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EQUALIZING DIFFERENCES IN THE LABOR MARKET*

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The theory of equalizing differences asserts that workers receive compensating wage premiums when they accept jobs with undesirable nonwage characteristics, holding the worker's characteristics constant. Previous research provides only inconsistent support for the theory, with wrong-signed or insignificant estimates of these wage premiums fairly common. An oft-cited reason for these anomalies is that important characteristics of the worker remain unmeasured, biasing the estimates.

In this paper, longitudinal data are used to test this conjecture. Although such data improve the control for worker characteristics, the plausibility of the estimates is not markedly improved. Alternative explanations for these results are considered.

"It's indoor work and no heavy lifting."

—Senator Robert Dole, explaining why he wanted to be Vice President.

The theory of equalizing differences—that individuals are induced to accept less attractive jobs by compensating differences in their wage rates—is an important tool in economists' attempts to understand the labor market. Both as a test of the theory and in order to improve the measurement of compensation, researchers have attempted to estimate "prices" for nonwage characteristics. Despite evidence from studies of the internal wage policies of firms that equalizing differences are present [Doeringer and Piore, 1971, pp. 66–68; and Reynolds 1974, p. 210] recent research on the determinants of individual earnings has provided rather limited support for the theory (see Table I).[1]

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1. Studies included in Table I were those using micro-data to explain the wages of broad groups of workers. Interesting studies excluded by this criterion were Antos and Rosen [1975], and Toder [1972] (both dealt only with teachers) and Mixon [1975] (who used time series data on 3-digit SIC industries). Each of these studies focused on job characteristics quite different from those in Table I. Antos and Rosen found that "equalizing wage differentials on working conditions are important sources of teacher wage variation." Toder found that communities with high proportions of nonwhites must pay a wage premium to attract teachers of given quality, but there was no analogous differential for teaching children in poor communities. Mixon reported that increases in the minimum wage led to statistically significant revision in at least one (out of five) nonwage characteristics in fifteen out of twenty low-wage industries studied.

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TABLE I

SUMMARY OF PREVIOUS MICRO-DATA STUDIES

Author	Source of individual data	Source of job data	Sample	Dependent variable	Job characteristic	Expected sign	Coefficient (std. error)	Notes
Lucas [1977] Table 1	Survey of Econ. Oppy.	Dict. of Occ. Titles	White males 12 yrs ed	ln (wage/hr)	Repetitive work	+	0.103* (0.043)	
					Requires phys. strength	+?	−0.170* (0.038)	
					Bad working conditions	+	0.068* (0.028)	
					Supervise people	+	0.152* (0.053)	
Bluestone [1974] Table 5.8, col. 2	Survey of Econ. Oppy	Dict. of Occ. Titles	White males	wage/hr (in $)	Requires phys. strength	+?	−0.13* (0.04)	Measured on 1 to 5 scale
					Bad working conditions	+		Insignificant
Quinn [1975] Table 5–9	Longitud. Retirement History Survey	Dict of Occ. Titles	White males Age 58–63	ln (wage/hr)	Work under stress or phys. strength	+	0.058* (0.031)	
					Other phys. demands	+	−0.015 (0.030)	
					Bad working conditions	+	0.005 (0.030)	
Hamermesh [1977] Table 3 col. 2	ISR Survey of working conditions	ISR Survey of working conditions	White males age 21–65	ln (annual earnings)	Noise	+	0.151 (0.10)	Jointly insignificant ($F = 0.091$)
					Weather and heat	+	0.075 (0.07)	
					Dirt	+	−0.007 (0.77)	
					Hazardous materials	+	0.037 (0.03)	
					Hazardous	+	0.033	

					equipment		(0.07)	
					Misc. hazards	+	0.029 (0.04)	
Thaler-Rosen [1975] Tables 3 & 4 col. 1	Survey of Econ. Oppy.	Society of Actuaries	Adult males in risky occupations	wage/week (in $)	Deaths per 1,000 manyears	+	3.52* (2.10)	
				ln (wage/ week)	Deaths per 1,000 manyears	+	0.0206 (0.0167)	
Smith [1973] Table 1 col. D	Current Pop. Survey	U.S. Dept. of Labor	White males	ln (wage/hr)	Deaths per 1,000 manyears	+	0.360* (0.151)	
					Lost workyrs per 1,000 manyears	+	−0.0011* (0.0005)	
Taubman [1975] Table III-5	NBER-TH	NBER-TH	Above-avg. IQ men age 44–52	Annual earnings	Prefer salaried	−	−0.10*	Coefficients expressed as fraction of avg. earnings of not self-employed high school grads
					Teacher	−	−0.18*	
					Reason took job:			
					Pros. finan. suc.	+	0.17*	
					Independ. work	−	−0.11*	
					Interper. contact	−	−0.01	
					Help others	−	−0.08*	
					Was a challenge	+	0.17*	
					Job security	−	−0.13*	
					Provide free time	−	0.02	
Duncan [1976]	Quality of Employment	Quality of Employment	Males age 21–65	(wage + est. fringe	Safe and healthy working conditions	−	−0.21	Expressed as fraction of mean
Table 1	Survey	Survey	working full time	benefits)/ hr	Control of overtime	−	−0.56	wage. Based on standardized
					Employment stability	−	−0.20	weights from canonical
	Panel Study of Income Dynamics	Panel Study of Income Dynamics	Male hshd. heads in same job in 1970 and 1971	wage/hr	Free to increase work hours	−	−0.23	correlation (see text fn. 2).
					Freedom to reduce work hours	−	−0.23	No tests of significance
					Income stability	−	−0.14	

(*continued*)

TABLE I

(cont'd.)

Author	Source of individual data	Source of job data	Sample	Dependent variable	Job characteristic	Expected sign	Coefficient (std. error)	Notes
Duncan-Stafford [1977] Table 2	Time Use Survey	Time Use Survey	Adult blue-collar workers	ln (wage/hr)	Works with machines	+	0.169* (0.086)	
					Not free to take time off	+	0.031 (0.092)	
					ln (index of work effort)	+	0.066* (0.032)	
				ln (wage/hr)	Works with machines	+	0.157 (0.099)	"hours" = time at work − break time − training time
					Not free to take time off	+	0.098 (0.105)	
					ln (index of work effort)	+	0.000 (0.037)	
Lazear [1977] Table 1	NLS Young Men	NLS Young Men	Men 14–24 in 1966	Change in ln (wage/hr)	Change in enrollment status	−	−0.132* (0.025)	
Schiller-Weiss [1977] Table 1	Social Security LEED file	U.S. Dept. of Labor	Employees of 133 large firms	ln (annual earnings)	Eight pension-plan characteristics			jointly significant for 26–34 and 35–39 age cohorts; not for older cohorts

* = Statistically significant at the 0.05 level.

Lucas [1977, pp. 554–55] found evidence of significant compensation for repetitive work and somewhat smaller (though statistically significant) compensation for jobs with bad working conditions (hazards, extreme temperatures, etc.). Jobs requiring physical strength appeared to command lower wages, other things equal, and these differences were "significant" statistically. Neither Bluestone [1974, pp. 132–222] nor Quinn [1975, pp. 112, 115] nor Hamermesh [1977, p. 65] found consistent evidence of wage compensation for jobs requiring physical strength or involving hazards or extreme temperatures.

Smith [1973] found that "the probability of [work-related] death may be fully reflected in wage rates, but evidence of compensating differentials related to nonfatal injuries is scant." Thaler and Rosen [1975, pp. 289–94] concluded that, for workers in the most hazardous occupations, occupation-specific mortality rates do exert a positive influence on wage levels. Even here, the point estimates of this premium and their statistical significance are sensitive to the other variables included in the estimating equation and the functional form (linear or log-linear) employed.

Taubman [1975, pp. 51–52] analyzed the relationship between wages and the reasons that individuals in the NBER-TH sample of above-average ability males gave for choosing their occupation. Interpreting the latter as reflecting differences in their actual job characteristics, he found that most had statistically significant, plausibly signed, and often substantial coefficients. Duncan [1976, p. 472] found substantial compensating differentials for some job characteristics (freedom to control hours worked, safe working conditions, and employment and income stability);[2] however, the probability of observing such estimated differences by chance was not explored. Duncan and Stafford [1977, p 15] report positive premiums for work effort and for jobs that restrict "opportunities to choose an individual or flexible work schedule and an individual work pace"—but these differences become statistically insignificant when a theoretically preferable wage measure is employed. Lazear [1977, p. 175] reports significantly lower wages for young men enrolled in school, "consistent with an equalizing-difference explanation which argues

2. Because Duncan reported standardized weights from canonical correlation rather than unstandardized regression coefficients, his results were adjusted to make them more comparable to others in Table I. The unstandardized weight as a fraction of the mean wage equals the standardized weight times the coefficient of variation of the wage divided by the standard deviation of the job characteristics. However, the latter was adjusted so that the range of the job characteristic was zero to one in all cases, making them more nearly comparable to the dummy variables used by other authors in Table I.

that students optimally choose more flexible and easier jobs at the cost of lower wages."

Evidence on other important job characteristics is more limited. For example, human capital theorists maintain that individuals gain entry into occupations with prospects of higher future wages only by accepting lower current wages. Empirical support for this proposition has been rather indirect (e.g., inferences from the pattern of the variance of earnings by age [Mincer, 1974, pp. 58–59], or differences in age-earnings profiles between groups [Lillard, 1977, p. 521].[3] Schiller and Weiss [1977, p. 17] investigated the relationship between pension benefits and wages in a sample of workers in large firms. They found support for the equalizing-differences hypothesis among younger workers but not among those nearing retirement. The relationship between wages and other fringe benefits (vacations, health insurance, etc.) has not been examined, although the existence of a tradeoff is often asserted in collective bargaining contexts [Reynolds, 1974, p. 217].

The overall pattern that emerges from Table I is one of mixed results: some clear support for the theory but an uncomfortable number of exceptions. Among the studies that fail to find equalizing differences, the most common explanation is the omission of important worker abilities, biasing the coefficients of the job characteristics.

The purpose of this paper is to provide a more appropriate test of the theory. In Section I a model of labor-market choice that emphasizes equalizing differences is presented. The model formalizes the omitted variable argument and suggests that even "favorable" results could underestimate the magnitude of these differences. Moreover, the analysis in Section II suggests that a more appropriate test of the model can be conducted when longitudinal data are available.

In Section III the data are described. The National Longitudinal Survey (NLS) Young Men's sample provides seven years' data on the labor market experiences of males age 14–24 in 1966. Data on occupational characteristics are taken from several sources and matched to individuals on the basis of their occupation or industry. The results are presented and discussed in Section IV. Concluding observations are offered in Section V.

3. Hause's [1973] study of the covariance between earnings at different points in time (using Swedish data) is an exception.

I. THE MODEL

The central focus of the theory of equalizing differences is the choice made by individuals *with given* personal characteristics (X) among jobs with different wages (w) and differing nonwage attributes (Z). In order to attract labor of a given quality, an employer offering jobs that are hazardous or otherwise undesirable must pay higher wages than employers offering jobs with more desired nonwage characteristics. Therefore, an individual faces a set of jobs with differing combinations of w and Z, and is assumed to choose among these opportunities in order to maximize utility.

Formally, let Z be the vector of nonwage characteristics, measured so that larger values of Z represent less desired jobs. ("Less desired" is understood to reflect the preferences of the marginal individual.) If $f(w,Z;X)$ is the function relating wages to nonwage job characteristics for persons with personal characteristics X, the theory asserts that $\partial w/\partial Z_j > 0$ for all j.

This prediction can be tested once the function $f(w,Z;X)$ is specified. Unfortunately, theory provides no guidance in the choice of functional forms, at least for most Z's.[4] A convenient specification is the semi-log equation,

$$W \equiv \ln(w) = XB + ZA + u, \tag{1}$$

where u is a random disturbance.[5]

Equation (1) is the estimating equation used in most of the studies mentioned earlier, where the estimates of the A's were often insignificantly different from zero and sometimes "significant" but wrong-signed.

It is not difficult to explain this result, even if the theory is correct ($A > 0$). Consider what happens to the estimate of A when some of the X's are omitted from equation (1). For simplicity, suppose that

$$W = B_0 + X_1B_1 + X_2B_2 + ZA + u, \tag{2}$$

4. For a discussion of the relationship of f to production relationships and workers' tastes, see Thaler and Rosen [1975, pp. 268–86].

5. Equation (1) has been estimated by ordinary least squares in previous studies. Several readers of earlier drafts of this paper have questioned the appropriateness of such estimation, arguing that u and Z are inevitably correlated. Ordinary least squares might be appropriate if *all* the systematic determinants of earning capacity were included among the X's; one might then argue that transitory variation (due, for example, to measurement error in reported wages) was independent of Z. Of course, X is never fully specified; but the resulting difficulties are more easily understood from the "omitted variable" discussion in the text than from a simultaneous-equations-bias perspective. In any case, finding instruments for the Z's that are not themselves X's would be extremely difficult.

where B_1, B_2, and A are scalars, X_1 and X_2 are orthogonal, and data on X_2 are unavailable. The bias in the estimate of A will have the same sign as the correlation between X_2 and Z. If the people with more X_2 "spend" some of their greater earning capacity on reducing Z, this correlation will be negative,[6] and the estimate of A will be biased toward (or even beyond) zero. The omitted-variable argument is the most frequently encountered explanation for insignificant or wrong-signed coefficients [e.g., Lucas 1977, p. 555; Toder, 1972, p. 440; Quinn, 1975, p. 103; and Duncan and Stafford, 1977, p. 3]. Moreover, it implies that even right-signed estimates may be too small [Antos and Rosen, 1975, p. 137].

Rigorous generalization of this argument to cover more than one Z does not appear possible.[7] However, in general, the bias in estimating A_k will have the same sign as the coefficient of Z_k in a hypothetical regression of the omitted X of the included X and all of the Z's. If that hypothetical coefficient is negative, the estimate of A_k is biased toward (or beyond) zero.[8]

II. The Importance of Longitudinal Data

Clearly, it is important that the determinants of earning capacity be specified as completely as possible. However, the data usually

6. Evidence in Lucas [1974] on the relationship between *observable* determinants of earning capacity (schooling and age) and nine job characteristics is consistent with this hypothesis. Bowles [1972, p. S238] argues that this positive relationship holds for social class and nonmonetary job characteristics generally. Bailey and Schwenck [1972, p. 15] report "an increasing emphasis [on employer-financed retirement and insurance plans] accompanying higher levels of earning power." Duncan [1976], however, found that, controlling for education, other determinants of earning capacity (e.g., experience, SES, tenure, test score) were not significantly related to nonpecuniary job characteristics.

7. With two Z's, equation (2) would become

$$W = B_0 + B_1X_1 + B_2X_2 + Z_1A_1 + Z_2A_2 + u. \tag{2'}$$

The bias in estimating, say, A_2 has the same sign as

$$[\mathrm{cov}(X_1,Z_1)\,\mathrm{cov}(X_1,Z_2) - \mathrm{var}(X_1)\,\mathrm{cov}(Z_1,Z_2)]\,\mathrm{cov}(X_2,Z_1) + [\mathrm{var}(X_1)\,\mathrm{var}(Z_1) - \mathrm{cov}(X_1,Z_1)^2]\,\mathrm{cov}(X_2,Z_2).$$

Assuming that $\mathrm{cov}(X_i,Z_j)$ is negative for all i and j, the sign of the bias is still indeterminate, since we cannot rule out the possibility that the first term in brackets is negative and large enough to make the bias positive. A moderately helpful assumption is that $r(X_1,Z_i) = r(X_2,Z_i)$, $i = 1,2$, where r is the simple correlation. In this case, the bias has the same sign as $r(X_1,Z_2) - r(X_1,Z_1)r(Z_1,Z_2)$, which will be negative unless $r(Z_1,Z_2)$ is positive *and* both $r(Z_1,Z_2)$ and $r(X_1,Z_1)$ are substantially larger than $r(X_1,Z_2)$ in absolute value.

8. Given two individuals with identical observed X's and identical Z's (except for Z_k) and even-money odds, would you bet that the individual with greater Z_k had more or less of the unmeasured X? This question elicits one's subjective expectation of the hypothetical coefficient: betting that the individual with greater Z_k will have less unmeasured X is equivalent to expecting that A_k is biased toward zero.

available are deficient in that they provide no information on many potentially important personal characteristics. For example, the Survey of Economic Opportunity (the source of wage and personal characteristics data used by Lucas, Bluestone, and Thaler and Rosen) contain no measures of intelligence or fluency in oral communication, and its measures of social background are far from ideal.[9]

To the extent that most of the omitted dimensions are individual-specific (i.e., do not change over time), they can be summarized by an individual-specific intercept. With cross-sectional data, including individual-specific intercepts among the X's would be impossible, since there would be one such "variable" for each observation. Given data on wages and jobs of individuals at several points in time, the use of individual-specific intercepts is a feasible strategy for controlling for individual characteristics that do not change over time.

Of course, some determinants of earnings capacity do change over time (e.g., an individual's age, work experience, formal job training, and marital status). To the extent that such dimensions remain unmeasured, the omitted-variable problem persists, albeit (hopefully) reduced. Fortunately, the NLS data include fairly detailed information on many of these characteristics.

Including several hundred individual-specific intercepts in the X-matrix would exceed the capacity of almost any computer program that calculates regressions. Fortunately, there is a computationally feasible alternative.

Define

$$\tilde{W}_{it} = W_{it} - \frac{1}{T}\sum_{t=1}^{T} W_{it}, \qquad i = 1, 2, \ldots, \text{(number of individuals)} \quad t = 1, 2, \ldots, T$$

$$(3)\quad \tilde{X}_{ijt} = X_{ijt} - \frac{1}{T}\sum_{t=1}^{T} X_{ijt}, \quad j = 1, 2, \ldots, \text{(number of } X\text{'s)}$$

$$\tilde{Z}_{ikt} = Z_{ikt} - \frac{1}{T}\sum_{t=1}^{T} Z_{ikt}, \quad k = 1, 2, \ldots, \text{(number of } Z\text{'s)}.$$

It can be shown[10] that regressing $\tilde{W}$ on $\tilde{X}$ and $\tilde{Z}$ gives the same estimates of B and A as regressing W on X, Z, and the set of individual-specific intercepts.

9. See Bowles [1972, section III].

10. The key is to partition the independent variable matrix so that the set of individual intercepts is in one block and the X's and Z's are in the other. Note that the standard errors for each coefficient calculated by standard computer programs from the transformed data must be corrected to reflect the loss of degrees of freedom due to the (swept-out) intercepts. See Pacific Consultants [1976, pp. 202–15].

Equation (3) emphasizes that, with individual-specific intercepts, it is changes in W, X, and Z over time for each individual that identify B and A. This would cause difficulties if individuals remained in the same occupation, since the changes in Z would presumably be negligible. Fortunately, occupation-changing is the rule rather than the exception for young men. In the sample analyzed below, 85 percent of the individuals changed 3-digit occupations at least once between 1966 and 1973, and 60 percent moved to a new broad (one-digit) occupational group.[11]

III. The Data

The NLS Young Men's sample provided data in each of the seven years 1966–1971 and 1973. Eliminating individuals who were college graduates, were in school at any of the survey dates, or did not provide usable data on wages, industry, occupation, or the other variables described below reduced the sample from 5,225 (roughly 2,000 of whom were out of school in 1966) to 470. The subsample studied here was older, had more stable labor force attachment, and came from less "advantaged" backgrounds than the larger sample.

Given the individual-intercept strategy, only determinants of earning capacity that change over time need be (or can be)[12] included explicitly in equation (1). The effects of those determinants of earning capacity that do not change over time (e.g., schooling or race) are reflected implicitly in the intercepts.

The Young Men's surveys provide information on seven determinants of earning capacity that change over time:

1. Six dummy variables (which equal one for observations from the 1967, 1968, 1969, 1970, 1971, and 1973 surveys, respectively, and zero otherwise) were included. These dummies capture the general growth in wages due to technical progress, price inflation, etc., and the effects of time spent not working.

2. Three types of variables reflect human capital investments. The first is the cumulative time spent in civilian formal training programs, measured in hours of training/(40×52). Three types of training programs could be distinguished: company training, part-time school courses, and "other."[13] The second is cumulative work

11. Individuals who do not change jobs are still "useful" observations, in that they help to determine the coefficients of the individual characteristics.

12. Including a characteristic that did not change over time would make that characteristic an exact linear combination of the individual-specific intercepts.

13. Part-time school included business and technical school, regular school, and correspondence courses. Because formal apprenticeships are coded separately only in 1966 and 1973, they were included in the "other" category.

experience since 1965, measured in weeks employed/52. Presumably, this variable measures the productive effect of past on-the-job investments associated with informal "learning by doing" rather than formal training programs. Finally, tenure with current employer, measured in months/12, is included to capture differences in firm-specific human capital investments.

3. Two measures of unionization were employed. First, the fraction of workers unionized in the individual's (3-digit Census) industry was coded separately for office and production workers, based on data in Freeman and Medoff [1980]. Second, the probability that the individual worker was himself covered by a collective bargaining agreement was included. For 1969–1971, this was a dummy variable (1 = covered, 0 = not covered) from the NLS file. Whether the worker was covered by a collective bargaining agreement was not ascertained in the remaining years. If the worker was working with the same employer in, say, 1968 as in 1969, the 1969 dummy was coded for 1968. If the worker was with a different employer, the probability of his being covered in 1968 was taken to be the fraction of (office or production) workers in his industry who were covered.

4. Marital status was represented by a dummy variable that equals one whenever the respondent is currently married and zero otherwise.

5. Geographic variation in wage rates is reflected in three dummy variables, which equal one whenever an individual works in an SMSA, lives in the South, or lives in the West, respectively, and zero otherwise.

6. The effect of involuntary job separation was represented (crudely) by the cumulative number of times an individual has been fired or laid off.

7. The effect of health problems on earning capacity was represented by a dummy variable that equals one whenever the respondent reported a health problem that interfered with work activities.

In order to compare the results presented here to those of other researchers, conventional determinants of workers' wages that do not change over time were included *instead of* the individual-specific intercepts in some regressions. These variables were years of schooling, months of military training programs, labor-market experience in 1965, knowledge of the world of work, an index of the socioeconomic status in which the individual grew up, and race.

An important source of data on job characteristics was the Dictionary of Occupational Titles (DOT) file. Originally, each of over

13,000 DOT occupations was assigned a dummy variable for each characteristic (1 = present, 0 = absent). These 13,000 occupations were then aggregated into the nearly 300 3-digit Census occupations using a conversion matrix based on the October 1966 Current Population Survey. Thus, the "score" for each occupation represents the probability that a randomly selected individual in that 3-digit occupation has the given job characteristic. For details see Lucas [1974]. Both "laborers, not elsewhere classified," and "operatives, not elsewhere classified," are subdivided according to Census industry, in order to reflect the heterogeneity of these important "residual" categories.

Four characteristics were selected for study here:

1. the job requires performing repetitive functions;
2. the job requires working under stress;
3. the job requires physical strength;
4. the job involves bad working conditions (extremes of cold or heat, humidity, vibrations, or hazards).

An alternative, more specific measure of bad working conditions, the increase in the actuarial probability of death associated with hazardous occupations [Society of Actuaries, 1967] was also used. Since these data measure the *extra* risk from working in such occupations, occupations not covered were assigned a value of zero. This made it imperative that Society of Actuaries' occupations be matched to Census occupations (or occupation-industry pairs) whenever possible. These annual extra-mortality probabilities have been multiplied by 1,000 as a scaling factor; thus, we have the extra deaths per thousand man-years in each occupation. (Among all males age twenty-five, about two out of 1,000 die each year; thus, the coefficient of this variable may be interpreted as the compensation for increasing the normal risk of death by one-half.)

Data were available from various sources on four other potentially important job characteristics:

1. The number of hours usually worked by the individual was recorded each year by the NLS. The logarithm of hours worked and a separate dummy variable for part-time work (less than thirty-five hours per week) were used.

2. A dummy variable that equals one whenever the individual worked for a federal, state, or local government, and zero otherwise, was constructed. It was intended to reflect the greater job security of government employment [Blechman, Gramlich, and Hartman, 1975, p. 68], although other interpretations are possible.

3. When an individual was currently enrolled in a formal training program (e.g., company training program), the NLS file gives the

number of hours per week the individual spends in training. The ratio of training hours to usual hours worked was coded as a measure of self-investment opportunities of the job. The NLS did not determine whether the firm paid the employee for time spent in training, or for tuition, or other expenses. This seems almost certain for company training, unlikely for part-time schooling, with "other" training in between. Given Lazear's argument that students select more flexible, less demanding jobs while enrolled, one would expect a negative sign even for part-time schooling, but the interpretation of this coefficient is unclear. Finally, workers who reported their occupations as being in Census "apprentice" categories often did not report themselves to be enrolled in formal training.[14] Whatever the "formality" of apprenticeship, there is strong reason to believe that substantial on-the-job training occurs in these occupations. Therefore, a dummy variable that equals one whenever the respondent's occupation is one of the Census apprentice categories, and zero otherwise, was created. Human-capital theory leads one to expect a negative sign for this variable.

4. Ideally, total compensation (including fringe benefits) would be used as the dependent variable. Lack of appropriate data has precluded this in the past, and the NLS data are no exception. However, data on total expenditures for labor compensation, divided into wage and nonwage components, are available for 2-digit SIC industries.[15] Annual data from 1965 to 1969 were averaged, and the values assigned to each Census industry were those of the 2-digit SIC industry to which it belonged. For manufacturing industries these estimates were refined using data on a 3-digit SIC level for 1967 from the *Annual Survey of Manufacturers* [1973]. From these data the ratio of nonwage compensation to wages was calculated, and matched to each individual according to his Census industry. If the logarithm of total compensation is some function of X and the other job characteristics (i.e., $h(X,Z)$), then

$$\begin{aligned}\ln(\text{wage}) &= h(X,Z) - \ln(1 + \text{nonwage compensation/wages})\\ &\simeq h(X,Z) - (\text{nonwage compensation/wages}),\end{aligned}$$

so that the coefficient of our measure of nonwage compensation to wages would be -1.0.

Two other "job characteristics" taken from the DOT file were also included in some regressions: GED (general educational devel-

14. For example, in 1966 and 1973 (the only years in which formal apprenticeship programs were recorded separately), only half of those who were in apprentice occupations were recorded as being in apprenticeship programs.

15. Unpublished data, Bureau of Economic Affairs.

opment) and SVP (specific vocational preparation). Their inclusion, in addition to the other determinants of earning capacity described above, can be justified in several ways: as measuring omitted productive characteristics of individuals; as reflecting perceived productivity differences among individuals that are not explained by the other X's; as reflecting wage differentials that workers in some jobs receive over identical workers in other jobs.

IV. Results

Estimates of the parameters of equation (1) appear in Table II. Columns 5 to 8 include individual-specific intercepts, while columns 1 to 4 do not.

In columns 1–4, coefficients of the individual characteristics are generally in line with expectations and previous research using these data [e.g., Griliches, 1977; Kohen, 1972]. The human capital variables are generally significant. The exceptions are cumulative company training and tenure; the near-zero coefficient for tenure probably reflects the fact that job search and consequent job changes are important for workers in their twenties. If anything, the cumulative experience variable has an implausibly large effect. The unionization variables, marital status, and geographic variables have the expected impacts. The (crudely constructed) layoff-discharge variable has no effect on wages; health problems have a small negative impact, but the coefficient is about the same size as its standard error. Race, knowledge of the world of work, early experience, and years of schooling have the expected effects on wages, while military experience has little impact.

The individual characteristics in columns 1 to 4 are more comprehensive than those typically included in the studies in Table I. However, the coefficients of the job characteristics in columns 1–4 display the same inconsistent relationship to theoretical predictions one observes in Table I. The government worker coefficient is significantly negative and reasonable in magnitude. The supplements variable is wrong-signed and very significantly different from −1.0. Time spent in school or "other" training programs has a fairly large negative impact on current wages, but time in company training or being an apprentice does not. The four DOT variables are either "significant" and wrong-signed (repetitive work in columns 1 and 2, and bad working conditions) or insignificant. A zero coefficient for jobs requiring physical strength is sensible for workers in their twenties, but the other three characteristics should generate positive

TABLE II

ESTIMATES OF EQUATION (1)

Variable	Mean (st. dev.)	1	2	3	4	5	6	7	8
Constant	1.00	5.71	5.66	5.71	5.68				
	(0.00)	(0.127)	(0.126)	(0.126)	(0.125)				
Year = 1967	0.143	−0.013	−0.015	−0.007	−0.009	0.003	0.003	0.004	0.004
	(0.350)	(0.023)	(0.022)	(0.022)	(0.022)	(0.013)	(0.013)	(0.013)	(0.013)
Year = 1968	0.143	0.012	0.009	0.026	0.023	0.035*	0.035*	0.036*	0.036*
	(0.350)	(0.033)	(0.032)	(0.032)	(0.032)	(0.018)	(0.018)	(0.018)	(0.018)
Year = 1969	0.143	0.013	0.006	0.031	0.025	0.035	0.034	0.035	0.035
	(0.350)	(0.045)	(0.045)	(0.044)	(0.044)	(0.024)	(0.024)	(0.024)	(0.024)
Year = 1970	0.143	−0.011	−0.022	0.014	0.005	0.018	0.017	0.019	0.018
	(0.350)	(0.058)	(0.057)	(0.057)	(0.057)	(0.031)	(0.031)	(0.031)	(0.031)
Year = 1971	0.143	−0.049	−0.061	−0.019	−0.029	−0.020	−0.021	−0.020	−0.021
	(0.350)	(0.071)	(0.070)	(0.070)	(0.070)	(0.038)	(0.038)	(0.038)	(0.038)
Year = 1973	0.143	−0.071	−0.090	−0.027	−0.043	−0.042	−0.043	−0.041	−0.043
	(0.350)	(0.098)	(0.097)	(0.097)	(0.097)	(0.053)	(0.053)	(0.053)	(0.053)
Cum company train school	0.078	−0.003	−0.003	−0.005	−0.006	−0.001	−0.001	0.000	−0.001
	(0.467)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Cum part-time school	0.100	0.035*	0.036*	0.033*	0.034*	0.026	0.026	0.026	0.026
	(0.368)	(0.014)	(0.014)	(0.014)	(0.014)	(0.016)	(0.016)	(0.016)	(0.016)
Cum other school	0.096	0.049*	0.047*	0.043*	0.042*	−0.007	−0.008	−0.008	−0.009
	(0.459)	(0.011)	(0.011)	(0.011)	(0.011)	(0.013)	(0.013)	(0.013)	(0.013)
Cum work exper. since 1965	3.91	0.103*	0.105*	0.096*	0.097*	0.101*	0.102*	0.102*	0.102*
	(2.17)	(0.014)	(0.014)	(0.014)	(0.014)	(0.008)	(0.008)	(0.008)	(0.008)
Years tenure current job	3.06	0.000	0.001	0.001	0.002	0.006*	0.006*	0.006*	0.006*
	(2.85)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)

(continued)

TABLE II

(cont'd.)

Variable	Mean (st. dev.)	1	2	3	4	5	6	7	8
Prob resp covered by union	0.378	0.130*	0.126*	0.134*	0.132*	0.081*	0.080*	0.080*	0.080*
	(0.448)	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)	(0.016)	(0.016)
Union coverage, office	0.015	0.274*	0.332	0.233*	0.271*	0.178*	0.201*	0.176*	0.210*
	(0.073)	(0.074)	(0.073)	(0.074)	(0.073)	(0.076)	(0.074)	(0.076)	(0.074)
Union coverage, nonoffice	0.396	0.223*	0.178*	0.207*	0.172*	0.200*	0.190*	0.203*	0.190*
	(0.326)	(0.023)	(0.022)	(0.023)	(0.021)	(0.027)	(0.026)	(0.027)	(0.026)
Currently married	0.750	0.132*	0.133*	0.129*	0.130*	0.065*	0.065*	0.064*	0.064*
	(0.433)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
Job in SMSA	0.678	0.135*	0.135*	0.135*	0.135*	0.105*	0.105*	0.104*	0.104*
	(0.467)	(0.011)	(0.011)	(0.011)	(0.011)	(0.020)	(0.020)	(0.020)	(0.020)
Residence in South	0.440	−0.016*	−0.107*	−0.107*	−0.109*	−0.003	−0.002	−0.004	−0.002
	(0.496)	(0.012)	(0.012)	(0.012)	(0.012)	(0.045)	(0.045)	(0.045)	(0.045)
Residence in West	0.095	0.034	0.031	0.040*	0.038*	0.135*	0.138*	0.133*	0.137*
	(0.293)	(0.018)	(0.018)	(0.018)	(0.018)	(0.064)	(0.064)	(0.064)	(0.064)
Cum layoff + discharge	0.439	0.001	0.000	0.002	0.002	0.013	0.013	0.011	0.012
	(0.888)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Health limits work	0.069	−0.021	−0.022	−0.020	−0.022	−0.009	−0.010	−0.007	−0.009
	(0.253)	(0.020)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Race = White	0.760	0.125*	0.128*	0.110*	0.111*				
	(0.427)	(0.014)	(0.014)	(0.014)	(0.014)				
Knowledge of World of Work	33.9	0.0053*	0.0055*	0.0051*	0.0053*				
	(8.50)	(0.0008)	(0.0008)	(0.0008)	(0.0008)				
SES Index	91.3	0.0016*	0.0017*	0.0016*	0.0016*				
	(20.9)	(0.0003)	(0.0003)	(0.0003)	(0.0003)				
Cum experience in 1965	3.07	0.012*	0.011*	0.012*	0.011*				
	(2.34)	(0.002)	(0.002)	(0.002)	(0.002)				
Years schooling completed	11.0	0.031*	0.032*	0.031*	0.031*				
	(2.00)	(0.004)	(0.004)	(0.004)	(0.004)				
Cum military training	0.150	0.005	0.006	0.005	0.006				
	(0.635)	(0.008)	(0.008)	(0.008)	(0.008)				

Government worker	0.091	−0.074*	−0.091*	−0.060*	−0.071*	−0.076*	−0.080*	−0.074*	−0.079*
	(0.287)	(0.019)	(0.019)	(0.019)	(0.019)	(0.024)	(0.024)	(0.024)	(0.024)
Sup/wages in industry	0.110	0.010	0.185	0.050	0.178	0.170	0.223	0.161	0.217
	(0.051)	(0.119)	(0.117)	(0.118)	(0.116)	(0.163)	(0.161)	(0.163)	(0.161)
Time now in company train	0.008	−0.009	0.003	0.003	0.012	−0.036	−0.033	−0.032	−0.029
	(0.067)	(0.079)	(0.079)	(0.078)	(0.078)	(0.061)	(0.061)	(0.061)	(0.061)
Time now in part-time sch	0.008	−0.357*	−0.360*	−0.361*	−0.360*	−0.200*	−0.204*	−0.205*	−0.209*
	(0.058)	(0.088)	(0.088)	(0.087)	(0.087)	(0.071)	(0.071)	(0.071)	(0.071)
Time now in other training	0.008	−0.193*	−0.181*	−0.191*	−0.182*	−0.116	−0.112	−0.117	−0.113
	(0.063)	(0.082)	(0.082)	(0.082)	(0.081)	(0.065)	(0.065)	(0.065)	(0.065)
Occupation = apprentice	0.018	−0.007	−0.005	−0.028	−0.030	−0.098*	−0.097*	−0.098*	−0.098*
	(0.132)	(0.039)	(0.039)	(0.039)	(0.039)	(0.034)	(0.034)	(0.034)	(0.034)
Repetitive work	0.395	−0.137*	−0.143*	−0.036	−0.029	−0.049*	−0.050*	−0.056*	−0.048*
	(0.361)	(0.015)	(0.016)	(0.024)	(0.023)	(0.016)	(0.017)	(0.025)	(0.025)
Work under stress	0.067	0.041	−0.005	−0.012	−0.043	0.028	0.027	−0.019	−0.017
	(0.176)	(0.031)	(0.032)	(0.033)	(0.034)	(0.040)	(0.040)	(0.043)	(0.043)
Physical strength required	0.188	−0.006	−0.036	0.030	0.011	−0.028	−0.036	−0.009	−0.018
	(0.252)	(0.023)	(0.022)	(0.023)	(0.022)	(0.025)	(0.024)	(0.026)	(0.025)
Bad working conditions	0.561	−0.067*		−0.044*		−0.031		−0.037	
	(0.353)	(0.018)		(0.019)		(0.020)		(0.021)	
Deaths/1,000 manyears	0.225		0.060*		0.057*		0.009		0.007
	(0.448)		(0.012)		(0.012)		(0.012)		(0.012)
Ln (usual hours)	3.77	−0.375*	−0.381*	−0.368*	−0.369*	−0.254*	−0.255*	−0.254*	−0.255*
	(0.198)	(0.031)	(0.030)	(0.030)	(0.030)	(0.028)	(0.028)	(0.028)	(0.028)
Part-time worker	0.028	−0.095*	−0.105*	−0.087*	−0.089*	−0.052	−0.053	−0.053	−0.053
	(0.164)	(0.037)	(0.037)	(0.037)	(0.036)	(0.032)	(0.032)	(0.032)	(0.032)
Low GED requirement	0.584			−0.044	−0.067*			0.044	0.033
	(0.387)			(0.024)	(0.022)			(0.024)	(0.024)
Low SVP requirement	0.158			−0.212*	−0.204*			−0.075*	−0.075*
	(0.222)			(0.030)	(0.030)			(0.030)	(0.030)
Standard error of estimate		0.277	0.276	0.274	0.274	0.202	0.202	0.202	0.202
R-squared		0.635	0.636	0.641	0.643	0.833	0.833	0.834	0.834
Number of observations		3,290	3,290	3,290	3,290	3,290	3,290	3,290	3,290

* = Statistically significant at the 0.05 level.

wage differentials. The risk of death variable has a statistically significant positive coefficient that is roughly three times as large as that reported by Thaler and Rosen. Part-time workers receive lower wages (in line with analogous results for females; see Rosen [1976]) but otherwise longer workweeks generate lower hourly wages. Including GED and SVP makes the coefficients of repetitive work and bad working conditions less negative, but has little additional impact.

Individual-specific intercepts are included in columns 5 to 8. The coefficient of cumulative part-time schooling is reduced, and the effect of cumulative "other" training is eliminated. The impacts of the regional dummies are less in line with those in earlier studies. The coefficients of the unionization variables decline, but this was predictable. If some firms pay above-market wages, they should attract better qualified workers, thus offsetting part of the initial wage differential. If the individual-specific intercepts do in fact provide a superior control for variation in worker quality, their inclusion should reduce the impact of unionization.

The impacts of the intercepts on the coefficients of the job characteristics vary considerably, and there is no marked improvement in the correspondence between these coefficients and a priori predictions. The government worker and supplements/wages variables are nearly unaffected. The time in company training variable remains negligible, the coefficients of the part-time school and other training variables are reduced in absolute value, but the apprentice dummy acquires a significant negative coefficient. The effects of repetitive work and bad working conditions become less negative (the former remaining "significant"), while stress and strength are unaffected. The risk of death variable, however, loses its significant positive effect. The coefficients of the continuous workweek and the part-time dummy variables have become less negative.

The lack of consistent improvement in coefficients of the job characteristics due to the intercepts might be attributed to a lack of variation in the transformed variables (see equation (3)), leading to imprecise parameter estimates. In fact, however, the standard errors of these coefficients are not substantially raised by the addition of the intercepts. This reflects the fact that occupation-changing among young workers is common, so that "within-individual" variation in job characteristics is substantial.

Several experiments with the estimating equation are not reported in Table II. First, the cumulative training variables were coded using months rather than hours of training (i.e., neglecting hours spent per week). Second, the extra risk of death variable was replaced by a variable that took the values used by Thaler and Rosen in their subset of risky occupations, and zero otherwise, to test whether the coding of the additional occupations was responsible for the differences from their results. Third, rates of growth in employment in the individual's industry and occupation from 1960 to 1970 were added as explanatory variables, to test the idea that employers in growing industries or occupations might offer both higher wages and better working conditions to attract more employees. None of these changes led to results appreciably different from those in Table II.

Various restrictions of the basic sample were also considered. The sample was divided by race, and also by years of schooling (did/did not graduate from high school). While some of the job characteristics (government worker, time currently in training) were more often significant for whites and high school graduates, there was no clear pattern to the disaggregated equations. Next, the sample was restricted to those with scores of at least 28 (out of a possible 56) on the "knowledge of world of work" test. The motivation was to exclude those with the least information about the job market. Unfortunately, the test emphasizes questions that would be verifiable from Census tabulations rather than reflecting detailed knowledge about the individual's local labor market. In any case, the results for the resulting 2,639 observations were not markedly different from those in Table II. Finally, the sample was restricted to individuals who had been out of school for at least two years at the 1966 survey (2,674 observations), to check whether individuals finding their way in the labor market were obscuring the more systematic behavior of other workers. However, the results were qualitatively similar to those in Table II.

V. Conclusions

The hypothesis that the inconsistent support for the theory of equalizing differences that characterized previous studies was due to the omission of important dimensions of worker quality was not supported by the data. Despite reasonably adequate measures of those worker characteristics that change over time and a statistical technique for holding constant differences that do not, the coefficients of job characteristics that might be expected to generate equalizing differences in wage rates were often wrong-signed or insignificant.

One is left with several explanations for this failure, none of which is entirely convincing.

1. "Labor markets are simply not as competitive as the theory of equalizing differences assumes." While the assumptions of the

perfect-information profit- and utility-maximizing model most often used to explain the equalizing difference hypothesis—and relate it to applied welfare economics [Thaler and Rosen, 1975]—may be too strong, considerably weaker assumptions still imply such differences. Suppose that wages and working conditions are determined by collective bargaining without the threat of extinction compelling these decisions to conform to cost-minimizing outcomes. Suppose that workers lack information about working conditions and underestimate the differences in working conditions among firms. As long as workers prefer better working conditions and higher wages, and employers hire the applicants they perceive to be most qualified, the relationship between wages and unpleasant job characteristics holding worker quality constant should still be positive—though weaker than the stronger set of assumptions would imply.

2. "The *marginal* worker's tastes may be different from those assumed in the a priori signing of the coefficients." For example, while some workers abhor physical labor, others prefer it to more sedentary endeavors; thus, jobs requiring physical strength may not be unpleasant for the marginal worker, and no equalizing difference would be required. However, this conjecture is much less convincing for most of the other job characteristics in Tables I and II.

3. "The job characteristics are not well-measured." Undoubtedly, there is a large element of truth to this assertion, particularly for characteristics "matched" on the basis of occupation rather than being reported directly by the worker. It would be an attractive explanation for coefficients that fell a little short of plausible magnitude or statistical significance. But it is difficult to construct a measurement-error rationale for coefficients that are wrong-signed and significantly different from hypothesized values (e.g., repetitive work or supplements/wages).

4. "Omitted variables—both individual characteristics that change over time and job characteristics—may be biasing the results." Admittedly, some determinants of changes in individual productivity (e.g., intensity of informal on-the-job training) remain unmeasured. However, the results were little improved when individual-specific "abilities" were controlled. The omission of some job characteristics raises a more complicated issue. One might expect good job characteristics to be positively correlated in general. However, controlling for all X's, the partial correlation among job characteristics is more difficult to assess. It may be, for example, that individuals who do repetitive work have more freedom to work overtime or require less (costly) job search to find jobs. Lacking data to hold these omitted characteristics constant, one can only speculate.

5. "Testing the hypothesis on a sample in their early and mid-twenties is inappropriate." It is not obvious why workers in this age range should provide weaker support for the hypothesis than older workers. After all, the common stereotype of youth is one who is *overly* sensitive to working conditions, insufficiently willing to put up with repetitive, stressful, or otherwise unpleasant work in order to "make something" of himself.[16] In any case, as reported in Section IV, deleting the least experienced fifth of the sample failed to provide clearer support for the hypothesis.

One could undoubtedly construct a more convincing case for each of these explanations, but it is doubtful that it would be fully satisfactory, explaining the "successes" in Tables I and II as well as the failures. While the present paper provides little support for an oft-used explanation, the task of choosing (or combining) the alternatives remains.

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16. Moreover, using older workers would make it likely that current wages would be quite different from the wages anticipated by those workers when they made their career decisions.

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