# What Is a Good Job? A New Measure of Labor-Market Success[1]

Christopher Jencks
*Northwestern University*

Lauri Perman
*Pennsylvania State University*

Lee Rainwater
*Harvard University*

No currently available index allows investigators to estimate the overall desirability of specific jobs. With data collected in the 1980 Survey of Job Characteristics, an index of job desirability (IJD) can be constructed to fill this gap. The IJD incorporates 13 nonmonetary job characteristics along with measures of earnings and weights all job characteristics according to their effects on workers' judgments about how "good" their current jobs are compared with an average job. While earnings are the most important single determinant of a job's desirability, the 13 nonmonetary job characteristics together are twice as important as earnings. Unlike occupational status and earnings, the proposed index explains almost the entire effect of race, sex, educational attainment, and experience on job ratings. It also explains almost all the variation in job ratings provided by workers in different occupations. Furthermore, taking account of nonmonetary job characteristics more than doubles the estimated level of labor-market inequality. White skin, male gender, favorable social origins, high educational attainment, and extensive labor-market experience are also worth two to five times

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more when one considers both monetary and nonmonetary payoffs than when one considers money alone.

Some jobs are better than others. Everyone recognizes this fact, both when they discuss jobs in daily conversation and when they must actually choose among jobs. Yet social scientists have no comprehensive measure of a job's desirability. Sociologists have devised many schemes for ranking *occupations* but none for ranking the diverse jobs that fall into the same occupational category. Economists rank jobs according to their pay but have no global measure of jobs' nonmonetary benefits (or costs). Psychologists measure workers' subjective satisfaction with their jobs but have not, for the most part, tried to rank jobs on the basis of their objective characteristics.

This paper will argue that neither occupational status nor earnings nor job satisfaction is a good measure of a job's overall desirability. It follows that none of these measures provides a good measure of an individual's competitive success in the labor market. We therefore propose a new strategy for measuring labor-market success that takes account of both monetary and nonmonetary job characteristics and weights them according to their importance to the average American worker. The resulting index of job desirability differs from economists' preferred measure of jobs' desirability, namely pay, because it takes account of jobs' nonmonetary characteristics. It differs from sociologists' preferred measure because it takes account of differences among jobs in the same occupational category. It differs from psychological measures of job satisfaction because it is based on jobs' objective characteristics and ignores variation in the way different individuals evaluate jobs with similar objective characteristics. While it is not ideal for all purposes, we argue that the proposed index provides a better measure of a worker's competitive success in the labor market than any existing measure does.

The paper is divided into three parts. The first discusses the limitations of existing measures of labor-market success and proposes an alternative approach. The second describes how we selected the job characteristics we include in the index and how we estimated their effects on a job's overall desirability. The third shows how using the index changes our picture of the determinants of both labor-market success and overall labor-market inequality in the United States.

## RANKING JOBS AND WORKERS

Our aim is to develop an index that can serve two closely related purposes. First, it should rank the jobs available in the American economy and tell us how much better one job is than another. Second, it should

rank the workers who hold these jobs and tell us how much more successful one is than another in the eyes of outsiders. Both goals require some elaboration.

If all jobs had identical nonmonetary characteristics, knowing how much two jobs paid would provide a complete description of how much better one job was than another. Since jobs differ with regard to an almost infinite number of nonmonetary characteristics, a realistic scheme for comparing jobs must use some common metric to value both monetary and nonmonetary characteristics and must then sum these values.

Workers disagree, of course, about the relative importance of different job characteristics. This means that no index of job desirability can rank jobs in such a way that all workers will prefer all jobs with high scores to all jobs with low scores. A good index should, however, rank jobs in such a way as to maximize the proportion of workers who prefer jobs with high scores to jobs with low scores. To accomplish this goal, an index should identify those job characteristics that exert the greatest influence on workers' judgments about their jobs' desirability and should give each of these job characteristics a weight equal to its average importance in the eyes of the labor force as a whole.

No index can be ideal for all purposes. Our index tries to predict how "good" workers will say a job is and, ultimately, how workers with perfect information would choose among the jobs available to them. It does not try to embody all the job characteristics that influence other consequences of employment and is not a substitute for indices designed to predict other consequences. Kohn and Schooler (1983), for example, have tried to design measures that predict workers' psychological functioning, while Wright (1985) has tried to design measures that predict social and political behavior. We would not expect our index to serve as a substitute for theirs in research of the kind they conducted.

While our primary goal is to propose a system for ranking jobs, our second goal, as our subtitle suggests, is to propose a system for ranking individuals according to their success in the labor market. When we speak of labor-market success, we do not mean subjective success. Rather, we mean how well workers have done in the competition for what other workers regard as desirable jobs. If our index of job desirability succeeds in ranking jobs the way the average American worker would rank them, it should provide a valid measure of individual success as we have defined it. A measure of this kind is also appropriate for measuring the overall degree of inequality in labor-market rewards and for estimating the effects of personal characteristics on such rewards. Indeed, it is probably appropriate in most situations where investigators would ordinarily use either occupational status or earnings as a dependent variable.

Skeptics may wonder whether a new measure of either jobs' desirability or individuals' labor-market success is really necessary. Economists have been using hourly wages and annual earnings for this purpose since the 19th century. Sociologists have been using measures of occupational status for much the same purpose since the 1930s. Why, then, should we propose a new approach to measuring success at this late date? The answer is twofold. First, there are strong empirical reasons for doubting that either earnings or occupational status is a good proxy for a job's overall desirability. Second, investigations that use occupational status to measure labor-market success often reach conclusions quite different from those of investigations that use earnings to measure success (see, e.g., Sewell and Hauser 1975, or Jencks et al. 1979). This obviously raises the question of which approach is "correct" or, more precisely, which approach best approximates the results we would obtain using a more theoretically satisfying measure.

## Problems with Occupational Status

No measure of occupational status can be better than the occupational classification scheme on which it is based. In America, the most widely used measures of occupational status all build on the Census Bureau's occupational categories. These categories change from one census to the next, but there are typically 300–500 of them. While some of these detailed (three-digit) categories may be internally homogeneous, most are not. Both the chairman of IBM and the manager of a local typewriter repair store fall into the category "salaried manager: business services," for example, while both James Reston and an obituary writer for a local paper fall into the category "editors and reporters," and both a U.S. Supreme Court justice and a traffic court judge fall into the category "judges."

One way to assess the census classification scheme is to ask whether it groups together jobs with similar characteristics. If most of the variance in job characteristics is between occupations, occupational titles can tell us most of what there is to know about jobs. If most of the variance is within occupations, occupational titles cannot tell us much about specific jobs. To get a rough sense of the explanatory power of the Census Bureau's occupational classification scheme, we turn to the 1980 Survey of Job Characteristics (SJC). The SJC was a national telephone survey designed by the authors and conducted by the Center for Survey Research (CSR) at the University of Massachusetts, Boston. Its target population included approximately 1,065 men and women who were over the age of 18, who worked at least 20 hours a week for pay at one job, and who

TABLE 1

MEANS, STANDARD DEVIATIONS, AND PERCENTAGE OF VARIANCE BETWEEN THREE-DIGIT OCCUPATIONS FOR JOB CHARACTERISTICS THAT INFLUENCE JOB RATINGS IN THE 1980 SJC

| Variable, Question, and Coding | Mean (SD) | Percentage of Variance between Occupations* |
|---|---|---|
| lnEarnings: "How much do your total earnings from your present job figure out to be a year, before taxes and deductions?" (natural log of reported value/15,154)† | .029 (.674) | 40.1 |
| lnEarnings:$^2$ (ln[Earnings/$15,154])† | .454 (.802) | 5.4 |
| Hours GT 35: "How many hours do you usually work each week on your main job, not counting time you take off for meals?" (response − 35; minimum value = 0) | 8.48 (8.27) | 23.6 |
| Vacation weeks: "Other than holidays like the Fourth of July or Labor Day, how many paid vacation days are you allowed to take off each year?" (response/5) | 2.32 (2.21) | 16.6 |
| On-the-job training: "Do you feel that a person on your job *learns new things* that could lead to a better job or to a promotion?" (yes = 1, no = 0) | .849 (.359) | 15.6 |
| Risk of job loss: "What chance do you think there is that you will lose your job completely in the next two years? Could you give your answer as a percentage between zero and 100?" (response/100) | .130 (.245) | 6.5 |
| Educational requirements: "How many years of education do *most* people in jobs like yours have?" (0–17)‡ | 13.18 (2.23) | 48.8 |
| Proportion repetitive: "What percentage of the time do you do the same things over and over?" (response/100) | .526 (.350) | 27.2 |
| Gets dirty at work: "In your present work, do you get dirty?" (yes = 1, no = 0) | .515 (.500) | 29.2 |
| Decides own hours: "Can you decide what time to come to work and when to leave, either officially or unofficially?" (yes = 1, no = 0) | .361 (.481) | 29.6 |
| Frequent supervision: "About how many times an hour, day, week, month, or year does a supervisor check up on your work?" (never = 0, self-employed = 0, yearly or more but less than daily = 0.5, daily or more = 1) | .510 (.410) | 12.2 |

TABLE 1 (*Continued*)

| Variable, Question, and Coding | Mean (SD) | Percentage of Variance between Occupations* |
|---|---|---|
| Union contract: | | |
| "Is your current job covered by a union contract?" (yes = 1, no = 0) | .253 (.435) | 23.0 |
| State/local employee: | | |
| "Are you employed by . . . a government agency, a non-profit organization, or a profit-making business? [If government agency, ask:] Is that a branch of federal, state, or local government?" (state or local = 1, others = 0) | .103 (.304) | 43.6 |
| Federal employee: | | |
| Same questions (federal = 1, others = 0) | .055 (.228) | 26.0 |
| Boss has a boss: | | |
| "Does your boss have a boss?" (yes = 1, all others = 0) | .721 (.449) | 27.7 |

NOTE.—Preparatory and explanatory remarks by interviewers are not reproduced. The full questionnaire is available on request. All statistics are for respondents with complete data ($N$ = 621). Earnings are the primary source of missing data ($N$ = 89).

* Adjusted for degrees of freedom.

† For about 5% of all respondents, annual earnings were estimated from weekly earnings.

‡ Missing data coded 12. A dummy for missing data on this variable has a small and insignificant coefficient in table 2.

worked no more than 10 hours a week at any other job. The CSR interviewed 76% of these potential respondents ($N$ = 809). The typical interview took 40 minutes.[2]

The SJC collected data on 48 characteristics of the primary job held by each respondent. It also asked three open-ended questions that allowed us to assign each respondent to three-digit occupation and industry categories. As we shall see, 14 of these job characteristics had statistically significant effects on the way workers evaluated their jobs. Table 1 lists these 14 job characteristics and shows the estimated percentage of the total variance in each characteristic explained by the three-digit occupational categories that the Census Bureau used in 1970. The explanatory power of the census classification ranges from a high of 49% for Educa-

[2] Despite the restrictions we placed on our sample and the fact that we did not interview workers living in households without telephones, the ages, sexes, races, marital statuses, occupations, and industries of our respondents did not differ significantly from those of employed persons in the July 1980 Current Population Survey. Perman (1984) discusses the sample in more detail.

tional Requirements to a low of 7% for Risk of Job Loss.[3] At best, then, three-digit occupational titles can explain half the variance in those job characteristics that determine job ratings.

The results in table 1 are hardly surprising. The primary goal of occupational classification has traditionally been to group together jobs that required similar technical skills or activities. Technical considerations are fairly strongly related to educational requirements and economic rewards (but see Hodge and Siegel 1966). As a result, occupational classifications tend to group together jobs with similar educational requirements and economic rewards. Many other job characteristics that are important to workers vary with the organizational setting in which work occurs. Occupational classifications seldom take organizational settings into account, so they do not capture much of the variation in these jobs characteristics. Since measures of occupational status take occupations as their building blocks, they can never hope to capture the effects of job characteristics that vary mostly within occupations. Nonetheless, as we shall see, many such job characteristics are important to workers.

## Problems with Earnings

Focusing on earnings instead of occupational status would allow us to take account of variation among jobs with the same occupational title, but it has other costs. Many nonmonetary job characteristics exert significant effects on workers' evaluations of their jobs. In the 1972–73 Quality of Employment Survey conducted by the Survey Research Center at the University of Michigan, for example, willingness to recommend your current job to a friend correlated 0.18 with its educational requirements and 0.12 with whether the job provided steady employment, compared with only 0.09 with annual earnings (tabulations by the authors). In the SJC, as we shall see, the combined effect of nonmonetary job characteristics on job ratings is more than twice that of earnings. Equating a job's overall desirability with its pay is therefore likely to be quite misleading.

[3] We used the Bureau's 1970 occupational classification because we wanted to be able to assign occupations a score on Duncan's (1961) "socioeconomic index" and Siegel's (1971) "prestige" scale. Such scores were not available for the 1980 census's occupational categories. We used the SPSS "breakdowns" routine to estimate the within-group sum of squares ($SS_w$) and degrees of freedom ($df_w$). Since $SS_w/df_w$ is an unbiased estimate of the within-group variance for the population as a whole ($s_w^2$), the explanatory power of the census classification scheme ($\bar{R}^2$) is equal to $1 - s_w^2/s_t^2$ where $s_t^2$ is the total variance.

## Alternatives to Occupational Status and Earnings

A satisfactory measure of a job's desirability (or of labor-market success) must combine the best features of both occupational standing and earnings. Specifically, it should have two attributes: (1) Like an occupational prestige score, a good measure of a job's desirability should be as sensitive to nonmonetary as to monetary differences among jobs. (2) Like earnings, a good measure of labor-market success should be as sensitive to differences among jobs with the same occupational title as to differences among jobs with different occupational titles.

One obvious way to meet these requirements would be to ask workers how good their jobs are or, perhaps, how "satisfied" they are with their jobs. This approach has serious limitations, however. Suppose that women rate their jobs more favorably than men. Does this mean that women hold jobs that are better in some objective sense? Or does it just mean that women are more inclined to express positive views about everything, including jobs that are objectively identical to those held by men? Because completely subjective measures do not allow us to address this question, they are not promising substitutes for occupational status or earnings.

A second possible approach would be to ask outside observers how good jobs are. This approach escapes the pitfalls of pure subjectivism, but it is almost impossible to operationalize. The way we rank other people's jobs depends on what we know about them. If people know only a job's occupational title, for example, their rankings will closely approximate the Siegel scale (Siegel 1971). If people also know how much jobs pay, their rankings will depend more on pay (Coleman and Rainwater 1978). If they know all the characteristics listed in table 1, they will produce still another set of rankings, imperfectly related to the first two. Thus, if we want to predict how outside observers would rank specific jobs, we must first specify exactly what these outsiders know about the jobs in question. Since different outsiders usually know different amounts, this path leads quickly to the Slough of Despond.

To get around these difficulties, we adopted a third approach to measuring a job's desirability. We asked workers to evaluate their own jobs and then weighted each job characteristic according to its *average* effect on these ratings. This approach solves the problem of imperfect information by asking the person who knows a job best, namely the worker who holds it, to report its characteristics. Yet it avoids complete subjectivity by weighting job characteristics according to their average weight in the eyes of *all* workers, not according to the varying weights that individual workers assign them.

A critic might object that, while this is a plausible approach to ranking

*jobs,* it is unnecessarily cumbersome for ranking *individuals*. When ranking individuals, we could, after all, rely directly on their subjective judgments about their jobs. A surgeon who thought she had a terrible job would then be classified as unsuccessful, and a cleaning lady who thought she had an ideal job would be classified as successful. This approach makes sense for certain purposes. In this paper, however, we are concerned with measuring *competitive* success in the labor market. To do this, we must rank workers' jobs the way *others* would typically rank them if they knew what the jobholder knew, ignoring the jobholder's idiosyncratic values. For our purposes, in other words, labor-market success is a matter of getting what others want, even if this is not what you want.

## THE INDEX OF JOB DESIRABILITY

Our strategy for estimating a job's average desirability in the eyes of workers involved five steps. First, we searched the literature for job characteristics that might influence the way workers evaluated their jobs. Second, we designed a set of survey questions that asked workers whether their jobs had these characteristics. Third, we asked these same workers how "good" they thought their jobs were compared with the average job. Fourth, we regressed each job's perceived "goodness" on its objective characteristics. Fifth, we used the coefficients from this regression equation to construct an index of job desirability (IJD). This index predicts the mean rating of a job with any specified combination of objective characteristics.[4] We will discuss the five steps sequentially.

### Identifying and Scaling Job Characteristics

We were not able to identify any studies that explicitly related jobs' objective characteristics to workers' judgments about how "good" or "desirable" their jobs were. There is, however, a large body of research on the determinants of job "satisfaction" (see Gruneberg 1979; Locke 1976; and Seashore and Taber 1975, for reviews). Most of this research deals

[4] This approach to estimating a job's desirability is in many respects similar to Duncan's approach to estimating an occupation's socioeconomic rank. When Duncan (1961) devised his socioeconomic index for occupations, survey data on the standing of different occupations were available for only 45 census occupational titles. Duncan therefore used two objective characteristics of occupations, namely, the educational attainments and incomes of men working in them, to estimate their socioeconomic rank. To determine the relative weight of these two occupational characteristics, he looked at their weights in a regression equation predicting the "general standing" of the 45 occupations for which general standing was available.

with the way workers' overall feelings of satisfaction or dissatisfaction relate to their feelings about specific features of the job, such as whether the pay is "good," the work "interesting," or the supervisor "fair." Some investigators have, however, tried to relate satisfaction to what we will call "objective" job characteristics, namely those characteristics that all workers in the same job should in principle describe in the same way. (Pay, fringe benefits, occupational title, and working hours are obvious examples.) We culled this literature for potentially important objective job characteristics, trying to err on the side of inclusiveness.

A related research tradition in labor economics tries to estimate the monetary value that workers assign to various nonmonetary features of their jobs. Since labor markets are not in competitive equilibrium, and since we cannot measure all the worker characteristics that employers value, this literature on "compensating differences" does not yield reliable estimates of the value workers assign to most nonmonetary job characteristics (see the reviews by Smith 1979; Brown 1980). Nonetheless, we culled this literature for job characteristics that economists regarded as potentially important.

Finally, having surveyed these literatures, we conducted several large pretests in which we asked respondents open-ended questions about what they thought made their jobs good or bad. On the basis of these interviews and the research literature, we identified 48 job characteristics whose influence we would try to assess. For ease of presentation, we group them under 11 headings and identify those that exerted significant effects on job ratings with asterisks.

*Pay* (Earnings,* Variability of Earnings)

*Fringe Benefits* (Vacation Weeks,* Pension, Medical Insurance, Sick Days)

*Hours* (Hours per Week,* Night Work, Weekend Work)

*Occupation* (Duncan Score, Siegel Score, Three-digit Census Occupational Category)

*Training and Promotion Opportunities* (On-the-Job Training,* Proportion Promoted, Job Ladder Upward)

*Hazards* (Risk of Job Loss,* Risk of Layoff, Risk of Disability)

*Educational Requirements**

*Technical Characteristics* (Proportion Repetitive,* Gets Dirty,* Heavy Dirt such as Oil or Grease, Sweats at Work, Proportion Paperwork, Proportion of Time Spent at Desk, Proportion of Time Spent Talking)

*Autonomy* (Decides Own Hours,* Frequent Supervision,* Self-employed, Can Receive Phone Call at Work, Can Make Phone Call at Work, Can Receive Visitor at Work, Can Leave Work without Permission)

*Authority* (Supervises Others, Number of Subordinates, Controls Others' Pay, Has Budget)

*Organizational Setting* (Union Contract,* Federal Employee,* State or Local Employee,* Nonprofit Employee, Has a Boss, Boss Has a Boss,* Job Ladder from Below, Organization Size, Workplace Size, Multilocation Organization, Number of Immediate Co-workers)

This list omits job characteristics that seemed to us explicitly evaluative, such as whether the respondent judged the job "interesting," but it includes some with implicitly evaluative components, such as whether the job is repetitive ("you do the same things over and over"). The list also excludes "subjective" measures on which different workers in the same job seemed likely to disagree. The line between "objective" and "subjective" job characteristics is never precise, however, and some of our measures clearly have a subjective element.

Table 1 shows how we measured the 14 job characteristics that had significant effects on job ratings. Since one of our primary goals was to compare the effect of earnings with the effect of nonmonetary job characteristics, we paid special attention to the problem of scaling earnings. The relationship of job ratings to earnings is not linear in either dollars or the logarithm of dollars. The most satisfactory way to describe the relationship is with a quadratic in the log of earnings. We divided earnings by its median ($15,141) before taking its logarithm, so the variable denoted as lnEarnings is actually ln(Earnings/15,141).

We also conducted extensive experiments with the scaling of hours. Economic theory suggests that the market determines what workers can earn per hour and that workers then largely determine the number of hours they want to work at their "market" rates. In such a world there should be little or no correlation between a job's usual weekly hours and its average rating once we control the job's other characteristics.

Our data support this theoretical expectation for jobs requiring fewer than 35 hours a week. There is no relationship between usual weekly hours and job ratings among workers in such jobs. For jobs requiring more than 35 hours a week, however, the effect of additional hours is the opposite of what economic theory predicts. Every hour beyond 35 leads to a higher job rating, even with annual earnings controlled. This remains true with all the other nonmonetary job characteristics in table 1 controlled. We therefore constructed a variable (Hours GT 35) that measured the number of hours, if any, that our respondents worked in excess of 35 per week. This variable has a value of zero for those who usually work 20–35 hours a week. (The SJC excludes those who usually worked fewer than 20 hours a week.)

Rather than assuming that long hours raise job ratings, some readers

may suspect that a favorable job rating leads workers to put in longer hours. Long hours may, in other words, be an indirect job rating. To test this hypothesis we investigated the relationship between hours and job ratings among workers who said they did not control their own hours. The relationship was weaker for such workers, but it was still positive. We therefore assume that long hours are also a proxy for other job characteristics that workers value and that the SJC failed to measure. Identifying these unmeasured job characteristics should be a high priority in future research.[5]

The other job characteristics in table 1 are largely self-explanatory. Federal Employee, State or Local Employee, Decides Own Hours, Boss Has Boss, Union Contract, On-the-Job Training, and Gets Dirty at Work are dichotomous. Criterion scaling against workers' job ratings suggested that we could measure Frequent Supervision adequately using an equal-interval three-point scale. Vacation Weeks, Risk of Job Loss, Proportion Repetitive, and Education of Co-Workers are all continuous. We investigated the nonlinear effects of these variables, but our nonlinear models did not predict job ratings significantly better than the linear approximation. This does not mean that these job characteristics "really" have linear effects; it just means that our sample is too small to say anything meaningful about nonlinear effects.[6]

## Measuring Jobs' Desirability

We used magnitude estimation to measure a job's perceived desirability (Stevens 1971; Hamblin 1974). Near the beginning of the SJR interview, we asked:

> Taking everything into account—pay, fringe benefits, working conditions, kind of work, etc.—when *most people* think of *average jobs* they think of jobs like telephone operator, carpenter, or payroll clerk. Let's give an average job a rating of 100. Compared to an average job like one of these, I would like to ask you to rate your own job. If you think your own job is *twice* as good as an average job, for example, give it 200. If you think your job is *half* as good as an average job, give it a 50. You can give any number

[5] The best predictors of Hours GT 35 are being male, controlling other people's pay, having a budget of one's own at work, high annual earnings, and being self-employed. With the exception of high annual earnings, none of those variables has an appreciable effect on job ratings with other job characteristics controlled.

[6] We also looked for nonlinear effects among the 34 job characteristics that did not enter our index of job desirability. We did not conduct an exhaustive search for interactions, however, since testing even the two-way multiplicative interactions among 48 job characteristics would have meant creating more variables than respondents.

> you like. *Considering everything,* if an average job is rated 100, how would you rate your job?

Near the end of the interview, after respondents had described their jobs in considerable detail and had also rated a number of hypothetical jobs, the interviewer again asked:

> Now that you've rated so many jobs, I'd like to give you another chance to rate your own job. Compared to all the jobs in the country, if an average job is 100, what number would you give your job? Don't worry about remembering your first number—just tell me what number you think fits your job.

Since we had asked respondents to rate jobs on a scale for which the implied "distance" between ratings of 50 and 100 was the same as that between ratings of 100 and 200, we expected changes in job characteristics to change jobs' ratings by a fixed percentage rather than by a fixed numerical amount. Like other investigators who have used magnitude estimates, we therefore took the natural logarithm of job ratings before analyzing their relationships to one another or to other variables.[7] The distributions of responses to the two rating questions were almost identical, but the two sets of responses correlated only 0.77. While the first set of responses exhibited slightly higher correlations with most job characteristics than the second, the geometric mean of the two responses (denoted hereafter as $\ln R$) yielded even higher correlations, so we used it to estimate the overall "goodness" of jobs. Using the Spearman-Brown formula, the estimated "internal" reliability of the resulting measure is $(2)(0.77)/(1 + 0.77) = 0.87$. The "test-retest" reliability is presumably lower.

We used telephone operators, carpenters, and payroll clerks as examples of "average" jobs because these occupations had Siegel scores near the midpoint of the national distribution (Siegel 1971). Nonetheless, the median worker rated his or her job 150, not 100.[8] Because preferences

[7] Correlations involving untransformed job ratings are all much lower than those reported in the text, and the bivariate plots are strongly heteroscedastic.

[8] Two-thirds of all ratings were above 100, 11% were exactly 100, and only 21% were below 100. Seven respondents rated their jobs 10,000 or more on at least one occasion, and three rated them 10,000 or more on both occasions. The logs of job ratings from 10 to 2,000 exhibit a tidy linear relationship to most other variables, but respondents rating their jobs 10,000 or more did not differ from respondents rating their jobs between 500 and 2,000. This suggests that respondents who rated their jobs 10,000 or more were simply naming what they thought of as "a big number," not trying to convey stronger feelings than respondents who named values of 500–2,000. We therefore recoded values in excess of 2,000 to 2,000 for the analyses reported here. Analyses that set no ceiling on ratings are essentially identical to those reported here. With a ceiling of 2,000, the skew of the logged distribution is 0.66.

vary and workers can select jobs that fit their preferences, workers may typically be 50% better off than they would be if jobs were assigned randomly. Alternatively, the fact that workers typically rate their jobs 50% better than the average job could just indicate a tendency toward self-congratulation. There is, however, no obvious reason why this tendency should alter the average effect of objective job characteristics on job ratings.

## Regression Results

We can think of a job's rating as a function of the job's objective characteristics, weighted by the value that the jobholder assigns to these characteristics. Using the subscripts $i$ to denote the $i$th individual, $j$ to denote the $j$th job, and $1 \ldots n$ to denote all a job's possible characteristics (or combinations of characteristics), we can write

$$\ln R_{ij} = B_{i0} + B_{i1}C_{1j} + \ldots B_{in}C_{nj}, \tag{1}$$

where $\ln R_{ij}$ is the $i$th individual's rating of the $j$th job, $C_{1j} \ldots C_{nj}$ represent the $n$ characteristics of the $j$th job, $B_{i1} \ldots B_{in}$ represent the weights that the $i$th worker assigns these characteristics, and $B_{i0}$ is the rating the $i$th worker would give a job with values of zero on $C_1 \ldots C_n$. Averaging across all individuals, we get

$$\ln R_j = B_0 + B_1C_{1j} + \ldots B_nC_{nj}, \tag{2}$$

where $\ln R_j$ is the average rating of the $j$th job, $B_1 \ldots B_n$ are the average values that workers assign to characteristics $C_1 \ldots C_n$, and $B_0$ is the average rating of a job with values of zero on all the job characteristics in the equation.[9]

The regression of $\ln R$ on the 14 job characteristics in table 1 yields estimates of $B_1 \ldots B_n$. Table 2 shows these estimates for the 621 SJC respondents with complete data on the variables in table 1. These 14 job characteristics explain 41% of the variance in $\ln R$. The remaining 59% is attributable to some mix of specification error, omitted job characteris-

[9] The logarithmic transformation of the dependent variable in eq. (1) means that a one-unit increase in a given job characteristic multiplies a job's expected rating ($R$) by $e^B$. When $B$ is small, $e^B$ approximates $1 + B$. If $B_1 = 0.05$, e.g., a unit increase in $C_1$ will multiply the average rating by $e^{.05} = 1.0513$. Note, however, that a 10-unit increase will multiply the average rating by approximately $1.05^{10} = 1.63$, not by $1 + (10)(.05) = 1.5$. When the independent variable is also logged, $B$ is an elasticity. If the coefficient of lnEarnings is 0.25, e.g., a 1% increase in earnings leads to a 0.25% increase in job ratings.

TABLE 2

REGRESSION OF JOB RATINGS ON JOB CHARACTERISTICS: SJC RESPONDENTS WITH COMPLETE DATA

| Independent Variables | Equation (1) | | | Equation (2) | | |
|---|---|---|---|---|---|---|
| | *B* | SE | Beta | *B* | SE | Beta |
| lnEarnings | .226 | .040 | .214 | .290 | .039 | .275 |
| lnEarnings$^2$ | .068 | .028 | .077 | .064 | .028 | .074 |
| Educational requirements | .053 | .012 | .165 | .052 | .012 | .163 |
| Hours GT 35 | .012 | .003 | .145 | .013 | .003 | .151 |
| On-the-job training* | .265 | .065 | .134 | .266 | .066 | .134 |
| Gets dirty at work* | −.188 | .049 | −.132 | −.183 | .049 | −.129 |
| Vacation weeks | .035 | .011 | .107 | ... | ... | ... |
| Decides own hours* | .156 | .055 | .106 | ... | ... | ... |
| Frequent supervision* | −.180 | .059 | −.104 | −.218 | .056 | −.126 |
| Union contract* | .162 | .058 | .099 | ... | ... | ... |
| Proportion repetitive* | −.197 | .072 | −.096 | −.220 | .072 | −.108 |
| Federal employee* | .281 | .117 | .090 | .345 | .099 | .110 |
| State/local employee* | −.165 | .077 | −.071 | ... | ... | ... |
| Boss has boss* | −.131 | .061 | −.082 | ... | ... | ... |
| Risk of job loss* | −.213 | .094 | −.073 | ... | ... | ... |
| Constant | 4.213 | .195 | ... | 4.282 | .195 | ... |
| $\bar{R}^2$ | .414 | | | .387 | | |
| SD of residuals | .544 | | | .557 | | |

NOTE.—$N$ = 621.
* Variable ranges from zero to one.

tics, errors in measuring job characteristics, variation in the weights that individuals attach to various job characteristics, and errors in measuring respondents' feelings about how good their jobs are.

While tables 1 and 2 omit 34 of the 48 job characteristics measured in the SJC, readers should not conclude that those job characteristics have no effect whatever on job ratings. A job characteristic can have a standardized coefficient as large as 0.06 in this sample and still be "insignificant." Furthermore, some variables with observed coefficients of less than 0.06 have "true" coefficients of more than 0.06. We chose to omit job characteristics with statistically insignificant coefficients not because we believed their true coefficients were zero but because variables with standardized coefficients of less than 0.06 add almost nothing to the accuracy of an index. Were we to replicate our results on a larger sample, we would obviously expect some changes in the relative importance of specific job characteristics.[10]

[10] To estimate the correlation between our version of the IJD and the "true" IJD that we would have obtained in a sample of infinite size, we divided the SJC into two

TABLE 3

FREQUENCY DISTRIBUTION FOR THE INDEX OF JOB DESIRABILITY: SJC RESPONDENTS WITH COMPLETE DATA

| Percentile | IJD | Antilog of IJD |
|---|---|---|
| Minimum | 3.71 | 41 |
| First | 3.97 | 53 |
| Tenth | 4.47 | 87 |
| Twenty-fifth | 4.70 | 110 |
| Fiftieth | 5.00 | 149 |
| Seventy-fifth | 5.36 | 212 |
| Ninetieth | 5.65 | 284 |
| Ninety-ninth | 6.14 | 463 |
| Maximum | 6.54 | 689 |
| Mean | 5.038 | 172.4 |
| SD | .465 | 88.0 |
| Skewness | .215 | 1.67 |
| Kurtosis | −.089 | 4.24 |

NOTE.—$N$ = 621.

Nonetheless, two implications of table 2 deserve comment. First, none of our measures of authority at work exerts a significant effect on job ratings once we control earnings, hours, and the like. Most workers appear to value autonomy far more than they value authority. Second, neither a job's Duncan score nor its Siegel score nor its three-digit occupational title has a significant effect on job ratings with the 14 job characteristics in tables 1 and 2 controlled. (We discuss the way in which we tested the effects of three-digit occupational categories in n. 16.)

To obtain a job's score on the IJD, an investigator must obtain values for the variables in table 2, multiply each variable by its unstandardized coefficient, and add the constant. Since a job's IJD score is simply its expected rating, the sample mean of the IJD is the same as the sample mean of ln$R$. Table 3 shows the distribution of IJD scores for respondents with complete data. It also shows the antilogs of the relevant IJD values, so that readers can see the mean ratings of jobs at various points in the

random subsamples. Regressing ln$R$ on all 48 objective job characteristics in each of these subsamples gave us two alternative versions of the IJD. Their noncommon (or "error") variance averaged 0.0464. Since the full sample is twice as large as the samples used to construct these two indices, the error variance of the IJD in the full sample should be only half as large as in the two subsamples (0.0232). Our IJD has a variance of $0.4648^2 = 0.2160$. Its estimated correlation with another IJD derived from an independent sample of similar size is therefore $1 - (0.0232/0.216) = 0.893$, and its estimated correlation with a "true" IJD derived from an infinitely large sample is $0.893^{1/2} = 0.945$.

distribution. The best job in this sample has characteristics that give it a predicted rating of 689, which is more than four times the predicted rating of the median job (149). The worst job has characteristics that give it a predicted rating of only 41.

## Validity

The reader's first question about our proposed index should be whether the weights it assigns to specific job characteristics seem reasonable. Earnings, vacations, on-the-job training, job security, variety, cleanliness, and autonomy are all generally recognized as important components of a good job, and the variables that measure these attributes all have the expected signs. Holding a job with high educational requirements is presumably a proxy for complexity, variety, and perhaps social status. (It could also be a proxy for the respondent's own education, but it remains significant even with the respondent's education controlled.) Having a boss with a boss is presumably a proxy for lack of autonomy and bureaucratization.

The four variables whose presence—or sign—may surprise some readers are Hours GT 35, Federal Employee, State or Local Employee, and Union Contract. As we noted earlier, long hours are probably a proxy for unmeasured job characteristics and perhaps also for the respondent's attitude toward work. Government employment and a union contract may also be proxies for unmeasured job characteristics, but since table 2 controls the most obvious candidates, we are not sure what these unmeasured job characteristics might be. It is worth noting that the positive effect of a union contract on job ratings emerges only after we control for the fact that unionized jobs are low on autonomy and highly repetitive.

The most troubling feature of table 2, and hence of the IJD, is the low coefficient of earnings relative to the coefficients of nonmonetary variables. Since we divided earnings by its median before taking logarithms, Earnings equals one and lnEarnings equals zero for the median worker. The coefficient of lnEarnings with $(\text{lnEarnings})^2$ controlled therefore estimates the elasticity of job ratings with respect to earnings for workers near the median. This elasticity is 0.226.[11] A 1% increase in earnings

[11] The elasticity of ratings ($R$) with respect to earnings ($Y$) is $d(\ln R)/d(\ln Y)$. We can calculate this from table 2 as $0.226 + (0.136)(\ln Y)$. The positive coefficient of $\ln Y$ tells us that each unit decrease in $\ln Y$ decreases the estimated earnings elasticity of job ratings by 0.136. When $\ln Y$ reaches $-1.66$ ($Y = \$2{,}880$), the implied earnings elasticity of job ratings reaches zero. Below \$2,880, the implied elasticity is negative. More detailed analysis suggests, however, that the elasticity is actually positive throughout the entire range of earnings on which the SJC provides data. This should remind us that the quadratic specification of table 2 is only a rough approximation of the "true" (but unknown) functional relationship of ratings to earnings. With a sample of this size, we cannot say more.

TABLE 4

PERCENTAGE INCREASE IN EARNINGS REQUIRED TO OFFSET A ONE-UNIT CHANGE FOR THE WORSE IN EACH NONMONETARY COMPONENT OF THE IJD FOR WORKERS WITH EARNINGS NEAR MEDIAN

| Job Characteristics | Worker's Rating of Own Job | Interviewer's Rating of Job |
|---|---|---|
| Hours GT 35 | 6 | 1 |
| Educational requirements | 26 | 52 |
| Vacation weeks | 17 | 14 |
| On-the-job training | 223 | 157 |
| Gets dirty at work | 129 | 144 |
| Union contract | 105 | 24 |
| Frequent supervision | 121 | 93 |
| Proportion repetitive | 138 | 249 |
| Decides own hours | 99 | 62 |
| Risk of job loss | 157 | 22 |
| Federal employee | 246 | −4 |
| State/local employee | 110 | 27 |
| Boss has a boss | 79 | 113 |

NOTE.—All job characteristics range from 0 to 1 except Hours GT 35, which runs from 0 to 55, Educational requirements, which runs from 0 to 17, and Vacation weeks, which runs from 0 to 18.

(from \$15,000 to \$15,150) thus raises a job's expected rating by 0.226% (e.g., from 150 to 150.3).

Now consider a typical nonmonetary job characteristic, such as Gets Dirty at Work, which has a coefficient of −0.188. For workers with earnings near the median, lnEarnings must increase by about 0.188/0.226 = 0.832 to offset the effect of getting dirty at work. Actual earnings must therefore increase by a factor of $e^{.832} = 2.30$. Equation (1) thus implies that workers rate a clean job paying \$10,000 a year about the same as a dirty job paying \$23,000 a year.[12] Many readers will surely find that hard to believe.

Column 1 of table 4 shows the percentage increase in earnings required to offset a unit change for the worse in each nonmonetary component of the IJD.[13] Most of these implied monetary values are also likely to strike readers as too high. We therefore tried to check the validity of our results in a variety of ways.

[12] This estimate ignores the change in $(\ln Y)^2$, but in this example the distortion is negligible.

[13] Readers should bear in mind that the effects of continuous variables in our preferred specification are exponential rather than multiplicative. Among workers with earnings near the median, e.g., a four-year increase in Co-workers' Education is equivalent to increasing the job's pay by a factor of $1.22^4 = 2.22$, not $1 + (4)(.22) = 1.88$.

One possible source of upward bias is the job-rating task itself. The first SJC question defines "average" jobs not in terms of what they pay but in terms of their occupational titles (telephone operator, carpenter, or payroll clerk). Using occupational titles may focus respondents' attention on what people do at work rather than what they are paid for it. But this potential problem should have been less severe in our second question, which followed a long series of questions about what different jobs had to pay in order to be equally good. Earnings have similar effects on both sets of ratings, so we doubt that our description of the average job accounts for our results.

Furthermore, the SJC used more than one technique to estimate the monetary value workers placed on nonmonetary job characteristics. Our most direct approach was a "wage-equalizing" question that asked respondents to imagine two jobs and then posed questions such as "The first job pays $14,000 a year and you don't get dirty at work. What would a second job where you do get dirty at work have to pay for you to consider the two jobs equally desirable?" We asked questions of this sort about 15 of the 47 nonmonetary job characteristics on which the SJC collected data (see Perman [1984] for details). Averaging across all 15 characteristics, the "wage-equalizing" method implies monetary values 87% of those obtained using the job-rating method. This difference is neither statistically significant nor large enough to be of any practical importance.

While the wage-equalizing questions do not show any general evidence of *methodological* bias in the IJD, they do suggest some *statistical* bias. Only four of the 15 job characteristics for which we have wage-equalizing values appear in the IJD. These are Vacation Weeks, Decides Own Hours, Gets Dirty at Work, and Risk of Job Loss. These four characteristics appear to be worth only a third as much using the wage-equalizing method as using the job-rating method. The reason is presumably that, when we regress job ratings on job characteristics in a relatively small sample, some job characteristics have observed coefficients appreciably larger than their true coefficients, while others have observed coefficients appreciably smaller than their true coefficients. Since the IJD includes only characteristics with standardized coefficients larger than 0.06, it includes a disproportionate number of characteristics whose observed coefficients exceed their true coefficients. This bias occurs in all indices derived from small samples. For this reason, readers should not place much confidence in the coefficients of specific job characteristics. At the same time, the global comparison between the wage-equalizing and job-rating results suggests that the IJD does not exaggerate the *overall* importance of nonmonetary job characteristics.

We also checked the validity of the IJD coeefficients by comparing the

factors that influenced workers' ratings of their own jobs with the factors that influenced interviewers' ratings of these same jobs. We asked SJC interviewers to rate the respondent's job at the end of the 40-minute interview by using the same magnitude estimation technique that they had asked workers to use. Survey interviewers' criteria for evaluating jobs may differ from other people's criteria, but there is no reason to suppose that they are less interested in money than the average worker.

The median interviewer rating was 100, not 150, which is what we expect from outsiders rating randomly selected jobs. Interviewer ratings correlated only 0.52 with respondents' ratings of their own jobs, so interviewers did not accept respondents' judgments about their jobs uncritically. A job's Duncan or Siegel score was the most important single determinant of the interviewer's rating. With all the job characteristics in table 1 controlled, Siegel scores had a standardized coefficient of 0.21 in the equation predicting interviewer ratings, compared with only 0.02 in the equation predicting workers' ratings of their own jobs. Substituting Duncan scores for Siegel scores yielded similar results. Interviewers, it seems, rely on a job's occupational title to assess a job. Workers do not.

The critical question, however, is whether interviewers place more weight on jobs' pay than workers do. When we regressed the log of interviewer ratings on the job characteristics in table 2, lnEarnings had roughly the same effect on interviewers' ratings ($B = 0.270$) as on respondents' ratings ($B = 0.226$). Most of the nonmonetary job characteristics in table 2 had somewhat less effect on interviewer ratings than on respondent ratings. Column 2 of table 4 shows the monetary value interviewers implicitly assigned each nonmonetary job characteristic. The monetary values of nonmonetary job characteristics implicit in interviewers' job ratings average 66% of those implicit in respondents' job ratings. In part, no doubt, this reflects the combined effects of sampling error and eliminating variables with low coefficients from our index. In part, however, it may reflect systematic differences between interviewers and respondents.

Perhaps the most dramatic difference between interviewers and respondents is the low value interviewers assign to Risk of Job Loss. This is hardly surprising. Individuals who place a high value on job security are unlikely to take temporary jobs as survey interviewers. Interviewers also place less value than workers on long hours, union contracts, working for the federal government, and not working for a state or local government. If we are right that these four job characteristics are proxies for other unmeasured attributes of jobs, it is not surprising that they affect respondents' judgments more than interviewers' judgments. If we confine our attention to the nonmonetary job characteristics that have traditionally

loomed large in discussions of what constitutes a "good" job, table 4 suggests that interviewers assign these characteristics about the same monetary value that workers do.

Finally, we compared our results with results using job "satisfaction" as a criterion. The SJC asked half its respondents a question about job "satisfaction" drawn from the 1972 Quality of Employment Survey, namely: "All in all, how satisfied would you say you are with your job—very satisfied, somewhat satisfied, not too satisfied, or not at all satisfied?" Responses to this question correlate 0.49 with job ratings ($\ln R$). Four nonmonetary job characteristics (Risk of Job Loss, On-the-Job Training, Controls Own Hours, and Frequent Supervision) had very large effects on respondents' answers to this question. Taken together, these four questions explained 23% of the variance in reported satisfaction. Once we controlled these four nonmonetary job characteristics, lnEarnings had a standardized coefficient of only 0.03. Taken at face value, these results imply that earnings have almost no effect on satisfaction. Other surveys that have asked about job satisfaction tell a similar story (Jencks 1982).[14] Data on job satisfaction do not suggest, in short, that the IJD understates the importance of earnings relative to jobs' nonmonetary attributes.

While none of the foregoing tests indicates that the IJD underestimates the importance of pay relative to nonmonetary job characteristics, several additional checks would be desirable. First, our results should be replicated using a description of the "average" job that refers explicitly to each component of the IJD. Second, they should be replicated by asking workers how favorably they think *others* would rate jobs like theirs. Third, they should be replicated by collecting data on each respondent's previous job and investigating whether *changes* in job characteristics lead to changes in job ratings of the magnitude predicted by the IJD. Fourth, they should be replicated by asking workers to discuss their jobs with one another and then rate them.

[14] The bivariate correlation of lnEarnings with job satisfaction in the SJC is 0.24, which is slightly *higher* than in the 1972 Quality of Employment Survey or in the General Social Survey. The modest effect of earnings on satisfaction may be attributable to the fact that standard questions about job satisfaction do not give different respondents a common benchmark with which to compare their jobs. In the absence of explicit instructions, some respondents presumably compare their present jobs with an "average" job, while others compare it with their previous jobs, with the jobs they expect to hold in the future, with their friends' jobs, with the worst job they can imagine, or with the best. When respondents' current job characteristics are correlated with the characteristics of the jobs they use as benchmarks, the coefficients of these job characteristics in an equation predicting reported satisfaction will underestimate their effects on "true" satisfaction. This kind of bias is likely to be especially severe for earnings.

TABLE 5

RELATIONSHIP OF PERSONAL CHARACTERISTICS TO JOB RATINGS WITH AND WITHOUT IJD CONTROLLED: SJC RESPONDENTS WITH COMPLETE DATA

| | MEAN | SD | BIVARIATE CORRELATION WITH ln$R$ | STANDARDIZED REGRESSION COEFFICIENT | | |
|---|---|---|---|---|---|---|
| | | | | With Only IJD Controlled | With Only Duncan Score Controlled | With Only lnEarnings Controlled |
| White | .89 | .32 | .106 | −.009 | .050 | .042 |
| Male | .63 | .48 | .196 | −.037 | .206 | −.062 |
| Education | 13.2 | 2.5 | .382 | .034 | .263 | .249 |
| Experience | 15.5 | 11.2 | .132 | −.006 | .122 | −.011 |

NOTE.—$N$ = 621.

In the absence of evidence from such experiments, we believe that the most plausible interpretation of our findings is that nonmonetary job characteristics are far more important to workers than most of us ordinarily assume. Readers should bear in mind, however, that our results apply only to the way in which workers rate jobs they have actually held. They do not purport to describe the way workers evaluate jobs that they have not held. Workers who have to choose among jobs they have not held usually have less information about these jobs than they have about the jobs they currently hold. Like our interviewers, such workers are likely to rate jobs they have not held on the basis of characteristics that are easy to ascertain in advance, like pay and occupational title. As a result, they probably put more weight on pay when making prospective judgments than when making retrospective ones.

## How Complete Is the Index?

Our index of job desirability surely omits some job characteristics that are important to the average worker. One way to assess the importance of omitted job characteristics is to exploit the fact that personal characteristics influence workers' bargaining positions in the labor market. We know from both everyday observation and past research that highly educated workers, experienced workers, white workers, and male workers get a disproportionate share of the "good" jobs. Table 5 shows that such workers also rate their jobs more favorably than other workers. If the IJD included all the job characteristics that make such workers rate their jobs especially favorably, personal characteristics should not affect job ratings with the IJD controlled. If the IJD omits important job characteristics, personal characteristics should serve as proxies for unmeasured job char-

acteristics and should appear to influence job ratings even with the IJD controlled.

We can test the hypothesis that the job characteristics in the IJD account for the effect of personal advantages on job ratings by comparing columns 3 and 4 of table 5. Column 3 shows the bivariate correlation of each personal advantage with job ratings. Column 4 shows the standardized regression coefficients of personal advantages with the IJD controlled. These standardized coefficients average −0.004. Since their sampling errors are all about 0.03, none is significantly different from zero. Table 5 thus suggests that the IJD directly or indirectly includes almost all the job characteristics that advantaged workers take into account when they rate their jobs.

For comparative purposes, table 5 also tests the hypothesis that a job's Duncan score explains the effect of personal advantages on job ratings. Column 5 shows that a job's Duncan score explains less than half of the effect of race and education on job ratings and almost none of the effect of gender or experience.[15] Column 6 repeats this exercise using earnings to explain the effect of personal advantages on job ratings. Earnings do a fairly good job in accounting for the effects of race, gender, and experience on job ratings, but they account for only a third of the effect of education on job ratings. Overall, it seems clear that our index does a far better job of accounting for the effect of personal advantages on job ratings than either Duncan scores or earnings do. This is not surprising, since the IJD is designed to explain these particular job ratings, but it is still reassuring.

An even more stringent test of the IJD's comprehensiveness is its capacity to explain the way workers rate jobs with different occupational titles. After adjusting for random sampling error, the Census Bureau's three-digit 1970 occupational classification accounts for 24% of the variance in individual job ratings. If the IJD included all the characteristics that made one occupation seem better than another, it should account for all this interoccupational variance in job ratings. In fact, the IJD accounts for 99.4% of the nonrandom interoccupational variance in job ratings.[16] This means that the IJD includes almost all the job characteristics that account for occupational differences in job ratings, or at least

[15] Including Experience$^2$ does not alter this conclusion.

[16] The total variance of job ratings is 0.5024. Using the ANOVA procedure in n. 2, we find that the nonrandom interoccupational variance of these ratings is 0.1225. When we regress job ratings on the IJD, the total variance of the residuals is 0.2871, while the nonrandom interoccupational variance of the residuals is 0.0007. The IJD thus explains all but 0.0007/0.1225 = 0.6% of the nonrandom interoccupational variance in job ratings.

that it includes good proxies for these characteristics. The IJD compares very favorably in this regard with the Duncan and the Siegel scales. Each of these scales explains 54% of the nonrandom variation in occupations' mean ratings, compared with the IJD's 99.4%.

## Should the Weights Vary by Age, Sex, or Education?

We know from experience that different individuals put different weights on different job characteristics. That is presumably one reason why the job characteristics in table 1 explain only 41% of the variance in individual ratings. But it need not follow that these weights vary systematically by gender, age, education, or other demographic characteristics. To test for such differences, we looked at the multiplicative interactions of the 15 variables in the IJD with gender, age, years of education, and a dummy for having a bachelor's degree.[17] We expected one of these 60 interactions to be significant at the 0.02 level simply by chance. Three were actually significant at this level.[18] By far the strongest interaction was between education and deciding one's own hours. The weight that workers attach to deciding their own hours increases steadily with education ($P = .0001$). The other two interactions that were significant at the 0.02 level had the "wrong" signs from a conventional viewpoint. Older workers put *less* weight on the estimated risk that they would lose a job than young workers did, and men were *more* averse than women to getting dirty at work.

Overall, we are impressed by the degree of consensus among different groups about the relative importance of specific job characteristics. This finding is consistent with studies of occupational prestige, which show great variation in individual estimates of occupations' general standings but few systematic differences among demographic groups (Reiss 1961).

## Cost Considerations

Investigators who want to use the full IJD must ask workers 14 different questions about their jobs. For economists who would otherwise rank jobs entirely on the basis of what they pay, the incremental cost of using the IJD will therefore be substantial. For sociologists who would otherwise use the Duncan or Siegel scale, the IJD could save time and money.

[17] We also looked for race interactions, but because our sample included so few blacks, none was significant.

[18] We expected five additional interactions to be significant at the 0.02–0.10 level by chance. Since only five were in fact significant at this level, we will not discuss them.

In order to assign jobs Duncan or Siegel scores, an investigator must first assign them three-digit occupation and industry codes. Accurate assignment of such scores requires three separate open-ended questions, dealing with the kind of work the respondent does, the respondent's main duties and activities, and the kind of organization the respondent works for. Interviewers must record each respondent's answer to each of these questions in detail. Coders must then read the answers and decide which occupational title best fits each respondent. Supervisors must resolve ambiguous cases. All this takes scarce interview time and expensive coding time. Our 14 closed-ended questions probably take no more interview time than these three open-ended questions, and they cost far less to code. In the near future, however, few sociologists are likely to drop occupation or industry coding, so our questions will add significantly to their costs.

Investigators with limited resources can, however, abbreviate our index by using only the seven nonmonetary characteristics with the largest standardized coefficients. Equation 2 in table 2 shows the weights for this abbreviated IJD. It explains 38.7% of the variance in job ratings, compared with the 41.4% explained by the full IJD, but both estimates are inflated by the way we selected variables for inclusion in the equation. Investigators who have data on some other subset of the variables included in the IJD can construct their own version of it using the correlation matrix in the appendix.

Whenever one asks 14 questions, missing data are also inevitable. The problem is especially severe for earnings. Fortunately, since our index of nonmonetary job characteristics (see below) correlates 0.951 with a job's total IJD score, investigators can estimate a job's IJD score quite accurately even when respondents fail to report their earnings.

## SUBSTANTIVE IMPLICATIONS OF THE INDEX

A new index of labor-market success is of little interest unless it tells us something new about the real world. This section examines three substantive issues. We begin by looking at status-attainment models, asking how substituting the IJD for the Duncan scale would alter our understanding of status attainment. Next, we separate the rewards of work into monetary and nonmonetary components, asking how taking account of jobs' nonmonetary characteristics alters the estimated benefits of being male, having high-status parents, having white skin, having a lot of schooling, and having a lot of labor-market experience. Finally, we ask how substituting the IJD for wages changes our picture of the overall level of inequality among jobs.

## The IJD versus the Duncan Scale

Sociologists' collective understanding of the status-attainment process has been profoundly affected by the fact that Blau and Duncan's classic, *The American Occupational Structure* (1967), measured labor-market success using the Duncan scale. We therefore begin by asking how substituting the IJD for the Duncan scale would alter our understanding of the status-attainment process.[19]

Table 6 shows the bivariate correlations of workers' personal characteristics with their Duncan and IJD scores. Gender has almost no correlation with a worker's Duncan score. This is also true if we rank jobs using the Siegel scale (see table 6) or using Treiman's "international" scale (Treiman and Terrell 1975). As others have noted (see, e.g., Powers 1982), this situation raises serious questions about the meaning of such scales. After all, men earn almost twice as much as women in our sample. Men are also far more likely than women to be in positions of authority (Wolf and Fligstein 1979). The fact that occupation-based scales fail to detect differences between the jobs held by men and women is thus a bit like the thirteenth chime of a clock: it tells us something is fundamentally wrong with such measures. Unlike the Duncan and Siegel scales, the IJD is strongly correlated with gender. The fact that the IJD yields results consistent with everyday experience strengthens our confidence in it.

The same story recurs when we look at the effects of labor-market experience. Everyday observation suggests that experienced workers get better jobs than inexperienced workers. Yet the bivariate correlation of the Duncan scale with labor-market experience is extremely low. This is not surprising, since career advancement after the age of about 30 is typically advancement within a given occupation. Nonetheless, the fact that the IJD picks up these differences while the Duncan scale does not is surely a point in favor of the IJD.

The IJD and the Duncan scale have roughly similar correlations with mother's education, father's education, father's occupation, race, and respondent's education. This means that substituting the IJD for the Duncan scale would not have made much difference to Blau and Duncan, who looked only at men, did not investigate the effects of experience, and

[19] After adjusting for degrees of freedom, the Census Bureau's detailed 1970 occupational classification scheme accounts for 52.7% of the variance in SJC respondents' scores on the IJD. The Duncan scale explains only 28.6% of the variance in the IJD, and the Siegel scale explains only 28.3%. The Duncan and Siegel scales do not, therefore, estimate the perceived desirability of jobs in different occupations very accurately. In addition, 47.3% of the nonrandom variation in the IJD is *within* occupations. The IJD is therefore measuring something quite different from what the Duncan or Siegel scale measures.

TABLE 6

EFFECTS OF RESPONDENT CHARACTERISTICS ON DUNCAN SCORES, SIEGEL SCORES, lnEARNINGS, AND THE INDEX OF JOB DESIRABILITY: 1980 SJR

| | BIVARIATE CORRELATIONS | | | | STANDARDIZED REGRESSION COEFFICIENTS | | | |
|---|---|---|---|---|---|---|---|---|
| | IJD | Siegel Score | Duncan Score | lnEarnings | IJD | Siegel Score | Duncan Score | lnEarnings |
| White | .175 | .131 | .156 | .137 | .047 | .028 | .058 | .037 |
| Male | .348 | .036 | −.028 | .508 | .237 | −.054 | −.118 | .411 |
| Father's education | .153 | .187 | .219 | .032 | −.024 | −.015 | −.001 | .028 |
| Mother's education | .219 | .210 | .230 | .103 | .063 | .056 | .068 | .017 |
| Father white collar | .147 | .123 | .083 | .108 | .056 | .061 | −.020 | .029 |
| Education | .547 | .524 | .539 | .341 | .544 | .530 | .541 | .351 |
| Experience | .193 | .031 | .025 | .298 | .231 | .162 | .162 | .248 |
| $\bar{R}^2$ | | | | | .446 | .296 | .319 | .405 |

NOTE.—$N$ = 621.

used standardized coefficients to assess the relative importance of family background and schooling.

Substituting the IJD for the Duncan scale would, however, alter the unstandardized results of Blau-Duncan models. If we convert the Duncan scale to the IJD's metric, we find that a one standard-deviation increase in a job's Duncan score typically leads to a 28% increase in the job's rating relative to that of the average job. A one standard-deviation increase in a job's IJD score typically leads to a 59% increase in the job's expected rating. The fact that family background and schooling have the same standardized effect on IJD scores as on Duncan scores thus means that their unstandardized effects are about twice as large using the IJD as using the Duncan scale.

## Monetary versus Nonmonetary Rewards

One reason the IJD captures the effects of gender and experience better than traditional measures of occupational rank is that it is sensitive to differences among jobs with the same occupational title. Measures of individual earnings also have this virtue. If an analysis of individual earnings told essentially the same story as an analysis of the IJD, investigators would have little reason to bother with the IJD. In reality, though, substituting the IJD for earnings reduces the relative importance of gender and experience while increasing the relative importance of race, parental advantages, and education (see table 6).

To get a clear picture of the bias inherent in looking only at earnings, we can decompose the IJD into two additive components, which we label Money and Nonmoney. These two indices together include all the job characteristics in the IJD and assign each characteristic the same weight that the IJD assigns it, namely, the unstandardized weight shown in table 2. The Money index measures the effect of a change in earnings when nonmonetary characteristics remain fixed. It is therefore defined as

$$\text{Money} = .226(\ln\text{Earnings}) + .068(\ln\text{Earnings})^2.$$

The Nonmoney index measures the effects of changes in nonmonetary characteristics when earnings remain fixed. It is therefore defined as

$$\text{Nonmoney} = .053(\text{Educational Requirements}) + .012(\text{Hours GT } 35) + \ldots - .213(\text{Risk of Job Loss}) + 4.213.$$

Readers should bear in mind that, although the coefficients of the specific nonmonetary job characteristics we used to construct Nonmoney are inflated by sampling error, other nonmonetary job characteristics have

been omitted from Nonmoney because their observed coefficients were less than their true coefficients. Thus, there is no reason to believe that our Nonmoney index exaggerates the overall value workers place on their jobs' nonmonetary attributes or that it yields systematically biased estimates of the association between workers' personal characteristics and their jobs' nonmonetary rewards. Since Money and Nonmoney include all the variables in the IJD,

$$\text{Money} + \text{Nonmoney} = \text{IJD}. \tag{3}$$

This decomposition allows us to partition the effects of personal attributes into monetary and nonmonetary components. If $B_{IS}$ represents the unstandardized effect of an extra year of schooling on IJD scores, and if $B_{MS}$ and $B_{NS}$ represent the effects of an extra year of schooling on Money and Nonmoney, respectively, equation (3) tells us that

$$B_{IS} = B_{MS} + B_{NS}. \tag{4}$$

The ratio of $B_{MS}$ to $B_{IS}$ in turn tells us the proportion of schooling's overall effect that is accounted for by the fact that schooling leads to higher earnings. Table 7 shows the values of $B_{IS}$, $B_{MS}$, and $B_{NS}$ for various combinations of personal characteristics.

*Gender.*—Equation (3) in table 7 shows that the average worker would rate the kinds of jobs held by men $e^{.324} = 1.38$ times higher than the kinds of jobs women hold. Readers should bear in mind that this coefficient does not, in principle, measure the effect of gender on the way men and women rate their *own* jobs. Rather, it measures the way workers in general would rate jobs with the characteristics reported by the average man and the average woman.[20]

Equations (1) and (2) in table 7 allow us to decompose the overall difference between men's jobs and women's jobs into monetary and nonmonetary components. Comparing equations (1) and (3), we see that pay accounts for $0.154/0.324 = 48\%$ of the overall difference between men's and women's jobs. Since a unit increase in earnings typically raises IJD scores by 0.226, equation (1) tells us that men's jobs pay $e^{.154/.226} = 2.0$ times more than women's jobs in this sample. Equation (2) tells us that men have jobs with nonmonetary advantages equivalent to their being paid $e^{.171/.226} = 2.1$ times more than women. Thus, if jobs' nonmonetary characteristics were to remain as they now are, women's jobs would have to pay $(2.0)(2.1) = 4.2$ times more than they now do in order to be rated as favorably as men's.

[20] Since none of the personal characteristics in table 7 has a significant effect on job ratings independent of the job characteristics in the IJD, the unstandardized effects of personal characteristics on job ratings ($\ln R$) are in practice very similar to their unstandardized effects on the IJD.

## TABLE 7

### UNSTANDARDIZED EFFECTS OF PERSONAL CHARACTERISTICS ON MONETARY AND NONMONETARY JOB CHARACTERISTICS: SJC RESPONDENTS WITH COMPLETE DATA

| | Money (1) | Nonmoney (2) | Total (IJD) (3) | Money (4) | Nonmoney (5) | Total (IJD) (6) | Money (7) | Nonmoney (8) | Total (IJD) (9) | Bivariate Correlations with Money/ Nonmoney (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| White | .047 (.018) | .172 (.043) | .219 (.053) | .037 (.018) | .128 (.043) | .165 (.053) | .012 (.017) | .058 (.037) | .071 (.044) | .122/.168 |
| Male | .154 (.012) | .171 (.029) | .324 (.036) | .153 (.012) | 165 (.029) | .318 (.035) | .122 ( 011) | .101 (.025) | 224 (.030) | .462/ 237 |
| Father white collar | | | | .030 (.020) | .116 (.047) | .147 (.058) | 018 (.018) | .073 ( 040) | .091 (.048) | .105/.140 |
| Father's education | | | | .002 (.002) | .009 (.005) | .011 ( 006) | .001 (.002) | −.002 (.004) | −.002 (.005) | .063/.167 |
| Mother's education | | | | .003 (.002) | .018 (.006) | .021 (.007) | .002 (.002) | .008 (.004) | .010 (.006) | .124/.224 |
| R's education | | | | | | | .022 (.002) | .078 (.005) | .100 (.006) | 344/.544 |
| R's experience | | | | | | | 008 (.002) | .010 ( 004) | .018 ( 004) | .274/ 122 |
| R's experience$^2$ × 100 | | | | | | | −.011 (.004) | −.011 (.009) | −.022 (.010) | 228/.098 |
| $\bar{R}^2$ (SD of residuals) | .219 (.145) | .076 (.350) | .142 (.431) | .228 (.144) | .124 (.341) | .185 ( 420) | .364 (.131) | .369 (.290) | .449 (.345) | |

NOTE.—$N$ = 621. Values in parentheses are SEs.

These estimates do not support the hypothesis advanced by Filer (1985) that women are paid less than men primarily because women trade high pay for desirable nonmonetary characteristics. Women's jobs are inferior to men's on 12 of the 14 nonmonetary measures in the IJD. The two exceptions are Vacation Weeks and Gets Dirty at Work. If we take account of all nonmonetary differences, the overall difference between men's jobs and women's jobs is larger than the pay difference, not smaller.

*Race.*—Analogous calculations for whites and nonwhites bring out the importance of nonmonetary differences even more strongly. Pay differences between whites and nonwhites account for only a fifth of the difference in the expected ratings of the two groups' jobs. The other four-fifths derives from nonwhites' having jobs with less desirable nonmonetary characteristics. Thus, while whites hold jobs paying 23% more than the jobs nonwhites hold, the nonmonetary characteristics of "white" jobs are worth as much as an additional 114% pay advantage. Readers should treat these numerical estimates cautiously, however, since our nonwhite sample is quite small ($N = 74$).

*Parental status.*—Columns 4–6 of table 7 show the effects of three measures of parental status on job ratings. Like previous investigators (Duncan, Featherman, and Duncan 1972; Sewell and Hauser 1975; Corcoran and Jencks 1979), we find that parental status has very modest effects on earnings (see col. 4). When we turn to nonmonetary measures of labor-market success, however, we find much larger effects of parental status (see col. 5). Seven-eighths of the effect of parental education and two-thirds of the effect of having a white-collar father are traceable to the fact that these characteristics influence the nonmonetary characteristics of children's jobs.

*Education and experience.*—Columns 7–10 show the estimated effects of education and experience. Only a quarter of the job-related benefits of schooling take monetary form. Overall, a one-year increase in education increases the median worker's expected job rating by as much as a pay increase of $e^{.102/.226} - 1 = 57\%$. This means that those who focus exclusively on monetary returns to investment in education drastically underestimate the overall value of schooling to workers. We plan to explore the implications of this finding more carefully in future work.

Most of the payoff to labor-market experience also takes nonmonetary form, but the balance between monetary and nonmonetary benefits is more nearly equal for experience than for education.

*Overall inequality.*—Since we measured Money and Nonmoney in the same metric, their variances allow us to compare the contributions of monetary and nonmonetary factors to overall inequality. Using the sub-

scripts $M$ to denote Money, $N$ to denote Nonmoney, and $I$ to denote the IJD, the standard deviations are

$$s_M = 0.164$$
$$s_N = 0.365$$
$$s_I = 0.465.$$

Since $M$, $N$, and $I$ are logged ratio scales, their standard deviations are direct measures of inequality. They tell us that:

1. Equalizing jobs' pay while leaving their nonmonetary characteristics as variable as they now are would lower inequality by 22% (from 0.465 to 0.365).

2. Equalizing jobs' nonmonetary characteristics while leaving the dispersion of pay unchanged would lower inequality by 65% (from 0.465 to 0.164).

3. The overall level of inequality in the labor market is 0.465/0.164 = 2.8 times greater than inequality in pay alone.

If our estimates are even approximately correct, analyses that focus exclusively on wage inequality may prove very misleading. Economists, for example, have often investigated the reasons why earnings inequality changes over time (see, e.g., Chiswick and Mincer 1972). They ordinarily treat earnings inequality as if it were a measure of overall inequality in labor-market outcomes. This assumption is valid if monetary and nonmonetary inequality change in the same way at the same time. If the two trends are even partially independent of one another, however, the trend in nonmonetary inequality will largely determine the trend in overall inequality. Using the trend in monetary inequality as a proxy for the trend in overall inequality may then yield very misleading results.

Suppose, for example, that monetary inequality ($s_M$) were to rise 10% between 1980 and 1990, while nonmonetary inequality ($s_N$) fell 3% and the correlation between them ($r_{MN}$) fell 1%. Conventional economic analysis would conclude that labor-market outcomes had become more unequal and would search either for supply-side explanations, such as increasing inequality in workers' cognitive skills, or for demand-side explanations, such as declining demand for skilled blue-collar workers. In this example, however, overall inequality ($s_I$) would be no greater in 1990 than in 1980. All explanations of the alleged increase in inequality would therefore be misleading. Difficulties of this kind are likely to loom even larger when economists try to explain why wages are more unequal in some countries than in others (see, e.g., Lydall 1968).

## CONCLUSIONS

We have shown that neither a job's occupational standing nor its pay provides a satisfactory measure of how good the average worker judges the job to be. A combination of monetary and nonmonetary job characteristics, weighted by their average subjective importance to a representative sample of workers, can provide a much better measure of a job's overall desirability and, hence, a better measure of labor-market success. Our proposed index of job desirability is such a measure. It correlates 0.72 with a job's annual pay, 0.53 with its Siegel score, and 0.54 with its Duncan score.

Our index has four advantages over previous measures of jobs' overall desirability:

1. It can account for differences in the way workers in different occupations rate their jobs. Neither earnings, Duncan scores, nor Siegel scores can do this nearly as well.
2. It can account for the effects of race, sex, education, and labor-market experience on workers' ratings of their jobs. Again, neither earnings, Duncan scores, nor Siegel scores do as well.
3. Unlike the Duncan and Siegel scales, the IJD picks up the effects of both sex and experience on labor-market success.
4. Unlike earnings, the IJD picks up the effects of parental education and father's occupation on labor-market success.

Decomposing the index of job desirability into its monetary and nonmonetary components can also help us understand the workings of the labor market. Two conclusions are noteworthy:

1. Jobs' overall desirability varies far more than their pay. Measures of monetary inequality therefore underestimate the overall degree of inequality in labor-market outcomes.
2. While all personal advantages confer nonmonetary as well as monetary benefits, this seems to be particularly true for family background and schooling.

In assessing the index, readers should ask two questions. First, Is our general approach to measuring labor-market success reasonable? If not, what would be better? Second, Is our selection of job characteristics for inclusion in our index reasonable? If not, what are the crucial omissions? If the method and measures are reasonable, the principal remaining problem is to reduce the effects of sampling error on our proposed index. That means replicating our work, either on a single large sample or on a number of small ones.

# APPENDIX

## TABLE A1

CORRELATIONS AMONG JOB CHARACTERISTICS AND PERSONAL CHARACTERISTICS: SJC RESPONDENTS WITH COMPLETE DATA ($N$ = 621)

| | $W$ | $M$ | $S$ | $X$ | $C1$ | $C2$ | $C3$ | $C4$ | $C5$ | $C6$ | $C7$ | $C8$ | $C9$ | $C10$ | $C11$ | $C12$ | $C13$ | $C14$ | $C15$ | $C16$ | $Y1$ | $Y2$ | $Y3$ | $Y4$ |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| White ($W$) | 1.000 | | | | | | | | | | | | | | | | | | | | | | | |
| Male ($M$) | .065 | 1.000 | | | | | | | | | | | | | | | | | | | | | | |
| Education ($S$) | .160 | .071 | 1.000 | | | | | | | | | | | | | | | | | | | | | |
| Experience ($X$) | .058 | .259 | −.176 | 1.000 | | | | | | | | | | | | | | | | | | | | |
| Frequent supervision ($C1$) | −.046 | −.018 | −.119 | −.088 | 1.000 | | | | | | | | | | | | | | | | | | | |
| Vacation weeks ($C2$) | .013 | −.019 | .077 | .109 | .149 | 1.000 | | | | | | | | | | | | | | | | | | |
| Decides own hours ($C3$) | .162 | 220 | .299 | .096 | −.255 | −.170 | 1.000 | | | | | | | | | | | | | | | | | |
| Gets dirty at work ($C4$) | .031 | .137 | −.309 | .022 | 121 | −.127 | − 117 | 1.000 | | | | | | | | | | | | | | | | |
| On-the-job training ($C5$) | .108 | 175 | .214 | .040 | −.023 | .031 | .168 | − 059 | 1.000 | | | | | | | | | | | | | | | |
| Union contract ($C6$) | − 072 | .066 | .159 | .076 | 167 | .198 | − 290 | .156 | − 147 | 1.000 | | | | | | | | | | | | | | |
| Boss has a boss ($C7$) | −.073 | −.138 | −.032 | −.062 | .343 | .362 | −.319 | −.100 | −.032 | 304 | 1 000 | | | | | | | | | | | | | |
| State/local employee ($C8$) | 010 | −.022 | .162 | 009 | − 002 | 206 | − 100 | −.053 | − 019 | .120 | .104 | 1 000 | | | | | | | | | | | | |
| Federal employee ($C9$) | −.043 | .054 | .001 | .017 | .020 | .184 | −.107 | −.050 | .023 | .169 | .150 | − 082 | 1 000 | | | | | | | | | | | |
| Proportion repetitive ($C10$) | −.179 | − 201 | −.412 | −.017 | .132 | − 049 | −.264 | .148 | −.261 | 125 | .091 | −.107 | −.030 | 1 000 | | | | | | | | | | |
| Hours GT 35($C_{11}$) | .082 | .304 | .127 | − 111 | − 137 | −.044 | 240 | 093 | 096 | −.175 | −.279 | .041 | − 094 | −.176 | 1.000 | | | | | | | | | |
| Risk of job loss ($C12$) | −.064 | −.061 | −.089 | −.090 | .120 | −.043 | −.111 | .077 | −.113 | 071 | 185 | −.051 | −.007 | 171 | −.092 | 1 000 | | | | | | | | |
| Educational requirements ($C13$) | .148 | .013 | .707 | −.049 | −.100 | .125 | 283 | −.381 | 183 | −.148 | .010 | 160 | .006 | −.378 | .115 | − 108 | 1 000 | | | | | | | |
| lnEarnings ($C14$) | .137 | .508 | .341 | 298 | −.140 | .057 | .342 | − 058 | 228 | 025 | −.138 | − 022 | −.005 | − 304 | .349 | − 142 | 350 | 1 000 | | | | | | |
| lnEarnings$^{S2}$ ($C15$) | − 015 | −.032 | .078 | − 008 | −.129 | −.193 | .157 | −.039 | −.048 | − 112 | −.164 | −.093 | − 056 | .037 | −.006 | 076 | 058 | .038 | 1 000 | | | | | |
| Duncan score ($C16$) | .156 | − 028 | .539 | 025 | −.159 | .140 | .294 | −.410 | .210 | −.209 | −.001 | 100 | 097 | − 361 | − 062 | −.152 | 588 | .321 | 029 | 1.000 | | | | |
| Money index ($Y1$) | .122 | .462 | 344 | 275 | −.174 | −.012 | .370 | − 067 | .196 | −.014 | −.183 | − 051 | −.023 | −.270 | 322 | − 107 | .345 | .943 | .361 | 309 | 1 000 | | | |
| Nonmoney index ($Y2$) | .168 | .237 | .544 | .122 | −.392 | 216 | .508 | − 441 | 456 | −.102 | −.248 | −.023 | 210 | −.562 | 414 | − 351 | 642 | 501 | 022 | 544 | 473 | 1 000 | | |
| IJD ($Y3$) | .175 | .348 | .548 | .193 | − 368 | .165 | .529 | − 370 | 427 | −.085 | −.259 | − 036 | 157 | − 536 | .438 | −.313 | 625 | .725 | 147 | .535 | 723 | .951 | 1 000 | |
| lnRating ($Y4$) | .106 | 196 | .382 | 132 | −.241 | 106 | .347 | − 240 | .279 | −.056 | −.171 | −.024 | 102 | −.351 | .292 | − 205 | .408 | .475 | .096 | .363 | .474 | .622 | .655 | 1.000 |

NOTE.—$N$ = 621.

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