National Income and Product Accounts). The log of the real wage, denoted by WCPI, is used in all analysis (mean equal to 1.065 in 1967 CPI dollars). The unemployment rate (denoted by U-RATE) used in the analysis is the monthly national rate for all civilian workers 16 years and older (seasonally adjusted).

The specification of selection models is typically based on the assumption that the log wage equations should contain only variables that directly affect an individual's marginal product, and that the utility indices should include all these variables (since the wage enters the utility and is a determinant of employment status) plus additional ones that may affect hours of work and employment status independent of the wage. Since no variables used here obviously fall in the latter category, I have included the same controls in both the wage and choice equations. To implement the fixed effects estimator, any variables which are constant over time or collinear with the time trend must be excluded. This leaves the time trend, experience squared, WIFE, KIDS, SMSA, SOUTH, EDUC×EXPER, DEG×EXPER, and WHITE×EXPER as available controls. The list of controls used in OLS regressions is reported in the table of results.

4. Results

A. The Cross-Sectional Industry Wage Structure

As I noted in Section 2, the Krueger-Summers results based on the January 1984 CPS displaced workers survey, which they consider their most convincing results, are actually based on a sample of only a few years. In light of this, I have used the NLS to test the sensitivity of fixed effects estimates of wage differentials to the particular (short) sample period used. Fixed effects estimates using various pairs of waves from the NLS (always one or two years apart) are reported in Table 3. These results show a great deal of sensitivity to sample period. For example, the estimated differential for FIRE is positive 13 percent using 1975–76 data, but negative 12.2 percent using 1977–80 data. That for transportation and utilities is positive 15.2 percent using 1969–70 data, but only positive 5.4 percent using 1970–71 data. That for nondurable manufacturing is only positive 1.7 percent using 1978–80 data, but is positive 10.7 percent using 1980–81 data. And the negative differential in agriculture trends steadily downward over the whole set of periods. These results may stem solely from period-to-period variation in the net flow of workers out of the various industries due to sectoral shocks.

Regardless of the source of the year-to-year variation in estimated wage differentials, it is clear from Table 3 that the typical wage changes of movers between certain industries changes considerably from one year to the next. This contradicts the view advanced by Krueger-Summers that the structure of wages is remarkably stable over time, but does not necessarily indicate that industry wage differentials have any long term trends. Given these results, it is obvious that long sample periods are necessary in order to obtain reasonable estimates of average industry wage differentials.

Another problem with the CPS data that was noted in Section 2 is that the roughly 30 percent of respondents who move during the year are eliminated from the sample. To gauge the importance of this problem, the analysis of Table 3 was repeated with a geographic mover screen applied to the NLS data. These results are reported in Table 4. Comparing the Table 3 and 4 results, it is clear that may of the estimated wage differentials are sensitive to the geographic mover screen. For example, compare the transportation and utilities differentials for 1970–71, 1975–76, and 1978–80, the FIRE differentials for 1970–71 and 1975–76, the durable manufacturing differentials for 1978–80, or the agricultural differentials for 1975–76.

In light of the obvious problems created by short sample periods and geographic mover screens in the CPS, I turn to the relatively long NLS panel (12 surveys over 16 years) in order to obtain more reliable fixed effects estimates of industry wage differentials. First, table 5 reports the results of OLS estimates for comparison purposes. The large group of controls for observable worker heterogeneity (see table) explain only 33.4 percent of the variance in log real wages. Four occupation dummies explain an additional 1.81 percent of variance. Given these controls, the 10 industry dummies (government is the omitted industry here and throughout the rest of the paper) explain a substantial 8.63 percent of the total variance in log real wages. The estimated wage differentials are not only highly significant, but large in magnitude. For example, the column 3 estimates imply that workers in retail trade have wages approximately 26.8 percent lower than workers in government, observable characteristics being equal.

Fixed effects estimates are reported in Table 6. Comparing the first columns of Tables 5 and 6, observe that individual fixed effects (that is, unobservable individual productivity differences) explain an enormous 41.5 percent of the variance of log wages in the NLS data. After controlling for individual effects, the industry dummies explain only 1.38 percent of the total variance of log wages. Comparing this with the 8.63 percent of variance explained in the OLS regression, observe that 84 percent of the variance in log wages across industries not explained by observed characteristics is explained by unobserved differences in labor quality.[12]

Recall that Krueger and Summers' argument in favor of efficiency wage theory rests primarily on the assertion that the variance of wages explained by industry dummies is simply too great to be plausibly attributable to the industry dummies acting as proxies for individual effects and unobserved job

[12]Another way to look at this is that (in the models including occupation dummies) the sum of squared residuals drops from 3026.3 to 1100.8 when both fixed effects and industry dummies are included. When the industry dummies are included first, they account for 21.0 percent of this drop. When they are included last they account for only 3.4 percent of the drop. The total drop in unexplained log wage variance is 41.2 percent, and 21.0 percent of this is 8.63 percent, while 3.4 percednt of this is only 1.38 percent.

characteristics. Since the Table 7 results were obtained without any controls for specific job characteristics, it does not seem impossible that differences in unobserved characteristics of jobs across industries can explain the remaining 16 percent of interindustry log wage variance (or 1.38 percent of total log wage variance) in the NLS data that is not attributable to individual effects. Thus, it is not at all obvious that the data force us to resort to noncompetitive wage setting to explain the interindustry wage structure.

Exactly the same argument I make here is made by Murphy and Topel (1986). They use matched CPS data from 1977–84 and regress wage changes of industry-occupation movers on experience level and the implied wage change given by industry-occupation dummy coefficients estimated from a cross-section. A coefficient of one on the implied industry-occupation differentials indicates that these are pure industry-occupation effects, while a coefficient of zero implies pure ability sorting. Their preferred estimate is 0.365, indicating that 63.5 percent of the observed industry-occupation wage differentials are due to differences in unobserved ability. However, Dickens and Katz (1987) criticize Murphy-Topel for confounding industry and occupation effects: ". . . those being promoted to higher paying occupations move from relatively high paying jobs in the occupations that they are leaving to relatively low paying jobs in the occupation they are joining. Thus, their wages would not be expected to go up by the full difference between the average wages of the two occupations." This same criticism of Murphy-Topel is also made by Krueger-Summers. This criticism does not apply to the present study, because industry effects are estimated separately from occupation effects.

Even though individual effects explain most of interindustry log wage variance, several of the industry dummies (and the set of industry dummies as a whole) remain highly significant after controlling for these effects. The substantial positive wage differentials of durable manufacturing, construction, and transportation-utilities industry workers over government workers essentially disappear in the fixed effects estimates. However, mining continues to have a significant positive differential and the service sector industries (wholesale and retail trade, FIRE, and services) as well as agriculture continue to have significant negative differentials relative to government. The significance of industry dummies is not in itself evidence for noncompetitive wage setting, because, as I have previously argued, the possibility that these dummies simply proxy for unobserved job characteristics cannot be ruled out.

Another possible explanation for the remaining differentials is that slopes of the industry wage-experience profiles may be influenced by deferred payment schemes (see Lazear 1981). Because the extent of payment deferment may differ among industries, and since only the beginning of the life cycle is observed in the NLS (workers are from 21 to 39 years old in the data) this could generate spurious industry differentials. To address this issue, I plotted the estimated wage-experience profiles by industry. In general, the wage-experience profiles move roughly in parallel, so it does not appear that differences in slopes of wage-experience profiles are biasing the estimated wage differentials in Table 6.[13] The one notable exception is mining, where the positive wage differential clearly diminishes with experience.

Since both a time trend and experience cannot be included in a fixed effects model, the estimates of industry wage-experience profiles may confound time and experience effects if wage differentials are not fixed through time. Whether wage differentials vary through time is an interesting question in itself. Sectoral shocks, which require reallocation of labor across industries, may generate short run wage differentials which are eliminated in the long run by labor migration. Table 7 presents estimates of trends in industry wage differentials. These results give no indication that the differentials are closing over time. Of the five industries with significant negative differentials, only wholesale trade and services have significant trends in their differentials, and that for services is negative. Most of the estimated trend effects are insignificant and small in magnitude.[14] Note, however, that the failure of industry wage differentials to close over time is not really evidence in support of efficiency wage theory. If industry wage differentials are supported primarily by differences in unobserved labor force quality, as is indicated

[13]This parallel movement of the wage-experience profiles is an additional contradiction of efficiency wage theory if the same considerations which lead to efficiency wage payments also generate rising wage-experience profiles. It has been pointed out by Carmichael (1985) that posting of performance bonds by primary sector workers would obviate the need for efficiency wage payments to elicit work effort. Bulow and Summers argue that bonding cannot solve the effort elicitation problem because of liquidity constraints and moral hazard problems. However, following Lazear (1981), they state that ". . . a number of the features of actual primary sector firms may perform some of the same functions that bonding might perform. In particular, these firms are characterized by rising age-wage profiles, while similar phenomena are not observed in the secondary sector." Contrary to this view, however, the wage-experience profiles of the "low-wage" industries are not noticeably flatter than those of the "high-wage" industries.

[14]Note that there is no trend in the agriculture differential, as had appeared to be the case in the sequence of single period estimates reported in Table 3. It is important to note that industry wage trends reported in Table 6 are relative to a common trend. This was estimated at 0.039 with a standard error of 0.004. The rather high value of the common trend results from the aging of the sample.

by the results in Tables 5 and 6, one would not expect them to have long term trends. Nor would one expect the differentials not explained by individual effects to have long term trends if they are supported by differences across industries in job characteristics.

B. Cyclical Variation in the Industry Wage Structure

I next turn to the issue of how wage differentials move over the business cycle. OLS results in table 8 show significant procyclical real wage movement in construction, retail trade and services, and significant countercyclical real wage movement in transportation and utilities. These results change substantially when fixed effects are included in the wage equation. The fixed effects results in the 4th column show significant procyclical real wage movement in construction, services, and mining (and marginally significant countercyclical real wage movement in agriculture).

The last three columns of Table 7 present the wage and choice equation estimates of the U-RATE coefficient as well as the implied derivatives of industry employment probabilities with respect to U-RATE, obtained from the selection adjusted fixed effects models. A separate selection model is estimated for each industry as described in Section 2. In every industry, the estimate of ρ_j was insignificant and close to zero. Thus, the only source of difference between the fixed effects results and the fixed effects selection model results is that the selection models are estimated separately by industry. This allows for industry specific β_j and μ_{ij} (see equation (3)). As was discussed in Section 2, it is necessary to allow for industry specific β_j and μ_{ij} in order to estimate labor force quality constant industry wage movements.

According to the fixed effects selection model estimates, there is significant procyclical real wage movement in construction, FIRE and services (and marginally procyclical movement in durable and nondurable manufacturing). Particularly important biases resulting from failure to allow for industry specific β_j and μ_{ij} are apparent in FIRE, services and mining.

If high wage industries are industries which pay efficiency wages, then high wage industries should exhibit relatively rigid real wages (that is, real wages should fall less in these industries in response to adverse nominal or real shocks than they do in the low wage, competitive, industries). Then, industries with (positive) negative wage differentials should have (positive) negative U-RATE coefficients relative to the mean U-RATE coefficient. Table 9 presents summary statistics derived from tables 6 and 8 that address this issue. The industry dummy coefficients from Table 6, column 4 were used to construct industry wage differentials (deviations from the industry size weighted mean dummy coefficient). In Table 9, industries are ranked according to their differential. Wage cyclicality differentials were also constructed from the U-RATE coefficients in Table 8, and are reported in the last two columns of Table 9. These are deviations from the industry size weighted mean U-RATE coefficient, so a negative value indicates above-average procyclicality.

The results in Table 9 show no clear pattern of high (low) wage industries having less (more) procyclical wages. According to the fixed effects estimates of the U-RATE coefficient, among the six industries with above-average wages mining and construction have greater than average procyclicality, while transportation and government have less than average procyclicality. Among the "low wage" industries, only services has above-average procyclicality. The size weighted correlation of wage differential rankings with wage cyclicality rankings is only -0.16 (this should be -1 given a perfect pattern of higher-wage industries having less procyclical wages). The size-weighted correlation of the wage differentials with the cyclicality differentials is only 0.15 (this should be 1 given a perfect pattern of industries with lower wages having more procyclical wages).

To give an idea of the magnitude of the increase in industry wage differentials during a recession implied by the U-RATE fixed effects point estimates, Table 10 also reports the standard deviation (σ) of log wages across industries both at the mean of the data and following a 3-point increase in the U-RATE variable. The standard deviation of log wages across industries increases from 0.105 to 0.110, indicating that the relative wage position of an industry with wages 1 standard deviation above the mean improves by only about 0.5 percent.

While the fixed effects results indicate a slight widening of wage differentials in recessions, the fixed effects selection models tell a different story. Services, which was the only "low wage" industry to have above average wage procyclicality according to the fixed effects estimates, has roughly average procyclicality according to the fixed effects selection model estimates. Mostly due to this change, the size weighted correlation of wage differential rankings and wage cyclicality rankings goes essentially to zero, while the size-weighted correlation of industry wage differentials with cyclicality differentials goes to -0.16. This negative value indicates that a slight <u>narrowing</u> of industry wage differentials occurs in recessions once industry level variation in labor force quality is controlled for.

In contrast to the result here, the existing aggregate data studies find that wage differentials widen in recessions (see Okun 1982, Hall 1975, and Wachter 1970). In particular, Hall found that the aggregate wage is most strongly procyclical in retail trade, wholesale trade, FIRE and services—all low-wage industries according to the aggregate data. The results here indicate that after correcting for cyclical variation in labor force quality, only FIRE among these four industries has significantly above average procyclical wage movement. And, after controlling for individual characteristics, FIRE is found to be an average rather than a low wage industry in micro data.

5. Conclusion

In NLS data, unobserved differences in labor force quality explain fully 84 percent of the residual variance in log wages across industries that is not explained by observable worker characteristics. This is true in models which contain no controls for specific characteristics of jobs which may vary systematically across industries. Thus, it is not clear that these data force us to resort to noncompetitive wage setting in order to explain the remaining 16 percent of across-industry residual log wage variance. Given the great difficulty involved in measuring job characteristics, it is probably impossible to convincingly demonstrate that they cannot explain this residual variance. Furthermore, the paper's use of a specialized sample (young men) actually strengthens the finding that individual effects explain such a high proportion of interindustry wage variance for two reasons: First, the predictions of competitive theory for the behavior of industry wage differentials are the same within any one demographic group as they are for the population as a whole, and second, the use of a single demographic group should, if anything, make individual heterogeneity less important.

The results of the present paper also contradict a well-established stylized fact concerning labor market dynamics: that industry wage differentials widen in recessions. On the contrary, it was found that after controlling for industry labor force quality, industry wage differentials are essentially acyclical. This is evidence against efficiency wage theories of industry wage differentials, because such theories predict that real wages should be relatively rigid in high wage industries where firms pay efficiency wages. Thus, adverse real or nominal shocks should produce smaller wage reductions in these industries than in low wage industries where wages are set competitively, causing wage differentials to widen in recessions.

Table 1

Means of Variables in NLS Analysis Samples

Variable	Mean
Log real wage -- WCPI	1.065
Real Price of refined petroleum -- OIL	1.53
Unemployment rate -- U-RATE	6.38
Education (years) -- EDUC	12.57
Labor market experience (years since leaving school or military) -- EXPER	7.90
Experience squared -- EXPER^2	87.05
White race dummy -- WHITE	0.74
Wife present dummy -- WIFE	0.69
SMSA resident dummy -- SMSA	0.70
South resident dummy -- SOUTH	0.41
Children in household -- KIDS	1.30
College degree dummy -- DEG	0.23
Employed dummy	0.89
Occupation Dummies:	
Professional and Technical Workers (0-370)	0.31
Craftsmen and Foremen (401-545)	0.19
Salesmen (380-395)	0.05
Services (801-890)	0.05
Operatives, Laborers, Farmers (200-222, 601-775, 901-985)	0.29

NOTE: Census 3-digit occupation codes are used.

Table 2

Sample Size by Industry

Industry	CIC Codes	Person-Year Observations	Percentage of Total Observations	Fixed Effects Model Sample Size
Durable Manufacturing	206–296	4693	.201	3258
Nondurable Manufacturing	306–459	2580	.108	1832
Construction	196	2217	.093	1688
Transportation & Utilities	506–579	1852	.077	1193
Wholesale Trade	606–629	1039	.043	888
Retail Trade	636–696	2343	.098	1713
Finance, Insurance, Real Estate	706–736	833	.035	601
Services	806–898	3252	.136	1935
Government	906–998	1389	.058	836
Agriculture	16–18	535	.022	379
Mining	126–156	327	.014	250
Employment with Industry n/a	–	143	.006	–
Unemployed	–	2724	.114	–

NOTE: Person-year observations are for employed workers only. Total is 21,004. The number of person-year observations on workers employed in an industry that are effectively used to estimate the fixed effects models is less than the total number of person-year observations because workers who are always employed in the same industry (that is, workers who never move) do not contribute to the estimates.

Table 3

Fixed Effects Estimates of Industry Differentials, Selected One and Two Year Periods

	1969–1970	1970–1971	1973–1975	1976–1976	1978–1980	1980–1981
Durable Manufacturing	.076	.079	.084**	.077**	.089**	.059
	(.057)	(.058)	(.027)	(.032)	(.027)	(.036)
Nondurable Manufacturing	.017	−.027	.030	.059*	.017	.107**
	(.060)	(.058)	(.028)	(.033)	(.029)	(.040)
Construction	.047	.109*	.066**	.006	.071**	.037
	(.059)	(.056)	(.028)	(.032)	(.029)	(.036)
Transportation, Utilities	.152**	.054	.065**	.070**	.044	−.001
	(.065)	(.066)	(.030)	(.035)	(.028)	(.041)
Wholesale Trade	−.079	−.002	−.045	−.041	−.031	.011
	(.066)	(.062)	(.030)	(.034)	(.029)	(.039)
Retail Trade	−.115**	−.132**	−.122**	−.076**	−.143**	−.132**
	(.057)	(.058)	(.027)	(.030)	(.029)	(.039)
Finance, Insurance, Real Estate	−.007	.100	−.040	.130**	−.122**	−.019
	(.073)	(.076)	(.035)	(.042)	(.036)	(.050)
Services	−.069	−.127**	−.107**	−.103**	−.069**	−.100**
	(.058)	(.055)	(.026)	(.029)	(.025)	(.035)
Government	.006	.102	.055	−.071	.067	.018
	(–)	(–)	(–)	(–)	(–)	(–)
Agriculture	−.473**	−.344**	−.232**	−.209**	−.181**	−.008
	(.082)	(.075)	(.041)	(.047)	(.047)	(.066)
Mining	.113	.126	.069	.021	−.028	−.041
	(.083)	(.100)	(.046)	(.051)	(.047)	(.063)

NOTE: The reported differentials are not industry dummy coefficients but deviations of these dummy coefficients from the industry size weighted mean dummy coefficient. Government is the omitted industry. Standard errors are in parentheses. A ** indicates significance at the 5% level; A * indicates the 10% level. Controls are a time trend, experience squared, a marriage dummy, number of children, an SMSA dummy, a South dummy and interactions of experience with education, a college degree dummy and a race dummy. Note that variables which are constant over time or collinear with the time trend must be eliminated.

Table 4

Fixed Effects Estimates of Industry Differentials, Selected One and Two Year Periods:

Geographic Movers Screened from Sample

	1969–1970	1970–1971	1973–1975	1975–1976	1978–1980	1980–1981
Durable Manufacturing	.045	.091	.105**	.054	.043	.058
	(.073)	(.075)	(.034)	(.036)	(.031)	(.038)
Nondurable Manufacturing	.047	−.052	.006	.046	.024	.071*
	(.075)	(.073)	(.035)	(.038)	(.034)	(.043)
Construction	.040	−.021	.052	.000	.087**	.068*
	(.072)	(.070)	(.035)	(.036)	(.033)	(.039)
Transportation, Utilities	.212**	.264**	.071*	−.015	.087**	.002
	(.082)	(.086)	(.038)	(.040)	(.031)	(.042)
Wholesale Trade	−.001	.000	−.069*	−.005	−.027	.031
	(.082)	(.077)	(.037)	(.038)	(.033)	(.041)
Retail Trade	−.033	−.144**	−.154**	−.092**	−.166**	−.126**
	(.076)	(.073)	(.033)	(.035)	(.034)	(.041)
Finance, Insurance, Real Estate	.007	.235**	.017	.243**	−.179**	.039
	(.088)	(.100)	(.044)	(.048)	(.044)	(.055)
Services	−.079	−.097	−.083**	−.070**	−.041	−.121**
	(.074)	(.074)	(.032)	(.033)	(.029)	(.038)
Government	−.118	−.041	.028	−.041	.107	.040
	(–)	(–)	(–)	(–)	(–)	(–)
Agriculture	−.640**	−.389**	−.209**	−.080	−.185**	−.119
	(.099)	(.100)	(.049)	(.053)	(.052)	(.075)
Mining	.077	.102	−.052	.010	−.008	−.027
	(.098)	(.109)	(.052)	(.054)	(.054)	(.072)

NOTE: The reported differentials are not industry dummy coefficients but deviations of these dummy coefficients from the industry size weighted mean dummy coefficient. Government is the omitted industry. Standard errors are in parentheses. A ** indicates significance at the 5% level; A * indicates the 10% level. Controls are a time trend, experience squared, a marriage dummy, number of children, an SMSA dummy, a South dummy and interactions of experience with education, a college degree dummy and a race dummy. Note that variables which are constant over time or collinear with the time trend must be eliminated.

Table 5

Estimated Industry Dummies: OLS

Variable	(1)	(2)	(3)	(4)
Durable Manufacturing			0.071**	0.047**
			(0.011)	(0.012)
Nondurable Manufacturing			0.021*	−0.002
			(0.012)	(0.013)
Construction			0.125**	0.079**
			(0.013)	(0.013)
Transportation and Utilities			0.111**	0.086**
			(0.013)	(0.013)
Wholesale Trade			−0.080**	−0.126**
			(0.015)	(0.016)
Retail Trade			−0.224**	−0.268**
			(0.012)	(0.013)
Finance, Insurance, and Real Estate			−0.001	−0.062**
			(0.016)	(0.016)
Services			−0.213**	−0.241**
			(0.018)	(0.012)
Government			0.000	0.000
			(–)	(–)
Agriculture			−0.453**	−0.455**
			(0.019)	(0.019)
Mining			0.216**	0.206**
			(0.022)	(0.023)
Occupation Dummies	NO	YES	NO	YES
Sum of Squared Residuals	3111.2	3026.3	2682.6	2623.7
R^2	0.3337	0.3518	0.4255	0.4381

NOTE: Standard errors are in parentheses. A ** indicates significance at the 5 percent level. A * indicates significance at the 10 percent level. Data set is the NLS, 1966-81. Sample size = 21,004. Controls are a time trend, education, experience and its square, four dummies for types of college degrees, five dummies for fields of degree, an SMSA dummy, a south dummy, a race dummy, a marriage dummy, number of children, and interactions of experience with education, a college degree dummy and a race dummy. Total sum of squares is 4669.0.

Table 6

Estimated Industry Dummies: Fixed Effects

Variable	(1)	(2)	(3)	(4)
Durable Manufacturing			0.009 (0.015)	−0.004 (0.016)
Nondurable Manufacturing			−0.019 (0.016)	−0.031* (0.016)
Constructions			0.041** (0.016)	0.024 (0.016)
Transportation and Utilities			0.029* (0.017)	0.019 (0.017)
Wholesale Trade			−0.125** (0.017)	−0.141** (0.017)
Retail Trade			−0.205** (0.016)	−0.219** (0.016)
Finance, Insurance, & Real Estate			−0.067** (0.019)	−0.088** (0.020)
Services			−0.191** (0.015)	−0.199** (0.015)
Government			0.000 (−)	0.000 (−)
Agriculture			−0.365** (0.022)	−0.373** (0.023)
Mining			0.066** (0.025)	0.055** (0.026)
Occupation Dummies	NO	YES	NO	YES
Sum of Squared Residuals	1171.9	1165.4	1104.2	1100.8
R^2	0.7490	0.7504	0.7635	0.7642

NOTE: Standard errors are in parenthesis. A ** indicates significance at the 5 percent level. A * indicates significance at the 10 percent level. Data set is the NLS, 1966-81. Sample size = 21,004. Controls are the same as those in Table 5 except that to implement the fixed effects estimator, any variables which are constant over time or collinear with the time trend must be eliminated. This leaves the time trend, experience squared, a marriage dummy, number of children, an SMSA dummy, a South dummy and the experience interactions. Total sum of squares is 4669.0.

Table 7

Trends in Industry Wage Differentials

	OLS Estimates		Fixed Effects Estimates	
	Dummy Coefficient	Trend	Dummy Coefficient	Trend
Durable Manufacturing	.063**	−.0015	.000	−.0002
	(.027)	(.0026)	(.025)	(.0022)
Nondurable Manufacturing	−.008	.0012	−.025	−.0003
	(.029)	(.0028)	(.027)	(.0024)
Construction	.163**	−.0096**	.045	−.0024
	(.030)	(.0029)	(.028)	(.0024)
Transportation and Utilities	.054*	.0038	.007	.0015
	(.031)	(.0030)	(.029)	(.0025)
Wholesale Trade	−.126**	.0004	−.186**	.0055**
	(.035)	(.0034)	(.031)	(.0028)
Retail Trade	−.217**	−.0057**	−.211**	−.0007
	(.029)	(.0029)	(.027)	(.0024)
Finance, Insurance, and Real Estate	−.029	−.0033	−.123**	.0041
	(.039)	(.0038)	(.034)	(.0031)
Services	−.145**	−.0103**	−.140**	−.0066**
	(.029)	(.0028)	(.026)	(.0023)
Government	.000	.0000	.000	.0000
	(–)	(–)	(–)	(–)
Agriculture	−.442**	−.0009	−.394**	.0034
	(.039)	(.0041)	(.036)	(.0034)
Mining	.279**	−.0078	.116**	−.0065
	(.052)	(.0050)	(.048)	(.0042)
SSR	2616.1		1098.5	
R^2	.440		.765	

NOTE: Standard errors are in parenthesis. A ** indicates significance at the 5% level. A * indicates the 10% level. Controls for OLS regression same as in Table 5. Controls for fixed effects estimates same as Table 6. Occupation dummies are included. The trend coefficients reported are from interactions of the time trend with the industry dummies, government being the omitted industry.

Table 8
Cyclical Movements in Industry Log Wage Differentials

					Selection Model Fixed Effects		
	OLS		Fixed Effects		U-RATE		
Industry	Dummy	U-RATE	Dummy	U-RATE	Wage Equation	Choice Equation	∂P/∂ U-RATE
Durable Manufacturing	0.091*	0.0010	0.030	−0.0040	−0.0056*	−0.041**	−.0115
	(0.047)	(0.0035)	(0.038)	(0.0025)	(0.0030)	(0.009)	
Nondurable Manufacturing	0.017	0.0051	0.009	−0.0048	−0.0056*	−0.003	−.0006
	(0.051)	(0.0044)	(0.041)	(0.0033)	(0.0033)	(0.010)	
Construction	0.252**	−0.0196**	0.105**	−0.0116**	−0.0134**	−0.007	−.0011
	(0.052)	(0.0048)	(0.042)	(0.0036)	(0.0040)	(0.011)	
Transportation and Utilities	0.044	0.0142**	−0.002	0.0046	0.0046	−0.001	−.0001
	(0.055)	(0.0054)	(0.044)	(0.0040)	(0.0043)	(0.011)	
Wholesale Trade	−0.088	0.0019	−0.164**	0.0051	−0.0011	−0.011	−.0010
	(0.061)	(0.0068)	(0.048)	(0.0050)	(0.0050)	(0.013)	
Retail Trade	−0.149**	−0.0109**	−0.1-78**	−0.0051	−0.0042	0.025	.0043
	(0.051)	(0.0046)	(0.041)	(0.0034)	(0.0036)	(0.010)	
Finance, Insurance, and Real Estate	0.065	−0.0117	−0.066	−0.0023	−0.0135*	0.007	.0005
	(0.068)	(0.0080)	(0.053)	(0.0060)	(0.0058)	(0.015)	
Services	−0.096*	−0.0146**	−0.081**	−0.0172**	−0.0074**	−0.011	−.0024
	(0.050)	(0.0042)	(0.040)	(0.0032)	(0.0032)	(0.010)	
Government	0.000	0.0073	0.000	0.0010	0.0030	−0.002	−.0002
	(--)	(0.0064)	(--)	(0.0048)	(0.0045)	(0.012)	
Agriculture	−0.465**	0.0101	−0.431**	0.0117*	0.0151	0.007	.0004
	(0.071)	(0.0091)	(0.056)	(0.0069)	(0.0099)	(0.018)	
Mining	0.335*	−0.0121	0.178**	−0.0173*	−0.0028	0.010	.0003
	(0.089)	(0.0116)	(0.073)	(0.0091)	(0.0100)	(0.021)	
All Workers					−0.0054**	−0.074**	−.0148
					(0.0017)	(0.010)	
SSR	2617.1		1098.1				
R^2	0.440		0.765				

NOTE: Standard errors are in parentheses. A ** indicates significance at the 5% level. A * indicates the 10% level. Controls for the OLS regression same as in Table 5. Controls for the fixed effects estimates same as in Table 6. Occupation dummies are included. Controls for the fixed effects selection models same as for the fixed effects wage equation estimates except the occupation dummies are omitted. The OLS and fixed effects results are from a single regression each. For the selection models, each row of the table is a separate model for the particular industry indicated.

Table 9

Industries Ranked by Wage Level and Degree of Wage Cyclicality

Industry	Employment Share (%)	Industry Wage Differentials: At Mean of Data	Industry Wage Differentials: Add 3% to U-RATE	Wage Cyclicality Differentials: Fixed Effects	Wage Cyclicality Differentials: Fixed Effects Selection Model
1. Mining	1.5	.128	.092	−.0121	.0018
2. Construction	10.5	.097	.078	−.0064	−.0088
3. Transportation and Utilities	8.7	.092	.122	.0098	.0092
4. Government	7.3	.073	.092	.0062	.0076
5. Durable Manufacturing	22.1	.069	.073	.0012	−.0010
6. Nondurable Manufacturing	12.2	.042	.044	.0004	−.0010
7. Finance, Insurance and Real Estate	3.9	−.015	−.006	.0029	−.0089
8. Wholesale Trade	4.9	−.068	−.037	.0103	.0035
9. Services	15.3	−.126	−.161	−.0120	−.0028
10. Retail Trade	11.1	−.146	−.145	.0001	.0004
11. Agriculture	2.5	−.300	−.249	.0168	.0197
		σ = .105	σ = .110		

Industry Size Weighted Correlations:

	Fixed Effects	Fixed Effects Selection Model
Wage Rankings with Wage Cyclicality Rankings	−.16	−.02
Wage Differentials with Wage Cyclicality Differentials	.15	−.16

NOTE: Wage differentials at mean of data are deviations from the industry size weighted mean differential, and may be constructed from results in table 6, column 4 or the fixed effects results in table 8 (by multiplying the U-RATE coefficients by the mean of U-RATE and adding this to the dummy coefficients). The fixed effects U-RATE coefficients are used to obtain the wage differentials with 3 percent added to the mean U-RATE but with other variables at the mean of the data. The wage cyclicality differentials are deviations from the industry size weighted mean U-RATE coefficient in table 8.

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